An Analysis of Empirical Equivalence: Its Foundation, The Evidence-Theory Distinction, And Its Entailment, Underdetermination.

By

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A Thesis
Submitted to the Faculty of Graduate Studies
In Partial Fulfillment of the Requirements
For the Degree of

MASTER OF ARTS

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THE UNIVERSITY OF MANITOBA

FACULTY OF GRADUATE STUDIES
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Abstract

Recent anti-realistic accounts of science have relied heavily on the warrant of the Underdetermination of Theory by Evidence argument. The problem of underdetermination has a long and varied history, but what began as a methodological skepticism for Descartes has become the basis for a serious problem in the philosophy of science. Underdetermination occurs if our best scientific theories have rival theories (empirical equivalents) that meet the demands for a satisfactory theory (minimally: one that covers all the available data). If our best theories have rivals that meet the demands for a satisfactory theory, then we have no reason to believe that our sciences tell us about the hidden world (the world of theory – e.g. atoms), since choosing between theories would be based on arbitrary criteria. I believe that the problem of underdetermination can be successfully defended from all opponents, and that the underdetermination argument can be levied in certain cases to provide reasonable grounds to embrace anti-realism.

It is often argued that the underdetermination argument can be eschewed, since it rest on an incoherent distinction between evidence and theory. Both Bas van Fraassen (a prominent anti-realist) and Jerry Fodor offer distinctions that appear to avoid the charge of incoherence. I argue that both van Fraassen and Fodor provide distinctions that should not appeal to the anti-realist. These distinctions rest on the belief science providing us with a non-underdetermined theory of observation; it opens the anti-realist to a charge of being arbitrary in their division of the world to accept that some theories are not underdetermined. Further, there are other problems that need to be worked through before either offering for a possible distinction can be deemed coherent. Thus, I offer my own evidence-theory distinction founded on scientific practice. It seems that scientists already hold that there is a distinction between evidence and theory, and the success of science justifies the distinction.

The foundation of the underdetermination argument is the notion of empirical equivalence. Empirical equivalence occurs when there are two or more theories that cannot be distinguished by their observable consequences. That is, when two theories cover all the scientific data, there is no way to distinguish between them. Larry Laudan and Jarrett Leplin argue that the notion empirical equivalence is conceptually flawed; thus, it cannot be used to found the underdetermination argument. I argue that the force of their attack demands a realistic reading of science, and, as such, it cannot be used to question the notion of empirical equivalence. However, even though the notion of empirical equivalence cannot be shown to be incoherent, it must be argued that the notion is coherent. I defend a method for generating empirical equivalents that is advance by Andre Kukla in his most recent book. He argues that there is a method for generating rivals to any of our best theories: for any theory we can generate a rival by combining it with another theory that contains observations that we cannot ever observe. I argue that this method can be successfully defended against all attacks.

Finally, once the foundation, empirical equivalence, has been satisfactorily defended, the task of demonstrating that underdetermination follows from empirical equivalence must be undertaken. Even if our best theories have satisfactory rivals, it may be the case we can choose between them using non-arbitrary criteria. Historically, there have been three different forms of arguments to show that we can choose between theories in a non-arbitrary, and truth-tracking fashion: non-empirical virtues of theories provide a truth-tracking criteria, the evidence itself allows us to choose between the theories, and the historical success of science provides evidence for one theory over its rivals. I argue that each of these arguments fails to defeat the claim that, when faced with empirically equivalent rivals, selecting one theory over the others is not arbitrary. However, a defense of the claim that theory choice is arbitrary does not amount to the claim that theory selection is arbitrary. Thus, I argue that there is a motivation for the claim that we should not select any given theory, and that it is in principle possible to defend such a claim. However, my defense can only justify the claim that theory choice is arbitrary in a narrow set of cases: when it is used against people who are undecided with respect to realism and anti-realism, and when it used against people who are realists for reasons other than the belief that there are non-empirical virtues of theories.

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Chapter 0
The Introduction

1. The Problem

I will suppose, then, not that Deity, who is sovereignly good and the fountain of truth, but that some malignant demon, who is at once exceedingly potent and deceitful, has employed all his artifice to deceive me; I will suppose that the sky, the air, the earth, colors, figures, sounds, and all external things, are nothing better than the illusions of dreams, by means of which this being has laid snares for my credulity; I will consider myself as without hands, eyes, flesh, blood, or any of the senses, and falsely believing that I am possessed of these; I will continue resolutely fixed in this belief, and if indeed by this means it be not in my power to arrive at the knowledge of truth, I shall at least do what is in my power, viz. [suspend my judgement], and guard with settled purpose against giving my assent to what is false, and being imposed upon by this deceiver, whatever be his power and artifice.

- Descartes[1]

While Descartes held his skepticism to be merely methodological, his evil genius has been an epistemological horror. In the philosophy of science the Cartesian Evil Genius again rears its ugly head; underdetermination of theory by evidence is the philosophy of science's evil genius.[2] Just as the Evil Genius provides us with an alternate theory to our commonsense theory about the constitution of the perceived world, calling into question the epistemic justification of our beliefs about the perceived world, the proponents of underdetermination claim that there could be compelling, competing, and observationally equivalent alternate theories to our best scientific theories. The possibility of such alternate theories, empirical equivalents, may usurp any potential justification for belief in the truth of our scientific theories.

Underdetermination of theory by evidence is appropriately ubiquitous if two criteria are jointly satisfied:[3] first, each theory has at least one competing and incompatible empirically equivalent rival theory (call this criterion EE),[4] and, second, at least one of the competing theories is as believable as the theory in question (call this criterion 'EE entails UD').[5] If these two criteria are satisfied, then belief in the truth of any particular theory is unfounded – such an epistemic commitment would be arbitrary.

Following Earman,[6] I take the realist position in science to be composed of at least two core features. One feature of realism is Semantic Realism, the commitment that theories are to be read literally.[7] Another feature is Epistemic Realism, the commitment that we have good evidence, through observation and experimentation, to believe what a theory, read literally, says about the world.[8] Clearly, the existence of empirically equivalent rival theories creates a tension for these two commitments of the realists: where there are rival theories, composed of at least one incompatible feature, only one theory can consistently be believed at a time. It seems, then, that to preserve Semantic Realism when faced with two theories, which, when read literally, say different things about the world and that have equal epistemic status (such as is the case with empirical equivalents), requires the rejection of Epistemic Realism. Further, in order to avoid other skeptical worries,[9] scientific realists are committed to the belief that our best theories track the truth, so they will find it unacceptable to switch between belief in one theory and belief in another without a change in the evidence. Where there are empirically equivalent rivals, the choice of which theory to believe is arbitrary and we are unable to claim our theories track the truth.

The Underdetermination of Theory by Evidence is an argument normally advanced by scientific anti-realists. The scientific anti-realist, or at least the one that I am happy to defend by means of this thesis, holds the epistemic position that science cannot provide us with knowledge about anything other than the empirical consequences of our theories. That is, we are not justified in our beliefs concerning electrons, or quarks, or what-have-you, since these 'objects' are not observable. Further, we are not justified in any part of our theories, not just their postulated entities, other than the parts of the theory that are observable. Thus, differently from the skeptic, the anti-realist believes that it is possible to know about observable things; the anti-realist does, however, agree with the skeptic that it is impossible to know about the 'unseen world.' It is important to note that I do not hold the anti-realist to be making claims about the ontology of such objects noted above. My study of Empirical Equivalence is, thus, a part of a greater project, that of the anti-realists.

2. The Structure Of This Thesis

Empirical Equivalence is the fundamental building block of the anti-realist project. For Empirical Equivalence to do its job in justifying this project two conditions must be met: 1- it must be possible that there are empirically equivalent rival theories to our best scientific theories, and 2- these equivalents must be sufficient to make our choice between them and our best theories epistemically arbitrary (i.e. they must generate underdetermination).

Chapter 1 – The Evidence Theory Distinction

Not only is it critical for the anti-realist, who believes that we can know about the empirical consequences of our theories (the observable entailments of our theories), but it is critical for the possibility of empirical equivalence that there be a distinction between what is merely observation and what is theory. Chapter 1 is dedicated to an exploration of the distinction between evidence and theory.

I begin Chapter 1 with an analysis of two candidates for founding the required evidence-theory distinction. The first candidate belongs to Jerry Fodor, who offers a distinction based on his belief that perception is modular. He claims that the distinction lies in the difference between our inferences based on perceptual modules and the modules themselves. The second candidate is offered by Bas van Fraassen, who suggests that there is a distinction to be drawn between two types of objects in science. Our relation to the objects of science, whether or not they are observable by us, is the grounding for his distinction. In the first half of Chapter 1, I consider these two candidates and find them wanting: both distinctions require that there be at least one scientific theory that has no empirically equivalent rival. If the theory-evidence distinction requires that there be a theory that has no rival, it should be rejected by the anti-realist project, since such a theory opens the door for other theories with no rivals that could very well lead to realism. Thus, in the second half of Chapter 1, I offer another candidate for the evidence-theory distinction.

My candidate for the evidence-theory distinction is founded on the differential justification for our beliefs in observables (evidence) and theory. I believe that it can be shown that evidence is justified externalistically and theory interalistically. Definitions of these terms can be found in Chapter 1. My candidate does not, however, suffer from the critical problem that there must be at least one theory that has no rival. Also, my evidence-theory distinction provides adequate grounding for my claim in Chapter 2: what counts as evidence is determined solely by what humans can observe, not also by what they could observe.

By the end of Chapter 1, I hope to have established there is a viable candidate for an evidence-theory distinction. Such a distinction should be able to provide an adequate foundation for empirical equivalence: Chapter 2 will rest in part on whether or not there is a distinction between evidence and theory. Also, it should be noted that establishing this distinction serves to show that the underdetermination argument has a place in an anti-realistic project, rather than simply a skeptical one.

Chapter 2 – The Ubiquity of Empirical Equivalence

Empirical equivalence occurs when two theories have the same empirical consequence classes (i.e. they have the same observational entailments). The core claim of my thesis is that all scientific theories have empirically equivalent rivals. In Chapter 2, I will elaborate on the notion of empirical equivalence. Chapter 2 is divided into two major parts: 1- the presentation and critique of Larry Laudan and Jarrett Leplin's argument that Empirical Equivalence is impossible, and 2- the presentation and defense of Andre Kukla's method for generating empirically equivalent rivals.

Laudan and Leplin argue that all seeming cases of empirical equivalence are defeasible: due to the nature of scientific evolution, it is impossible that two theories remain equivalent over time. Of course, the anti-realist's program rests on the claim that we cannot ever have justified beliefs about the truth of our scientific theories; thus, if the rivalries to not obtain 'transcendently,' her program is defeated. I will argue that critical points in Laudan and Leplin's Defeasibility Argument beg the question against the anti-realist.

Andre Kukla provides the method for generating empirically equivalent rivals to all of our best scientific theories. In the second major part of Chapter 2, I will advance

Kukla's method and defend it. Kukla argues that it is always possible to generate a rival theory by 'cloning' our best theory and adding some object or event that humans will never observe. The newly generated theory will now have the same empirical consequence class as our best theory, but this new theory will be a theory unto itself (i.e. an empirical equivalent). My defense of Kukla's method rest primarily on demonstrating that attacks on the newly generated theory consist mostly of genetic fallacies and realist question begging.

By the close of Chapter 2, I hope to have demonstrated that empirical equivalence is not only possible, but also actual and appropriately ubiquitous. If all of our scientific theories have empirically equivalent rivals all that remains for the anti-realist is to show that underdetermination is indeed an entailment of these equivalences.

Chapter 3 – Does Empirical Equivalence Entail Underdetermination?

Underdetermination only occurs, to the level that would justify the anti-realist project, if the empirically equivalent rival theories are equally epistemically warranted. Should it turn out that we have greater evidence for one theory over another, it does not follow that we have no reason to believe that our best scientific theories are true (as the proponent of the underdetermination argument would have us believe).

Chapter three is devoted to several common arguments that claim there is more evidence for the truth of a theory than simply its empirical consequences. I examine three different classes of arguments for such evidence. First among these arguments are the ones that claim there are certain virtues of theories that make them more likely true than their rivals. I argue that these arguments beg the question against the anti-realist.

Next is the claim, similar to the defeasiblity argument, that the structure of science demonstrates that underdetermination does not follow from empirical equivalence. Here Laudan and Leplin argue that connections between theories allow them to provide indirect evidence for one another. There are actually a few versions of this objection, I consider and reply to each in this chapter. Finally, it is often argued that the success of our best scientific theories provides us with evidence of their truth (this is more commonly known as the No Miracles Argument). I argue that this argument fails to undermine the rivals generated by Kukla's method; thus, the No Miracles Argument provides no additional warrant for our best scientific theories.

By the end of Chapter 3, I hope to have shown that we have no reason to believe that underdetermination is not an entailment of empirical equivalence. I also hope to demonstrate that there is a place for the Underdetermination Argument outside the anti-realist's coherence project: the Underdetermination Argument may beg the question against certain realists, but this does not preclude it's use in debates against other realists, or those who have so far remained 'agnostic' in the debate.

3. What Will Be Accomplished

When all is said and done, I hope to have demonstrated that Empirical Equivalence can play a critical role in the anti-realist project. I should have shown that there is a proper foundation for the limited skepticism of the anti-realist (the evidence theory distinction is in tact), that empirical equivalence is a sound concept (the critical building block of the anti-realist's limited skepticism), and that there is no way to block the obvious entailment of an appropriately ubiquitous empirical equivalence (underdetermination follows from empirical equivalence: we cannot have warranted beliefs in our best scientific theories past the observable level). Further, I hope to show that there may be a place for the Underdetermination Argument in arguments against certain sorts of realists and those who have not made a judgement between realism and anti-realism: the argument could well be used to sway individuals in these positions to anti-realism.

Chapter 1 – The Evidence-Theory Distinction[10]

0. Introduction

According to most, the underdetermination of theory by evidence argument requires an important distinction to be drawn, the distinction between theory and evidence. In science the evidence are the phenomena to be properly described or explained; it is commonly held that these phenomena are ordinary observable objects and events, like tables and erosion. The important question to be answered here is - is there a difference in epistemic kind between evidence and theory? Without such a difference in kind, it is difficult to see how the underdetermination of theory by evidence argument is meant to proceed, since the underdetermination thesis depends on certain phenomena (i.e. observable phenomena) being of a different epistemic sort from theoretic postulates.

Commonsense seems to make a distinction in epistemic kind between objects like tables and objects like electrons. Certainly, tables, to which we seem to have a sort of immediate access, are epistemically safer than the likes of atoms. It is often objected that commonsense is not rigorous enough to produce the groundwork for a powerful anti-realistic thesis such as underdetermination. I concur. The critical problem with the commonsense distinction is that it does not draw a difference in kind between observables and unobservables: this particular distinction merely demonstrates a difference in degree. I am more certain that there are tables than electrons because I can see them and touch them. Of course, I also see and touch particle clouds, but I just don't have the same sort of "feeling" of certainty about their existence. Also, how certain we can be that we know about perceptual objects themselves is a problem in its own right, but it is one whose answer is not of import to the scientific realism/anti-realism debate, where the underdetermination argument lives.

It has been argued that the scientific realist can win a quick and decisive battle against the scientific anti-realist, whose conception of science depends on the underdetermination thesis, by dissolving the observable-unobservable distinction. Both Fodor and Van Fraassen offer distinctions that may serve as appropriate for the anti-realist's needs. I believe that neither generate sufficiently acceptable evidence-theory distinctions. However, I will argue that the evidence-theory distinction can be drawn based on a limited externalism.

1. The Fodor Distinction

Jerry Fodor offers a distinction between observation and theory. He believes that the realist requires such a distinction to avoid Kuhnian relativism: if a theory-neutral language does not present itself, we are then unable to make impartial judgements between different scientific paradigms, and paradigmatic solipsism follows on its heels. Fodor believes that a combination of a modular perception and the possibility of a 'language of thought,' can create a case for a theory-neutral language.[11] Whether or not Fodor can win this battle is of little import to my purposes in this paper; however, if his distinction does withstand scrutiny, then the anti-realist may avail herself of it in order to motivate scientific anti-realism.

