SCIENCE AND KNOWLEDGE: A POST-MODERN APPROACH TO EMPIRICISM

by

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Chapter 1: Introduction

Science today makes up a large part of what we know of the world. From the tiniest little particle to the shifting of the mountains, the journey of earth through space, and the inner workings of the human mind, science is making progress in understanding how the world works. Or is it? The debate on scientific realism is not a new topic, but, largely due to the pragmatic success of science, the debate has largely gone ignored in the public sphere. For the average person with limited time and practical concerns, why bother questioning science if it works? It gives us better healthcare, better environmental policies, better entertainment, better buildings, better methods of transportation, etc., and so it must also give us, for at least those interested enough to care, a better understanding of what the world really is like. This naïve realism, the belief that even the accepted theories of science provide truth *simpliciter* about the world, has largely been rejected by philosophers of both realist and anti-realist leanings. This paper will argue that realism is not really still philosophically defensible. Because we are only able to guess at the approximate truth of theories and we struggle with the empirical equivalence of theories, we will see that science is capable of producing knowledge, but not knowledge that is universal.

The primary claim of science that makes realism so difficult to maintain is its claim to know unobservables. And so, it is important to remember that when I refer to 'science' in this paper I am referring primarily to commonly accepted and peer reviewed science, which claims some sort of intersubjective and testable hypothesis about unobservables. Even if one is able somehow to defeat or ignore the extremely skeptical claims of Cartesian skepticism (Descartes, 1641, 1996) which denies knowledge from sense experience, and accepts that we can know things about the world through our direct experience of it, and thereby accept science's claim to

knowledge about the observable world, there is a whole new level of skeptical arguments when one begins to claim to know things about things that cannot be sensed. How is it that we can claim to know something that we cannot even see? If the modern phrase "Seeing is believing" is true, and knowledge includes proper belief then unless we can observe something in some way, then we cannot ever have proper belief, and without proper belief knowledge is impossible. Thus it is problematic that science would claim knowledge of unobservables. Though a more in-depth discussion of what it means for something to be observable will follow later in this thesis, right now I believe it is prudent to state it simply. Something is observable if we can sense it, that is, observation is not limited to only using our eyes but also our unaided senses of touch, taste, smell, etc. Further, it is important to note that science often takes sensible effects which are a part of human experience, for example, electrical current, and attempts to explain it by reference to unobservables; what is at issue in this thesis is not our experience of being electrocuted but the explanation of how electrocution occurs.

Stances

Because my thesis is going to be an investigation into the philosophy of science, and specifically a comparison between the stances of realism and constructive empiricism, I must first begin by making the case for thinking about metaphysics in terms that I will be using. This section will begin by demonstrating some of the flaws in traditional metaphysics as represented by Michael Jubien (1997). After I find that the traditional view of metaphysics falls to the same complaints that Popper's Falsificationism does, I move to outline and defend Bas Van Fraassen's stance-based metaphysics.

Metaphysics has long been disparaged in the philosophy of science because it was nearly synonymous with un-observable; however, with the failure of positivists like Carnap, Hempel,

Schlick, and other members of the Vienna Circle to remove reference to the unobservable from science, it is becoming more and more recognized as being a necessary evil. Michael Jubien argues that metaphysics is needed to defend a realist interpretation of science because realism has three foundational assumptions: "(1) that there exists a physical reality independent of our mental states; (2) that the interactions of the stuff constituting this reality conform to certain general laws; and (3) that we are capable of grasping physical laws and obtaining evidence that favors or disfavors specific proposed laws" (Jubien, 1997, 1). There is no imaginable observable evidence that could prove or disprove any of these three assumptions and so Jubien concludes that in order to defend this position one needs to resort to metaphysics. I believe that acknowledging the necessity of metaphysics as being necessary for the philosophical foundation of science is becoming the default and accepted position in modern philosophy of science.

However, some disagreement has occurred over how it is that metaphysics proceeds. Jubien proceeds to argue that the primary tool of the metaphysician is the thought experiment, where careful thought supplemented by the use of logic and unbiased openness to the truth are used to provide counter-examples and search out inconsistencies in positions, thereby ruling them out as possible candidates for truth. In this way, metaphysics progresses much like how Karl Popper (1968) argued that science progresses; through a process of falsification so that the theories that survive are probably more verisimilar than those that are falsified. For Popper, the primary problem of science was that of induction, i.e., how is it that we can ever be sure of the truth of our scientific theories, which are typically universal statements, if we have not looked at every possible case where they apply? Popper's solution was to concede the problem and admit that we can never know the truth of a theory, but that knowledge of the falsity of a theory is possible, because if we can find a single case that contradicts a universal theory then it is obvious

that what the theory says should be true all the time about the universe is not the case and so it is false. There are many parallels between Popper's Falsificationism and Jubien's take on metaphysical progress and so they will probably share the same faults as well.

The first objection is Nicolas Maxwell's (1972) observation that any attempt at falsification must occur between two theories, i.e., in practice it is not just with single observed instances that scientists falsify theories but with whole new universal theories of the way things are. Thus, Maxwell argues, that we must first assume the truth of one universal theory in order to falsify another. The problem is that we then have two universal theories that merely contradict one another, each with a substantial amount of empirical evidence (which must be true, or else neither theory would have stood any chance at acceptance), so no way exists to get beyond the problem of induction for either one, and thus no way to determine which one falsifies the other. This problem is pertinent to metaphysics as well. Jubien uses an example from ethics to demonstrate a clear cut case of metaphysical progress, in which he argues against a proposed ethical principle by use of a thought experiment that brings into view a theoretical situation which demonstrates another ethical principle and concludes that the first principle is false.

> "Suppose someone claimed that people are morally responsible for causing injuries only if they cause them intentionally… a little reflection reveals that [this claim] isn't really all that plausible. Let's assume for the sake of this reflection that people really are morally responsible for some of the things that they do, as we normally take them to be. Now, given this assumption, it is certainly true that people are often morally responsible for injuries that they intentionally cause. And it is also true that people are sometimes not morally responsible for injuries that they cause unintentionally. But it is not hard to think of examples where we find moral responsibility without intent to injure. Cases of gross negligence are probably the clearest examples. Say a pharmacist carelessly fills your prescription with the wrong pills and you take them and die as a result. Here there is no intent to injure but the pharmacist is morally responsible. Examples of this general sort are easy to multiply." (Jubien, 1997; p. 8).

He has provided no evidence for thinking that the 2nd principle is true aside from falsifying the first one by use of the new, unproven principle. His appeal is to the same intuitive knowing that was the original defence of the first principle. If this is a case of clear cut metaphysical progress, as Jubien claims that it is (1997; p. 8), then the view is more problematic than he would want his readers to think.

The second problem commonly associated with Popper's Falsificationism was discovered by David Miller (1974) and further strengthened by Joseph Agassi (1975). For Popper, scientific progress was defined by an increasing degree of verisimilitude; this complex concept basically is the idea that if a theory's truth content is greater than another, while also having lower falsity content, then it is considered to be closer than the other to the truth. Thus, progress is made by removing error and adding more truth. Miller's insight was that it is logically impossible for one theory to be closer to the truth than another unless the theory with greater truth is not simply more verisimilar, but actually true.

In the case where A and B are both false but the truth content of B is ≥ the truth content of A, then if B is false or A is true, and the truth content of B entails the truth content of A, then B entails A and if B entails A and B is false then the falsity content of A is less than the falsity content of B. This is because if the falsity content of A were equal to the falsity content of B, then if A were false then B and A would be inter-derivable. Thus if we have two theories, A and B, which are both false, and B has at least as much if not more truth content than the other, they are incomparable because B exceeds A in both truth and falsity content (Miller 1974, 172).

Thus, progress is impossible for Popper unless the new theory is completely true, which by the problem of induction cannot be assumed to be the case. This same problem must be overcome by Jubien's metaphysical method. If he is to defend metaphysical progress then I

believe it is safe to assume, based on his emphasis on the refutation of false metaphysical theories and his search for the 'solution' to difficult metaphysical problems, that he is thinking in similar terms as Popper's verisimilitude and that metaphysical progress occurs when error is removed and truth is gained. Thus, unless metaphysicians are in the habit of jumping from false theories to irrefutably true ones while experiencing metaphysical progress, which I do not believe to be the case, then the idea of metaphysical progress is impossible, since comparing the verisimilitude of two false theories with differing truth contents seems to be impossible.

With these considerations in mind I am inclined to agree with Bas Van Fraassen (2002) who concludes that because metaphysics is not scientific and since there is no way to determine the truth values of metaphysical questions, that we ought to reject metaphysics as an epistemological enterprise altogether. For Van Fraassen, contemporary metaphysics has become nothing more than idle word play which, while meaningful, is ultimately irrelevant, passing, and insignificant. Van Fraassen, in light of the failure to find any rationally binding and conclusive arguments in metaphysics concludes that according to traditional epistemology any attempt to adopt and defend a metaphysical position is irrational. However, as a proponent of a new kind of empiricism Van Fraassen needs some metaphysical foundation for his project. He attempts to solve this problem by introducing the idea of 'metaphysical stances' combined with a voluntaristic epistemology. In this view, acceptance of metaphysical positions is a discussion of values and emotion. He writes that metaphysics has "now become a question of how to view the role of emotion in our epistemic and valuational stance and the epistemic enterprise as a whole" (2002, 108). Stance-metaphysics then, is the view that metaphysics is a discussion about values and emotions while voluntaristic epistemology is an attempt to redefine what it means for a stance to be rational. What do the limits of rationality become? According to Van Fraassen,

consistency is the only way to determine if one can rationally hold a position or not, i.e., if a position contains some inherent logical contradiction then it would be irrational to hold it, and if it does not then there is no reason, aside from those personal valuations we make which might rule out a position as rational to hold.

Darrell Rowbottom (2011) compares and contrasts the concept of van Fraassen's stances with that of Kuhn's (1962) paradigms. The primary difference is that because paradigms have exemplars and stances do not, stances are personal while paradigms are communual. Exemplars are the shared techniques, values and beliefs which partially determine what new puzzles will be investigated (that is, what counts as a 'proper' puzzle). They determine what approaches are available in searching for solutions to those puzzles and "it is the standard by which the quality of a proposed puzzle-solution can be measured" (Rowbottom 2011, 115). Stances do not have any penultimate form with which we can measure one's performance to a stance's standards precisely because they are personal.

While Daniel Ratcliffe (2011) believes that an open mind about stances combined with a greater appreciation of the role of feelings in adopting implicit stances will help to progress critical philosophical dialogue, Anjan Chakravartty (2011), has some reservations against this voluntarist stance-based metaphysics. Chakravarrty points out that in order to adopt a voluntarist stance we need to make the assumption that "there are in fact domains of inquiry in which more than one stance is rationally permissible" (39). He then proceeds to argue against voluntarism on the grounds that if it is true then it leads to relativism, and if that relativism is true then there can be no explanation as to why someone would adopt any particular philosophical stance (2011).

To Chakravartty's first point I can provide two lines of response. The first being that it does in fact appear that there is at least one domain of inquiry such that more than one stance

looks to be rationally consistent. The philosophy of religion, and more specifically, the debate on the existence of some all-powerful, all-loving, and all-knowing creator-like being is one with no clear drag-em-out, knock-em-dead argument either way. Michael Bergmann's *Readings in Philosophy of Religion* (2007) is a fine survey of the prominent arguments by respected philosophers in the field, who generally conclude that there is no such thing as a definitive argument and one can be equally rational either in accepting the conclusions in favour or against theism or in rejecting them. Consider William Rowe's (2001) position that he calls "Friendly Atheism" which holds that even once both parties are exposed to the same evidences for or against a position, one party may find them "more telling" than the other does (62).

But one might respond that the positions only seem rationally consistent for now, and upon further study and research in the area then we will one day be able to confidently exclude all positions but one. When that day comes, it would certainly rule out a voluntarist, stance-based view for that field; however there is no reason to believe that that day will come any time soon for most fields of metaphysics, philosophy of mind and the philosophy of religion, just to name a couple that are commonly known to be rife with disagreement. Even in fields where disagreement is rare, e.g., over such questions like the existence of an external world, it is often recognized that the only way to avoid the skeptic's arguments is to simply assume that they have some wrong premise but not be certain of which one is wrong in the spirit of G.E. Moore's 'proof' for the existence of an external world to be found in his hands. In cases like these, where there is a definite solution but it is currently beyond our reach, a stance based solution is, if nothing else, a prudent and expedient solution in the mid-term.

The relativism that Chakravartty is worried about has also been written about by Dien Ho (2007), who wrote that while van Fraassen's portrayal of philosophical positions of stances

appears correct it leads to a dilemma regarding philosophy as a whole. The dilemma is that either we blur the lines between philosophy and sophistry and lose any possible distinction that philosophy has gained in order to be able to arbitrate between the value based stances, or "we embrace a kind of radical relativism" and accept that stances are merely "lifestyle choices", which means that philosophy is still concerned with the way the world really is but also implies that "at some point, philosophy will be silent in the face of incompatible hypotheses as to which one is correct" (Ho 2007, 332).

This is a valid concern. However, we must be careful not to mischaracterize the view. There are, I think two strains of relativism which they could be talking about. The first is strong relativism, which would state that any view which is acceptable is also true, or correct; the second sense of relativism could be weak relativism, which would be more along the lines of admitting that we have no way of distinguishing between true and false theories, thereby losing our concern with correspondent truth and instead focusing on investigating different ways in which humanity has dealt with the experience of reality in the past and present so that we have more ideas as to how to handle new experiences in the future. It is obvious then, that the strong sense of relativism would result if it were a part of voluntarism to equate 'rationally acceptable' with 'true'; however, since it does not do this, and Ho's concern seems to be more nuanced than that, the concern must be with a slide into weak relativism. Ho writes that if voluntarism is correct then "at some point, philosophy will be silent in the face of incompatible hypotheses as to which one is correct" (Ho 2007, 332). If philosophy is silent then it is neither judging as true or false.

So, how should the stance metaphysicist reply to claims of weak relativism? There are two possible solutions. One is to simply bite the bullet and acknowledge that we are limited in

our abilities and ought to consider ourselves lucky that we can even do metaphysics to the extent that we do, in formulating positions and checking them for consistency. The second solution is to take a more optimistic view and say that, though we are currently trapped in a form of weak relativism, it is only temporary, as continued effort and work, combined with careful open-minded thought, will eventually and slowly weed out all of the positions that are both internally inconsistent and unable to mesh with our experience of the external world.

Along these lines Teller (2011) provides a defense of voluntarism. He begins first by briefly going over van Fraassen's argument in favor of voluntarism. This is the application-regress argument which focuses on the impossibility of setting a basic foundation for knowledge because that foundation will itself need a foundation which will then need another foundation and so on. Teller concludes that we are not currently justified in believing that there is some greatest system of beliefs and rules; voluntarism would only need some self-evident justification if we are to first beg the question and assume foundationalism, i.e., any argument against the potential relativism of voluntarism must first assume some form of foundationalism or else it will slip into radical skepticism and self-defeat. At the heart of objections against voluntarism, Teller speculates, might be the fear that if true then we have no explanation for the reliability of our methods, rules, and basic beliefs. Against this he argues that one could easily turn to evolutionary explanations, or at least, that this question is a scientific one and not a philosophical one. Van Fraassen has a different solution, and I will get to that in the third chapter.