1. How the distinction is drawn

Fodor claims that the language of scientists' includes a differentiation between observation and inference. This scientific practice provides the grounds to believe that there is an epistemic difference in kind between 'table,' an object of observation, and 'electron,' an object of inference. As far as an argument for the distinction in question, this is obviously insufficient to sway anyone. Fodor makes his case by defending, rather than arguing for, the distinction. Kukla argues that the best case made for the Fodor distinction is to be found in Fodor's claim that psychology (science) has left room for a form of direct perception.[12]

Fodor believes that he can demonstrate that at least some of our perceptions are immune to the sorts of inferences that would make them theory-laden. Certainly, some level of inference is involved in all perception, but, according to Fodor, there may be two different levels of inference: 1- the kind that all perceivers use, and do not bias (so to speak) the observation to fit with one theory or another, and 2- the kind that do stem from theory. For example, perceiving a red ball includes few inferences, and could be done in the same manner by all perceivers, regardless of background theories. However, perceiving electron tracks in a cloud chamber requires all sorts of training and, consequently, high-level inferences.[13]

2. Fodor's best case

The Kuhnian program for scientific relativism gains strength from some recent studies in psychology. In 1957, Bruner and his colleagues performed a number of studies that demonstrate that the observations of the everyday perceiver are biased (infected) by the perceiver's background theories.[14]

Fodor points out the fact that there are many well-known illusions which demonstrate that our observations can resist our background theories. Consider the stick that appears to become bent when we place it about halfway into a pool of water.[15] Now, the better part of the populace knows that the stick does not become bent when it is placed in the water, it merely appears bent, and this is, certainly, included in peoples' background theories. Despite the inclusion of this knowledge in our background theories, the stick still appears bent. Fodor claims that this shows that perception is modular (shielded from infection by our theories).

What Fodor seems to show is that at least *some* of our observations are immune to inference infection. Recall that Fodor only requires that enough of our observations be inference free to attain his goals in the overthrow of the relativists. How many types of observation, if any, we can count as uninfected is a matter for empirical study. Further, the upshot of such a study will be of import only to those who believe that relativism cannot be avoided in another way. Notice also that the occurrence of illusion serves only to show that (possibly?) some observations are not (perniciously?) inference infected; Fodor must make a much stronger case for the claim that modular perceptions can generate a set of observations that are not theory-laden. However, I will now continue to a more pressing matter: can the Fodor distinction serve to defend scientific anti-realism?

3. The utility of the observation-inference (Fodor) distinction

The observation-inference distinction could help the anti-realist. The scientific anti-realist requires that there be a difference in epistemic kind between our *belief* in observable phenomena (*e.g.* tables) and our *belief* in theoretic postulates (*e.g.* quarks), which are inferred from the observables. Fodor's distinction draws a line between (some of?) our perceptions and our inferences, but it tells no story about how this leads to a difference in epistemic kind between the aforementioned beliefs. Of course, we could develop a story about how the beliefs were formed; this may provide a difference in epistemic kind.

We could imagine an anti-realist who claims that beliefs formed from our perceptual modules (*e.g.* there is a table) are of a separate epistemic kind from the beliefs formed from our theories (*e.g.* the table is composed of atoms) due to the difference in their genesis. But is this a distinction without a difference? Assume for a moment that Fodor's modularity of perception is true: perceptual modules are immune to pernicious inference. Given this assumption, when I see a table, I know that this table appearance is not a result of my background theories. Thus, when I assert that I believe that there is a table, I seem to be making a claim that is on relatively stable evidential footing. Further, when I claim that I believe a table is composed of atoms, I am certainly asserting a very different sort of belief: beliefs based on theory, which do not arise from some sort of direct contact with the world, seem to be on a different epistemic level than the former beliefs. Of course, this is little in the way of argument, but it does seem to match strongly with our intuitions.

So far, my arguments and comments on the observation-inference distinction have been pretty vague. This is due to the fact that I believe there is a prima facie problem for the anti-realist's appeal to this sort of distinction.

4. The problem

While it seems that the scientific anti-realist could build her case based on the observation-inference distinction, as outlined above, I believe that she should not.

The reason that the anti-realist needs an evidence-theory distinction in the first place is to show that empirical equivalence is a problem for the realist's conception of science. Why is this a problem with respect to the observation-inference distinction? The anti-realist can only generate a difference in epistemic kind for beliefs, with respect to observables and inferences, if it turns out that perception is appropriately modular (direct, in a sense). Whether or not perception is modular is based on the results of an empirical study. If empirical equivalence is to be as pernicious as the anti-realist wants to claim, then why would she want to admit that our theory of perception does not have an empirical equivalent? The anti-realist must admit that our theory of perception (in this case, the one that shows that perception is modular) has no empirically equivalent rival (or, at least, that it is not underdetermined in the same way *all* other theories are), otherwise the whole distinction cannot get off the ground.[16]

This is by no means a devastating criticism of the anti-realist who wishes to employ the observation-inference distinction to support his/her argument for underdetermination. Why, though, would she want to admit that there are some theories that are not underdetermined at the outset? This simply allows room for the realist to build his case. I believe, however, that there is an evidence-theory distinction that does not suffer from the same problem. Before I explore the distinction that I believe serves the anti-realist best, I will provide an examination of the other contender: Van Fraassen's observable-unobservable distinction.

2. The Van Fraassen Distinction

Bas Van Fraassen, a proponent of the underdetermination argument, provides his own version of the evidence-theory distinction: the observable-unobservable distinction. Van Fraassen believes that the difference in epistemic kind needed for the underdetermination argument is to be found at the level of entities: there are observables (e.g. tables) and unobservables (e.g. quarks). If there are very different relations we can have to entities, it is possible that our beliefs about these types of entities are of a different kind as well.

Van Fraassen does not try to produce a distinction that will provide us with a theory-neutral language, in fact, he embraces quite the opposite: he believes that "All our language is thoroughly theory infected."[17] He is interested only in showing that there is a dichotomy between beliefs about observables and unobservables, even within our theory infected language. Van Fraassen's distinction is intended to show that some objects, whether or not they exist, fall in the category 'observable,' like tables, and others in the dichotomous category 'unobservable,' like quarks.

1. How the distinction is drawn

"X is observable if there are circumstances which are such that, if X is present to us under those circumstances, then we observe it."[18] Van Fraassen notes that this is not to be held as the formal definition, but a guideline for the discussion of observation and observability that follows. Notice that Van Fraassen's distinction is completely determined by the limits and capacities of human sense organs: to be observable is to be measurable by humans. "The human organism is, from the point of view of physics, a certain kind of measuring apparatus."[19] In the end, what is measurable by humans will be determined by the final biology and physics: "... [the human organism] has certain inherent limitations – which will be described in detail in the final physics and biology."[20] Thus, the distinction is to be drawn based on what is measurable by the human perceptual apparatus versus what is not.

2. The epistemic significance of the distinction

The common charge against the observable-unobservable distinction is that it is arbitrary to carve the world up in this manner. Maxwell argues that it makes little sense to divide the objects (entities of the world) of science in this way.[21] Notice that Van Fraassen will admit that describing what we know to exist is a proper goal of science. In this case, any entity that exists is an object of science. The charge is that to divide this category of existents into observable and unobservable categories would lead to ontological relativism. Van Fraassen argues that ontology and what is observable are indeed separate:

> ... even if observability has nothing to do with existence (is, indeed, too anthropocentric for that), it may still have much to do with the proper epistemic attitude to science.[22]

It seems to me that Maxwell is also demanding that Van Fraassen demonstrate that an epistemic difference in kind, required for the underdetermination argument, is provided by the observable-unobservable distinction.[23]

Recall that Van Fraassen believes that the human organism is merely a certain sort of measurement device. I believe that this claim is meant simply to reinforce his claim that the goal of science is to describe the observable data of the world appropriately (i.e. develop empirically adequate theories).

> The question is what aim scientific activity has, and how much we shall believe when we accept a scientific theory. What is the proper form of acceptance: belief that the theory, as a whole, is true; or something else? To this question, what is observable by us seems

> eminently relevant. Indeed, we may attempt an answer at this point:
> to accept a theory is (for us) to believe that it is empirically adequate
> - that what the theory says about what is observable (by us) is true.[24]

Certainly, his observation that the human creature is a particular form of measurement device is not conclusive evidence for his proposed goal of science. However, in light of the fact that his opponents have offered no argument for a different goal,[25] Van Fraassen can, and does seem to, claim that attempting to develop an empirically adequate theory is an as yet unrefuted and reasonable pursuit of science. If it is the goal of science to develop empirically adequate theories, and the human organism is limited in his/her ability to examine the world (i.e. it has a limited access to the empirical facts about the world), then there is no apparent difficulty in ascribing a higher level epistemic significance to beliefs about objects that the organism can measure (observe). That is, it is not too strange to claim that there is an epistemic difference between objects we do 'see' and those that we <u>can't</u> 'see.' Thus, the epistemic significance of the distinction may be found in the goal of science.[26] Certainly, this is not intended as a deductive argument for an epistemic difference in kind between beliefs about observables and beliefs about unobservables, but it does lend weight to the claim that there is such a difference.

3. Some Problems

A wide array of literature with respect to the observable-unobservable distinction can be found in recent journals. In light of this fact, I will survey only a few of the more prominent problems.[27] A caveat: I intend to offer and defend a third, and I believe better, version of the evidence-theory distinction and, thus, have no serious interest in saving the observable-unobservable distinction as Van Fraassen asserts it.

3.1 The electron microscope eyed mutants

Only in philosophy could an objection known as 'the electron microscope eyed mutants' not be laughed out of contention. This objection belongs to Paul Churchland; he points out that it possible that the process of evolution (or some other mechanism of mutation) could give rise to people who can see as though they have electron microscope eyes.[28]

Why is the electron-microscope-eyed mutant troublesome for Van Fraassen? Van Fraassen has claimed that there are in-principle-unobservables and has sought to draw the line where something becomes unobservable on the basis of human biology and physics. Presumably he believes that no matter how the final biology and physics turns out, there will always be some unobservables. Clearly, given Van Fraassen's distinction, then electrons are unobservable. If there is the possibility of mutations that move electrons into the observable camp, then we have the first step onto a slippery slope. For this particular 'slippery slope' the mutants give us no more than the first data point for the induction to the claim that 'there is nothing which is in-principle-unobservable.' Kukla points out that there is certainly a continuum of such possible mutations, which would, in turn, provide us with a rather large number of data points for our induction. If it turns out that there is no reason to believe that there are no in-principle-unobservables, then Van Fraassen's distinction is impotent.

Van Fraassen's reply seems little more than the claim that there can be no changes whatsoever in the range of the observable. On the mutants he writes:

> In [this] case, we call the newly-found organisms "humanoids" without implying that they bear more than a physical resemblance to us. Then we examine them physically and physiologically and find that our science (which we accept as empirically adequate) entails that they are structurally

> like human beings with electron microscopes attached. Hence they are, according to our science, reliable indicators of whatever the usual combination of human with electron microscope reliably indicates. What we believe, given this consequence drawn from science and evidence, is determined by the opinion we have about our science's empirical adequacy - and the extension of "observable" is, *ex hypothesi*, unchanged.[29]

He does not seek to deny the nomological possibility of electron microscope eyed mutants (an avenue that I believe may well be worth pursuing); instead, he argues that our sciences will tell us that these mutants are no more than the equivalent of a current human with an electron microscope 'strapped' to her head. Thus, whatever the mutant sees should be treated in the same manner that we would treat a human with the scope strapped to her head. In turn, there is no change in what is observable to humans qua human, merely a combination of two epistemically distinct measurement devices.

This reply gives preference to the sort of creature we are today, ignoring the possibility of what we could be. It appears obvious that Van Fraassen has got to limit who, or what, can be a member of *our* scientific community; he does appear to argue that what is observable is relative to the community in question. If he cannot, Churchland shows that we have no reason to believe that there will be any entity which is unobservable given Van Fraassen distinction.

I believe that Van Fraassen ought to reply that the laws of nature of our world seem to prohibit the development of a human with electron microscope eyes. This is not a stretch; all we need to do is ask ourselves what sort of mutations are nomologically possible. Nowhere in our vast history can we find examples of the sort of survival conditions that would evolutionarily cause creatures to need to observe things that are of an order like electrons. But this is an avenue of defense that I will pursue no further in this paper. One can see, though, that such a reply would prevent many 'slippery-slope' arguments, including Churchland's.

To recapitulate, I argue for Van Fraassen that it is not arbitrary to divide the world up into 'observable by humans' and 'unobservable by humans,' since the goal of science could well be to provide empirically adequate theories.[30] Van Fraassen claims that the final biology and physics will tell us what is observable and what is not. Churchland offers the electron microscope eyed mutant thought experiment to show that there is no reason to believe that there will be anything that remains in-principle-unobservable in Van Fraassen's sense. Van Fraassen's reply is to claim that there are limits to who or what counts as a member of our scientific community, an argument I will pursue in some detail later. It follows that there are different sciences (and different ranges of observables) for each scientific community,[31] which likely strikes many as counter-intuitive.[32] I suggest there may be room to argue that the electron microscope eyed mutants are not a nomological possibility, and, as such, should not trouble the proponent of the observable-unobservable distinction. At any rate, the upshot of the electron microscope eyed mutant argument is that the observable-unobservable distinction can be "saved" with some form of bullet biting (demanding a rigorous definition of 'scientific community').

3.2 The Freidman Argument

Kukla claims that the argument which devastates Van Fraassen's distinction is offered by Friedman (1982). Friedman argues that Van Fraassen cannot have his cake and eat it too: he cannot consistently hold both 1- we can believe the observable consequences of our theories and 2- we can only express our theories in theory-laden language. He argues as follows:

> "The observable objects" are themselves characterized from within the world picture of modern physics: as those complicated systems of elementary particles of the right size and "configuration" for reflecting light in the visible spectrum, for example. Hence, if I assert that observable objects exist, I have also asserted that certain complicated systems of elementary particles exist. But I have thereby asserted that (individual) elementary particles exist as well! I have not, in accordance with van Fraassen's "constructive empiricism," remained agnostic about the unobservable part of the world.[33]

The idea is just this – I believe that there are observable objects such as tables, but this belief can only be expressed if I include current physics: tables are comprised of atoms which reflect light in a particular way, making them observable. Thus, when I hold the belief that there are tables, I also hold (i.e. believe) that there are a wide variety of unobservable particles (e.g. atoms which reflect light and light waves themselves). This argument, therefore, shows that it is incoherent to believe in observable entities, and our successful sciences, and not the unobservable entities postulated by our best sciences.

It strikes me that this is a dishonest charge of incoherence. Friedman seems to be overstating the import of language in our beliefs. Why is it the case that my beliefs must be formed in language at all?[34] However, rather than push a solution from the philosophy of language, I believe a less arcane solution could be proffered.

According to Kukla, the question put to Van Fraassen is simply this: can you present a story about how it is possible to believe the observable consequences of a theory that does not carry with it belief in the rest of the theory? Certainly Van Fraassen can hold that language is theory-laden, and that one need not believe *all* the consequences of our best theories; it is surely possible to hold that the observational consequences of a theory are believable, while the rest of the theory is merely a useful fiction. The proponent of the observable-unobservable distinction is free to use any arbitrarily selected scientific theory to express her beliefs about the observable. Thus, she can divorce her beliefs about the observable data set and the theory with which she chooses to express these beliefs. Thus, the anti-realist does not believe the observable consequence class of *a* theory, she believes the observable consequences of *some* theory. If this is possible, and certainly it is,[35] then the anti-realist does not have to carry the theoretical postulates of our current theories into her belief set along with the observables. Now, the anti-realist is free to believe in tables (an observable consequence of some physical theory) but not atoms (part of a particular theory), since she can describe her beliefs in tables using any arbitrarily selected theoretical framework.

3.3 Incoherence Arguments

Alan Musgrave,[36] among others, claims that the observable-unobservable distinction is incoherent. His argument is as follows:

1. The observable-unobservable distinction is a theory, call it T.
2. The anti-realist can only believe the observable consequences of a theory.
3. T contains critical statements about unobservables (e.g. electrons are in-principle-unobservables).
4. The anti-realist cannot believe these critical statements.

Therefore,

5. The anti-realist cannot believe the whole theory T.
6. If the whole theory T cannot be believed, then there can be no meaningful distinction.