Chakravartty's second fear is that on voluntarism we would not be able to explain why someone would adopt any particular philosophical stance. This misrepresents voluntarism. Voluntarism is not the view that any view is permissible for any person, i.e., if one holds values that are contrary to an otherwise logically consistent position then it would be logically

inconsistent, and thus rationally impermissible, for that person to hold that position. To imply that there is no reason why one would hold a metaphysical position is to think too narrowly about what could possibly be a reason. In this case the philosopher's personal values and feelings towards the topic are considered to be reasons for holding or rejecting a position. The critic might reply that it may possibly be the case that a philosopher might hold to a value which is logically consistent with two contradictory metaphysical positions and yet still choose between them. In this case, the voluntarist must reply that there is yet some second value at play which has influenced the philosopher's decision making. One side effect of this, which many philosophers will not like, is that it becomes nearly impossible to convince someone of a position that is contrary to their values without first convincing them to accept the prerequisite values. This will lead to an increased emphasis on the appeal to emotion rather than to logic or reason in order to convince your opponents. However, as distasteful as this is to philosophy, this seems to be already the case, to great success, in the pragmatic fields of business, marketing, politics and countless other fields in which the primary goal is to persuade people. Perhaps this is a sorry admission for philosophy to make but if it is in fact the way things are, that is, if everyone's (philosophers included) decisions are primarily decided by their values then even the decision to try and be logical is a decision which is primarily value driven and perhaps it is an admission that philosophy ought to make in order to progress and gain greater influence in the world.

Some closing comments on metaphysical stances before moving on to the main body of my paper. As said before, voluntarism is not relativism, it is merely the acceptance of our limitations as they stand. It is not a call to stop doing metaphysics. Metaphysics can still advance through a process similar to Popper's Falsification combined with creative individuals developing new ideas and synthesizing old ones in new ways. But perhaps "advance" is the

wrong word to use as that implies some sort of progress towards something, specifically towards truth, which I've just argued is impossible. Van Fraassen wrote that metaphysics is the way that, each century, humanity re-evaluates itself, determining what it means to be human and what it hopes to become. Metaphysics is like art, necessary because it directly addresses issues of import to everyday life in a unique way which will resonate with some more than others, and provides more options for understanding the existence that we experience. Metaphysical progress then is an increase in understanding of reality and how others have chosen to interpret that experience.

Finally, I would like to point out that this whole discussion on stances has itself been a metaphysical discussion, or perhaps meta-metaphysical, and so must abide by the same rules that I have argued for. Thus, stance-based metaphysics is simply a stance that one can take towards metaphysics that is consistent and may be wrong (in fact, probably is wrong). However, I adopt it because I believe that it is motivated by the values of open-minded truth seeking, humility, co-operation, and tolerance. To work within a stance based framework one must be open to the likelihood that you are wrong in some way and that the best way to fix that is to work with those who hold opposing views in order to gain new perspectives on old problems. These two things can only be done if one is humble about their abilities, i.e., they do not assume that truth is something easily gained, and are tolerant of those who they disagree with.

For most of the rest of this paper I will consider the conversation on stances closed and will simply work within the framework provided by stance based metaphysics. There will be a section at the end of this thesis where we will return to the topic but for the majority of the work we will simply act as if stance-based metaphysics is the case.

Chapter 2: The Realist Stance

The debate over scientific realism has always had two primary contenders, each of which has enjoyed popularity at different times: empiricism and realism[1]. Empiricism was popular in the early 1900s and the heyday of positivism. But with the fall of positivism empiricism went into decline as well and during the mid-to-late 1900s realism took the spotlight, championed by philosophers like Karl Popper, Anjan Chakravartty, Richard Boyd, and Ikka Niinuluoto. Bolstered by the fact that scientific realism seems to be the default position of most practicing scientists, scientific realism has gained momentum in recent years and has had a strong influence on how science is understood generally.

Stathis Psillos (2002) defines scientific realism as the thesis that there is an unobservable mind-independent world that science "tries to map" and we can succeed in mapping it. He argues that we need not compromise on either of these two claims, because being unknowable does not follow from mind-independence. Further, he believes that the scientific realist ought to hold to the thick version of the presumptuous claim and believe that the ampliative-abductive methods of science can provide justification for believing the mapping of the unobservable to be true or at least approximately so. There are four separate claims here, and these I will take to be the basic tenets of scientific realism:

1) There is an unobservable mind-independent world
2) Science tries to map this unobservable mind-independent world.
3) Science at least approximately succeeds in mapping it.
4) The ampliative-abductive methods of science can provide justification for believing in at least the approximate truth of these maps.

[1] There have been other strains of anti-realism which differ from empiricism; however, due to the limitations of this and my interest in the work of Bas van Fraassen in reviving empiricism I have chosen to restrict the anti-realisms discussed to that of empiricism.

Of these tenets I will consider 1) to be uncontroversial. That is not to say that there are some who might disagree, but that their number is so few and their skepticism so great that it would be questionable to spend time arguing in favor of it. I am comfortable simply assuming the truth of 1), but 2), 3), and 4), however, are more open to criticism.

I separated 2) from 1) not because it's a controversial point over which to argue but because of how weak and ambiguous it is. To begin with, what is 2) referring to with 'science'? If it is talking about actual scientific practice then judging by the divide in the philosophy of science between empiricists and realists there probably are some scientists who are actively trying to map the unobservable world, and there likely are scientists who are doing science and not trying to map the unobservable world. If it is saying that scientists *ought* to try and map out the unobservable world because that is the true aim of science then it is stronger, yet still not relevant because an empiricist could accept that the aim of science is to map the unobservable and never succeed. Just like an athlete who aims for the perfect performance without ever actually achieving it. The real meat of scientific realism comes in 3) and 4).

Claim 3) is that science succeeds in approximately mapping out the unobservable world. This is where scientific realism gets the 'realism' part. According to 3) scientific knowledge is true, objective knowledge of the real world. The unobservable entities posited in theories as explanations of observable phenomena are thought to really exist and are not treated as useful fictions, i.e., scientific theory is at least approximately true, whatever that means.

Claim 4) states that the ampliative-abductive methods of science can provide justification for believing in at least the approximate truth of these maps. This means then that not only do the theories correctly latch onto reality somehow, but the methods that we use for selecting ones for acceptance also allow us to justify our belief that these theories are in fact true. The ampliative-

abductive method is that of adding on to what we currently accept by selecting the theories which we think best explain newly discovered phenomena.

Due to the problem of induction which was Karl Popper's biggest concern and its illustration in the history of science by philosophers like Thomas Kuhn (1962) and Larry Laudan (1981), it is commonly accepted in the philosophy of science that naïve realism, the view that science can provide us with truth *simpliciter* regarding the facts of the unobservable world, ought to be rejected. This has left realist philosophers scrambling to find a weaker version of scientific realism which is defensible. What is common amongst most of these accounts is something called 'approximate truth.' What is not common amongst these accounts is what exactly the term 'approximate truth' refers to. At first sight it seems like one is performing some sort of mental gymnastics in order to think of something that is approximately true. This is because truth is typically thought of as a binary value, either yes or no. Much like being pregnant, one is either pregnant or not pregnant. It would be absurd to think of someone being approximately or partly pregnant. What characteristic does truth have that makes it substantially different from other typical binary values in this way?

It turns out that for the major leading theories of approximate truth, it is not significantly different from other binary values. It is usually thought that approximate truth refers to theories as a whole and not individual facts. This means that a theory is thought to be approximately truth when it says some true things about the world despite also saying some false things about the world. There are many ways to think about approximate truth and why we should think that we have it.

Ilkka Niiniluoto (1999) argues in favor of a system called Truthlikeness. Truthlikeness is a measure of a theory's nearness to truth relative to some target. Niiniluoto argues that his notion

of truthlikeness allows philosophers to speak intelligibly about how a theory could be more or less true. Truthlikeness is measured by calculating the distance between statements and reality, this calculation is then used as a means for making sense of how some statements can be more truthlike than others. Judgements about truthlikeness are exercises in a fallibilist epistemology because it is a comparison of the distances from reality of competing theories when reality is currently unknown. Essentially this means that the more correct statements a theory makes about the world then the more truthlike (or approximately true) a theory is. This theory, if true, would solve the logical problem of approximate truth by defining it as a logically consistent concept. However, it fails to solve the epistemological problem because of its fallibilist nature and the reliance upon knowledge of reality for an accurate measurement; the measure of truthlikeness for any particular theory is beyond our grasp. Furthermore, this theory fails when one considers that a theory that makes a significantly large amount of both correct and false statements would be considered more truthlike than a theory that made few correct but even less false statements.

Thomas Weston (1992) nuances Niiniluoto's account by creating a distinction between comprehensiveness and accuracy. Weston believes that approximate truth is about accuracy of statement, not quantity of content, i.e., a theory can be approximately true even if it is not comprehensive as long as what it *does* say is approximately accurate. In taking this stance he distinguishes himself from theories about approximate truth which rely on truth, probability or comprehensiveness. This of course leads to the problem that any theory which does not include a quantity claim cannot be approximately true, but it is useful because it points to another distinction which approximate theories need to account for, that being the quantity vs quality for content.

Macintosh (1994) argued in favor of something called Convergent Realism, which is the belief that as empirically adequate theories converge in their statements about unobservables then it can be assumed that even if we do not know which one is true we can assume that they all in some way resemble the true one and so are all approximately true. Theories that are not approximately true are those that are not empirically adequate or do not resemble other theories in their statements about unobservables. Thus, approximate truth becomes that of similarity to and coherence with currently accepted theories and is both a logical and epistemological possibility. However, how do we justify our use of accepted theories as the standard? Presumably these other theories are only approximately true as well. The key is in convergence. As all theories converge then we have evidence that the similar parts are more likely to be true. But this still just leaves our measurements of approximate truth reliant upon the theoretical world rather than the actual world. Coherence with other theories becomes truth rather than correspondence with reality, and we have no real reason to think that other theories represent the real world other than that they cohere with other theories. The regression in justification makes it difficult to see how we could really ever be justified in believing in the truth of the set of currently accepted theories even if they are converging on one another.

Liu (1999) presents two reasons which make Niinuluoto's descriptive theory and Weston's close possible world theory difficult to accept as possible understandings of approximate truth. He argues that there are two other factors one must consider when measuring approximation: what the laws are and how they are instantiated in the actual world. He concludes, that idealizations give good grounds for producing laws but that law-likeness does not indicate truthlikeness. Thus, for Liu, science is an attempt to "carve the world up at the joints," that is, it attempts to carve the world into different causal structures separate from outside

influences. For Liu, science is an abstract study full of idealization, which prevents it from latching onto the real world. That is, "many, if not all, of our scientific theories - whose essential ingredients are laws of nature - are idealized in the sense that they are only true under conditions which do not obtain in the actual world" (Liu 1999, 229-230). Liu believes that lawlike behavior is not something that actually exists in the real world, or at least if it does we have yet to observe it, and that in order to create a scenario where law-likeness is present and recordable we need to produce such an idealized state that is so isolated from outside influences we can no longer be justified in saying that our theories are about reality.

Perhaps it is considerations like these which led Roger Rosenkrantz (1975) to relativize approximate truth to the specific set of experiments that each theory applies to. That is, the definition of approximate truth is going to be different for each theory and experiment it is applied to. He denies that it is necessary for a grand theory of everything for scientific realism to stand. He argues that doing so allows us to invest in theories arbitrarily over others which may be truthlike but are less salient and interesting on the journey towards verisimilitude. For Rosenkrantz, science isn't the gradual accumulation of facts but a disjointed journey from salient fact to salient fact, and this disjointedness implies that truth will be approached differently. Thomas Weston (1987) agrees that the sense of approximation needed when thinking about approximate truth will be relative to the subject matter of the theories at hand.

Peter Smith (1998) as well provides two valuable insights into approximate truth, the first being that if approximate truth is going to work then it makes sense that due to the differing nature of things being approximated in different theories that there must be different ways of defining approximate truth for each different type of theory. The second valuable insight is his way of defining approximate truth for dynamical theories; that is to break theories into two parts,

their idealized geometrical/mathematical frameworks and the claim that they are isomorphic to the world. The idealized part is just assumed to be true while the claim about isomorphism is said to "closely follow reality." Thus, a theory is approximately true if and only if its models are close to the way that the world is. This is to be contrasted with Lewisian-possible world theories of approximate truth and Niinliuoto's theory that "most bullseyes" win.

Anjan Chakravarrty (2010) as well writes about the difference between Abstraction and Idealization. He believes that these two categories each require their own definition of approximate truth and admits that often scientific theories are some mixture of abstraction and idealization. Abstraction, due to its willful ignorance and removal of some factors, is to be seen as true *simpliciter,* though not in line with real world observations, while, on the other hand, idealization is simply false due to the fact that it idealizes some factors into obviously false values. Abstractions then are to be said closer to the truth as the comprehensiveness goes up, that is, as the number of factors that is included goes up the closer to the truth an abstraction will be. Idealizations are closer to the truth as the number of idealized values are reduced. Thus, approximate truth is going to be relative to the theory and the mix of idealization and abstraction is used.

One significant problem with what I will call the "there is no one way that science is" theories of approximate truth that I have just discussed is the relativity of approximate truth and lack of hard and fast rules in a field supposedly defined by its strict method. Undoubtedly it would appear that one would speak of approximate truth differently based on situation, for example some theories will deal primarily with quantities while others will speak of probabilities, some about living organisms and some about inert gases or explosive chemicals. The vastness of the world and the different approaches needed to properly experience and

understand it gives credence to the idea that science is not a catch-all term and that each individual field ought to be taken on its own and treated for what it is rather than as a part of some grand scheme. However, the problem is that no matter how few idealized values in idealizations or how many relevant factors are included in an abstractions, neither of these two styles of approximate truth are about the real world.

That being said, it would appear that, at this point scientific realism is a valid stance to take towards science, i.e., it is not unreasonable to believe that some form of approximate truth is both logically possible and epistemically achievable. In fact, there have been arguments to the effect that it is the only valid stance to take towards science. What has been dubbed the 'No miracles argument' was originally formulated by Hilary Putnam when he said "Realism is the only philosophy that does not make the success of science a miracle" (1975: 73). The argument goes something like this:

1. There must be some reason for the success of science
2. That reason is either that the success of science is a miracle or it is because science is actually correct in its statements about the world.
3. Realism believes that science is actually correct in its statements about the world.
4. All other philosophies have no explanation for the success of science and so much rely on the miracle explanation.