Therefore,

7. The anti-realist can never form a coherent observable-unobservable distinction.

Clearly, if Musgrave is right, then the observable-unobservable distinction is of no use to the anti-realist (its only real proponent).

The solution to this problem, according to Kukla,[37] lies in Musgrave's claim that the anti-realist must believe in unobservables to make the distinction (*i.e.* attack premises 3 and 6). Kukla paraphrases van Fraassen on this point as follows:

> Suppose that B exists and is observable. Then if theory T entails that B is *not* observable, T will fail to be empirically adequate. So if we believe T to be empirically adequate, we have to believe either that B doesn't exist or that it's unobservable – equivalently, that *if* B exists, then it's unobservable. Musgrave is right when he claims that van Fraassen can't allow himself to believe that B is unobservable. But there is no reason why he shouldn't believe that B is unobservable if it exists. What the anti-realist refuses to believe is any statement that entails that theoretical entities exist.[38]

This seems an adequate reply to Musgrave's charge of incoherence.

4. Shaky Ground

We have seen that so far there has been no deathblow dealt to the observable-unobservable distinction. While there may be some problems to be worked out and some bullet biting to be done, there is reason to believe that the observable-unobservable distinction, as it stands, is an acceptable distinction. However, I believe there is one more criticism that should be considered.

5. The Serious Problem

Against Fodor I argued that the anti-realist should not bank on his distinction: Fodor requires that our theory of perception is not underdetermined. Oddly, it seems van Fraassen is open to the same charge. Recall that he expects the final biology and physics will tell us definitively what human's can observe. This requires that we <u>believe</u> whichever theories are critical to this determination, since what human's can observe is not itself merely an observable (if it were we could merely <u>accept</u> the final physics or biological theories). Thus, van Fraassen's distinction appears committed, just like

Fodor's, that some theories are not underdetermined. It follows that the anti-realist who wants to make use either of these distinctions must provide more argument to reconcile belief in underdetermination and a realistic take on some theories. With this being a problem for current versions of the evidence-theory distinction, I will now turn to another suggestion for the distinction.

3. The Theory-Evidence Distinction

As I alluded earlier, I believe that the theory-evidence distinction can be formulated with a basis in perceptual entitlement. Entitlement is part and parcel of an externalistic account of warrant, which makes my distinction suitably different from those distinctions drawn by Fodor and van Fraassen. It remains to be seen if this change in accounts of warrant will make for a better distinction. In this section, I intend to offer my theory-evidence distinction and provide a rudimentary defense of it. In Chapter 2, I argue that one's theory-evidence distinction must have certain characteristics in order to justify belief in empirical equivalence, so, following my exegesis the theory-evidence distinction I favor, I will argue that my distinction has these characteristics. Of course, my distinction has not weathered the scrutiny either of the other two have endured, and I'm sure that critical problems will have to be resolved before it can stand as a genuine option for the theory-evidence distinction. However, with this said, I proceed to the task of outlining the distinction.

1. An Argument For Entitlement

When I was but a small child, I was faced with a vast array of experiences. Like you, and many others, I was able to take-in these "proto"-images, sounds, smells and so on.

Of course, I had not yet formed any theories, but I had reason to take these sensations as veridical. It seems in fact that I would have been unable to form any beliefs about the world if there were no reason for me to take these experiences as veridical. Further, I would have been unable to understand the world sufficiently in order to have learned to reason as we humans do. Of course, these reasons would be cognitively closed to me, and still are – I am unable to express the justification for beliefs based on my perceptual experience.

What is the nature of our justification for perceptual beliefs? It seems clear that children and animals are "justified" in their perceptual beliefs – at least to some degree. While it is impossible for these to express their justification, it seems to be the case that they do have warranted beliefs about their perceptions. This sort of justification is best called externalistic, as the justification we have is not accessible to us (i.e. we cannot express our justification, since we do not know that we know). In such a case our justification comes from our being connected with the world 'in the right way.'

Further, as noted earlier, I have learned to be critical reasoner, and as such I would have had to understand the world sufficiently to become so. I could not have understood the world as I do, were I unable to have accepted some of my perceptual beliefs as veridical.

2. The Manifest Image Defined

So far I have claimed that there may be reason to believe we have externalistic justification for some beliefs. These things for which we have externalistic justification are the objects of perception, or, rather, the objects of the manifest image. The manifest image is made up of those objects that appear to me by means of my perceptual systems and are the foundations of my perceptual beliefs. I am inclined to say that things like tables and cats are objects of the manifest image, whereas things like atoms or logical entailments are not. However, this would be to speak too quickly. I must be careful in my description of the objects of the manifest image; while appearances of cats and tables are the objects of the manifest image, the category, the name, 'cat' or 'table' certainly does not belong to the manifest image. In fact, it may well be impossible to describe the objects of the manifest image (MI) in language, since our language is filled with theory. However, I can present a sense of the MI: it does not include the 'table' or 'cat' before me, but it does include a brown, rectangular, four-legged object on which my small, furry four-legged, meowing, critter is perched. Of course, to describe the MI I've used all sorts of labels, but one should be able to understand the idea: the objects of the MI are those objects that I perceive immediately when I am observing.

3. The Role of Entitlement in Science[39]

While I have provided some intuition pumping for a version of externalism with respect our justification for belief in the manifest image above, I have yet to provide any good reason to think that it plays a role in scientific explorations. A rational reconstruction of science reveals that science is primarily, if not exclusively, an internalistic pursuit: the scientist seeks to acquire justification for their beliefs and theories, and he does not accept that he knows things simply by being in the right sort of relation to the world. The scientist is very concerned with being able *to provide* a justification for her beliefs. With this said, I do not believe the former is entirely true.

While some scientists are concerned with the mechanisms and functions of our perceptual systems, it seems that science on the whole is not concerned about justifying its acceptance of its initial position, the manifest image, as veridical. Of course, science, and consequently the scientist, is concerned with providing theories that begin in the manifest image – we would need no theories to describe the world if there were no perceived world. Also, it is considered well accepted that the manifest image is to be replaced by the scientific image: beliefs in tables are replaced by beliefs in particle clouds and atomic interactions. However, science, regardless of its end position with respect to actual existents, forms its theories with a starting point in the manifest image. Most importantly, why is it that no scientist is ever concerned that belief in the manifest image is, or is not, justified? There are a number of possible explanations, some of which are as follows: 1- Scientists are skeptics with respect to the manifest image, 2- scientists hold an externalistic account of justification with respect to the manifest image, or 3- scientists believe that the coherence of their model is all that is critical to a description (or explanation) of the world. I believe that the best explanation for scientific practice is the second of these options.[40]

The best explanation for the fact that scientists do not ask if there are tables, but, rather, of what tables are composed or how we perceive tables at all, is that they hold that such a question is cognitively closed to us and the others are not.[41] Given that science is in the internalistic justification game, and that this cognitive closure bars an internalistic account of justification for the manifest image, the scientist could become a skeptic with respect to the manifest image. However, I do not believe that a skeptical position would explain the activities of scientists: it would be rare, at best, to find a scientist who believes her passionate pursuit of quantum mechanics is done to make up a nice story to tell her grandchildren about a world she does not believe exists. Certainly it is more likely that she believes that her senses are justified (though such justification is cognitively closed to her), and that her quantum mechanics will be used to explain (or describe) those objects and events to which she and her grandchildren gain access through perception.

It could be the case that the scientist believes that integral to science is merely the coherence of the scientific image. In this case, the scientist would have no reason to justify belief in the manifest image, since it is of no import to the coherence of his story. However, this option seems equally divorced from scientific practice as the option that scientists are skeptics with respect to the external world. Of course, scientists are concerned with coherence, but not the point that they are happy to claim that all they do is tell coherent stories – ones which may, or may not, have anything to do with the world around us, or the truth for that matter. If the scientist reverts to the claim that the story must be coherent but include the objects and events of the manifest image (saving himself from a ridiculous position with respect to the operation of science), then he is left with no explanation for the fact that scientists do not concern themselves with justifying belief in the manifest image.

Therefore, it seems that best explanation for scientific practice with respect to the manifest image is a limited externalism. I say limited externalism, since no one is happy to say that they have strong justification in the belief that the stick in the water is actually bent. This is why the notion of entitlement is an excellent fit for science. Entitlement is an externalist account of warrant where it is acknowledged that our theory can 'overthrow' our justification, garnered by externalistic means.[42] Even though such a limited externalism is compatible with reductionistic picture of science (such as Rorty's, where the manifest image reduces to the scientific image), it still provides a place, a special role, for the manifest image – our entitled beliefs must be overthrown by justified scientific beliefs (i.e. our entitled beliefs occupy a position with a certain epistemic privilege and priority). This limited externalism seems to fit nicely with scientific practice as well. Certainly, scientists do not believe that they were unwarranted in their perceptual beliefs about the manifest image in their childhood, but now they hold theoretically justified beliefs that tables are mere particle clouds.

4. The Evidence-Theory Distinction

The evidence-theory distinction that follows from my argument for perceptual entitlement and its role in science should by now be transparent. The evidence for any theory are those objects to which we are perceptually entitled – the objects and events of the manifest image. Such objects and events are justified externally, by our proper connection with the world (and, thus, are objects of the evidence category). Even in cases where (a là Rorty) our externally justified beliefs appear to be overthrown by theory (supported by internal justifications), the distinction remains: we have reason to believe our perceptual beliefs in the absence of theory, and even if theory can sometimes usurp such justification it does not change the fact that the manifest image remains differentially justified.

Thus, the anti-realist can now form an evidence theory distinction based on the genesis of our justification in our beliefs. Beliefs based on perceptual objects are externally justified and count as evidence for our theories. Beliefs based on theory are internally justified. Thus, it would seem, there is an epistemic difference in kind between the objects of our perception and the objects of our theory: a difference in justification.

4. Some Clarification With Respect To The Manifest Image[43]

Remember when you first changed schools? For the first few weeks the faces in the hall are all a bit blurry and seem to have very similar features. Of course, after we have settled in and get to know people, the differences in facial features are more obvious and clear. Are the blurry faces part of the MI? I believe that the answer is no.

1. The Careful Observer Under Optimal Conditions

The reason that the faces appear blurry is because our observer is paying attention to many different things (e.g. how to get to class, not bumping into the obvious school bully, not looking like a fool, an so on). If our observer were to carefully look at the faces of passers by, he would notice that the faces are not, in fact, blurry or oddly similar. Thus, I think it is safe to say that the objects of the MI are those appearances that would appear to a careful observer under optimal conditions.

2. The Doctor and The Mole

The next question seems to follow. If we are talking about a careful observer under optimal conditions, then who better than the doctor who is intently studying her patient's mole to determine whether or not the mole is cancerous? Doctor's with adequate training have little trouble identifying the cancerous nature of some moles; further, it seems that this is a simple observation – cancerous mole, not cancerous mole. However, I think that the identification of the mole as cancerous is certainly more than simple observation: a good deal of theory is needed before such an assessment, that the mole is cancerous, can be made; recall the doctor must have adequate medical training to make such a judgement. The careful observer could, of course, describe the mole in great detail – but the judgement that the mole is cancerous relies a great deal on our medical theories about the development of cancer in certain types of moles and on theoretical predictions about the future state of the mole. While the mole's 'browness' and 'shape' are objects of the manifest image, its cancerous nature is not.

Certainly, in this subsection, there is little in the way of argument; hopefully, however, a better understanding of what I intend when I say manifest image has been gained from these examples.

5. The Lack of Normativity Problem

My evidence-theory distinction is drawn by pointing out that it is a descriptive fact that science, while in the business of providing justification for beliefs, does not bother to justify belief in the manifest image, arguably the starting point of science. I argue that the best explanation for this fact is their belief in the manifest image is provided by a different sort of justification, an externalistic justification. This creates two tiers in science – the MI level, which is justified externalistically, and the theory level, which is justified internalistically (in the manner science seems to operate). The problem is that the mere fact that this seems to be the case provides no reason that this *should* be the case. That is, why should science be divided in two in such a manner: why not justify beliefs externally at both levels or internally at both levels? Couldn't we justify our belief in atoms by an appeal to our proper connection with the world, or why couldn't we offer interalistic reasons to believe in the MI?

Off the cuff, I don't think that scientists are in the business of simply claiming that they know about unobservables because they are in the correct relation with the world, they honestly think they have to prove and support such claims. Also, I don't think that it is possible to offer an internalistic account of the MI. However, I think that this problem, the demand for a normatively motivated distinction (not simply a descriptive one), can be adequately solved by an appeal to the success of science. Neither the realist, nor the anti-realist, denies that science is successful. Thus, in this case, it seems appropriate to claim that science should continue to utilize a method (in this case a distinction) that is successful, in order to ensure future, and continued, success. I believe that this is sufficient, at least for now, to provide normative force to the distinction.

6. The Language Interface Problem

Recall that I suggested that words like 'table' and 'cat' do not belong to the manifest image. This presents a problem for the use of evidence (objects of the MI) in the context of theory building. It is commonly believed that theories are supported by their empirical content (i.e. the observational consequences of the theory). However, if it is the case that we cannot use language to describe the MI, how can we formulate the following necessary conditional: if T, where T is some theory, then O, where O is some object of the MI? It doesn't appear that we could formulate these statements if O's cannot be put into language. This is a serious problem for many accounts of science that on the confirmation or falsification of Os, the Hypothetico-Deductive model of science for example. Of course, if this problem persists, it doesn't undermine the distinction that I've made, it merely makes it a useless distinction if we want to hold onto various accounts of science.

There may be a solution in providing a second distinction: if there is a difference between descriptors (words used to simply describe the MI – maybe words like 'red') and classifiers (words that carry a good deal of theory with them – words like 'cancerous mole'). It may be possible to develop a language of descriptors for use in describing the MI and forming the conditionals required for a variety of accounts of science. I will pursue this solution no further here, since it would involve a thesis unto itself; suffice it to say that the language interface problem may seriously reduce the pragmatic value of my distinction.

7. The Concession To A Realistic Reading Of At Least One Theory

My objection to the earlier distinctions, both Fodor's and van Fraassen's, was that they both require that at least one theory is not underdetermined (our theory of perception for the former, and the final biology and physics for the later). This is not the sort of concession that the anti-realist should make if she needn't. Why allow the realist a foothold? My distinction does not rely on some scientific theory turning out in a particular way, nor does it require that a particular theory be believed (to be true). This seems to be good reason for the anti-realist to accept my distinction over those of the others.

8. Properties of the Evidence-Theory Distinction

So far I have presented an, albeit inchoate, evidence-theory distinction for the proponent of underdetermination. I argue in Chapter 2 that the evidence-theory distinction must have certain characteristics; thus, I will attempt to demonstrate that my evidence-theory distinction has these characteristics by means of the Churchland's objection to van Fraassen's distinction.

1. A Common Issue (The Electron-Microscope Eyed Mutants Revisited)

The evidence-theory distinction that I have drawn is similar to Van Fraassen's in that it is anthropocentric: those objects that count as evidence are those objects which appear in we human's manifest image. This leaves the properties of the distinction open to criticisms such as Churchland's – the range of observation is not, nor are the boundaries of our scientific community, fixed.

Recall, Churchland's challenge to Van Fraassen: it is possible that the human organism will change significantly, so as to become a different sort of measurement device. This challenge could be damaging to the utility of my distinction, which also relies on an anthropocentric and static analysis of perception (observation). There is difference in how the objection affects my distinction versus van Fraassen's. Against van Fraassen, Churchland's objection is intended to show that it is possible that no entities remain unobservable; Churchland's objection cannot usurp my distinction, since if there were a change in the objects observable to us, then there would simply be a change in the contents of the MI. Of course, I renew my objection that the burden of proof is on Churchland to demonstrate that mutations like those that would be damaging to both Van Fraassen and my distinctions could occur.

Furthermore, if we are to distinguish evidence and theory on the lines that I have detailed, there is a second consideration. Later, in Chapter 2, it will become evident that I need one of the properties of my evidence-theory distinction to be that what counts as observable remains static (over time) and anthropocentric. Recall that essential to

Churchland's objection is that mutations can cause our range of observations to change. I believe that the distinction I have drawn, in conjunction with a Principle of Credulity, can remove the threat of changes in the range of observables that may make it impossible to generate empirically equivalent rivals. My reply works by demonstrating that if such mutations were to occur, then we would not have a change in our scientific community, but the rise of an entirely new science. Since this new science would be distinct from our current science, we would have no reason to suspect there is a continuum of change, which threatens to make everything observable, possible in our sciences.