   Therefore,

5. Realism is the only philosophy that does not make the success of science a miracle.

Richard Boyd (1991) expands on this argument with the notion of instrumental reliability, i.e., airplanes are more likely to fly than crash; computers are remarkably good at computing etc. He argues that no form of anti-realism can provide adequate explanations of the

instrumental reliability of science. Thus he concludes that realism in science is the only plausible explanation and must be true.

Ian Hacking (1991) also defends realism about entities in a similar manner. His argument is that if we can use one unobservable entity to manipulate a second unobservable entity then we have good reason for thinking that the first entity actually exists, and that our theories about it are at least partly correct and that there can be no other explanation.

On a stance-based metaphysics, however, this no miracles argument, even if true would fail to invalidate any view which was fine with allowing miracles; i.e., it only would disprove those metaphysical stances which deny the possibility of miracles or unexplained occurrences. Admittedly these miracle allowing stances would probably not find wide acceptance, as their pragmatic value in helping us understand how it is that we know is going to be limited. The real weakness is in the fourth premise. It has been hotly contested and denied by Bas van Fraassen and other empiricists as we will see in the next chapter.

Not everyone accepts that realism can even explain the so-called success of science. Otávio Bueno (1999) argues that the realist claim is viciously circular, i.e., the realist assumes that true theories have true consequences and then concludes on the basis of true consequences that theories themselves are true. The typical explanation given for the success of science is that science is successful in producing reliable instruments because its theories are at least somewhat true. The problem is that the term ‘success’ is ambiguous. Science is successful in producing reliable instruments, this is commonly accepted; what is not commonly accepted is that science is successful in producing true theories about the unobservable world. In regards to the second it begs the question to assume that there is even success that needs to be explained, and, as we will see in the next chapter, scientific realism is not the only philosophy that can explain the

empirical success of science. Thus, according to Bueno, the No-Miracles argument fails to defend scientific realism.

David Resnik (1991) argues along similar lines. He argues that approximate truth has been picked up and developed by scientific realists for two primary reasons: 1) to provide a surrogate for truth and 2) to explain the success of science, but, he claims, there is no theory of approximate truth that can do both. Metaphysical theories of approximate truth which state that in some way our theories are saying something about reality would provide an explanation for the success of science, however, they fail as surrogates for truth because epistemically approximate truth is just as impossible to grasp as is truth *simpliciter*. Epistemic views of approximate truth which define it in terms of successful theories or accepted opinion do provide a surrogate for truth by moving it into the cognitive realm but fail to provide an explanation of science. Epistemic views fail at 2 because it is at best a circular explanation where something defined by the success of science is used to explain the success of science.

Reference change and the Pessimistic Induction (PI)

The biggest issue that scientific realism and approximate truth must overcome is the problem of theory reference change and what is called the pessimistic induction. Thomas Kuhn's (1962) *The Structure of Scientific Revolutions* is an empirical look at the history of science. His conclusion is that the progression of science has not been a steady one towards increasing knowledge, but rather a haphazard and a-rational journey through revolution and periods of normal science. A paradigm is very similar to one of Michel Foucault's (1969) discourses which he first presented after his own independent study of the history of medical science. The paradigm determines what theories are acceptable and which are not. It is the whole worldview that is accepted and it is within the paradigm where normal science occurs. Normal

science is the process of scientific inquiry intended to push the limits of the paradigm and expand our knowledge within the paradigm. As normal science progresses, faults of the paradigm will be discovered; initially, according to Kuhn, they are swept under the rug, so to speak. The coherence of the paradigm is maintained at the cost of ignoring inconsistencies. Eventually, however, Kuhn says the inconsistencies become so many and so great that the paradigm itself comes into question, and in a moment of scientific revolution a new paradigm is proposed and eventually accepted.

This revolution, according to Kuhn, cannot be a rational decision because the arguments used to ground the new paradigm are not rationally acceptable within the old paradigm. That is, the arguments are in the acceptable language of the new paradigm and are not translatable into the old paradigm. This is the basis for the pessimistic induction and the problem of reference.

Boyd (1976) sums up the pessimistic induction with the following:

> "Well-confirmed theories often turn out to have been, in some respects, dramatically wrong, even if they played a significant role in the history of science. Realists are at pains to insist that Newtonian mechanics is "approximately correct," but it is also true that (even leaving aside the esoteric question of Absolute Space, which is probably the conflation of several different questions) Newtonian mechanics is profoundly wrong: wrong about mass constancy, wrong about simultaneity, wrong about the relation between spatial and temporal distances" (633).

The induction is as follows:

1. Well confirmed and widely accepted scientific theories have been proven wrong in the past

2. We have no evidence to think that this situation will change

   Therefore, probably,

3. Contemporary scientific theories that are both well confirmed and widely accepted will be proven wrong sometime in the future.

The problem of reference flows from the pessimistic induction. In the history of science there have been multiple theories about the same unobservable entities, for example, the electron. That is, words like 'electron' when first used referred to something entirely different from what it does today; the thing that the symbol 'electron' used to refer to never existed and it is likely that the things that 'electron' refers to today do not exist either because it only exists within the paradigm that we currently live within. For example, when 'electron' refers to an unobservable entity with the properties *a, b,* and *c,* how can it be said to refer to some entity that actually has the properties *b, d,* and *e*? Furthermore, what happens when the word itself in the growth and shift of science comes to refer to a theoretical entity with the properties *d, c,* and *f*? The issue here is that as we learn more about a thing, our definition of that thing changes and the anti-realist wants to say that because our definition of a thing changes then we are no longer talking about the same thing and so we no longer have knowledge about something that exists in the world, it only exists in the world of our theories, and since the words only refer to entities in our theories and not in the world, then there is no way to differentiate between right and wrong theories by reference to the real world.

Larry Laudan's 1981 paper, "A Confutation of Convergent Realism," was an excellent discussion of this topic. He strives to show that "epistemic realism, at least in certain of its extant forms, is neither supported by, nor has it made sense of, much of the available historical evidence" (223). Laudan argues against two lines of realist thinking, the first being that there is

connection between approximately true and successful reference, the second being that mature sciences tend to preserve older theories and that this preservation is evidence of their truthlikeness. Against the first claim, Laudan questions the link between referring and approximate truth on logical grounds. He points out that many theories which we now know to be false were in fact very successful, and that to link reference and success is question begging because it assumes that successful theories are successful because they refer and not for other more pragmatic or idealistic reasons. Laudan attempts to confute the second line of reasoning by arguing that if true it would freeze science and prevent progress by requiring that the older theories be preserved and unchangeable; confirmed theories could never be questioned or refuted. All that could be done in science is the addition of new theories that do not change the old accepted theories in any way. As well he says that there are many examples of theories which did not preserve preceding theories, e.g., phlogiston into oxygen, but were accepted because they were supported by the evidence. As such, the pessimistic induction is a direct response to the No-Miracles argument for realism, the claim being that realism cannot actually make sense out of the success of science.

Some attempts to solve the problem of reference include Putnam's causal theory of reference, and Niinuluto's adoption of truthlikeness. Putnam (1978) believed that the problem of reference could be solved by tracing the causal chain behind how one comes to know and use a word. If you could trace a route through how you learned the word back through a similar ancestry as some contemporary of yours then you would both be referring to the same thing with that word. Thus, even though our knowledge of the objects that words are supposed to refer to changes over time, according to Putnam, the man who originally coined the term 'electron', and the physicist today who uses the word to describe a charged sub-atomic particle that has all the characteristics that we believe it to have, are in fact referring to the same object, even though the modern physicist undoubtedly believes things about the electron that the early scientists could not have even dreamed and no longer believes things about the electron which used to be

common. It seems odd that a word's reference would rely upon how one learned it, rather than its definition, especially when you take the law of identity into account which states that two things are identical only if they share all of the same properties. Since the modern use of the word 'electron' refers to a different set of properties than the original use, it is difficult to see how they could be referring to the same thing.

Niiniluoto's (1978) solution to the problem of the change of reference for terms in scientific theories is to adopt the notion of truthlikeness. The problem is undergirded by the idea that "a term *t* occurring in theory *T* refers to object *a* iff *a* statisfies the claims of *T* containing *t*" (547). This causes difficulties when two competing theories make contradictory claims about what *t* is, since it is impossible for a single object (or type of object) to satisfy both sets of conditions. Niiniluoto's solution works because he refines the above principle to be more generous and hold that the definition of *t* in a theory need only be truthlike rather than strictly true. One could call this 'approximate reference' where words only approximately refer to an entity, and as long as both modern and classic uses of the word approximately refer to the same thing then reference has not changed. Along with the other problems that come along with approximate truth, this view also adds the problem that it is conceivable to think of two sets of definitions of the same word that shared nothing in common, and possibly even contradict each other, but were both considered to approximately refer during their respective times of acceptance. Under Niinuluoto's theory of approximate reference these two completely different and possibly contradicting sets of properties would both be said to referring to the same thing.

The problem of reference is very closely linked to the problem of identity change over time. How many changes to the definition of a thing can a thing sustain before it becomes a new thing? Much like the Ship of Theseus, entities that endure in modern science are constantly having their parts replaced or thrown out. The whole purpose of the Ship of Theseus is to

demonstrate the paradox involved in the change of parts while retaining identity; it seems that the realists want to simply deny this paradox rather than solve it and maintain that somehow identity is not linked to the parts or definition.

Of course, a significant part of the problem of reference is the pessimistic induction which concludes, based on past experience, that we will always find some reason to declare our currently accepted theories false and replace them with something new. Philosophers of science from both sides of the scientific realism debate have responded to the pessimistic induction claiming that it is insufficient to the task for one reason or another.

The first argument for the pessimistic induction is the problem of reference itself; without the pessimistic induction then the problem of reference is no longer an issue because it is only when a theory that is said to refer is changed in some fundamental way or is outright rejected that terms change or lose reference. Since the pessimistic induction is nothing more than the claim that we will never hold a theory about the unobservable that will not eventually be rejected or changed in some fundamental way, the fact that the problem of reference is actually a problem it stands as evidence in favor, or at least continued confirmation, of the pessimistic induction. If we had reached a point in the scientific endeavour where reference and theory change were no longer happening then one would be able to easily make a strong case that the pessimistic induction had failed. And it would go like this:

1) The pessimistic induction claims that we will never hold a theory which will not be rejected or changed in some fundamental way.
2) The problem of reference is not an issue, i.e., we no longer reject or change accepted theories.

Therefore,

3) The pessimistic induction is clearly false.

An astute reader might notice that there is an easy out of the above argument for the defender of the pessimistic induction. Because the PI is an induction, the defender of PI needs only to add onto premise 2 the simple three letter word 'yet.' This is the fundamental problem that the PI is getting at: there is no way of knowing for certain that we will never have any reason for changing our theories and so it is unreasonable to assume that we will not regardless of how much confirmation they may currently enjoy. In fact, Laudan (1981) argues that should the problem of reference cease to be a problem then science would be dead. He argues that if older theories were always preserved in new theories, then progress would be impossible and continued studies and efforts in the field must be halted. For even the addition of a new relation to an old entity changes how we define terms that are supposed to refer to that entity, and thus would constitute a rejection of the old theory.

One should notice that the PI is going to suffer from the same problems that scientific realism does; the fact that it is an induction ensures that there will never be enough evidence to guarantee the conclusion. In fact, because it is an induction, Chad Mohler and Bradly Monton (2014) write, and Van Fraassen agrees, it is a bad argument against scientific realism for an empiricist to use. The pessimistic induction is self-contradictory, because it is in effect relying upon the same methods and reasoning that it is critiquing. One of the reasons why the empiricist wants to reject realism is that the universal theories of science are permanently underdetermined by the evidence, and there is no solution to this problem that has yet been discovered. This is why it is a self-contradictory argument for an empiricist to use. They reject these methods and line of reasoning and so cannot use it and hope to remain consistent in their beliefs. However,

given that metaphysics is stance based, and the empiricist stance contains a completely different set of assumptions and beliefs about reality and the usefulness of certain forms of reasoning than the realist stance does, it does not follow that it is a bad argument for a holder of the realist stance to consider. Note that the reason why it is a bad argument for non-realists to use is because they believe that the form of reasoning contained in the PI is flawed, thus, for a realist to reject it on the same grounds they must also agree that the form of reasoning is flawed and unlikely to produce truth. In order to do that though, because of the similarities in the methods of reasoning between the PI and normal scientific practice, they would need to reject the same methods that normal scientific practice uses and thus reject the philosophy of scientific realism. So even though it would be contradictory for an anti-realist, and more specifically an anti-realist who does not believe in induction, to consider the PI to be influential, it is not the case that the realist can simply reject it on these same grounds without also rejecting scientific realism as a whole.

But what exactly is it about scientific realism that mirrors the PI in such a fatal way? The scientific method relies heavily upon abduction and addition, not on induction, doesn't it? Karl Popper astutely pointed out that it does in fact rely upon induction. As I wrote in the introduction on metaphysics and stances, Popper (1968) believed that one of the biggest obstacles to scientific realism was the problem of induction. Induction is, once again, the logical process of grounding a universal statement on the study of a few. The larger the sample selection is, the firmer the induction will be. Theoretically, there is no cap on how firmly grounded an induction can become but it can never be a certain conclusion. This is the critique of the PI that empiricists bring but realists are denied. Scientific theories are almost all universal statements, i.e., when scientists collect data they are doing it in order to make an inference, even if it is an abduction

(or inference to the best explanation), about how things are all the time. If a scientist were to try and simply publish a collection of facts with no attempt to interpret them then she would be avoiding making an induction. This is not the normal method of science. In biology and *The Study of Island Biogeography,* which I will take to be a paradigm example of science done 'properly', you have biologists, in this case E.O. Wilson and Robert MacArthur, who collect data about the populations of certain species on specific islands, look for trends, and then generalize these trends to all islands and all populations. They believe that this work is representative of populations in other types of islands such as green spaces surrounded by urban sprawl, even though it is sourced primarily by studying islands surrounded and isolated by oceans. This reliance upon induction is what gives science its value, because we can make predictions about how things are going to be and not just were. We can prepare ourselves to take advantage of different phenomena in more efficient and advantageous ways. Thus, even if science does rely upon abduction and addition as well, it cannot avoid induction, and if you attempt to deny the PI because it is an induction then you must also deny scientific realism.

Monton and Mohler also argued that the PI is a poor argument because of its conclusion that belief in current theories is irrational, i.e., given a stance based view of metaphysics they argue that choosing to believe theories about the unobservable can be a rational thing to do. The problem with this argument is that the PI is not just an argument against belief in the truth of theories but a demonstration that the whole realist project is inconsistent. The argument goes like this:

1) If the pessimistic induction is true then realism fails.
2) There is no way to deny the pessimistic induction other than to deny the form of reasoning utilized by the PI.

3) If you deny the form of reasoning utilized by the PI then you must also deny realism. Therefore,
4) Scientific realism fails because either the PI is true or the methods of reasoning in science must be rejected as they are not truth-conducive.

Premise 1) is a pretty obvious and self-explanatory conditional statement. Premise 3) is controversial but I have already argued in defense of it. Premise 2) is usually where the attack comes from realists who wish to avoid this argument.