Recall, evidence in science is based on those objects to which *we* have external justification (i.e. these are the objects that count as evidence for our theories).[44] Thus, our theories (be they underdetermined or not) have been, and are, based on the objects of human perception to date. Since there have been no changes to our perceptual mechanisms of the sort postulated by Churchland, we have reason to believe two things: 1- there will never be such changes, and 2- if there were such changes they would count as a radical change in our science. The former point is akin to the objection I made earlier – no such mutations could occur – and, as I have already stated, requires elaboration beyond the scope of this paper. The latter, however, requires further analysis.

To illustrate the claim that electron-microscope-eye mutations represent a radical change in our science, it is instructive to consider a similar objection. Let us say that our scientists are suddenly confronted with a race of aliens that can see, naturally, as though they have electron-microscope-eyes. Is our scientific community required to now take these aliens' observations as evidence in theory construction? I believe the answer is no, and that the aliens represent an, pardon the choice of words, alien scientific community.

William Rowe argues that in the face of religious experience we should accept a principle of credulity. The principle is as follows: "if a person has an experience which seems to be of x, then, unless there is some reason to think otherwise, it is rational to believe that x [is veridically experienced]."[45] Certainly this is a principle that our scientists are willing to accept – among other examples theorists take the experiences of researchers to be veridical in the formulation of their theories.[46] This seems to apply to our scientific community with respect to the aliens. The aliens are, arguably, persons who have an experience of the particles postulated by our best sciences. And, we have no reason to think that they do not experience these particles. Thus, we ought to accept these aliens' perceptions as evidence.

However, the principle of credulity seems to be founded on at least one critical assumption: we have a way of knowing what would count as reason to doubt the aliens' experiences. It seems that we are willing to accept the aliens' perceptions as veridical *because* they match up with the postulates of our best sciences. If their perceptions were foreign to our scientific theories, they saw 'fairies' instead of atoms, would we then have reason to think that there perceptions were not veridical? Certainly, we would think that they may be 'having us on.' Thus, it seems that our own theories do not provide us with what might be a reason to doubt the aliens, unless we presuppose that realism is true – which is, of course, still in question at this point. Also, we would have no reason to doubt the aliens experiences by the use of our own senses either, since our aliens would act no differently from us with respect to physical objects if their eyes saw all matter to be composed 'fairies' instead of electrons. So, it is the case that neither our scientific theories nor our own observations can provide us with reason to believe or disbelieve the aliens' experiences. Thus, it seems we cannot know what counts as a reason to doubt that the aliens' experience is veridical; it follows that we cannot apply the principle of credulity to the aliens, and we should not include their experience as evidence for our theories.

Let us return to the case of mutations. Does the same argument hold for mutations as it does for aliens? Let us assume that some of the human population mutates so that they see as though they have the electron-microscope-eyes. Should we now count the mutants and non-mutants as members of the same scientific community? It seems that the non-mutants still have the same reasons to not exercise the principle of credulity: they have no way of knowing what would count as reason to doubt the mutants. The same seems to be true for the mutants. Imagine a member of each community talking about a table. One will describe the table as a wooden object on which to place a glass of water; the other describes the table as a similarly configured collection of atoms, with a variety of interesting atomic properties, on which to place, another collection of atoms, with an interesting, but different set of atomic properties. Neither has any way of knowing what would count as reason to doubt the other's perceptions, unless they both assume that realism is true, and their perceptions happen to 'match up' with their best theories.

Therefore, it seems that the mutants actually make up a different scientific community from our current sciences, since neither community has reason to believe that the perceptions of the other are veridical. Also, the distinction between evidence and theory still holds, since the mutants and the non-mutants are both differentially justified in their beliefs about their perceptions (these are justified externally) and their beliefs about their theories (which remain internally justified). Thus, there is no continuum of mutations or changes in the scientific community that can form a 'slippery slope' to the impossibility of an important property of my distinction – that for each scientific community there is a static and anthropocentric set of observables. It remains possible, however, that there is some scientific community for which there is no set of unobservables; but this fact does nothing to harm the claim that for our community there are some things that cannot be observed and there is no reason to believe that this will change.

2. The Properties of the Distinction

I will argue later that for empirical equivalence to be ubiquitous the evidence-theory distinction must have the following properties: 1- what counts as evidence must be limited to humans' light cone, 2- what counts as evidence to a scientific community must be limited to the members of that community, and 3- the range of observables must be invariant within a given scientific community. These must all hold so that there are some objects that count as the sort of thing that are normally considered 'observables' (evidence) for a given scientific community that can never be actually observed by members of that community.[47]

In the preceding argument against the electron-microscope-eyed mutants, we saw that what counts as evidence for a given scientific community is in fact invariant within that community, since each community has no reason to accept the evidence of another community.[48] Also, the range of 'observables' is limited to the manifest image for any community; the observables of a given community never change, since they are limited to the externally justified beliefs of that community, according to previous argument.

It, therefore, remains only to argue that it must be the case that evidence is limited to the scientific community's light cone. While there may be many observables that may be counterfactually counted as evidence for a theory (e.g. if I were at point x, in the distant reaches of the universe, I would be able to observe y), the argument I will advance later requires that such counterfactuals are not evidence for a theory (i.e. y is not an observable if it is at the point x noted above). For example, there may be a chair in the distant reaches of the universe, which, were we there, we could observe; for my argument to be successful, it must be the case that this chair does not count as evidence (is not an observable) for our scientific community. Here again, I think that I would like to argue that scientific practice demonstrates that such counterfactuals do not count as evidence. Certainly, there are few scientists who would deny that they assume that the objects they would observe 'were-they-there' are similar to, if not the same as, those we have access to within our light cone. However, when skeptical worries are raised, such as claims that the distant reaches of the universe are so very different from our immediate spacio-temporal location, most scientists will ignore you or deny that it matters. Such a fact suggests that objects to which we could never have access have little impact on what scientists count as evidence. Moreover, Einstein's theory of relativity tells us that it is impossible to observe objects outside of our light-cone, making it the case that there are objects that we simply cannot observe. Further, there seems to be no reason that they should accept these objects as evidence.[49]

9. Conclusion

I believe that I have argued that there are several problems with both Fodor's and Van Fraassen's evidence-theory distinctions. It seems that there is at least another candidate for the distinction, and evidence-theory distinction based on belief formation. Science indicates that the best explanation for scientific practice with respect to justification of the manifest image is a form of externalism. This externalism provides the basis for a distinction between 'observables' and theory. A partial defense of this distinction demonstrates some properties that will make it possible that empirical equivalence is ubiquitous.

Chapter 2 - The Ubiquity of Empirical Equivalence[50]

0. Introduction

The primary focus of this chapter will be the concept of empirical equivalence, and the ability of Andre Kukla's method for generating empirical equivalents which satisfy the EE criterion. In order to show the ubiquity and pernicious nature of empirical equivalence, I will consider arguments both for and against the proper satisfaction the EE criterion. Larry Laudan and Jarrett Leplin, who (here) defend realism, argue that empirical equivalence is not the hobgoblin that the scientific anti-realist claims; they offer an argument (the defeasibility argument) intended to show that the EE criterion is never properly satisfied. Laudan and Leplin suggest a second version of the defeasibility argument in a later paper; I will consider this argument as well. I argue that there are sufficient weaknesses in both arguments to allow for the satisfaction of EE. Further, I will present Kukla's method for generating empirically equivalent rival theories, in order to show that empirical equivalence does arise; I will also show that empirically equivalent rival theories of the sort generated by Kukla's method are sufficient to satisfy the demands of the EE criterion.

1. The Concept of Empirical Equivalence

There are at least two commonly accepted characteristics of empirical equivalence: the rival theory must be empirically indistinguishable from the original theory, and the rival theory must be logically incompatible with the original theory. The latter seems straightfowardly understood.[51] However, since logical incompatibility does not appear to be a necessary characteristic of empirical equivalence, I will put it aside for the remainder of the paper. The former characteristic somewhat more controversial: while all appear to agree that this characteristic is necessary, there is serious disagreement about what it is for two theories to be empirically indistinguishable. I will return to this controversy in section 7, but, for now, it will be sufficient to say that two theories are empirically indistinguishable if they have the same empirical consequence class. The empirical consequence class of a theory is the set that is composed of all the observational entailments of that theory.

2. Laudan and Leplin's Defeasibility Argument[52]

At the heart of Laudan and Leplin's argument is the concept of empirical indistinguishability: they argue that the empirical consequence class of a theory changes with advances in science.[53] According to Laudan and Leplin the fact that science evolves (advances) is sufficient to show that apparent cases of empirically equivalent rival theories do not properly meet the demands of EE.[54] In this section, I will attempt to recreate Laudan and Leplin's defeasibility argument; discussion of the argument will follow in Section 3.

Recall that empirical equivalence rests in part on empirical indistinguishability. A reasonable analysis of what it is to be empirically indistinguishable is as follows: for two theories to be empirically indistinguishable they must have identical empirical consequence classes. In order to determine the observational entailments of a theory, we must, to the best of our ability, identify the theory's total consequence class (given that the empirical consequence class is a sub-class of the logical consequence class, or total consequence class, of a theory). A theory's total consequence class is composed of <u>all</u> of the logical entailments of the theory. It should be obvious that once the total consequence class of a theory is determined, we can only determine the empirical sub-class of a theory's entailments if we have an understanding of how such a sub-class is determined. According to Laudan and Leplin, the method by which a theory's empirical consequence class is determined demonstrates the fact that EE cannot be properly satisfied.

Of course, to determine the empirical consequences of a theory, we must have an analysis of what is empirical (*i.e.*, observable). Laudan and Leplin claim that it is uncontroversial that the range of observation is variable. They call this thesis VRO (the Variable Range of the Observable). This thesis amounts to the claim that there is change in what is observable as science advances: the "line" that cleaves the observable from the unobservable moves back and forth with advances in science.[55] An example might be the advent of the electron microscope: according to proponents of VRO, we can now observe many phenomena, which were previously unobservable (*e.g.*, atoms). If VRO is warranted, then a theory's consequence class is determined contextually: elements of a theory's empirical consequence class are dependent on advances, with respect to observation, in science.

Next, it is well accepted that a theory's empirical consequence class cannot be determined without the use of auxiliary hypotheses (imagine trying to determine the observational entailments of any theory without a theory of optics[56]). Thus, Laudan and Leplin claim that there are two further theses that are uncontroversial with respect to the determination of a theory's empirical consequence class: NAP (the Need for Auxiliaries in Prediction), and IAA (the Instability of Auxiliary Assumptions). For justification of

NAP Laudan and Leplin appeal to the holist's thesis: we cannot determine a theory's empirical consequence class without conjoining it with available auxiliary hypotheses, since the empirical content of a theory cannot alone be determined by the theory in question. As W. V. O. Quine observed, no theory faces the tribunal of sense experience alone. Justification for IAA can be readily found in scientific practice: through empirical methods and advances in thought, our auxiliary assumptions are developed, rejected, and replaced. The combination of these two theses serves to show that science "evolves." Since science evolves, it is possible that two theories that were empirically equivalent before an advance will cease to be so afterward. Imagine two theories, T_1 and T_2, which have the same empirical consequence class at time t_0. Notice, of course, it is not really the case that T_1 and T_2 have identical empirical consequence classes, it is (T_1 & A_0) and (T_2 & A_0), where A_0 is the set of the best available auxiliaries at t_0.[57] All Laudan and Leplin are asking us to believe is that it is *possible* at time t_1 that the empirical consequence classes of (T_1 & A_1) and (T_2 & A_1) will diverge, where A_1 is the set of best available auxiliaries at t_1.

The conjunction of these three theses (VRO, NAP, and IAA) demonstrates that our determination of the empirical consequence class of any given theory is contextual: we can only determine a theory's empirical consequence class within the context of the current, or relevant, state of evolution in science. It follows trivially that our judgements of empirical equivalence are contextual. According to Laudan and Leplin, if empirical equivalence is contextual, then EE is not properly satisfied: we must defer to scientific practice to determine if empirical equivalence obtains between theories. Therefore, "the limitations on theoretical understanding that a defeasible empirical equivalence imposes need not be grievous."[58]

3. Problems with the Defeasibility Argument

This version of the defeasibility argument contains several serious flaws, which, in combination, spell its demise. Laudan and Leplin's use of VRO, NAP, and IAA is far from uncontroversial; furthermore, their take on IAA is itself questionable. Moreover, I believe that, in the end, the success of the defeasibility argument rests on a straw man account of the anti-realist's reading of the EE criterion. Finally, I will also show that the defeasibility argument can be obviated if Kukla's method for generating empirically equivalent rivals works.

1. Problems with Laudan and Leplin's use of VRO

While I believe that a version of the defeasibility argument without VRO is equally strong, it is of some note that VRO itself is not nearly as uncontroversial as Laudan and Leplin claim. Kukla notes that Laudan and Leplin's version of the range of observables begs the question against any who believe an analysis of observation is inherently limited by physiology, such as van Fraassen or myself. If the range of observation is physiologically limited, then the range of observation is unaffected by changes in technology, counter to Laudan and Leplin's suggestion. Laudan and Leplin do offer an argument, in a footnote, for their version of the observable/unobservable distinction. They claim that our concept of observation, our judgement of what is or is not observable, has nothing to do with our physiology: only once we have determined what it is to be observable can we decide if it is limited by physiology.[59] Three possible replies seem warranted. First, they need to advance more argument than this to show that VRO is uncontroversial, as they claim. The second reply, suggested by some comments in Kukla (1998), is that if their argument is sound, then VRO is really an auxiliary assumption (hypothesis). Laudan and Leplin's argument appears to be nothing more than the claim that we require a theory of observables, which, in the end, may or may not include a physiological limitation. If this is the case, then VRO is no more than a part of our theory of perception. Clearly, our theory of perception is a critical scientific hypothesis. Thus, VRO does nothing to demonstrate the contextuality of empirical equivalence independently from the combination of NAP and IAA. The final reply is, as was noted earlier, that VRO begs the question against the versions of observation that hold observation is limited by physiology. The VRO presupposes that the range of observation cannot be static for a given scientific community, which is exactly what van Fraassen's, as well as my own, theory of what it is to be observable contend. By now, it should be obvious that VRO, as it stands, cannot be made to help the scientific realist (hereafter 'realist'); however, consideration of NAP and IAA remains.

2. Critical problems arise for the defeasibility argument with the use of IAA and NAP

Laudan and Leplin's use of NAP-IAA conjunction is odd, to say the least. Recall that they use this conjunction to show that evolution in science (and, consequently, theories' empirical consequence classes) is dependent on changes in auxiliaries. Further, they claim that this sort of evolution provides the possibility that we will discover that apparent cases of empirical equivalence are empirically adjudicable. However, Kukla, using a strategy from Boyd,[60] shows that auxiliaries have no significant place in the empirical equivalence debate. Furthermore, Laudan and Leplin's take on the nature and use of auxiliaries is question begging, and even if it is not, their use of IAA is insufficient to show that EE is not properly satisfied.

Laudan and Leplin accept that a theory's empirical consequence class can only be generated by conjoining the theory and the available auxiliaries. So, the empirical consequence class of T, some scientific theory, cannot be generated on its own; it can only be generated by combining T with A_0, where A_0 is the set of best available auxiliary hypotheses. Thus, there is a strong sense in which the auxiliaries really are just a part of the theory. Kukla uses Boyd's notion of total sciences[61] to avoid the problem the defeasibility argument creates for empirical equivalence:[62] even if two apparently equivalent theories' empirical consequence classes diverge after a given evolution in science, we do not have a resolution of the equivalence. What we do have after the evolution is two new theories, the survivor of which may have an empirically equivalent rival, the new rival will simply not be the one we had previously imagined to be the empirical equivalent, but that is nothing the anti-realist would deny.[63] For example, it is not the case that T_1 and T_2 are the empirically equivalent theories, since they have no empirical consequences on their own, it is the case, however, that $T_1\&A_0$ is empirically equivalent to $T_2\&A_0$ at time t_0. Let's say that at time t_1, $T_1\&A_1$ is no longer empirically equivalent to $T_2\&A_1$, and that $T_2\&A_1$ is no longer empirically adequate. It is still possible that $T_1\&A_1$ has an empirically equivalent rival in $T_3\&A_1$.

Laudan and Leplin, however, claim that the equivalence of T_1 and T_2 is resolved after an evolution because $(T_1 \& A_1)$'s empirical consequence class is no longer identical to

(T_2 & A_1)'s. But as noted above, it is simply not the case that T_1 and T_2 were ever empirically equivalent, since they have no empirical consequence classes without auxiliaries, and, as such, are not the sorts of things that could be empirically equivalent. Furthermore, it is entirely possible that the *new theory* (T_1 & A_2) will have an equivalent; whether or not T_2 is part of this equivalent is of little import. If there is some way to guarantee that there will be such a new rival, then the satisfaction of EE is not at all jeopardized by auxiliary-type evolutions in science. I believe, as does Kukla, that his method can guarantee an empirically equivalent rival for any empirically adequate total science. If we are talking about total sciences, then changes in auxiliaries have no significant effect on the empirical equivalence debate, since they have no capacity to help us adjudicate apparent equivalences of total sciences, each of which contains its own auxiliaries.

Laudan and Leplin believe that a theory's total consequence class contains conditional statements that have as their antecedent some possible auxiliary and as their consequent the observational entailments of the theory with this named auxiliary. This allows them to separate the auxiliaries from the theory, and allows them, in some sense, to talk about the empirical content of a theory. On this view, it is possible that T_1 and T_2 are empirically equivalent, since we simply insert the best available auxiliaries into the theory to determine its empirical consequence class given a specified set of auxiliaries. However, even if we were to accept that the theory T_1 has, in some sense, an empirical consequence class of its own, there are still two serious problems for the defeasibility argument. The first of these problems is that Laudan and Leplin's use of auxiliaries is question begging; the second problem is that even if we grant them their question begging version of auxiliaries, the defeasibility argument fails to show that EE is not properly satisfied. I will consider each of these problems in turn.

Recall, again, that Laudan and Leplin's argument requires that it may be possible to resolve the apparent empirical equivalence between T_1 and T_2, since it is possible that the empirical consequence classes of (T_1 & A_1) and (T_2 and A_1) diverge. Notice that they require that T_2's empirical equivalence to T_1 is determined using the same set of auxiliaries. Presumably, Laudan and Leplin believe that auxiliaries are in some way independently confirmed. However, if auxiliaries are theories, and surely they are, then the anti-realist need not concede that it must be (T_2 & A_1) which is equivalent to (T_1 & A_1), it could just as easily be (T_2 & A_3), where A_3 is some other set of auxiliaries. The only way we can interpret Laudan and Leplin's demand that we use the same auxiliaries for both theories, is under a realistic view of auxiliaries (that they can be independently confirmed, do not have empirically equivalent rivals of their own, and so on), and, of course, this would beg the question. Even if it does not beg the question, it is an undermotivated claim that there is some particular auxiliary, which, when independently confirmed,[64] will adjudicate apparent rivalries.

Laudan and Leplin face the further problem that, even if we concede their realist take on the confirmation of auxiliaries, they still cannot show that EE is not properly satisfied. Changes in auxiliaries may defeat apparent cases of empirical equivalence; however, if we have a method for generating and guaranteeing an empirically equivalent rival for any theory, despite evolutions in science, then EE remains satisfied, since it merely requires that every theory has at least one empirically equivalent rival theory. This will be true, regardless of realism with respect to auxiliaries, as long as we believe that there is no auxiliary, independently confirmed, which alone confirms a particular theory and that particular theory alone (*e.g.*, auxiliary A_{101} is 'theory T_{12} is the only empirically adequate theory'). However, Laudan and Leplin offer no reason to believe that any auxiliary such as A_{101} will ever be available to our sciences. In the absence of such an argument, and the presence of Kukla's method for generating empirically equivalent rivals (discussed in section 5), which is able to generate the type of rivals required to satisfy EE, Laudan and Leplin appear to have failed to undermine our notion of empirical equivalence.

3. Laudan and Leplin's version of the EE criterion is stronger than the anti-realist requires

It should be apparent that Laudan and Leplin's argument hinges on the claim that EE is held out as a formal point: it is not the case that the EE criterion can be satisfied temporarily, it must be satisfied permanently, otherwise empirical equivalence cannot motivate a 'transcendent' skepticism. This suggests that Laudan and Leplin believe that certain realizations of empirical equivalence are unable to properly satisfy the EE criterion. They claim that "EE [is unsatisfied] if, to retain [empirical equivalence] we are forced to have recourse to changing rivals."[65] Thus, they would remain unaffected by my claim that 'EE can be properly satisfied, even in the face of a realistic account of auxiliaries, since it is possible to generate another empirically equivalent rival after each resolution of an apparent rivalry.' There appear to be two possible replies, and I will consider each in turn. First, if we agree that science evolves, we must accept the claim that our theories were and are, at least in part, wrong. There seems to be no reason to believe that we could not be equally wrong about any given theory's empirically equivalent rival. I see no reason that we are not allowed to change the rival theory in much the same way we are allowed to change the theories the realist asks us to accept. It is not enough to claim that EE cannot be properly satisfied because we do develop new and better theories, since we could develop new "and better" equivalents, could we not? The second, and somewhat stronger, reply is that their take on the EE criterion is too strong. All EE really claims is that for any theory there is some empirically equivalent rival; Laudan and Leplin seem to read the EE criterion as 'each theory has the same empirically equivalent rival for all time (i.e. over every evolution of science).' Certainly, the anti-realist program requires no version of EE which is this strong, thus Laudan and Leplin's attack on EE here is uncompelling.

4. The defeasibility argument can be obviated

There is a final objection to the defeasibility argument intended to show that the defeasibility argument can be avoided. The anti-realist can concede that the determination of the empirical consequence class of any of our best scientific theories is context dependent (i.e changes with changes in auxiliaries), but also claim that a rival, whose empirical consequence class is identical to that of any scientific theory in question, can be guaranteed before the context is even known (i.e. regardless of changes in auxiliaries). Perhaps an example will help. Let's continue with T_1 and T_2, our empirically equivalent rival theories at time t_0. At time t_1, let us say the equivalence is resolved, since T_1&A_1 and T_2&A_1 have different empirical consequences under the set of auxiliaries A_1. Further, let's say that T_1 is observationally vindicated at this point. The anti-realist can agree that such scientific evolution occurs, and she can still claim that empirical equivalence motivates a 'transcendent' skepticism. She can commit to both of these claims if there is a method for generating an empirical equivalent to T_1 in the face of <u>any</u> change in auxiliaries. If such a method exists, then it would cease to follow trivially from the claim that the determination of the empirical consequence class of our best scientific theories is contextual that rivalries of the empirical equivalence sort are also contextual (and thus possibly defeasible). The defeasibility argument would be obviated because we would not have to look to science for the determination of, the empirically equivalent rival, T_2's consequence class, since the rival's empirical consequence class would be guaranteed to be identical to T_1's regardless of changes in auxiliaries. If there is a method for generating T_1's empirically equivalent rival which would map, identically, the changes in T_1, for any given evolution in science, then T_1's empirical consequence class would be determined contextually the claim that there will be an empirically equivalent rival is not context dependent. Kukla offers a method that "algorithmically" guarantees just such an empirically equivalent rival for every scientific theory. Rivalries that are generated in this manner are sufficient to satisfy EE, even on Laudan and Leplin's version, for there is for every theory an empirically equivalent rival for all time.

I believe that I have demonstrated that, for the most part, the defeasibility argument rests on realistic assumptions about auxiliaries, and thus should not dissuade the anti-realist from holding her position. Furthermore, even if the realist can excise these assumptions, or the anti-realist grants him these assumptions, Kukla's method for generating empirically equivalent rivals can be used to show that the defeasibility argument is of little moment in the debate over the satisfaction of EE. However, before I turn to Kukla's method, there is a second version of the defeasibility argument that can be constructed from comments in Laudan and Leplin's 1993 paper.[66]

4. The Inductive Version of the Defeasibility Argument

There is a second reading of the defeasibility argument found primarily in their 1993 paper, "Determination Underdeterred: Reply to Kukla." Laudan and Leplin's defeasibility argument can be construed as an inductive argument. While this is not an argument that is explicit in either of their papers, comments they make in dealing with other problems, such as the following, suggest the inductive version of the defeasibility argument.

> There is no reason to expect the changes we describe in observability and auxiliary knowledge to be restricted in such a way as to ensure the continuing availability of temporary indiscriminable rivals.[67]

If this is the case, then we have no reason to believe that EE is properly satisfied, since EE requires that there be a guarantee of empirically equivalent rivalry with each evolution of science. At the heart of this argument is Laudan and Leplin's belief that apparent empirical equivalences will be resolved as science evolves. Of course, whether or not there is reason to believe that changes in auxiliaries are not restricted, as Laudan and Leplin suggest, is open to historical investigation. Further, the resolution of apparent rivalries by changes in auxiliaries should also be a matter open to historical investigation. Laudan and Leplin offer no such historical evidence for either claim. However, if such evidence were available, then we could determine the strength of an inductive or probabilistic argument against the satisfaction of EE. Notice that such an argument could well be stronger than the original defeasibility argument. Even if the original argument were successful, the anti-realist could retreat from the metaphysical claim - all of our theories have rivals - to an epistemological claim – we have reason to believe that our best theories will have rivals – giving them an epistemological skepticism rather than a metaphysical one. If the inductive version is correct, then the anti-realist will be unable to justify even the epistemological claim. However, the inductive version of the of the defeasibility argument falls prey to the same strategy I offered earlier: if we can guarantee an empirically equivalent rival 'algorithmically,' then no induction of the sort the realist requires is possible, since with every evolution there is an empirically equivalent rival to our best theories.[68]

While I believe that the inductive version of the defeasibility argument has greater potential that the original version, but that neither has much merit without further work, I have offered an IOU for an 'algorithmic' method for generating empirically equivalent rival theories; this method requires a careful discussion and defense before we can be assured that empirical equivalence is indeed ubiquitous and pernicious. It is to the task of explaining and defending Kukla's method to which I will now turn.

5. The 'Algorithmic' Method for Generating Empirically Equivalent Rivals

Kukla offers a method for generating empirically equivalent rivals of the sort that can properly satisfy EE. His program is a derivative of an instrumentalist attack on realism: supply a method for generating an empirically equivalent rival for any and every scientific theory. If such a method is available, then EE is properly satisfied, and scientific realism is in very serious danger, pending the satisfaction of the 'EE entails

UD' criterion. I will begin with a brief discussion of the instrumentalist's algorithm, and then I will turn the focus to Kukla's 'algorithm' for the remainder of the paper.

1. The instrumentalist's algorithm

The instrumentalists supply an algorithm that consists in stripping a theory of its theoretical postulates and replacing them with a set of conventions that account for the observable phenomena. Often such theories are known as objective list theories. Basically, any theory, T, is reduced to a set of rules that characterize the observable world. A recent example of such a theory would be methodological behaviorism in psychology. Behaviorism, among other things, claims that we are to develop rules regarding the connections between observable phenomena (stimuli and response), ignoring our belief in "hidden" mental states (to which T, let us say our common sense version of psychology, would be committed). Now, a theory T has an empirically equivalent rival theory T', where T' is merely T without ontological commitment to the unobservables of T.

Laudan and Leplin object to such instumentalized theories. They argue that T' cannot "match [the non-instrumentalized theory's] capacity for empirical commitment, once the role of auxiliaries in fixing such commitment and the variability of the range of observable are acknowledged."[69] It should be evident that they believe that T' type theories are easily dispatched by some version of their defeasibility argument. Furthermore, novel predictions are often considered to be evidence for a theory, and it should be obvious that an instrumentalized theory (one of simple conventions, rather than posited entities and laws) will be less fecund in this area. The former objection is currently unsatisfactory, since it must appeal to an argument that requires realistic assumptions about the status of auxiliaries that the instrumentalist would deny. The latter objection merely shows that T and T' are not equally believable, which is irrelevant to current debate, since it could, at best, show that the 'EE entails UD' criterion is unsatisfied.

Laudan and Leplin argue further that T' is not a rival theory to T at all.

> The challenge the instrumentalist poses is to justify endorsing more than the instrumentalized version, not to justify endorsing something instead of it.[70]

One can see the point Laudan and Leplin are trying to make: simply setting up a set of conventions that match a theory's empirical consequence class seems disingenuous and vapid. Such a theory is not a separate theory at all; it is merely the original theory without its commitment to theoretical postulates (unobservables). I believe the resolution to this current attack depends on whether or not T' must incompatible with T. However, I concede, perhaps too hastily, that T's rival must be incompatible with T. Therefore, I will leave this debate unresolved, especially in light of the fact that I believe that Kukla's 'algorithm' presents a better method for generating rival theories; furthermore, rivals so produced are clearly incompatible with the theory in question.

2. Kukla's 'algorithm'

Rather than simply posit a second theory, which is identical to the theory in question without ontological commitments to theoretical postulates as the instrumentalists suggest, Kukla's strategy is to provide a method that generates an empirically equivalent rival theory by combining two theories. Consider the theory T; we can generate T!, an empirical equivalent to T, by combining T with T*. T* is a theory which could be similar to T but includes at least one element (be it law, entity, or event) which is empirically distinct from, and logically irreducible to, those of theory T. T! is to be constructed in the following manner: when humans are observing, T! has identical observational entailments to T, since under observation T! is identical to T, but when no human is observing T! has observational entailments in accordance with T*. Furthermore, theories T and T! are logically incompatible, since T* has, by stipulation, at least one proposition which is logically incompatible with those of T. Therefore, T! is an empirically equivalent rival to T. Such an equivalence can be created with a little imagination, but, more importantly, it can be guaranteed for any and every scientific theory (past, present and future). Since this method can be used to generate an equivalent for every scientific theory, it satisfies the IOU I had given earlier in response to the defeasibility argument, and, furthermore, rivalries generated by this method show, independently, that the EE criterion is satisfied.

6. A Defense of Kukla's Method for Generating Empirically Equivalent Rivals

I will consider a variety of objections to Kukla. Each objection is intended to show that T! is not a genuine empirically equivalent rival to T.

1. T! is not empirically equivalent to T

The most obvious objection to T! is that it is not empirically equivalent to T. It has a different empirical consequence class than the original theory: while humans may never observe the empirical consequences of T*, it remains the case that T* has empirical consequences that diverge from those of T alone. There are two replies open to Kukla, and I will consider each in turn.

The reply that Kukla favors is to claim that T and T! do in fact have different empirical consequence classes, but then deny the import of empirical consequence classes for empirical indistinguishability.[71] Kukla claims that only actual observations are important for empirical indistinguishability: since we cannot ever have access to the difference between T and T!, they are, in fact, empirically indistinguishable. Whether or not two theories actually have the same empirical consequence class is irrelevant to our judgement of empirical equivalence, if we have theories where we, qua human, can never discriminate between them. Thus, Kukla believes that we need not provide empirically equivalent rivals with identical empirical consequence classes to satisfy the EE criterion. However, Kukla's suggestion leaves the theories T and T! counterfactually distinct, which could spell trouble for the notion of empirical equivalence.[72]

The second reply, which I will offer on Kukla's behalf, is to provide and motivate a version of the observable/unobservable distinction that shows that T and T! do have the same empirical consequence class. I certainly will not claim to resolve this debate here, though I have given it a fair shot in Chapter 1; however, I hope to show that my version of the distinction, or van Fraassen's should mine prove unacceptable, could aid Kukla.