Clyde Hardin and Alexander Rosenberg (1982) argued that the examples given by Laudan (1981) as evidence for the PI are not good examples because realists have typically claimed that their thesis "is intended to apply to the theories of a 'mature science', one that is in Richard Boyd's term, past a 'take-off point', which he characterizes as 'a point in the development of the relevant scientific discipline at which the accepted background theories are sufficiently true and comprehensive'" (1982, 609). Hardin and Rosenberg are hoping to "to make a distinction analogous to the Kuhnian division between pre-paradigmatic and paradigmatic science" (610). Thus, the PI can be avoided because the theories that were once accepted and are now rejected were not really accepted in the proper manner, like today's theories are, and so the PI is avoided. Or in the case of those which have actually been accepted properly by the proper groups in the proper times, they actually do refer and only are mis-naming some other thing that exists in current modern theories. Aside from the fact that an appeal to Kuhn's (1962) paradigms is actually anti-realistic (and I'll get to this later) does this argument succeed in defeating the PI?

Hasok Chang (2002) has written a paper investigating this exact question. He looks at what he calls a preservative realist argument presented by Stathis Psillos (1999) concerning the

caloric theory of heat. Psillos has two primary contentions: the parts of the theory which were rejected were not central to the theory, and there were in fact parts of the theory which were correct and have been preserved. Chang, after surveying the history of the caloric theory of heat, finds these contentions wanting. Chang begins by arguing that all of the parts of caloric theory which Psillos brings up as points which are retained from it are either irrelevant to caloric theory itself, and thus irrelevant to the issue at hand, or were "theoretical beliefs within the caloric theory [that enable empirical success] that were later rejected," (906) just as Laudan (1981) has described. Chang points out the ironic fact that, "The fact that the Laplacians got the 'correct' (still preserved) formula on the basis of the now-rejected ontology should alarm the realists, not reassure them" (904). He proceeds next to argue that the "materiality of caloric was in fact essential for the explanations of phenomena that were accepted," and thus was in fact a central term to the theory which was later rejected (910).

Kyle Stanford (2003) offers one primary conclusion that is drawn from multiple instances. He argues that recent attempts fail to reject the pessimistic induction and refute Laudan's (1981) argument against realism by re-interpreting the history of science to be favorable to the cause of scientific realists. They do so because the concessions they need to make are so great that they leave the pessimistic induction standing in some other way. Rosen and Hardin's account requires a divorce of reference and accuracy, that is, realism can be saved by weakening what we mean by 'accurate' "and thus *abandoning* the specifically theoretical beliefs of the very sort for which the realist hopes to convince us to share her realism in the case of the current theories" (556). So realism lives, but now we can assume that terms which now refer are not accurate either. The account of reference relativized to individual tokens and intentions only succeeds if we "eschew those specifically *theoretical* descriptions associated with

[the user's] terms," thus implying that the past theories referred "just where their being so, did not depend upon those theories actually getting anything much right about the natural world" (557). Finally, against Psillos, Stanford argues that attempts to rely only upon fundamental descriptions of terms fails because we can never know "which of our beliefs about an entity are *actually* part of its core causal description" (559). The other avenue which realists have travelled in an attempt to defend against the pessimistic induction has been to water down "approximate truth" in such a way that these old theories could be considered non-referential and yet still approximately true by current theories. Stanford believes that these fail because current theories may very well be approximately true in the same way as the rejected theories and share a similar fate.

So it seems that the PI cannot be avoided because in order to ensure that it does not actually apply to the history of science you have to perform some revisionist history, which ultimately waters down reference and approximate truth to the point where the realism being defended no longer means what you want it to mean. Further, we can never be sure that the same watering down does not also effect current theories. For all we know the same critiques leveled against past theories still stand against theories today. Antonia Dieguéz-Lucena (2006) has responded to this situation by admitting that the PI stands but that it actually refutes a realist position that is so strong that the realist does not need it. According to Dieguéz-Lucena, what Laudan (1981) has done is shown that there is no necessary connection between a theory's being approximately true and it's being successful. This is something that Dieguéz-Lucena, as a realist, is willing to admit. An approximately true theory, he argues, needs to be in the right prior environment before it will begin to display predictive success. Dieguéz-Lucena argues that a

weaker sense of explanation will still hold that approximate truth is the best explanation for scientific success if it is not the only possible one. He uses the following analogy:

> The realist's claim can be illustrated with a well-known example by van Fraassen. Every time that the cheese disappears in the house, it is not necessarily because of the presence of a mouse, and every time that there is a mouse in the house the cheese does not necessarily disappear. But if the cheese has gone missing on some occasion, the best explanation is that there is a mouse in the house. Any other explanation would require more unlikely hypotheses. The cheese might occasionally disappear because it was stolen by a neighbour or because it was devoured by ants, but its disappearance is usually caused by a mouse. Similarly, scientific success could be due to different causes (to chance, to the use of false but empirically adequate theories (as in Ptolemy's system), to some pre-established harmony, to the divine providence, etc.); for the realist, however, it is sufficient that in a significant number of cases the cause be truthlikeness. (Dieguéz-Lucena 2006, 400).

This is a clear example of the abductive method of argumentation, and as such will lead me into the next major critique of scientific realism: an attack on abductive reasoning directly. Before I make that move I would like to say a few things about this particular instance of it and its success as a defence against the PI. Dieguéz-Lucena writes that truthlikeness is the most likely explanation of scientific success. This assumes that there is something called the 'success of science' to explain, if the success that is being referred to here is successful reference then it is a circular argument which assumes realism then concludes realism. The fact that philosophers of science refer to the success of science is, I admit, something that confuses me; success in any other enterprise is something that can only occur at the end of the journey or once the job has been completed. This is obviously not the kind of success which they are referring to otherwise science would not be continuing. Perhaps they are referring to successful 'steps' but then it hardly seems appropriate to talk about the success of science without first defining in what way

science is successful, e.g., is it successful in reference? If so then then there is no real way to investigate the truth of this claim as any attempt to explain the successful reference of science by means of realism is going to be circular; "science refers because it refers" is not a good explanation. Philosophers of science, then, must not be talking about this sense of the success of science.

It is likely that they are referring to the instrumental or experimental success of science in predicting empirical results and effects such as the ones that Boyd or Hacking refer to. I do not believe that there is any good reason to think that the fact that science refers is simply the best explanation for this phenomena. To begin with, this is simply a disguised version of the No-Miracles argument given and discussed earlier, and so is just as ineffective now. But does it even match up as a good explanation under the normal criteria used for the selection of theories? Quine and Ullian, in *The Web of Belief,* listed at least six different traits to be considered in theory acceptance: conservatism, modesty, simplicity, generality, refutability, and precision. It is difficult at this time to do a proper comparison between realism and other possible explanations without first describing what other explanations may possibly be, as the 'best' explanation implies competition or else it would be the 'only' possible explanation. This being said, it is clear that realism is not very simple in that it posits countless unobservable entities and forces which complicate reality, and though it appears refutable, many contemporary iterations have had to rely on numerous ad hoc theories and ideas to keep it alive, thus complicating the theory even more and bringing into question its actual refutability on the basis of empirical evidence. But as I said, a more in-depth discussion of how realism compares as a possible candidate for 'best' explanation cannot be done until a competitor is outlined which will be the main focus of the next major section of my paper.

It is worth noting that if the PI does hold and realism does fail by contradicting itself then offering up realism as the best explanation for the success of science does not in fact save realism from the pit of self-contradiction.

To sum up then, the PI claims that, based on historic evidence we will never be able to know if we have a theory that, regardless of the acceptance it holds now, will not be rejected at some point in the future. The PI could be rejected on the basis that it is an induction but to do so would also be to reject much of the scientific method that realists are trying to protect and take as serious. This leaves the only option of rejecting the PI on the grounds that it is a poorly grounded induction, however, there has been little success in this area and the PI still stands. Thus, realism must be rejected, even in a system of stance-based metaphysics which has low standards regarding possible viable stances, because either the PI stands and so realism is not true, or the PI is built on a poor system of reasoning, i.e., induction, but this would be a contradictory stance for scientific realists. If science could be construed without induction then perhaps realism could survive but then what would be left of science?

A Note on Semi-Realism

There is yet one further attempt to save some semblance of realism. Anjan Chakravarrty (2007) attempted to develop a nuanced version of scientific realism called Semi-Realism. Chakravarrty rightly recognized that realism in its traditional, or naïve form is not sustainable. Semi-Realism is the belief that science gives us true belief about the structure of causal properties in a given system. To attribute a causal property is not to say that object *x* has the property of causing event *y,* but a causal property is a property that grants dispositions to objects. Thus properties like having mass or having a surface that absorbs certain wavelengths of light and reflects others are examples of causal properties because they confer the disposition towards causing some detectable phenomenon. So in Chakravartty's view scientific knowledge of unobservables is the kind of knowledge about the properties of a structure which confer upon it dispositions to act in a certain way when submitted to the right conditions; further, we can know which causal properties exist because we can detect the dispositions. Thus, if two structures tend

to be attracted to each other by a force that is equal to what the force of gravity ought to be between the two objects, given all the relevant factors, then we can know that these objects have mass, because mass is the property that causes the disposition to be attracted to and attract other objects with mass.

Chakravarrty is attempting to find a sort of mid-way point between realism and anti-realism. The strengths of Semi-Realism are numerous. It easily takes into account the instrumental reliability of Boyd by means of ensuring that it is our knowledge of causal properties that allows us to create reliable instruments. It is continuing in the tradition of Hacking's experimental realism by centering scientific knowledge around our knowledge of how to manipulate the theoretical world by using the causal properties we have previously discovered. Thus, semi-realism keeps intact some of the stronger points of scientific realists, and furthermore, it responds well when tested by anti-realists. In response to the pessimistic induction, the Semi-Realist can accept that the terms which refer to entities or theories lose their referents or change meaning because on semi-realism there is no concern for individual entities. What the semi-realist is concerned with is our knowledge of causal properties, and while the way that we group them and identify them may change our experience of the individual causal properties themselves do not change. For example; even if we were to find that what we call electrons do not exist, we will still know that in the structure where electrons used to exist there is still something which provides the disposition towards negative charge, and we would still be able to use that disposition to our advantage even if electrons themselves did not exist.

In response to empirical equivalency the Semi-Realist is unconcerned so long as they accept some form of approximate truth. This is because the knowledge of unobservables we can have is merely approximately true, and as such we must be open to the existence of many

different explanations. When we divorce knowledge from truth and replace truth with the much weaker notion of approximate truth we must be ready to accept the idea that there are many truths which are approximately correct, and even further, there are many of these truths which are approximately correct to the same degree.

However, where Semi-Realism is weak is in its likeness to Neo-Aristotelianism and its reliance upon dispositions and the dispositional identity thesis (DIT). The resemblance to Neo-Aristotelianism comes because when we speak of a causal property conferring the disposition to a concrete structure to behave in such a way it is eerily similar to speaking about the nature of a concrete structure, which is, as Psillos (2013) says an unnecessary metaphysical burden. However, even if one were to accept this aspect of semi-realism the big problem is with DIT. Psillos argues that DIT fails because it states that in order to know a property we must know how that property reacts in relation to all properties that it can be in relation to. Not only is this impossible but it makes identifying properties on the grounds of only some of the relations it enters into impossible as well. Since we are capable of identifying causal properties (e.g. mass) without first knowing or observing it in all of its possible relations then DIT must be false. Steven French (2013) takes a similar tack when he writes "what makes a causal property the property that it is, are the relations that it enters into with other such properties, with the conjunction of the laws comprised by these relations specifying the natures of all the causal properties there are" (11). French goes further and argues that DIT fails because dispositions are able to be interfered with. That is, while they are, unlike capacities, always present and "turned on" so to speak, they are capable of being enhanced, retarded, or altered in some way. French continues along these lines to conclude that DIT forces us to think of objects in isolation – a situation which does not exist and any conclusions made as a result of these isolationist thought

experiments must necessarily be underdetermined by the evidence and so there is no good reason to posit dispositions.

In response to these rejections of DIT, that is, that DIT makes it impossible to properly identify causal properties because that would require knowing how they react in all situations the Semi-Realist could argue for a progressive or cumulative view of knowledge combined with some form of approximate truth. This would be along the lines of allowing partial knowledge of causal properties based on the fact that even though our knowledge is not complete it is at least true insofar as it is and it does not deny the possibility of additional causal relations.

The true weakness of Semi-Realism is that it is not all that different from Empiricism. If we accept that observable evidence includes a sixth sense, – our 'causal sense' – then the concrete structures of causal relations that Chakravarrty argues that we have knowledge of are in fact observables. Since, as we shall see below, the line between observable and unobservable, that is, the definition of the two words is going to be defined on a person to person basis then I see no reason why one could not be both a semi-realist and a Constructive Empiricist at the same time.

Chapter 3: The Empirical Stance

If Realism is inconsistent, then it is not a viable metaphysical stance to hold and thus needs to be replaced. What is it to be replaced by? I have hinted in the previous chapter that Bas van Fraassen will be a key figure in the discovery and development of the replacement. Van Fraassen developed Constructive Empiricism (CE) because he wanted to preserve empiricism without falling into the same self-refuting problems that more hardline empiricist positions like the positivism of the early 1900s ran afoul of. Beginning in 1980 with *The Scientific Image* van Fraassen has written a series of books including *Laws and Symmetry* (1989) and *The Empirical Stance* (2002) in which he first defines CE and then refines and more clearly analyzes the position in response to critiques.

In *The Scientific Image* van Fraassen defines CE as the belief that "Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate" (12). In *Laws and Symmetry* van Fraassen expounds upon the semantic view of theories and why he believes that inference to the best explanation is not a reliable means of truth-seeking. Finally, in *The Empirical Stance* the question of the value of metaphysics to epistemology becomes an issue and how a person is capable of switching from one scientific paradigm to another if each are incommensurable with each other.

But what does it mean for theories to be empirically equivalent? Empirically equivalent theories fit the observable evidence but go beyond the empirical and postulate the existence of things that are unobservable and untestable, e.g., Newton's Absolute Space. By 'fit the observable evidence' van Fraassen (1989) (means that "all appearances [of the theory] (structures described in measurement reports) are isomorphic to empirical substructures in at least one of its models" (192), thus if both theories contain models that are isomorphic to each

other and to empirical substructures then they are empirically adequate and equivalent theories. Maarten Van Dyck (2007) summarized the effects of empirical equivalence in the following: "(1) All theories have empirically equivalent rivals… [and] (2) Since empirically equivalent theories are equally supported by all possible evidence, all of them will always be equally believable… [thus,] belief in any theory must be arbitrary and unfounded" (12-13). Van Dyck believes that van Fraassen was not arguing for the irrationality of scientific realism, but instead for the rationality of constructive empiricism, i.e., in van Fraassen's voluntarist epistemology proponents of either stance can be considered rational. Thus, the argument for empirical adequacy in van Fraassen's work is intended simply to show that a constructive empiricist's attitude is possible, not that it is mandatory.