There are at least two common versions of the observable/unobservable distinctions. Clearly, Laudan and Leplin's version of the distinction allows for objects perceived using detection equipment (*e.g.*, atoms "seen" through an electron microscope), as well as objects we normally consider observable (*e.g.*, cats and trees). Unlike Laudan and Leplin's version of the distinction, there is a Van Fraassian version, to which I have already alluded, where the range of observables is limited to what creatures with human-like physiologies can observe.[73] On versions of this second type, objects that are observable are the ones that are nomologically possible for us to 'see with the naked eye,'[74] and objects that require detection equipment are considered unobservable.[75]

I believe a stricter version of the Van Fraassian type observable/unobservable distinction can be motivated: not only is the range of the observable limited by physiology, it is limit by what we can actually observe. Such an account differs from the Van Fraassian account in that it further limits the class of observables: only objects within humans' light cone are observable. On this account, it is only those macro-level objects and events that we can observe that count as observables, this leaves out all of those objects and events which it might be possible for us to observe if the world were slightly different. For example, on Van Fraassen's account objects and events occurring in the distant reaches of the universe are observable, as it is the case that if we were there, we could observe them. On my account, these objects and events, in the distant reaches of the universe, are too far away in space-time ever to be counted in the class 'observable;' Kukla puts it thus, "these events are as observationally unavailable to us as ultraviolet radiation."[76]

Such a distinction can be motivated in the following way: science is a human endeavor, and, when we are theorizing, our concern is, at least, explaining the regularities of the world to which we have access (where 'to which we have access' refers to our ability, not whether will in fact observe these things). Thus, the set of objects and events with which we, and our science, are concerned are those that are, in a sense, immediate to us – why should we be concerned with objects and events to which we can never have any access? This separates this criterion from that of Van Fraassen's, since objects to which we cannot gain access, because it is just impossible for us to do so, are eliminated from a theory's empirical consequence class.

I believe that van Fraassen oversteps appropriate bounds when he suggests that anything that could in principle be observed by us counts as an observable. Certainly the theory of relativity suggests that objects beyond our light-cone may be of the observable variety, but they are not ever observable by us! It seems obvious to me that science is not, nor should it be, concerned with producing theories about objects that we do not have the ability to ever observe. The differential objects of T* are just like those beyond our light-cone, and, as such, do not count as observable at all. Thus, T and T! are empirically equivalent as I have defined it.

It is not my intention to resolve any debates regarding the observable/unobservable, nor the observable/detectable, distinctions; however, I believe that I have shown sufficient motivation for a third version of the distinction. If this third version can be successfully defended, then Kukla's T! does, in fact, have the same empirical consequence class as T, since the "observational" entailments to which we will never have access are not a part of the empirical consequence class of a theory.

2. Observation does not have the special powers T! requires

The first objection raised by Laudan and Leplin is that we have no reason to believe that observation has any "special powers" such that the world would change when someone was looking versus not looking. Here, they intend to show that T! is not a real theory at all. Clearly, on a certain reading of quantum theory, observation, or at the very least detection, plays an important role in the collapse of the wave function. This example provides at least some reason to believe that observation does have the "special powers" Laudan and Leplin deny. Laudan and Leplin anticipate such a rejoinder and note that, unlike Kukla's algorithm, theories such as quantum mechanics are confirmable. It is not so clear to me why it should be important whether a theory is confirmable or not; quantum mechanics justifies the belief that observation may have special powers, this is all that is required for the reply to their criticism.

Regardless, the belief that observation has no "special powers" rests on realist intuitions that the world operates in one way, and that its operation is invariant regardless of human intentions in any way, shape, or form.

3. Kukla's method is not algorithmic

Laudan and Leplin argue that Kukla has not given us an algorithm that guarantees an empirically equivalent rival: "Kukla has given us, not an algorithm, but a promissory note."[77] The fact that Kukla's method is merely a promissory note is only important (with respect to the satisfaction of EE) if, at some point in the future, we are unable to produce a T* for the generation of T! This can only be the case if T has a structure such that there is no way we can modify it using a single event or object that will create the incompatible proposition required of T*. Laudan and Leplin offer an appeal to our intuitions to justify their claim that we will be unable to produce a T*: "in contemporary theoretical physics it is often difficult to come up with one theory meeting well established desiderata, let alone two."[78] First of all, I believe Laudan and Leplin have misunderstood the nature of T*: T* need not be an entirely different theory from T, it merely needs to contain a single incompatible proposition. The human mind is not so feeble that it will be unable to imagine a single object or event that will create an incompatible proposition. Furthermore, it seems to me that T can only have a structure such that no T* can be developed if we take a realistic view of auxiliaries. Of course, the anti-realists need not concede such a view, nor has an argument been provided that we should take a realistic view of auxiliaries. Thus, it appears that the fact that Kukla's 'algorithm' is merely a promissory note is of little import to the satisfaction of EE.

4. T! is semantically parasitic on T

Kukla takes Laudan and Leplin to have objected that T! is semantically parasitic on T.[79] For T! to be semantically parasitic on T, it must make ineliminable reference to T. If this is the case, then T! is not a proper rival theory at all; T! is dependent on T for even its semantic existence. The claim then is that we would not have a genuine empirically equivalent rival for T in T!, but merely a secondary, lesser, and worse version of T itself. Certainly this is not the case for T!; it may refer to T for part of its empirical content, but its set of observational entailments[80] (and empirical content) is, albeit unobserved by us, larger than T's. Due to the addition of T* to the theory, genuine rival predictions are made by T!. It is simply the case that they cannot be checked. T! is not a lesser or bad version of T in any way, it is in fact a larger, separate theory unto itself. Certainly realists such as Laudan and Leplin will agree that in the search for the truth either T is true or T! is true, whether or not one appears to be parasitic on the other is of little moment. It is unlikely that Laudan and Leplin believe that the genesis of a given theory can serve to determine its truth-value, since a genetic fallacy is no reason to believe any claim. In any case, we could just as easily claim that T is semantically parasitic on T! for its reference, and T is not a candidate for the truth; this is not something the realist would be happy to concede.[81]

5. T! is parasitic on T for explanation and prediction

I believe that Laudan and Leplin had a different version of parasitism in mind. They seem to mean that T! is parasitic on T for its ability to explain and predict phenomena, since all of the predictive mechanisms of T must be built into T!. Therefore, there is no reason for us to believe that T! is a rival theory to T. Again, my argument that the claim of parasitism is little more than a genetic fallacy seems to obtain. How are we to tell that it is not T, a mere part of T!, which is not parasitic on T!, rather than vice versa? Furthermore, since T! is incompatible with T, the parasitism objection cannot be used to show that T! is not a rival theory in the manner they rejected the instrumentalist's algorithmic empirical equivalents.

It is difficult to see how either version of the parasitism is intended to show that T! is not a plausible candidate for an empirical equivalent to T. I have already shown that it cannot be used to show that T! is not a rival; further the objection cannot show that T is not parasitic on T!. Thus, we have no reason to be swayed that T! is not an empirically equivalent rival. Kukla believes that the parasitism claim is really a method for identifying whether a theory has the proper characteristics of a theory.[82]

Kukla claims that the parasitism objections are really intended as follows: there are some properties that any theory must posses to be a theory, and if it is parasitic it lacks one of these characteristics. Kukla argues that this criterion would be too strong, and I agree. It is interesting to note that C. Clavius raised an objection against Copernicanism that appears to parallel Laudan and Leplin's.[83] He took it to be a serious problem that the Copernican project was developed after the Ptolomaic and, thus, knew which predictions it had to save. Clearly, Copernicanism made use of the predictive powers of the Ptolemaic model for the position of the planets and so on, in addition, it was geometrically equivalent. Thus, Copernicanism would have been ruled out *a priori* by some criterion for theoryhood, such as the parasitism criterion. Indeed it would be problematic for the realist if Copernicanism were not allowed to stand as a rival theory because of a parasitism criterion for theoryhood. Parasitism is obviously a criterion for theoryhood that the realist ought to reject.[84]

I believe that there is little more to the parasitism objections that an appeal to intuitions. It is clear that the realist will have a gut-feel that it is somehow compelling, and that the anti-realist will feel that it is irrelevant, provided the theories are not logically reducible. However, no matter how we interpret the parasitism objections, it should be clear that additional argument is required (at minimum to show the direction of the parasitism – recall the genetic fallacy argument) in order for these objections to have any impact on the empirical equivalence debate.

6. T! is too odd to be a scientific theory

Laudan and Leplin offer a triad of objections that are intended to show that T! cannot be an empirically equivalent rival, because T! does not have the proper form for a scientific theory. They provide an example to help illustrate this set of objections.

Laudan and Leplin ask us to consider T to be the body of Kepler's laws, and T* to be the case where the planets are motionless. It follows that T!, which, recall, is the combination of T and T*, has the following content: when we look, the planets obey Kepler's laws (T), and when we look away, the planets are motionless (T*). With such an example, it should not be difficult to see that all sorts of problems arise for T!.

6.1 The miracles required for T! make it counter-intuitive

One critical problem the theory T! is that all sorts of miracles will have to occur within theory T! to account for the motionless planets of T*. Imagine the impact on tides and so forth when the planets stop moving. Furthermore, instantaneous jumps must occur when a human observes the heavens: the planets must jump from their motionless position to the one they would occupy under T.[85] Thus, as Laudan and Leplin claim, T! no longer resembles a theory that could be considered "genuine." However, the method Kukla offers need not use anything as grand as motionless planets. All that is required for Kukla's algorithm is that T* has some contents that are incompatible with those of T, not that it contains something grotesquely incompatible. What if T* were simply that the planets shrink the tiniest fraction of an inch, which is, of course, imperceptible at the distance we find ourselves from said planets, without a change in mass? If this were the case the "miracles" that plague T!, according to Laudan and Leplin, seem quite unmiraculous. Importantly, though, this reply only diffuses only part of this 'need for miracles' objection: the theory T! is not a real theory because it is counter-intuitive. However, our intuitions on this matter can also be easily explained. Since T*, by definition, contains elements we will never observe, we should not be shocked that T!, a theory that explains the conjunction of T and T*, seems counter-intuitive, for commonsense has no place for objects we never observe.[86] Therefore, the fact that T! is counter-intuitive seems to be of little moment. Furthermore, it is of some note that several of our best theories are highly counter-intuitive: Quantum Mechanics contains many elements that clash with our intuitions.

6.2 The miracles of T! make it impossible for T! to be a scientific theory

The realist could argue that T* is too miraculous, since it repudiates seemingly well confirmed assumptions. Laudan and Leplin's favorite assumption is "physical events characteristically initiate causal chains that we may observe."[87] This is the claim that no matter what we put in T*, it will always produce observable effects (there would then be a divergence in the empirical consequence class of T and T!). For example, in the case of the shrinky planets it seems clear that Laudan and Leplin will claim that we will notice things like changes in light patterns on the earth due to the shrinking. I believe that the best response available to Kukla is to point out that these 'well confirmed assumptions' are really just hypotheses; thus, it begs the question to use them to dismiss other hypotheses – this is just another appeal to independently confirmed auxiliaries for which no argument for their confirmation has been given. This is easily illustrated using Laudan and Leplin's own favorite assumption: whether or not there are even causal chains is an open question: it has, as Laudan and Leplin themselves note, an empirically equivalent rival – Hume's *de facto* constant conjunction.[88]

6.3 The miracles of T make it impossible to conjoin it with T and still be a scientific theory*

Notice that T does, of course, include propositions like the causal chain that Laudan and Leplin suggest. This makes it difficult to see how T! could be a scientific theory if it denies these sorts of claims, as it appears it must for T*. However, it is not clear that T! must deny claims such as the causal chain in order to conjoin T and T*. The causal chain can remain intact and we can still have T!-type theories: we simply need to apply some brainpower to come up with an appropriately elaborate conspiracy theory. In the case of the shrinky planets we need at least a clever way to account for our inability to observe the differences in light patterns. The planets are "shrinky," but not only do they not change in mass they emit a field that causes light rays to be deflected just as if the planet had not been "shrinky." We could repeat this process for each recalcitrant observable, which would cause T and T!'s consequence classes to diverge, given by the "causal chain characteristically initiated by physical events." In anticipation of criticism, an attack on Kukla's algorithm cannot be formed on the grounds that most of us lack the time and imagination to provide an element that can fit in T*, since this does nothing to challenge the rivalry – it merely claims that developing theories takes time and effort (a fact that the realist will surely accept).

7. My earlier appeal to naturalism rules out T! as a rival theory

In Chapter 1, I appealed to the success of science to provide normative force to my evidence-theory distinction. It seems that the realist could use a similar argument to rule out T! as a scientific theory (in which case T! could not be a genuine rival theory to T). The realist could argue that successful science ignores silly theories like T!; thus, we too should ignore T!s when we are doing our philosophy of science.

However, the Naturalism to which I appealed is not a strategy that suits the realist in this case. The Naturalism to which I appealed, that we should take note of the methodology of successful science, makes no call one way or the other about the truth of our theories, something that the realist holds near and dear. The realist makes claims that supercede naturalism, and thus is open to attacks on his claims that exceed naturalism. That is, since the realist wants to claim that our theories are true, a claim that oversteps claims about the operation of science, he cannot avoid attacks that make claims that overstep the operation of science as well. Thus, the naturalist may well be able to ignore Kukla's T!s, on the grounds that our successful science ignores them, but the realist cannot argue in this fashion, since he has set higher standards for himself.

Furthermore, it is unclear to me that science does ignore theories such as T!. Of course, if science does not ignore such theories, the realist's criticism being considered in this section fails. Also, a further problem arises for a proponent of this criticism: how are we to determine if science does ignore theories such as T!? This may seem like a silly defense; however, it is not clear how we should go about analyzing the operation of science with respect to realism versus anti-realism – it seems that both camps are compatible with most, if not all, of the methodological operations of science. [89]

It seems at any rate that there may be a number of avenues of defense for the anti-realist who employs some naturalism in her arguments for the underdetermination debate as a whole. I will leave this question unanswered at this time.

8. *T! is simply not a scientific theory*

Not only do problems specific to Kukla's method obtain, according to Laudan and Leplin objections to algorithms in general arise. T!, the theory generated by the algorithm, is not a genuine scientific theory, and, as such, it cannot stand as a rival theory to T. Laudan and Leplin are happy to let the experts (*i.e.*, scientists themselves) decide what counts as a genuine theory.[90] However, in lieu of deferral to expert decision on the issue of genuine scientific theories, Laudan and Leplin offer a demarcation criterion for what is to count as a genuine scientific theory. They suggest "a theory must, at least in principle, be open to test."[91] Laudan and Leplin offer reason to accept such a criterion: "the purpose of theorizing is, at least in part, to gain predictive control over the subject matter under investigation."[92] While this may in fact be a key goal for theorizing, it does not provide us with good reason to believe that all aspects of a theory must be open to test. In fact, there are aspects of any theory that are not open to test. How are we to test whether or not the laws we postulate obtain at the far reaches of the universe, or in the remote past? Furthermore, even if the criterion they advance is reasonable, two problems remain. First, this criterion cannot be used to rule out T! as a genuine scientific theory. T! is open to test, all of the observational entailments of T! that are in line with those of T are in fact testable. Laudan and Leplin anticipate this maneuver, but they consider it perverse. Since the empirical consequences of T and T! are identical because they are so dictated or, one might say, parasitic on T, it would be wrong to claim that T! is testable and as such is a genuine theory. However, I have already demonstrated the direction of the parasitism is in question, it may be the case that T is dependent on T! for its testability. Of course, the realists may be demanding that T* is untestable and is thus unscientific. However, T* is not the rival theory to T, the rival theory is T!. Therefore, the lack of testability of T* is irrelevant, and T!, the rival in question, does meet the criterion just as well as the realists' favored T. The second problem is that the criterion rules theories out on pragmatic grounds. They demand that a theory be testable so that we can gain predictive control over the subject matter. Such a demand is not one that suits the realists, who demand theories to track the truth, even if Laudan and Leplin could rule out T! using this criterion, they would become some sort of pragmatists.[93] Thus, it appears that Laudan and Leplin's demarcation criterion, if it is acceptable at all, is both unable to rule out T! without further argument and unable to track a realistic picture of science.[94]

9. T! is not the right sort of theory to be a rival theory

It might be objected that theories must have certain virtues, such as simplicity or coherence, and that T! has few if any of these virtues. One can see how it might be objected that T! does not instantiate the virtue of simplicity (with respect to T). T! postulates all sorts of objects and events, not to mention the fact it likely requires "conspiracy theories," that T does not have. I mention this argument only for the sake of completeness; Kukla argues that these virtues really only play a role in determining whether a theory is equally believable, and I agree, if these virtues play any role at all in the underdetermination argument. Thus, such objections are really attacks on the 'EE entails UD' criterion, and as such are of little import to the debate at hand.

7. Conclusion

It was my goal in this paper to demonstrate that there are empirical equivalents that properly satisfy the EE criterion of underdetermination. To attain this goal, I considered Laudan and Leplin's defeasibility argument, which is intended to show that the EE criterion can never be properly satisfied, an inductive defeasibility argument, which I attributed to them, and Kukla's method for generating empirically equivalent rival theories.

It turns out that the core of the defeasibility argument rests on a realistic view of auxiliaries, which means that the use of IAA in this argument is question begging. Furthermore, the defeasibility argument can be avoided entirely if Kukla's method can guarantee empirically equivalent rivals for any theory.

The inductive version of the defeasibility argument requires further argument to demonstrate that the evolution of auxiliaries will adjudicate apparent cases of empirical equivalence. Furthermore, if Kukla's method can produce a guarantee that rivals exist for every theory, then the induction is impossible.

I believe that Kukla's method does provide the guarantee and the rivals required to properly satisfy the EE criterion. Thus, it shows independently that the EE criterion is satisfied, and it provides a critical attack against the realist's defeasibility argument. Thus, it appears that the first criterion for underdetermination is properly satisfied.

Chapter 3
Does Empirical Equivalence Entail Underdetermination?

0. Introduction

Given the success of a certain version of the evidence-theory distinction, and the ubiquity of empirical equivalence, the only remaining avenue of attack for the realist is to show that the underdetermination thesis does not follow from empirical equivalence. Recall that the underdetermination of theory by evidence thesis is as follows: belief in any particular scientific theory is unfounded.[95] Thus, an attack on underdetermination can be formulated: it does not follow from the fact that empirical equivalence is ubiquitous that we have no reason to believe our best theories. Historically, the preceding objection has taken several forms. I will survey these objections and show that each is insufficient to defeat the entailment of underdetermination from the ubiquity of empirical equivalence.

1. The Believability Criterion

As I alluded in Chapter 2, it is possible to object that the empirical equivalents generated by Kukla's method are not as believable as our best theories. Of course, the idea is that we have reason to believe our best theories, since the equivalents are not "real" rival theories.

Kukla claims that we can generate empirical equivalents by simply having any given data point (i.e. an observation) for our current theories change when no one is looking (and adding the appropriate conspiracy theory to cover up any possible changes due to physical causation).[96] This will, of course, give rise to a different theory with the same empirical consequence class as the one from which it is generated. The realist's charge, then, is that such a theory is not as believable as the one from which it is generated (whichever current theory is in question). Being that the equivalents are less believable, we ought not consider these particular theories to be rivals to our "best" theories. Recall that I had intimated that the second criterion that must be met for underdetermination is that there is an equally believable theory.

It seems clear to me now that "believability" is a red herring. It is not the case that the equivalents must be equally believable, merely that they be conceivable. Why the distinction? Earlier, I noted that believability does not track the truth (truth, approximate truth, or whatever); this is exactly why believability[97] is unimportant to the debate. Conceivability is important, since we have reason to believe that only conceivable objects exist. If it were impossible to conceive of a theory such as T!, then we would have no reason to believe that it is an empirical equivalent to T, and there would be no problem of underdetermination.

This seems to be the end of any objection that Kukla's rival theories cannot serve to satisfy the second criterion (EE entails UD) on the grounds that they are not equally believable. Objections that claim Kukla's rivals are not conceivable have been dealt with in the previous section.

2. The Virtues

Another attempt to show that underdetermination doesn't follow from Kukla's empirically equivalent rivals is to claim that the appropriate Supra-Empirical Virtues (SEVs) can help us to select the best theory.[98] Much like the previous objection, the claim is that there is another form of evidence, other than the empirical consequence class, which indicates the truth (truth, approximate truth, or whatever) of any given theory: the theory's match with certain supra-empirical virtues. We are all familiar with the sorts of things that are normally considered in this group of virtues: simplicity (a la Ockham), consistency (internal or external), explanatoriness, and the like.

I admit that conceivability is critical in satisfying the 'EE entails UD' criterion; this entails that internal consistency be respected.[99] However, after the claim that internal consistency is critical, and I have shown that Kukla's rivals are indeed internally consistent, what can the realist get out of the supra-empirical virtues? Two problems face the realist before the SEVs can be used to eliminate Kukla's empirical equivalents: 1- they must provide a compelling analysis of each virtue (or, at least, the one they want to use to kill the empirical equivalents), and 2- they must demonstrate that the virtues track the truth (or approximate truth) and are not merely pragmatic instruments or simply aesthetic virtues. I will examine these two problems in turn.

The difficulty of providing an analysis of the different virtues is readily apparent when we consider the case of the virtue of simplicity. When asked which is the simpler theory, formulating an answer to this question is difficult, if not impossible. Is the simpler theory the one with the least laws, the least entities, or the one with the smallest set of propositions? Clearly, no easy answer, if any, is forthcoming. However, we may have reason to believe that some of the virtues may be unproblematically analyzed: it is easy to understand the virtue of internal consistency.[100] Thus, the realist can reply that we have reason to believe that (at least some of) the virtues can be analyzed. I am willing to concede this last point; however, simply having an adequate analysis of each virtue does not mean that the virtues can serve the realist's purpose – denying the 'EE entails UD' criterion is ever met.

Even with the first problem successfully navigated, the realist remains saddled with the problem that the SEVs may not track the truth. There is no reason that the anti-realist cannot accept the pragmatic value of the SEVs in theory choice, while denying that these virtues have any place in identifying true theories. We may make use of, or even accept, certain theories because they are simpler, or explain, or cohere; however, these virtues provide us with no reason to believe a theory.[101] I am unaware of any work that demonstrates how the SEVs track the truth; at minimum, the burden of proof rests with the realist to provide a reason to believe that the SEVs are in fact evidence at all.

The SEVs, however, do provide the realist with an attack on some proponents of the underdetermination thesis.[102] The realist can point to a tension in the anti-realist's project: the anti-realist accepts that we can know about the empirical consequences of our theories (i.e. we know about observables), but the anti-realist cannot rule out empirical equivalents to our observable world without an SEV. The anti-realist holds that we can believe the empirical consequence of our theories, but they do so in light of the fact that Goodman's "grue world" remains an empirical equivalent to the observable world. Thus, it seems, the anti-realist deploys some sort of SEV in resolving the Goodman equivalence at the empirical level but, inconsistently, refuses the acceptability of such a move at the theory level. That is, it is quite acceptable to rule out an empirically equivalent rival when forced with a choice between a "green" world and a "grue" world, but it is not when force with a choice between a world filled with "electrons" and one with some other theoretical postulate. At the risk of sounding silly, this is inconsistent.

Kukla argues that the anti-realist can deny the universal application of the SEVs. That is, the anti-realist could employ a rule of the following form: the SEVs (or an SEV) count(s) as evidence at the empirical level but do(es) not at the theory level. Given that the realist has not offered any reason to believe that the SEVs track the truth, such a rule is not any worse than his rule (the SEVs are evidence categorically). The anti-realist would then be free to accept that the grue-world is ruled out by some SEV (which counts as evidence), and continue to accept that at the theory level SEVs cannot be deployed to rule out empirical equivalents (where the SEVs do not count as evidence).[103] This would avoid the inconsistency charge, but now the anti-realist would be open to claim that her rule is arbitrary

My own particular reply to this problem in Chapter 1 was to argue that scientific practice gives us reason to hold an externalist account of warrant at the observable level and an internalist account of warrant at the theory level. This provides the philosopher with a reason to dismiss the evil demon at the empirical level. There is then no need for an SEV to perform a role as evidence at all.

3. The Flow of Evidence Objection

Laudan and Leplin argue that there are two types of evidential relations that will demonstrate there is no entailment from the ubiquity of empirical equivalence to underdetermination. The first relation they ask us to consider is one between individual empirical consequences and the theory they support. Laudan and Leplin claim that individual data points can fail to be evidence for some (or one) parent theories, or that individual data points can support some (or one) parent theories over their rivals. If such a relation holds, then underdetermination does not necessarily follow from empirical equivalence, since it is possible that there are cases for which evidence differentiates empirically equivalent rivals. The second relation is between the parent theory and its sub-theories. The claim is that there is a web of theories, where a theory boasting strong evidence can provide evidence for other theories in its particular web. Again, if such an evidential relation exists, then there may be evidence for one rival over another that has nothing to do with a particular theory's own consequence class. I will consider each of the following relations.

1. Empirical consequences can support rivals differentially

If it were the case that even theories with the same consequence classes could be supported differentially by the evidence, then the realist could argue that most (if not all) empirical equivalences could be resolved by the evidence (individual empirical consequences – in this case) itself; Laudan and Leplin provide an example of this very phenomena.[104]

Our stock realists argue that there are some theories that are not supported (made believable or more believable) by their empirical consequences. They ask us to consider the following case: an individual is convinced that drinking coffee will remedy the common cold. Our friend, after drinking coffee for several days, finds that his cold has dissipated; thus, he concludes that his hypothesis is supported. Clearly, the empirical content of our 'foolish' friend's hypothesis is the dissipation of the symptoms of his cold after a few days of drinking of coffee; his hypothesis is supported by the empirical consequence noted above. Our realists offer two arguments against the possible satisfaction of the 'EE entails UD' criterion based on this example.

1.1 The evidential relation is misunderstood by proponents of underdetermination

Laudan and Leplin argue that the case of the coffee drinker illustrates the reason we believe there are constraints (i.e. experimental controls) on what counts as evidence for any given hypothesis.

> No philosopher of science is willing to grant evidential status to a result *e* with respect to hypothesis H just because *e* is a consequence of H. That is the point of two centuries of debate over such issues as the independence of *e*, the purpose for which H was introduced, the additional uses to which H may be put, the relation of H to other theories, and so forth.[105]

It would be constructive if Laudan and Leplin indicated how they believe the aforementioned debates turned out; I believe that such an admission would show their realist(ic?) intuitions in this case. It appears that, at minimum, it is their intention to reject the Hypothetico-Deductive model of science.[106] However, since the debate surrounding the analysis of evidence and how it is connected to theory is far from resolved (according to Laudan and Leplin themselves) and certainly quite complex, they do little but appeal to our intuitions that empirical consequence are not always, nor the only, evidence for a theory. In fact, the last several pages of their 1991 paper amount to a tirade against current analyses of evidence and the evidential relation. They take van Fraassen's notion of saving the phenomena to be as good as a reductio ad absurdum of the whole project; as a result, they ask us to look beyond semantics and syntactics to find an account of "epistemic warrant unfettered by semantics [that] has rich and varied sources yet to be exploited."[107] However, if van Fraassen's, or some other's, project turns out to be coherent, I find no reason to accept Laudan and Leplin's claim of a reductio, and, in turn, no reason, except perhaps the realist's intuitions, to reject the project – which would, of course, be no reason at all.

Furthermore, even if Laudan and Leplin are able to show that there are constraints on what is to count as evidence, there is still a good deal of work to be done by the realist. The realist must show that the constraints on the evidence, as well as, of course, the evidence itself, track the truth.

1.2 The possibility of differential confirmation

Before we leave Laudan and Leplin to the task of providing further argument for this particular "counter-argument," it is clear that the pair intend this objection in a second manner as well. As I noted earlier, if it is the case that some hypotheses are not, and others are, supported by the same empirical consequence (i.e. evidence), then some empirical equivalences can be resolved by the evidence alone.

Recall the coffee drinker. If it is the case that his hypothesis is not supported by its empirical consequences and one of its rivals is, then we have no reason to believe that 'EE entails UD' in the manner the proponent of underdetermination desires.[108] However, Laudan and Leplin do not tell us why the coffee drinker's hypothesis is not supported by the dissipation of the cold. The coffee drinker ought to remind us of the Gettier examples – this is a case where the hypothesis is supported "by accident." Of course, in the Gettier cases, and this case, the "accident" can be uncovered by further empirical study. Thus, we can admit that the coffee drinker's hypothesis <u>is</u> supported by his evidence, without giving up on the success of science. It seems to me that his hypothesis is supported.

Certainly 'supported' can be given an anti-realistic spin – to be a supporting instance is to maintain the empirical adequacy of a theory – to demand more would beg the question against the anti-realist. The problem here is that his hypothesis does not enjoy the same evidential support as our "best" theories, since it cannot account for the fact that the colds of those who do not drink coffee dissipate just as quickly, not that the theory is not supported by the data point.[109]

Kukla argues that Laudan and Leplin need not show that some hypotheses are not supported by their empirical consequences but, rather, that some hypotheses are differentially supported by the same consequences. However, as Kukla points out, this is no easy task, and, certainly, they have not demonstrated that such differential support could occur (especially between empirically equivalent rivals).

So far I've used the word 'supported' in a general fashion; Laudan and Leplin use 'confirmed' in lieu of 'supported,' and it seems that they intend it to have special meaning. Clearly, the coffee drinker's hypothesis is not confirmed by the dissipation of his cold. Kukla correctly points out that this is just another case of question begging on the part of our realists.[110] Neither the anti-realist nor the skeptic will admit that theories can be confirmed (other than on matters of its empirical content, in the case of the anti-realist). Furthermore, claiming that a theory is supported is not to claim that it is more likely true in any sense – it is to merely admit that it remains empirically adequate in light of the data point in question. Thus, for the previous discussion, I have eliminated the question begging by merely talking about empirical support for a theory (eliminating the demand for confirmation). It seems that Laudan and Leplin's point is lost as a result: the coffee drinker's hypothesis is no less well supported (not shown to be less empirically adequate) by the dissipation of his cold than our best theories are by the said dissipation, and, while it remains the case that his theory is not confirmed by the evidence, no theory is confirmed by the evidence unless we are realists to begin with.

Furthermore, Kukla characterizes the project for demonstrating that individual data points can support empirically equivalent rivals as follows:[111]

> ... if [empirical equivalence is appropriately ubiquitous], then there are indefinitely many theories that have the same empirical consequences as T. To suppose that we can ascertain which one deserves the confirmatory boost from [the individual data point] is already to suppose that the problem of underdetermination has been resolved.

It seems, then, that more argument is required on the realist's part in order to show that differential support for one empirically equivalent rival over the others can occur. At the very minimum, the realist must excise the question-begging in this argument – a feat that may well prove to be impossible.

2. There is an evidential web that provides the differential support

Many accept the claim that no theory lives in a vacuum: it is impossible to separate any given theory from at least several others in order to test it. This means that all theories are some how connected. Laudan and Leplin have a picture of this "web" of theories that is more like a tree. Rather than all theories being connected haphazardly, theories are connected to each other in some cases by parent theories. The following is Laudan and Leplin's example:

> Consider, for instance, the theory of continental drift. It holds that every region of the earth's surface has occupied both latitudes and longitudes significantly different from those it now occupies. It is thereby committed to two general hypotheses:
> H_1: There has been significant climactic variation throughout the earth,

the current climate of all regions differing from their climates in former times.

H_2: The current alignment with the earth's magnetic pole of the magnetism of iron-bearing rock in any given region of the earth differs significantly from the alignment of the regions' magnetic rock from earlier periods.

During the 1950's and 1960's, impressive evidence from studies of remnant magnetism accumulated for H_2. Clearly, those data support H_1 as well, despite the fact that they are not consequences of H_1. Rather, by supporting H_2 they confirm the general drift theory, and thereby its consequence H_1.

Certainly such a picture of the evidential connection among theories is not arcane. However, there is no reason to think that this unseats the claim empirical equivalence entails underdetermination. At least two objections are available to the anti-realist with respect to this argument: 1- the anti-realist can deny that this sort of evidential relation is important to the debate, or 2- the anti-realist can claim that indirect confirmation is possible, but it does nothing to scuttle underdetermination. I will present and examine both in turn: the former appears to be a failure as a reply, but the second (with some modification) is a success for the anti-realist.

2.1 Underdetermination is a claim about total sciences

Indirect confirmation is irrelevant to the debate concerning underdetermination, if we take, at least part of, Boyd's version of underdetermination to be correct. Boyd holds that underdetermination is a thesis about total sciences.[112] That is, we are not to be concerned about the individual hypotheses of science, rather we are to ask ourselves whether or not it occurs at the end of science. If this is the proper take on underdetermination, then indirect confirmation is of little import.