This empirical adequacy is not limited to only theories about entities but also theories about structure as well. Van Fraassen (2006) believes that we ought to take an empiricist stance towards structure, i.e., one in which the only requirement which new theories about structure ought to share with old ones is their empirical content. Because the forces and powers that are hypothesized about in structuralist accounts of nature are invisible, a constructive empiricist can only accept these hypotheses and not believe them. In this case 'accept' means that we can agree that the theory is a logically coherent and empirically adequate possible explanation but that our confidence in it as the correct explanation is not to such a high degree that we are willing to believe it is true. Forces like gravity or magnetism, which are explained by reference to unobservable entities, are useful and empirically adequate but not likely to be true.

In the semantic view of theories, theories are construed as a "family of models" at least one of which is isomorphic to the phenomena. They can be true or false. They say what the world is like, and it is literally meaningful information "in the neutral sense in which the truth

value is 'bracketed'" (van Fraassen, 1989; 192). That is, theories are constructs that help us to understand the connections between empirical data.

Constructive Empiricism has gained popularity in contemporary philosophy of science because of its success in responding to the No Miracles argument for scientific realism, and its ability to explain the anomalies described in the history of science by the likes of Laudan (1981), Kuhn (1962), and Foucault (1969) while allowing empiricists to remain empiricists. So how does science progress from one paradigm to the next? Under CE there is no a priori attachment to the current paradigm because it is 'true.' A paradigm, or 'discourse' as Foucault would call it, is simply accepted for the sake of pragmatic unity and explored. Because there is no belief in the veracity of the paradigms all that is required for a paradigm to be accepted is for it to be empirically adequate and appeal to the correct values like simplicity, cohesiveness, comprehensiveness, etc. A paradigm switch will occur when one of three situations come about:

1. There is a switch in the priority of values and a new paradigm better satisfies these values than the old.
2. The current paradigm is found to be inconsistent with itself and a new one must be found.
3. A new paradigm is discovered which better satisfies the current values.

The empirical adequacy requirement of a paradigm ensures that science can be successful, because success is defined by the instrumental reliability, or empirical success, of scientific theories. Science is empirically successful because the only constant requirement for theory acceptance is empirical adequacy, or empirical success. This is not circular, and thus realism is not the only possible metaphysics that does not make the success of science into a miracle.

It should be noted that Constructive Empiricism depends upon stance metaphysics. This is because empiricists deny the validity of metaphysics as an epistemological endeavour and stance metaphysics allows an empiricist to hold a metaphysical position like empiricism without

having to defend it beyond showing that it is logically consistent. I have endeavored to show in the previous chapter that scientific realism is in fact inconsistent. This section will focus on a defense of the consistency and internal coherence of CE.

Acceptance vs Belief

A key distinction which is necessary for Constructive Empiricism is the distinction between acceptance and belief. A scientific realist argues that scientific theories give us knowledge of the unobservable world. Knowledge according to traditional epistemology is composed of true, justified belief (Lemos, 2007); thus, the scientific realist requires that we not only accept the theories of science but that we ought also to believe them to be true. Van Fraassen has had a hard time, and for good reason, accepting the realist stance so he has instead argued that all that is required for science to progress is simply acceptance of theories rather than belief in their veracity. In practice, a scientist who is a constructive empiricist will look and work similarly to a scientist who holds to realist values except that a constructive empiricist will be less committed to the truth of the theories they are working with, and perhaps are able to be more open minded about competing theories and new ideas.

Gideon Rosen (1994) disagrees that such a distinction exists. For him belief is determined by our actions, i.e., he takes a physicalist approach to belief and holds that one is said to believe in something if they act towards it in a certain manner. If those who think they only accept instead of believe a theory yet still act as if they believed it, then there would be no distinction, because acceptance would be belief in effect. This is echoing Sam Mitchell (1988) who objected to Constructive Empricism (CE) by pointing out that if belief is determined by behavior and if a person who is agnostic about the unobservables in a theory but who behaves exactly the same as one who does believe in the truth of the statements made about the unobservables, then there is

no actual difference between agnosticism and belief. In other words, he holds that it is impossible to remain agnostic towards unobservables while still accepting theories which contain them in the way that van Fraassen wants. Rosen is right. If it is in fact true that belief is defined wholly by action then acceptance and belief would be incredibly similar, maybe even identical; but there is no consensus currently on the actual definition of 'belief' and it seems from our phenomenological experience that belief is something that, though often connected to, is separate from action, at the very least, it appears to be prior to action. Not only this, but Rosen runs into the problem that there would in fact be a distinction in actions between one who accepts a theory and one who believes it. Namely, the one who accepts a theory would be likely to, if questioned, state that they do not believe the theory but merely accept it. This would be only one among many other minute differences that could potentially exist, such as being more willing to question or reject a key hypothesis in light of a new theory with no change in empirical evidence, or a changed perspective rather than ignore the inconsistencies in favor of preserving the established paradigm.

Furthermore, Rosen argued along with John O'Leary-Hawthorne (1994) that CE is a poor stance because, as he saw it, it is a sociological statement about what scientists do, and since there had been no surveys and no actual sociological research done then there was no reason to accept it. Van Fraassen (1994) wrote back that this argument missed the mark because CE is not a sociological project but is in fact a statement about the aims of science. He believes that it is logically consistent for someone to be participating in the larger project of producing empirically adequate theories while also believing them to be true. In fact, Van Fraassen believes that this is largely what happens. One does not need an understanding of the reality of a situation in order to live and exist within it. To put it another way, if the limits of a system are unknown they still

exist, that is, the participants cannot reach beyond the limits even if they believe that they are doing more than they actually are. It's like the Miss Universe contest in which we definitively crown the most beautiful woman in the universe; we all believe we do but it might very well be the case that there is another planet with an even more beautiful woman that we just simply do not know about yet. Put simply, whether the majority of scientists are realists or constructive empiricists does not influence the truth of the matter. Even if every scientist held some sort of realist philosophy then it is still possible that CE be the true state of affairs and all these scientists are doing is producing empirically adequate but false theories about the unobservable world.

I do not believe there to be a good reason why we should think that there is no possible distinction between acceptance and belief. It is a very simple thought exercise to accept some proposition on the grounds of some value other than truth-likeness such as pragmatics or simplicity while keeping an open mind about the truth value of the proposition.

Observable vs Unobservable

Since Constructive Empiricism's primary goal is to stop believing theories about the unobservable without doubting our empirical experience, finding a well-defined distinction between the observable and unobservable is going to be on the list of things to do. One would think, at first glance, that doing so would be easy; all that would need to be done is something along the lines of "if we can see it then it is observable, and if not then it is not." However, the actual doing of this has not been so easy. To begin with, a quick survey of some of the challenges that must be overcome followed by the Constructive Empiricist's response.

Stephen Leeds (1994) echoed Michael Friedman (1982) when he argued that because the received view of the observable content in theories which defines observables in terms of unobservables, the received view is then unavailable to the constructive empiricist as it would

make the position incoherent. That is, because the currently accepted scientific worldview defines observable objects (e.g., apples, chairs, etc.) as being composed of large numbers of unobservable objects (e.g., atoms, electrons, other sub-atomic particles, etc.) it would be inconsistent for constructive empiricists to accept this view, even if they do not believe it. Thus, even to speak of observables in contemporary terms is something that empiricists are unable to do because they are collections of unobservables. In this objection Leeds misses the point. It is common day-to-day practice for the Constructive Empiricist to accept theories about unobservables, even ones which are so rare that they do not compose parts of observable wholes. If science tells us that an apple is composed of many unobservables, a constructive empiricist can accept this theory in the same way that they accept that electrons are real, and invisible forces can be explained by reference to these same electrons. The important part is that the atomic worldview is empirically adequate. Were it not then it would conflict with the constructive empiricist, but were it not empirically adequate then I doubt very many realists would be interested in accepting it either, let alone believing it.

Paul Teller (2001) presents a more potent challenge to the observable/unobservable distinction. Teller asks the writer to consider a simple tube microscope with which one can observe many things which are not visible to the naked eye and yet are not so small that to believe in them is not unrealistic, such as microbes and cells. Along the same lines Teller speaks of the stethoscope, which simply amplifies sounds that are barely audible or in-audible yet are presumably considered empirical. The point being that the naïve observable/unobservable distinction is an oversimplification because not all cases that are considered to be observable are unaided. The quick reaction would be to argue that these examples are over complications and that they are not real cases of observation because the instruments used are built upon theories

about unobservables, like how light bends as it travels through a medium or how to amplify sound waves. This response fails because when one considers a further form of aided observation, the telescope, you can see that aided observation, despite its dependence upon theory and instruments, is reliable. One can easily set up an experiment to test this by taking a telescope and looking at some object that is far enough away to be difficult to discern with the unaided eye yet made clear by a telescope. Take note of certain features and compare the experience you had of the object through a telescope with the experience of the object you have once you move closer to it. You may notice that the telescope has skewed things somewhat, by making them appear smaller than in person but these are slight changes and can be accounted for reliably because they will be consistent across all test cases. Thus, the case of observation aided by telescope seems reasonable, and since it uses the same theory that the tube microscope does then it would only be logical to include microscopic observation as well as telescopic. A similar experiment could be set up to demonstrate that the theory that the stethoscope relies on is reliable in a similar manner. Thus, Teller's examples stand as direct examples of one of the reasons why the observable/unobservable distinction is so difficult to define: it is hard to determine which instruments distort our ability to observe and which do not.

Arthur Fine (2001) argued that the Constructive Empiricist does not even need the distinction between observable and unobservable because for Fine "the name of the game is reliability… [and] the only thing that counts is our experience" (113). The Constructive Empiricist is only concerned with experience and not the distinction between observable and unobservable. This view would require the abandonment of the semantic view of theories because that view requires that we map unobservables onto the observable world, but according to Fine that would not be a problem because isomorphism never actually occurs anyways; even

our best and most accurate theories are still just approximations. Fine's pragmatism is interesting but I do not think it is necessary for the Constructive Empiricist to reject this rejection of the unobservable. To take Fine's stance and accept an experientially based knowledge of unobservables would be to ignore the epistemic distrust that empiricists have of the unobservable world altogether. Even though Teller has provided cases of aided observation that seem to be on the same epistemic level as unaided observation, there are still cases of instrumentally aided observation that appear to have a different epistemic standing, such as electron microscopes and other systems of observing which rely on interpreting signals received by some device. Here the observation is not simply a magnification but a translation both into the language of the measurement device and again into our experience. If we begin to believe explanations of experience given in terms of unobservables then we are missing the whole point of Constructive Empiricism, which is to simply accept these theories and work as if they are true while withholding belief in their veracity.

The idea that there is in fact a privileged divide between the observable and unobservable has been defended by Nancy Cartwright (2007). She wrote that "We have a special primitive justification of self-defence for forming beliefs about what is observable: these beliefs help us control the experiences and perceptions that are thrust upon us" (43). Her argument hinges on two primary facts, the first is that the difference between the observable and unobservable isn't epistemic and the second is that we are experiential beings who have no ability to deny our experiential nature. So, insofar as the observable world affects our experiences, and the unobservable world does not, beyond providing explanations for the observable phenomena, we are justified in forming beliefs about the observable world. For Cartwright, the key isn't that there is some grand epistemic difference between the observable and unobservable but that the

observable is what directly affects us and we need to respond to it in order to survive, whereas what our theories say about the unobservable world are irrelevant to our survival except insofar as they can help us to understand the experiential world. The unobservables posited in explanations are only hypothesis which we can accept as useful for understanding, but for which we do not have good reason to believe actually exist. This view works but does not really solve the problem of actually finding us a distinction between what is observable and what is not. Simply taking the step back and saying that 'observable' is defined by 'experiential' still requires that we define what types of experience matter. For example, there are experiences which have no bearing upon our survival which we are justified in forming beliefs about, but experiences, such as the experience of reading the output of some measuring device, which we must simply accept as having some specific meaning rather than another.

Van Fraassen (2001) provides a response to both Teller and Fine. He begins with reflections on the distinction between observable and unobservable using light as a paradigm case of what is unobservable. He argues that contra Teller, the microscope is not a unique case of observation for it is simply the same refraction and reflection involved in our experience of rainbows, which are not real things, only mirages. That is, even if microscopes use the same theory as telescopes then it is still a theory which distorts reality and since, unlike the telescope, we cannot verify the observations simply by moving to a point a where we can observe these things unaided, we have no reason for thinking that the same theory applies to macro objects as it does to micro objects. For van Fraassen this distinction (Observable/not-Observable) is important to CE and cannot be done away with because there is too much risk involved in believing things about the unobservable, and only the observable world can provide an obligation towards belief.

The divide between observable and unobservable is difficult to define. In fact some have argued that it's a scale of degrees with no ultimate epistemic difference between one end and the other, while some have conceded that there is a gradual slipping of epistemic reliability but with no hard epistemological cut off. Perhaps the key is that the voluntarism applies to this distinction as well. Each person will be comfortable with differing amounts of risk and so will choose to set the cut off where they wish; what they ultimately decide to be considered unobservable will depend on how much they value risk taking against the possible rewards of taking those risks, where the risk is being wrong and the reward could be true understanding. The reality is that since Constructive Empiricism is a metaphysical position and since this distinction is a metaphysical one, given stance-metaphysics and voluntarism, there is no argument that can be given for or against any person's particular position on where to draw the line. The issue, like all metaphysical disputes, will be decided on a person by person basis in which the relevant values will play a crucial role in determining the final stance of the individual, even if we are not aware of which ones are relevant or even which ones we hold. There may be no real hard solid distinction, but that is not necessary for constructive empiricism, for all we need is the ability to accept theories rather than believe them to be true, and the belief that there is in fact nothing about the scientific method which gives us reason to believe in unobservables.

Modality

Constructive Empiricism has been criticized on the grounds of modality. Even if we grant that there is a distinction between observable and unobservable and that we can agree on what that divide is, there is still the problem of modality. The argument against Constructive Empiricism on the basis of modality is composed of two primary points. The first being the problem of induction, which, as stated before, is the inability to make universal predictions from

incomplete sets of observations, and the second being more unique to Empiricism: predictions based on theories about future observable data (based on historical observations) entails that there be belief in constancy of the unobservable future. Thomas Grimes (1984) put the problem of modality like so: How can we know if a theory is empirically adequate for all occurrences, past, present, and future of the relevant observable data if we cannot observe them? Consider what we know about copper. It has a certain melting point, weight, and the property of conducting electricity well, but how do we know that copper always has had these properties or that it always will? Perhaps there is a place in the universe where the copper there is identical to copper here in all ways but that it does not conduct electricity to the same degree as the stuff we have here. James Ladyman (2000) pointed out that not only does CE suffer from modality in this way but it also runs afoul of modality by way of possibility. There are many theories which at best provide only approximate knowledge of if something may occur, due to a certain element of randomness or unpredictability. These theories rely on probability theory and statistics, but once again, how would we ever know about these possibilities because they are possible phenomena from the past, present, and future; i.e., the majority of them are unobservable. If we are to limit ourselves to only knowledge of the observable, or experienced, world then with what are we left? We can no longer make predictions about the future based on trends or patterns. Any time there is a statement made about the future to any degree of certainty we cannot ever be said to know it. For example, according to this interpretation of CE we could not know that objects with mass near earth will continue to be attracted to each other by the empirical experience that we have theoretically labeled ‘the force of gravity’; but this seems absurd. Surely we should be able to know that tomorrow we will not have to worry about floating off into space. Because every scientific theory is subject to modality it appears that the Constructive Empiricist is caught

between a rock and a hard place. Either CE must reject scientific theories and find some substitute, or else contradict itself and allow knowledge of some unobservable things. The first option would require a complete overhaul of science and make it impossible for the constructive empiricist to even believe theories about empirical phenomena, and the second option would rule it out as a viable metaphysical stance.

The response has been to take a third way, and that is to defend knowledge of possibilities and future states of affairs by way of what are called occurrent properties. Bradley Monton and Bas van Fraassen (2003), along with Joseph Hanna (2004), elaborate on this idea. To return to an earlier example, the ability of copper to conduct electricity in the way that it does gives us knowledge that it always will, because it is an occurrent property of copper. It is observable and it is knowledge about an observable thing, therefore it is not unobservable. What the critic is doing is similar to if we were to declare planets, or other similarly large objects that we cannot currently observe directly, to be unobservable, even though by any standard, the only thing preventing us from directly observing them is the distance between us and them. A planet is clearly an observable object; just because we cannot directly observe Saturn now does not imply that we will never develop the technology to send a person into orbit around Saturn one day.

It must be admitted though, that this solution does not completely answer the question. For example, even if the tendency of objects with mass to be attracted to each other is an occurrent tendency, then how do we know that they will continue this behavior in the future? This problem is not fatal to constructive empiricism though. The point of constructive empiricism is to avoid putting too much emphasis on the truth or falsity value of scientific theories. It is a healthy skepticism grounded in a history littered with discarded theories and

revisions. The problem of induction and modality is a problem for all philosophies of science to date, not simply Constructive Empiricism, and it does not unearth an inconsistency in Constructive Empiricism but in the scientific method, and displays a limit to human abilities. If worse comes to worst a Constructive Empiricist could be a modal realist by accepting modal statements about observable objects and not about unobservable ones.

Constructive Empricism

Constructive Empiricism is a metaphysical stance on what science is possible of achieving. It is not a sociological statement about what scientists think they are doing. It is ultimately underwritten by what I believe are the values of humility and respect for truth. Constructive Empiricists do not want to ascribe truth values to theories about unobservables because they recognize our limits as human beings and do not want to be caught believing falsehoods. Constructive Empiricism helps in understanding the history of science as it has shifted from paradigm to paradigm by identifying the mechanism at work as being the values of the scientific community and the advancement of understanding of the implications of our theories. The empirical adequacy of competing theories, and the fact that every theory has competitors that are equally empirically adequate, makes choices between theories non-rational, as there is no evidence or line of reasoning to which one can appeal to beyond the empirical world of experience. By merely accepting the paradigms and theories of the day, scientists are able to progress within the paradigm, continue to search for more empirically adequate theories, and create new phenomena which will spawn new empirically adequate theories and so on.

This is not to say that Constructive Empiricism is a perfect theory. It is weak in that there is no clear definition of observable and this bleeds over into the problem of modality. These problems can be solved either by means of examining one's own values and coming to your own

conclusion as to what is observable and just how much you are willing to risk, or by accepting even more and greater limits to human knowledge. Overall, while not irrelevant, these problems are not fatal to Constructive Empiricism's overarching theme, which is that of being humble before the limits of human understanding.

Not only is Constructive Empiricism not a perfect theory, it is in fact probably wrong. For all the same reasons that we cannot believe scientific theories about unobservables we also cannot believe theories about metaphysics. We can however, accept it for now because it aligns with our values until someone devises a new theory which aligns with our values better or our values change. In this way constructive empiricism and stance metaphysics are the same thing. They both call us to accept theories rather than believe them. Theory acceptance in both is based on considerations other than truth value, such as fit with our values and other accepted theories, and the defining feature of both is a dislike of being caught *believing* in falsehood. The implication is that anything which attempts to be scientific and goes beyond the search for empirical adequacy is going to be determined entirely by voluntarism, and because a significant part of the scientific method is the peer review process and communal acceptance of theories by specialized experts, paradigms will be largely chosen simply by appealing to the values of the majority rather than any evidence-based reasoning. It is important to remember that the fact that a paradigm, theory, or stance matches one's values is in fact a reason for selecting one to accept; i.e., we do not select stances randomly or without reasons. However, these reasons are sometimes not reasonable in the sense that they will not always appear logical or display the best use of critical thinking and reasoning, such as being objective and characterized by cold, clear, reason, because they may be emotional, taste-based, or even something taught to us as children.

An example of this is raised by Evelynn Keller (1987) who argued that the development of science and scientific theories has been disproportionately influenced by males and the sociological factors that make up male-ness. Keller is concerned that there are avenues of knowledge and truth which are simply over-looked because of the male bias in scientific culture. Keller relates the story of a female scientist who developed a theory of DNA which viewed it not as a dominating tyrant of the cell but as a product of the relations and an equal member partner in the cellular eco-system. She believes this to be an issue because the new theory of DNA has been by and large ignored, which she believes can be attributed to the fact that men tend to take a domination and power view towards the world while women lean more towards relations of equality and co-dependence. If the new theory is empirically adequate and is relatively equal to the old theory across all of the other usual metrics considered for theory selection, such as those listed by Quine and Ullian in *The Web of Belief* then we have a perfect example of what Stance metaphysics and Constructive Empiricism is predicting to be the case. The dominant theory of DNA, i.e., the one which holds the most support, has been determined by sociological factors relating to the predominantly male population of scientists and the way in which males are socialized from childhood. This is not an evidence based decision. It is rational, but only in the sense that there is a reason for the choice, not that the reason is particularly logical or good and there is no objective sense in which this reason is better than any other reason. It is simply a reason held by the majority of the scientific community now.

Chapter 4: Science and Voluntarism: Into a New Understanding of Knowledge

Scientific knowledge is much more individualistic than the peer review process would lead us to believe, even if theories are something that are accepted or refuted by the community. The way the process works is through peer review. A hypothesis is created, then an experiment is devised to test it, and then the whole thing, method and results, are published so that other experts can attempt to reproduce the experiment and either come to the same conclusion or else find some error that the original group made. This process assumes that everyone has at least a similar understanding of what it is the theory is saying about the world and how the empirical evidence provided by the experiment is supposed to relate to the theory about unobservables, i.e., it assumes that there is such a thing as a homogenous community of scientists who are all equal in understanding and background knowledge. There is a problem with this assumption: scientists are individuals, and, as such, have their own individual understanding of the world that is going to be different from how even other scientists on their own research team will understand it. This is because our understanding is shaped by our life experience, history, and who we are. Consider for example an electrician's understanding of electricity against that of a physicist who is on the leading edge of science. The layman will likely know electricity as a thing to be manipulated and controlled in order that he can be paid and eat, or perhaps as a thing which, if it is not respected and handled properly, can cause severe injury. The physicist's knowledge and understanding of electricity is going to be more technical and dependent upon theory. Thus, an electrician's experience of electricity defines his knowledge of electricity; he or she may have a rudimentary understanding of the theory behind electricity but it is not required that there be complete knowledge of that in order to do the job properly.

This is as Michel De Certeau (1984) argued: we know the world as we use it, i.e., knowledge is produced by our interactions with and uses for the world that we live in. De Certeau spoke of what he called "lived spaces," which are the places of everyday life that are given meaning through our use of, and interaction with them; these lived spaces are to be compared against the 'empty spaces' which remain unused by us though may become 'lived spaces' for others. This is how an empty building can become a thriving community center, a place of occupation, crime or charity, all while still remaining the same building occupied by the same people who continue to do the same things. Each patron has a different use and experience of the building and its occupants, such that it comes to have these different meanings to each patron.

But surely, between scientists who are experts in the respective fields, there is a similarity in understanding. According to Homi Bhabha (1994) this not so. Homi Bhabha's concern is with issues of race and culture. He believes that modernity has failed humanity in a post-colonial world by making it too easy to set up one homogenous culture against another homogenous culture. In fact, Bhabha believes that there is no such thing, in a post-colonial world, as a homogenous culture. He argues that the true location of culture is not stored in the centre of a homogenous group of people but it is to be found in between groups of people, in the spaces in between and in the hybrid mixing of people, i.e., a person's true identity and history is a mix of traditions and experiences which set him or her a part as an individual while simultaneously connecting them to a diverse and often misrepresented history. Even in stereotyping and discriminating against the other we ourselves are defining ourselves in contrast to the other and are forcing them into our own discourse. As the other sees us, we are defined and they define themselves. Bhabha concludes that culture is a hybrid thing where there is no 'pure' and

homogenous group but that, like individuals, cultures are constantly shifting and changing and are always influenced by all the other cultures and events it comes into relation with. Thus, culture is a hybrid entity. Bhabha shows this by utilizing themes of mimicry, hybridity, stereotype, fantasy, difference, and ambivalence in many different places which are typically thought of as pure and homogenous sources of culture.

The relevance of Bhabha lies in the fact that just as there is no homogenous culture, there is no homogenous scientific culture either. That is, there is no such thing as a pure scientist, or a pure science, or a pure scientific community that all share the exact same history. The aims and motives of science will always be composed of a mix of the aims and motives of the stated community of scientists and their personal goals. There is no such thing as someone who is purely a physicist, or purely a biologist. Everyone is part philosopher, part theologian, part layman, etc. A person's experiences and interactions, that is, their personal history, shape the way that they know and understand the world. Even in a group of highly specialized individuals like a research group, the individuals will come with their own presuppositions about the way that the world is, which will colour their understanding of the theories and their link to the experimental data. Each scientist will come with their personal concerns and interests that will separate them from the group as an individual with their own nuanced interpretation of the evidence and understanding of the theories.

This difference might not always be significant, as the dominance of the paradigm in the education of new scientists will go a long ways towards ensuring that each person has a similar experience and thus a similar understanding. The difficulty though, is that when dealing with unobservables there is no way to ensure that everyone has the exact same experience, because they exist outside of our experience–an unobservable cannot be experienced. We can only rely

on our understanding of the terms which are used to define them and, if philosophy has taught us anything, it is that language is ambiguous, interpretation is contextual, and the context is the history of the interpreter. Thus, I conclude that scientific knowledge is individualistic in nature; i.e., it is not communal, and thus is susceptible, to the injection of values, taste and other pre-rational considerations which are going to be embedded in history and not empirical evidence. Our individual understandings, or stances towards, unobservables and the interpretation of empirical evidence is not going to be determined by some universal scientific community.

The recognition of the importance of values, taste, and other pre-rational, historically shaped, factors in theory acceptance is going to inevitably lead to the critique that this is a relativist metaphysics. With no evidence-based reason for choosing between theories, and the addition of taste, there is no way to prevent anybody from developing an empirically adequate theory which fits their personal agenda and declaring it true while ignoring all other and possibly more 'reasonable' theories. Or alternatively, one might argue that this system of metaphysics and science will end discussion, debate and any hope of progress by introducing a way out for any person to believe whatever they want. How could two people with different values have a discussion about whose belief is right when there is no logical reason for compelling belief; only empirical adequacy and internal coherence exist as measures for falsifying beliefs and thus removing them from competition? Dien Ho (2007) wrote that while van Fraassen's portrayal of philosophical positions as stances (and by extension through Constructive Empiricism all non-empirical subjects, including those which are normally under the purview of what we call 'science' and the interpretation of such empirical subjects) appears correct it leads to a dilemma regarding philosophy as a whole. The dilemma is that either we blur the lines between philosophy and sophistry and lose any possible distinction that philosophy has gained in order to

be able to arbitrate between the value based stances or, “we embrace a kind of radical relativism” (332) and accept that stances are merely “lifestyle choices.” This means that philosophy is still concerned with the way the world really is but also implies that “at some point, philosophy will be silent in the face of incompatible hypotheses as to which one is correct” (332).

The view advocated in this thesis concerning of both science and metaphysics is not a relativist one. As stated before, relativism holds that all beliefs are true; while this mistake is easily made, neither Constructive Empiricism nor Stance Metaphysics holds that all beliefs are true. In fact it is quite the opposite; they treat all statements and theories that go beyond empirical adequacy as if they were false. Constructive Empiricism calls for one to merely accept theories, not believe them, while, similarly, Stance Metaphysics is open to the existence and acceptance by two separate parties of two contradicting stances so long as they are both internally consistent. This openness would not be possible if there were a hierarchy of stances. If one stance were to be considered objectively better than others, based on some measure like truth-likeness, then it would be difficult to see how one would not be obligated to adopt that one over and above the others. So, in a sense this is a relativist view because it treats all empirically adequate and internally consistent views equally, but, it does not make the next step and declare all such viable views truthful. It may, however, declare these views to be knowledge.

Traditionally, knowledge has been defined as true, justified, belief. However, as many skeptics have pointed out, the idea that truth is necessary for knowledge may be too high of a bar and leads to skepticism. If we remove the requirement of truth and speak only of justified belief then the search for knowledge becomes not only epistemically possible but probable and useful. What would it look like? I have already endeavored to define part of what it means for a belief to be justified: it has to be both empirically adequate and logically consistent both internally and

with the holder's other beliefs. There has also been much discussion in the field of epistemology on the topic of justification which could be added to these requirements, such as proper functionalism as presented by Alvin Plantinga (1993), which would require that our beliefs be formed by properly functioning cognitive faculties in order to be considered warranted; this really is just an extension of the requirement for empirical adequacy, where empirical adequacy is communally defined as this: it is impossible for one person to have an empirical experience that no-one else would have if they were in the exact same situation if their faculties were functioning properly, though they still may have a different understanding of those experiences.

Regardless of how justification would be defined, the end result would be that there would be many possible expressions of knowledge, where a knowledge is both a way of understanding the world and a way of living in the world. Each knowledge would be equal to the next because there is no way to judge between them, i.e., each knowledge would be of equal value in understanding the human experience. Though, each way of knowing the world, that is both empirically adequate and logically consistent, may be epistemically equal, this does not mean that they need be considered equal in all regards; some may be better at providing a foundation for technological advancement while others may be better at providing an understanding of human values and making sense of the world around as a lived space. If a knowledge is essentially the summary of what it means for one who has that knowledge to be human, then in order to fully understand the full human reality we would need to account for the knowledge of the world contained within that person's experience of it.

This view trades heavily upon the distinction between 'seeing as' and 'seeing that.' What is universal to humanity is our empirical experience, that is, the given realm of experience, of the world and the restraining force of logic. However, just because every person has the same

experience (seeing that) of a rabbit in the bushes it is not true that this experience is interpreted (seeing as) the same way. The experience of the rabbit could be interpreted in many different ways, which are all logically coherent and empirically adequate: perhaps the rabbits appearance is a sign of good fortune, a portent of future fertility; alternatively the rabbit could be simply another data point on a graph representing the growth and change of species population over time as E.O. Wilson's *The theory of Island Biogeography* leads us to believe could be the case for all species.

The world is known as we live it. Human experience is the source and foundation of knowledge. In Michel de Certeau's *The Practice of Everyday Life* (1984) we see the production of knowledge in how we use the world. Places become lived and thus are known, e.g., a forest might be known as a refuge and a place of peace which needs to be protected to one with environmentalist leanings while to others it may be a barrier to future financial stability and the last obstacle before technological and societal advancement. In a world where there is no means of convincing beyond an appeal to values and emotion, there is no way in which we can judge between these knowledges of the world. However, what we can do is recognize each view as valid and valuable in order to create discussion in an attempt to better understand the values underlying each position. This is where real Knowledge lies; Knowledge of the values which are important to humanity; Knowledge of what we hope, dream, and desire; Knowledge of how the world shapes us and makes us into what we are. Hans George Gadamer (1975) wrote that historical knowledge is not law orientated like science but it is an attempt "to understand how this man, this people, or this state is what it has become, or more generally, how it happened that it is so" (4). It is the values that are undergirding our choices of stance and theory, which make

this man, this people, or this state what they are. An understanding of these values will lead us to Knowledge.

Knowledge is not systematic, it is not spoken, but felt; it is what defines humanity. It does not defy scientific knowledge for what it is worth: scientific knowledge helps us by providing one way of interpreting the world and enables our manipulation of it to better fulfill our values. Foucault (1969) wrote about discourses, the idea that there are unwritten rules about what can or cannot be said (or meant). The Discourse is the home of knowledge; knowledge is relative to the discourse and the discourse is relative to the dominant values and pragmatic needs of those in the discourse. The scientific discourse is necessarily focused on classifying, categorizing, and analyzing the natural world, while an example of a more lived discourse would be focused on job, community, self, and family. Knowledge lies at the intersection of discourses. Values define the discourse and are thus beyond the discourse, that is, a value which helps to define one discourse will by virtue of being a part of a meta-discourse shape other discourses as well an example of this is ethics, which affects all aspects of human life from the rules and regulations regarding test subjects in a scientific experiment to the ways in which a store owner ought to treat their customers. The purpose of inter-discourse discussion, and by this I mean not only between discourses on different topics but between discourses which disagree with one another about the same subject, is to reveal these values and to help us to gain a better understanding into what it means to be human.

Knowledge is more than simply useful information about the world, it is a search for understanding and refinement of the self. It is a valuing of the other and a death to tribalism and partisanship. It is not a death to individualism; this is not a call to forget the individual in favor of the collective. Knowledge is both subjective and communal. Gadamer, in his quest to legitimize

the human sciences as a non-structured field of knowledge, believed that ‘taste’ was key. Taste is subjective because it is personal, it is a matter of personal preference and desire. Yet, Gadamer also writes that taste is communal in that there is something called ‘good taste’ and is something that we strive to have. Our culture sets a standard for what is good taste and what is in bad taste, but that culture is informed by the collection of individuals. Should a critical mass decide that what was once in good taste, for example slavery, become bad taste, there is a shift and a new knowledge is produced. Taste, Gadamer writes, is not something that can be taught or demonstrated. One must have it and one can develop good taste, but it is not bound to a system of rules, or even fixed in time. Taste changes and the changes are nearly unpredictable. Taste, in this sense is a form of knowledge, one which we need in order to live in community and be human. It must be stressed again, that though ethics has been reduced to aesthetics here this is not an attempt to declare that all ethics ought to be is aesthetics, simply all that we can make of it. There may in fact be a mind-independent truth about ethics, just as there is a mind-independent reality for science but just as in science we cannot escape our own filters to view it as it is in itself and so all we are left is to investigate the human experience of ethics.

Gadamer believes that ‘taste as knowledge’ is only relevant to the human sciences. However, if Constructive Empiricism is true, or at least if one accepts Constructive Empiricism then taste becomes important even in the so-called hard sciences. Taste is composed of all of the social factors and is both a reflection of and determinant of our values. If it is in bad taste for scientific theories to be overly complex, then scientists will strive to have good taste and develop simpler theories. For a poignant example of taste in science, consider geology. Alfred Wegner, in 1912 (Hughes, 1994) posited the idea of continental drift, which is the theory that the surface of the world is not stationary and is subject to change. Wegner initially had a difficult time getting

his work on the subject published, with universities threatening to boycott publishers who took his book seriously. He received much personal dislike for his ideas and his trials are likened to those of Galileo's struggles with church dogmatism (Hughes, 1994). He was denied professorship at many universities and the reaction to his ideas was "militantly hostile" (Hughes, 1994). His views have later come to have wide acceptance. Even though some claim that there is evidence for the existence of tectonic plates, they have never been directly observed. Their so-called effects are used as evidence for their existence, but you must first assume that they exist in order for them to cause the effect which needs explained. Nonetheless, it is an empirically adequate theory which was ridiculed and insulted by big names in the field of geology that is now widely accepted. Wegener's mistake was not that he did not have enough evidence to back up his theory, as even with the accumulation of more evidence tectonic plates still remain unobservable, but that his idea was in bad taste for the times. This man, who is now considered among the visionaries and great thinkers, was an outcast due to the dogmatism that comes with the established taste and values of the scientific community. This reaction is indicative of the presence of emotion and value in the reasoning of the scientific community. If someone has simply made a logical error, or mis-interpreted the data (which it is believed today that he in fact did not do), then there is little reason to react in such an aggressive and career-destroying manner unless there is some further reason. It is generally considered bad taste to try and branch off from the established theories and paradigms of science, even though this is precisely how science makes its biggest and greatest advances. This culture within science means that it is generally not the big-out-of-the-box thinker who is successful, in the sense that they get funding and can continue to afford to work as a scientist, but the scientist who has a good sense of taste and is able to develop new ideas within already currently accepted ideas. Even though, according to

Constructive Empiricism, these ideas which define the rules and which make or break careers, are no more defensible than any other set of empirically adequate rules. Simply put, having taste is important not only to the human sciences but in the hard sciences as well. As long as science is a communal exercise, i.e., governed by the peer review process that attempts to understand the realm of unobservables, success will largely be determined by one's ability to toe the line and have good taste.

Kuhn (1962) wrote that the history of science is described by revolution followed by periods of normal science. The revolutions were spawned by changes in taste and value, while normal science, which is the majority of science, depends upon a knowledge of taste. A scientist today dare not attempt to explain something by reference to phlogiston even if it is as empirically adequate as oxygen for explaining. Not because the theories about oxygen are true, but because oxygen has greater explanatory power, is simpler, or is more comprehensive than phlogiston. That is, it is a matter of taste or preference which has set the rules of the discourse in such a way that taking phlogiston seriously is a good way to end your career. Though it may seem contradictory to say that oxygen "has greater explanatory power, is a simpler explanation, or is more comprehensive than phlogiston" and then in the next sentence say that it is only a matter of preference to choose oxygen over phlogiston, it is not, precisely because it is those characteristics which we choose as being the measuring stick which are chosen by our pre-rational values. For example, it could easily have been the case that history had conspired to leave us desiring more complex explanations or less comprehensive ones. In fact, van Fraassen argues, the criteria used in selecting theories are not linked in any way to an increase in likelihood of being true, and in some cases, such as comprehensiveness actually decrease the chance that a given theory is true. As the number of claims that a theory makes goes up, it

becomes more and more probable that it is wrong in at least one of them and so the more comprehensive a possible explanation is the more likely it is to be false.

Once again however, this is not to say that there is no value in the scientific discourse. The values that drive science have been successful in producing many technologies that improve our quality of life in both trivial and meaningful ways. The development of the internet has enabled the spread of ideas and information in ways that could not be imagined before. Advances in medicine have helped stave off disease and prolong life spans, while agriculture is more productive than ever before. Our options for entertainment are nearly limitless and during all of this revolution we are discovering more and better ways to protect the environment from our rapid expansion and development. These are but a few small things that science has given us; however, to assume that the scientific worldview is in any way objectively better than another is to ignore the fact that at the base of all human knowledge is the same thing: taste. Science has given us the world, but what good is it if we lose our soul in the process? Humans are primarily a communal being, moral philosophy throughout history has all mostly agreed that the communal effects of actions are relevant to their value and that recognizing the value of the communal is important to the individual's well-being and ability to live the 'good life.' William Craig (2010), in order to drive the meaninglessness of a purely material life home, uses the story of an astronaut stranded on a meteor, all alone. The astronaut has in his possession two vials filled with liquid. Whichever vial he drinks first, the effects of that liquid are permanent and cannot be undone even by drinking the other. One vial contains a life-giving serum that causes one to become immortal, whereas the other contains a deadly poison. The astronaut, in his despair, decides to end his suffering and drink the poison, but mistakenly drinks the serum, thus damning himself to eternal life alone on a rock flying through space. The utter despair of a life spent

without meaning, alone travelling through space, that the astronaut would experience is similar to what humanity is doomed to if categorized scientific/analytic knowledge were deemed to be the only way of knowing. Even if we are here together, if what is properly deemed as scientific knowledge is the only kind of knowledge to be discovered and verified true universally, then we essentially are just one collective travelling through space on an asteroid in a universe that does not care.

One does not need a story about astronauts and asteroids with vials filled with miracle working liquids to make this point. All you have to do is look at nursing homes and the way in which western society treats the elderly. Atul Gawande (2014) argues, on the basis of personal experience, anecdotes, studies, and the historical development of elderly care in the West, that the best way to take care of the elderly is not to simply ensure that their material needs are taken care of as described by physical science, but that people tend to live longer, happier, and more enabled lives if we provide them with an avenue which brings meaning into their lives than if we simply treat them as patients with a list of needs and prescriptions. Humans need value and value judgements in order to make their lives feel worthwhile. These things come from philosophy, art, drama, etc, i.e., they come from the social sciences. And by the social sciences I do not mean the science-envious analytic philosophy, psychology, or sociology (not that they do not have their place and purpose as well), but from the locations of knowledge production which are less methodical like literature, art, drama, or the interactions one can have with family, friends, and even pets.

In order to refine his idea of taste Gadamer turns to the Aesthetic Problem. For those who want to systematize knowledge, aesthetics has always presented an insurmountable problem. All that has been previously said about taste applies, that is, in art there is such a thing as good taste

and bad taste as well as personal preference. All of these things require knowledge of taste, but there is no way to quantify and measure and this is demonstrated in aesthetics. Gadamer believes that there is no thing such that it has a perfect example of itself, i.e., there is no such thing as a perfect house because 'house' is an insufficiently defined term. Homes all around the world are judged by different standards and as such a perfect home will vary by location. The same goes for more standard forms of art like paintings: there is no such thing as a perfect painting. The standards change through time and vary from person to person. Paintings can be in good taste or bad taste, but even a painting done in bad taste can still have the value and convey the meaning that the painter wants. Some may argue that in man-made art there is in fact a line to be drawn between objectively good and objectively bad art. Gadamer's response is to develop the distinction between natural aesthetics and man-made aesthetics. In nature it is eminently not the case that there is any standard of perfection. Even a mangled and diseased tree is beautiful in its silent struggle for survival and in adding contrast to the world. In nature, preference, or taste, is what determines the beautiful. Someone who grew up in the prairies prefers the flat plains of Saskatchewan with the sweeping views of a grand sky while someone from Western Alberta prefers the sharp ruggedness of range after range of mountain stretching on as far as the eye can see; and though they disagree in their preference neither is wrong and both are right. From this conclusion in natural aesthetics Gadamer concludes that in art it is the same: all claims for universality are simply mistaken.

Foucault (1969) would write that any discourse which tries to set itself up as the right one is not merely mistaken but is actually violently and arbitrarily oppressing those who are in opposition. By setting one way of knowing, or one set of values, or declaring one taste as objectively good taste which we are morally obligated to align ourselves to for all time, we are

destroying the process through which knowledge is created. We are limiting our ability to be human and to fully open ourselves up to the wide variety of human experiences. When we deny the sub-Saharan tribe's worldview as valid and impose our own western view we de-legitimize their experience of the world, and we do so completely arbitrarily. Simply because we live longer, or have more technology, there are still reasons why one might prefer the ways of the tribe to that technological society we have created. This is made obvious in Aldous Huxley's *Brave New World* where we see John "The Savage" ultimately reject the happier and easier life of the so-called 'civilized' for the sake of a simpler and harder life where he believes that true emotional growth and development is possible. To turn to a previous example, one could look at the suppression of different empirically equivalent views on the role of DNA in the cell. It is doing a disservice to human creativity and experience to deny the new view a proper investigation, just to defend the status quo which is nothing more than the product of history. To drive the point home even further and demonstrate why it is important that we be aware of this violence, I turn to the work of Roland Barthes who writes about something which he calls Myth.

Barthes is not referring to the myths of Greek or Norse legend but to the modern myth that both inhabits and defines our contemporary world. Myth is what is left when the meaning of a thing is emptied out and something else is left. Symbols come to mean more than simply what they are and can be used to influence how people act. Barthes showed how this works with myriad examples and it is key in advertising. Consider a luxury car like a Cadillac Escalade. It is more than just a car, it is not simply a means of moving from place to place but a method of expressing oneself. The Escalade has become associated with wealth and power. When you buy an Escalade you are in fact not buying a car you are buying an image that is associated with the car. You can see the same thing with sex all throughout western culture. Sex is no longer a

means for reproduction, it is pleasure and status; sex in the public sphere is not intimacy, the sex we are sold is the opposite of intimacy. The emphasis on sexual freedom has removed sex from itself and made it into something else. There is nothing inherently wrong with this creation of myth but it is important to be aware of what is going on because it is too easy to not be aware and to unconsciously become a perpetrator of this violence. It was John MacMurray (1933) who wrote that these philosophies, or myths, "holds [us] in their grip and tosses [us] about helplessly from one surprise to another" (2) if we are unaware of them, and it on this grounds that he concludes that philosophy is a practical endeavour. Too easily do we divide and caricaturize the other, creating straw men who we can stand against while dehumanizing both them and ourselves. It is through processes like these that you can divide the entirety of the approximately four hundred million Americans into two distinct and hostile camps: Democratic and Republic. The oversimplification of issues into partisan party politics occurs in Canada as well. Most issues are too complex for a single party stance to properly evaluate and solve the problems. Further, it is simply impossible for a whole coherent picture which takes into full account all values and opinions to be represented by an 'us vs them' mentality.

This pluralistic view of knowledge paves the way for an easier transition to a more empathetic view of our opponents. When we consider that their arguments and ideas are primarily undergirded by their emotional concerns, values and personal taste, which is just as strong as our own position we can move to the conclusion that the best dialogue is one which approaches our interlocutors as whole persons with concerns, fears, and values not dissimilar to our own and not simply as our opponents. C. S. Lewis argued in *The Abolition of Man* that morality actually is universal and that this is evidenced in history. His thesis was that the apparent difference in morals was simply a difference in facts and not of values. According to

Lewis, every culture has valued Bravery, Justice, Honor, and Veneration of Elders among others but has simply expressed these values in their own way. Thus for example, when the North American Inuit peoples were discovered to be setting their elders afloat on ice bergs to die it isn't that they do not value their elders or human life; it is that it was believed that this was the only way for the elders to achieve their version of the afterlife. Regardless of whether or not one is willing to accept Lewis's argument, it is at least important in that it illustrates the fact that for one value there can be many different manifestations, and that there is at least reason to think that maybe we do in fact share similar values with others, even those who we consider our enemies. Thus, discussion exists to discover not only what our interlocutors value but how they value it. This holistic approach to knowledge will enable greater understanding and a growth in cultural and scientific knowledge.

Conclusion

This thesis has covered much ground. The project has been ambitious. The topics have ranged across metaphysics, philosophy of science, into epistemology and back again to metaphysics. I doubt that I have done proper justice to many of the topics discussed but I have hopefully been able to properly communicate my message.

The Scientific discourse has come to dominate western society; the expert has replaced the sage while scientific studies have almost achieved the level of holy writ in their ability to dictate to the layman the way things are. Gone is the reliance upon common knowledge, tradition, and cultural norms as a way of knowing the world. Today the concern is with being scientifically correct, and this is disturbing only because the overemphasis on one way of knowing is devaluing humanity. Without even considering the inherently reductionist theories of physics which reduces all things, human emotion and decision-making included, into the end result of deterministic processes occurring at the sub-atomic level, the scientific discourse has successfully turned following one's cultural heritage, and thus cultural knowledge, into a social faux pas. Given that metaphysics is largely based on value and an individual's history, why is it that the scientific discourse has come to such a dominant position in recent times? The naïve answer would be simply that this is so because science has been successful and it is only now that the layman is collectively beginning to recognize this success and value it. This is naïve because success is undefined. Success in common parlance is typically used to refer to the positive completion of a challenge, the victorious end of a journey, or a triumph over some opponent or trials. Science cannot be said to have done any of these things can it? Larry Laudan (1981) answers 'no' because if it had, then further progress would be impossible and we would either be at the limits of human understanding or else have complete knowledge of the universe.

In fact, it is not even possible to say that science successfully gives us approximate truth regarding unobservables.

In a stance based system there are only two metrics by which a philosophy can be judged: the first is empirical adequacy and the second is internal consistency. Regarding empirical adequacy scientific realism is successful, i.e., if science gives us truth about the universe then it would easily explain the instrumental and experimental reliability of scientific theory. However, regarding internal consistency scientific realism runs into rocky ground. This is seen in the Pessimistic Induction. Even though as empiricists people like Bas van Fraassen deny the value of induction and thus the value of the pessimistic induction, they forget that stance metaphysics judges stances based on their *internal* consistency, and so, if induction is something which scientific realism internalizes, then it is a viable method of critiquing realism from within the realist stance. If scientific realists defend the reliability of induction in producing knowledge of the world then they cannot deny instances of it simply because it contradicts their beliefs; they must respond to the specific claim of the Pessimistic Induction. There is no good realist response to the claim that the history of science is full of widely accepted scientific theories which have been rejected and replaced, and that this trend will likely continue with contemporary theories. Thus, scientific realists are skewered by their own method. Science, and the methods of science, including induction, are believed to produce knowledge, but, the historical evidence provides good grounds for the inductive reasoning included in the Pessimistic Induction. Therefore, scientific realism does not provide real knowledge about the unobservable world. Thus, scientific realism is logically inconsistent. The Pessimistic Induction serves as a piece of the whole which scientific realism needs to coherently account for, and since it cannot without both affirming and

denying the epistemic value of induction, scientific realism is incoherent. As such it is not a viable stance.

Semi-Realism is an excellent attempt by Anjan Chakravarrty to save scientific realism. It easily accounts for the things which scientific realists wish to save, such as instrumental reliability and the experimental manipulation of unobservables with other unobservables. By rooting knowledge in our causal contact with the causal properties in the world Chakravarrty secures realism against the Pessimistic Induction because entities and structures can come and go so long as the properties that we have causally experienced remain. Knowledge of unobservable is not based on our understanding of or acceptance of theories but on our experience of the causal properties. In this way one may argue that Chakravarrty relies too much upon the dispositional identity thesis or accepts too much metaphysical baggage in allowing dispositions or causal properties in the first place. But if one can reconcile themselves with a form of neo-Aristotelianism and accept approximate identities for causal properties, Semi-Realism survives. However, since the foundation of this knowledge is our experience, the constructive empiricist has no qualms with this. Insofar as concrete causal properties are things which we have experienced one could possibly argue that our causal knowledge of the world is based in some sixth sense which is tuned for detecting causation and causal properties in a similar way to our other more obvious senses. Furthermore, these causal properties are empirical in the sense that they can be quantified, for example mass can be quantified by measuring the forces (the mechanism by which mass is a causal property) produced by that mass. So my argument is not so much that Semi-Realism fails for any particular reason, but that it simply is not necessarily realism about the unobservable world but is instead an empirical stance.

The strongest argument that scientific realists have been able to produce in favor of their position is that scientific realism is the only philosophy that can explain the success of science without reference to miracles. The No Miracles defense of scientific realism has failed because there is at least one philosophy of science which can explain the success of science without reference to either realism or miracles. Bas van Fraassen's Constructive Empiricism and its reference to the notion of empirical adequacy has successfully provided a possible explanation for how science could be performed without miracles or realism. The idea is that science exists to produce empirically adequate theories regardless of truth value. Thus, the instrumental reliability of scientific achievement is explained by saying that theories are empirically adequate, which is the only possible measure of scientific success, because they are made to be and judged on their empirical adequacy.

Constructive Empiricism is not without its challenges. The distinction between observable and unobservable is complex and murky. The problem is that there is no agreement about that distinction and there in fact some instances of non-natural observation which do seem to be at least as reliable as unaided observation, e.g., the sound of a heartbeat through a stethoscope or the view through a telescope. These appear to provide examples where some instrumentally aided observation is good for producing knowledge and it is difficult to see why we should discount these observations. The argument is then that if we can trust these instruments then there is no reason why we distrust more abstracted (from unaided human senses) forms of observation. However, this misses the point because these forms of observation are inherently theory laden, and the problem with that is that if the Pessimistic Induction is true then the theory which underlies these instrumental observations are probably going to be found wanting in some respect and thus are not reliable for generating knowledge about the world.

Ultimately, since the divide between observable and unobservable is a metaphysical distinction, i.e., it is not an empirical one as it would be circular for us to decide if something were observable, and thus empirical, through the use of empirical methods, the divide is probably going to be decided on an individualistic and voluntaristic basis. Essentially the value which underlies the empiricist's denial of the instrumentally aided observation is the risk inherent in believing something to be true which is in fact not true. Thus, where the line is drawn for each individual will be drawn based on the amount of risk that individual is willing to take on. The primary thing needed for Constructive Empiricism is not some definitive and universal dividing line between observable and unobservable but the consensus that there is greater risk in the unobservable than in the observable and the desire to minimize that risk.

Along these same lines is the problem of modality. Modality is a problem for Constructive Empiricism because scientific theories do not simply tell us about how things are but how they will be as well, and this stands not just for unobservable entities but for clearly observable ones as well. This calls into question the enduring nature of a theory's empirical adequacy, i.e., just because a theory is empirically adequate now unless we are a modal realist and allow some inductive reasoning there is no grounds for thinking that it will continue to be. The Constructive Empiricist's response has been to speak of what are called 'Occurrent Properties;' these are properties which are those currently held by the entities described by science. For example, the melting point of copper is an occurrent property and this is what science tells us about, and for as long as copper continues to have this occurrent property then the scientific theories about copper will continue to be valuable. Should copper ever lose this property then we would simply have to re-discover what it had been replaced with and the new causal relations that it could be useful in.

To those who would argue that this does not solve the problem of modality, it is open to the Constructive Empiricist to argue that because no scientific theory is believed to be true it does not really matter how one solves the problem of modality as long as one is able to simply accept the popular theories of the day.

All of this being said in defence of Constructive Empiricism, yet it is probably still wrong. As a human theory, constructive empiricism is most likely incomplete or just plain wrong, and further, even if it were complete there would be no way to definitively know that it is so. The same restrictions that apply to our theories of unobservables in science apply to our theories of science. That is to say that the greatest commitment one ought to make to constructive empiricist is mere acceptance and not belief. Ultimately, what one chooses regarding what to believe about science will be tempered only by the belief's logical consistency, the belief's empirical adequacy, and the values of the believer. Constructive Empiricism is logically consistent and is empirically adequate. This is to say that there has yet to be found any internal contradictions and that it can account for all of the evidence, both measurable and repeatable data as well as subjective experience regarding science. Furthermore, Constructive Empiricism is motivated by a humility towards humankind's ability to hold an objective and complete point of view which has been born of years of failing to do just that. The desire to respect truth and be wary of declaring anything true that we are not certain of is another defining value of Constructive Empiricism. Constructive Empiricists value truth too much to be willing to be caught handing that title out to incomplete and half-understood theories about which we have no evidence.

This personal element of scientific knowledge, which, with the advent of analytic philosophy and the growth of logic as the foundation of knowledge, has been the most dominant

discourse in the twentieth century, must lead us to a yet another re-conception of what knowledge is. Science is not objective in the sense that modernity has taken the word to mean, nor is it communal. 'Objective' in modernity has come to mean something like Kant's "thing in itself" however, given that there is no such thing as 'the thing in itself' within human knowledge (for how can there be a knowledge of something that is mind-independent – the thought is absurd) then nothing can be objective in this sense. If one wanted to, one could preserve the word 'objective' by changing the meaning to something like "the object as it appears to us." The problem of empirical adequacy and the under-determination of theories by evidence ensure that scientific theory is chosen based upon value and not upon logico-deductive methods which supposedly connect empirical evidence to theories about unobservables in some way which ensures the truth of the conclusions. The path from experience to knowledge is much murkier than that. There is no method to it; the values with which we determine which theory is best oftentimes are such that they do in fact reduce the likelihood of a theory being true, e.g., the more comprehensive a theory is the more content it contains and the chance of error increases with each new bit of data included.

This dependence upon value in the production of knowledge is key to understanding what knowledge really is. But first we must answer the question, "What is value?" Value is the worldview of the individual. Worldviews are individualistic in nature. Every person is born into their own experience with their own understanding. What we are taught by experience is tempered by what we are taught by society, elders and circumstances. The individual is a free being who attempts to realize themselves within the context of the limitations placed upon them by experience. It is their understanding of this experience that shapes their values and determines their worldview. Given that our understanding is produced by our history and culture and the fact

that it is highly unlikely that two individuals would have exactly the same personal history (not even identical twins grow up with the exact same experiences) it is also highly unlikely that no two individuals will ever have the same understanding of the same experience, (if it even is possible that there can be the same experience if experience is composed of both empirical sensory stimulation combined with an interpretation of these sensory signals), and there is no way to objectively value any one individual's understanding as somehow better than some other person's understanding. Thus, the production of knowledge is dependent upon the individual's personal understanding of that experience. Knowledge, in this view, is not objective, nor methodical, neither universal.

Even if the experiences which shape us and make us into what we are and our understanding into what it is could be codified and even if there is a set of laws which can determine how any specific individual will understand and react to the limitations given to them, by the nearly infinitely large sample size of starting conditions that each person is faced with, the idea of a pure homogenous culture group is a stretch. Some person may be born into a similar situation to some other person and their understanding of it could be so drastically different that one ends up overcoming the obstacles given and becoming a productive member of society while the other makes decisions which lead to a total failure in moral character. Even amongst scientists on the same research panel there will be individual understandings of the findings and individual understandings of the theories used, and these will lead to completely individual understandings of the conclusions of that research group.

This is not to say that the knowledge produced by the analytic methods of science are not useful in their own way. Science tends to be more practical than this other form of knowledge which I am writing about. For example, in physical geology, part of which is the study of the

minerals in the earth's crust an important part of the science is the naming and analyzing of different minerals and rocks, e.g., quartz, granite, shale etc. These names and divisions are important for the development of many different industries, included in which are some pharmaceuticals. However, this system of labelling and dividing nature up is arbitrary and has its only foundation in our desire to analyze. There is a whole world of experience contained within the field of physical geology which is not taken into account by the analyzing of rocks and mountains down into their parts. This reductionism is useful but it is not knowledge in the sense that it is the way things are. A mountain presents itself to us in some particular way and it is in how we understand that presentation that meaning and knowledge are produced. For some a mountain is a grand demonstrating of the power of a deity, for others a mountain is a representation of the next major obstacle that one must overcome in order to be successful in some endeavour; mountains may be viewed as beautiful, imposing, bright, dark or even annoying obstacles in the way of a better view. To break a mountain down into simply nothing more than rocks, trees, dirt and water with some animals mixed in, while not wrong, is to completely miss what it is to be a mountain. Mountains have meaning and it is in understanding that meaning that knowledge is produced. The reductionism in science has practical value but there is no meaning in pushing it too far, for when we reduce things beyond our experience then what value is there? We are given possible explanations for phenomena which may be true but there is no way to know since we will simply be choosing theories which already fit with the values of our day. Meaning only exists during interpretation and in absolute reduction there is nothing to do the interpreting.

The problem does not exist when science is performed but when science is raised above other ways of knowing. Even if one does admit that there are Natural Kinds which somehow

divide themselves into categories and that science somehow latches onto these, there are still no grounds for believing that science is a more valuable form of knowing than others. First because that would assume some realist stance on science when as we saw in chapter two realism suffers from some serious difficulties, e.g., how would we ever know that what we have described as Natural Kinds are how they are actually divided up? The second and more important reason why the existence of Natural Kinds would not warrant holding scientific knowledge in higher esteem than other knowledges is that humans do not experience the world in terms of natural kinds. There is never an isolated kind of thing taken in and of itself. Every experience of everything that can be experienced is always done in the context of some background composed of other things. We never experience a tree, or a piece of quartz, in a complete vacuum, and even if we did the vacuum in and of itself is contextual background in which we would know these things. Suffice to say science, even an empirically minded science, is not representative of the way which we actually experience the world. But then, why would it be? That is not what science is trying to do; science is trying to help us better use the world and improve the quality of our experiences based on certain subjective measures, like increased health, easier survival, and greater pleasure to name a few.

Hopefully this thesis will help western society move beyond its infatuation with facts and back into a more holistic and person-centered view of the world. The 20th century's obsession with scientific truth, and thus what they believed was objective truth, is less a means of salvation for the human race and more a means of prolonging our existence while leaving us to die empty and alone as just another patient in an institutionalized, one-size-fits-all health center.

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