Total sciences occur when all of science is complete (i.e. we have a completed account of the world – all the observations are covered). Such total theories are, of course, composed of all sorts of individual hypotheses in any number of relations. But the interrelation of these hypotheses is of no consequence, since no indirect confirmation can occur at the level of total sciences:[113] if there is an empirical equivalent to a total science, then underdetermination cannot be questioned by an appeal to indirect confirmation internal to the total science.

A defense based on completed total sciences is not one that either the realist or anti-realist should favor. Carl Hoefer and Alexander Rosenberg argue that the debate over underdetermination should be located at the level of completed total sciences;[114] however, they conclude that if the debate is to be located here, we have no reason to accept either realism or anti-realism. Rosenberg and Hoefer argue that there are a variety of considerations neglected by both groups that are contingent, not *a priori* as the groups presume. The best example of this is the question of whether or not the world even admits a complete total science (let alone two, or more). This seems a contingent truth at best, and neither group seems to have an argument to show otherwise if they leave arguments about local theories behind.[115] Thus, while the displacement of the debate to the level of complete total sciences may seem, prima facie, a good idea, it turns out that such a move leaves the debate unresolved (and perhaps unresolvable).

2.2 The indirect confirmation argument makes question-begging assumptions

The second argument against Laudan and Leplin's claim, that underdetermination does not follow from empirical equivalence on the grounds that there can be indirect confirmation for a theory, is made by Kukla.[116] He argues that the anti-realist can accept that indirect confirmation occurs, but they are in no way forced to accept that any theory is confirmed.

Kukla agrees with our realists: indirect confirmation (the increased support for a branch theory by the increased support for a parent theory, call it T, by its other branches) is uncontroversial. However, here again, the realist begs the question against the anti-realist. If empirical equivalence occurs, and I believe I have established that it does, then there are an indefinitely large number of Ts. Thus, if the realist assumes that T is confirmed, and that, in turn, the confirmation can flow back 'down' to a branch theory, then he has assumed that underdetermination does not occur (which, of course, is exactly what is in question), since it would otherwise be impossible to establish which of the set of Ts was confirmed in the first place.

It is, however, unclear to me how the anti-realist can accept indirect confirmation and maintain that underdetermination occurs. As Kukla so aptly points out, accepting that indirect confirmation can occur amounts to accepting realism. Thus, it is difficult to see what use the anti-realist may have for indirect confirmation. In the case of Laudan and Leplin's example, the anti-realist can accept that the theory of continental drift gains empirical support from evidence for H2 (*i.e.* it is on the road to empirical adequacy – the goal for theories according to the anti-realist). But why should she accept that H1 gains support from such an occurrence? I believe that the anti-realist should simply deny that there is any form of indirect confirmation, since she is apt to deny that there is confirmation of any theory in the first place. Further, nothing seems lost by such a claim. Should it turn out that the theory of continental drift is empirically adequate, then that would entail that we had checked the empirical consequences of H1 anyway: those phenomena proper to H1 would have to be saved by the theory of continental drift – the empirical adequacy of the theory of continental drift could not be established otherwise.

Kukla argues that indirect confirmation rests on the 'well supported' thesis that no theory can "enjoy a greater degree of confirmation than any of its consequences." This rule would be violated if the theory of continental drift obtained a greater degree of confirmation than H1; thus, the degree of confirmation of H1 must be increased with increases in the degree of confirmation of the parent theory. Yet, it seems that the anti-realist need not accept this 'thesis'. Consider Laudan and Leplin's example again. H1 and H2 are not empirical consequences, they are merely theoretical consequences, of the theory of continental drift. Since 'confirmation' is awarded by the anti-realist only in cases where there is empirical support for the theory, she can accept that the theory of continental drift has gained support from its empirical consequences (which also support H2), and still deny that its empirical consequences that would support H1 have gained any 'confirmation.' To claim otherwise would be to accept that theories can gain 'confirmation' which is not strictly empirical (i.e. to accept realism).

4. The Ultimate Argument

So far, it appears that there are no objections that can block the move from the ubiquity of empirical equivalence to underdetermination. There does however remain one final possibility. The Ultimate Argument, often known as the no-miracles argument, provides the realist with one more avenue to explore. The realist could argue that a theory's 'track record' is evidence for the truth (approximate truth, or what have you) of a given theory. The truth of a theory is obviously non-empirical evidence in favor of one theory over another; therefore, empirical rivalry need not entail underdetermination.

Likely we are all familiar with the no-miracles argument, the idea in this context is that our best theories have been inordinately successful, such success is evidence for the approximate truth of our best theories. In this final section, I will explore Carl Matheson's objection to the no-miracles argument (the NMA) and offer an objection of my own.

1. The no-miracles argument

The no-miracles argument comes in a number of variations. Matheson lays out the canonical version as follows:[117]

1. Science has progressed.
2. Scientific realism provides us with a better explanation for this progress than any other philosophy of science.
3. All other things being equal, we should believe the philosophy of science that best explains facts about scientific practice.

Therefore,

4. We should believe that scientific realism is true.

If this argument is sound, then we would have no reason to believe that underdetermination occurs, even in light of empirical equivalence. Since our best theories would be at least approximately true, we would have reason (the fact that they've latched onto at least a little bit of the truth) to accept these theories over their empirically equivalent rivals.

Matheson argues that premise two is the false premise; I agree. Clearly, the realist and anti-realist will agree that science has progressed,[118] leaving only some skeptics out (something I am happy to do). Also, in a debate about the proper epistemic attitude toward the posits of science premise three seems warranted. Furthermore, it seems that the argument is valid.

2. Matheson's objection

The heart of Matheson's objection is the claim that there is a better explanation for the success of science that scientific realism. This is the sort of objection that spells the end of the use of the no-miracles argument against the underdetermination thesis.

Recall that the no-miracles argument blocks the entailment from the ubiquity of empirical equivalence to underdetermination by providing some non-empirical evidence (the approximate truth) for one of the rivals. If it is the case that we cannot infer the approximate truth of a given theory from its track record, then there is no additional evidence provided by the no-miracles argument to resolve an apparent rivalry and underdetermination holds. Matheson argues that the no-miracles argument does not give us reason to infer the approximate truth of any given theory.

Recall that I have already established the ubiquity of empirical equivalence. Since this is the case, the realist must admit that the world admits of observationally equivalent theories (even if he does not have to admit that this entails underdetermination). Matheson asks us to imagine a world where there are two complete total sciences: Wave Theory and Particle Theory.[119] These two theories are radically different in the nature of their entities, laws, and so on. It should not be difficult to see that both the Wave theorist and the Particle theorist can deploy the no-miracles argument to infer the truth of their favored theory, since both would enjoy the grand successes of a completed total science.[120] Now, of course, only one of these theories could be true; therefore, there is a serious problem for the no-miracles argument.

Matheson asks us to imagine that the existence of these two complete total sciences entails that there are, at least, two different possible worlds - one where Wave Theory is true and one where Particle theory is true.[121] These possible worlds are otherwise identical to our world where we have a pair of completed total sciences. Thus at Particle world the no-miracles argument arrives at a true conclusion for the Particle theorist and a false one for the Wave theorist, and vice versa. However, since each theory enjoys the exact same level of success no matter which world the theorist is in, the inference to the truth of his favored theory can be no more than probability 0.5.[122] That is, the best either theorist could say is that they might be in the world where their favored theory is true, regardless in which world they were.

The fact that a probability of no more that 0.5 can be assigned to one's preferred theory, even if one is in the world where this theory is true, amounts to a dilemma for the no-miracles argument, according to Matheson. The proponent of the no-miracles argument can admit that, in the cases of empirical equivalence, the success of science is not best explained by the approximate truth, since the probability of the truth of any given theory is, at best, 0.5. Or, the proponent can maintain the claim that the success is still explained by the approximate truth but give up on argument to the best explanation: we do not infer the truth (or approximate truth) of our theories from their success.[123] This dilemma leads Matheson to provide his better explanation for the success of science – one which does not require the truth (approximate or otherwise) and is compatible with the entailment of underdetermination from the ubiquity of empirical equivalence.

Matheson claims that the best explanation for the success of science is not that science tracks the truth, rather it tracks empirical adequacy. He defines empirical adequacy as follows:

> Empirical Adequacy: A theory, T, is empirically adequate to degree n at world w1 if and only if there is a world, w2, observationally indistinguishable from w1, such that T is approximately true to degree n at w2.[124]

Notice that in the former case of Wave Theory and Particle Theory no matter which world we are in both are empirically adequate to degree 1 (there is a world where each is true – by stipulation – and they are observationally indistinguishable). Now is possible for each tradition to use the no-miracles argument to infer the empirical adequacy of their theory, no matter which world they are in, without generating the aforementioned dilemma, since in the case of truth there can be only one theory which is true but this is not the case for empirical adequacy. Thus, we now have an explanation for the success of science that does not jeopardize abduction. I believe that this argument is sound; however, I believe that there is no reason for this argument given Kukla's method for generating empirical equivalences.

3. Another objection

The no-miracles argument posits a set of data, the success (or progress) of a given scientific theory, and a best explanation for the data, the approximate truth of the theory in question. There is an obvious problem for the entire no-miracles project, if empirical equivalence is ubiquitous as I have claimed.

Recall that each theory T has at minimum one empirical equivalent rival, T!, generated by Kukla's method. 'Kuklizing' our best theory at each point in time generates the rival theory T!. Thus, the rival T!, enjoys the same success over time as T, since it is essentially the same as T, with the exception the unobservable Kuklized bit. Take T to be Kepler's theory of planetary motion and T! to be identical to T but with the shrinky planets and appropriate conspiracy theory to cover the shrinkage.[125] Notice that if we can infer the approximate truth of Kepler's theory on the basis of its success, then we can also infer the approximate truth of T! on the same grounds.[126] Thus, Kepler's theory does not gain any epistemic ground, so to speak, over its rival(s) on the basis of its historical success – both are equally approximately true. We can abduct from these present and past rivalries to rivalries at the end of science; therefore, there is no reason to believe that a no-miracles argument, wherever it is situated in the debate, will provide a resolution of underdetermination.

Therefore, even if we ignore the problems with the no-miracles argument (and the subsequent resolution of the problems in a manner that is compatible with underdetermination), the no-miracles argument cannot provide any additional evidence for one rival over its competitors, as the realist had hoped. It seems, then, that there is no recourse for the realist to claim that there may be other ways of resolving the equivalence.

5. A Final Critical Problem

In this chapter, I surveyed a variety of arguments, each intended to show that underdetermination does not follow from the ubiquity of empirical equivalence. In each case I demonstrated that there is no reason to deny that underdetermination follows from the ubiquity of empirical equivalence demonstrated in Chapter 2. However, what I have not shown is that EE *does* entail UD. Recall that empirical equivalence is in fact ubiquitous, the debate at this point has moved to the claim that this ubiquity impels us to accept that we should not believe that our best scientific theories are true (i.e. that EE does entail UD). Kukla argues that the anti-realist has failed to provide motivation for the claim that the entailment of underdetermination follows from the ubiquity of empirical equivalence. I will outline Kukla's argument, and I will suggest a possible strategy for resolving the debate.[127]

1. Motivating the entailment

Kukla notes that simply defending, as I have done in this chapter, the entailment is insufficient. The anti-realist cannot simply claim that the realist cannot unseat the entailment claim and then pack-up and go home; it is critical that she provide a motivation for the belief that underdetermination does, in fact, follow from the ubiquity of empirical equivalence.

Kukla argues that the belief that EE does entail UD can be motivated by an appeal to the Vulnerability Criterion of Belief (VCB).[128] This criterion works as follows:

> Let's say that two hypotheses T_1 and T_2 are *equivalently vulnerable* if there's no possible observation that disconfirms one of the hypotheses but not the other. ... if T_1 and T_2 are equivalently vulnerable, and if T_1 is logically stronger than T_2, then we should not believe T_1.[129]

Basically, if T_1 entails T_2, but there is no empirical evidence for one over the other, we should believe the theory that makes no additional claims.

We can see how this criterion provides reason to accept the ubiquity of empirical equivalence does entail underdetermination. If two theories, T_1 and T_2, are empirically equivalent, then they are equivalently vulnerable, since, as I have defended, there is no evidence that can point to one being more likely true than the other. Of course, the anti-realist will claim there is another theory, call it T_3, which is a theory that ascribes to only the empirical content of these theories (i.e. it is empirically adequate only). It follows that both T_1 and T_2 are logically stronger than T_3; thus, we ought only to believe T_3 – we have no reason to believe either T_1 or T_2. Therefore, in the VCB we find reason to believe that EE does entail UD.

2. A problem with the use of the vulnerability criterion of belief

While the VCB does seem to motivate the anti-realist's belief that EE does entail UD, it remains to be seen whether or not it should sway the realist. Kukla argues that the will not accept the VCB. He suggests that the anti-realist banks on the intuition that the VCB is some sort of default position:

> I think that the lure of VCB is due to an obscure sense that nonbelief is a default position for rational beings – that we abide in a state of nonbelief unless we are to encounter persuasive reasons that impel us to move. Perhaps some law of least effort is involved.[130]

He also believes that this is simply a ridiculous suggestion:

> To articulate this notion is to see that it's without foundation. At the moment we become reflective about our mental life, we already find ourselves immersed in a tangled net of beliefs. That is our de facto default position, and if we follow a law of least effort, we'll abide in our prereflective beliefs until circumstances force us to give them up.[131]

Thus, it seems there is no reason to accept the VCB if it amounts to the claim we ought to follow a law of least effort in belief formation. Furthermore, even if one were to accept a law of least effort reading, one could remain a leap-of-faith-realist (an individual who, in the initial position, believes that realism and has been offered no reason to change):

> It's true that no experience can compel us to adopt an invulnerable belief about the world. But it's also true that no experience can compel us to abandon one that we already have.[132]

So, if we already hold realistic theories about the world, there is no need to abandon them, especially if we accept a law of least effort. Therefore, at least, the anti-realist is left to provide a motivation for the VCB, one that cannot be formed based on a claim that nonbelief is a default position, since this simply appears to be wrong and cannot, at any rate, compel certain realists.

Moreover, the big problem for the VCB, according to Kukla, is that it begs the question against the realists:

> What turns people into scientific realists above all else is their belief that there are nonempirical properties of theories, such as their simplicity or explanatoriness, that have a bearing on their epistemic status. This is tantamount to the negation of VCB. To be sure, we've seen that realists have been unable to find any rational means of compelling antirealists to accept their intuition. But to base the argument *for* antirealism on VCB is simply to stipulate that the realists are wrong.[133]

This seems to be true when referring to a virtue-realist (i.e. one who believes that realism is true because at least one of the SEVs does, or will be shown to, track the truth). Kukla believes that the failure of the VCB to compel the realist amounts to a failure to motivate the claim that EE does entail UD, and consequently the failure of the Underdetermination Argument, as well as an impasse between realism and anti-realism.

3. A place for the underdetermination argument

Notice that Kukla's critique of the VCB is two pronged: 1- the anti-realist has failed to provide a reason to accept the VCB, and 2- the VCB begs the question against the (virtue-)realist. However, he has not ruled out the possibility that a case can be made for the entailment of underdetermination from the ubiquity of empirical equivalence to two possible groups: 1- people who are undecided between acceptance of the VCB (and the corresponding anti-realism) or one, or more, of the SEVs (and the corresponding realism), or 2- the leap-of-faith-realist may also be compelled to accept the VCB, where a different analysis of the VCB given (i.e. not a law of least effort reading). Thus, despite the fact that accepting that EE does entail UD begs the question against the virtue-realist, the anti-realist may be able to compel fence-sitters and leap-of-faith realists with the underdetermination argument; all that is required is that a motivation for the VCB is uncovered.

3.1 A possible motivation for the acceptance of the VCB

In epistemology, since Descartes himself, a standard question has been 'what is limit of human knowledge?' We are concerned with the discovery of just how much we can know. There seem to be two possible ways of discovering this limit: 1- we can assume that we know a lot, and then try to undermine our beliefs by generating skeptical problems, or 2- we can start with claims that are virtually – if not totally – certain, and build up as far as we can. Both of these methods appear to be underwritten by a single goal: epistemic safety. The goal is to hold only those beliefs that can be appropriately justified, or are the safest ones to which to ascribe. So, insofar as epistemology is the pursuit of the limit of human knowledge, I believe that a fundamental goal of this pursuit is to be safe (i.e. avoid error).

Likely one can see where this line of reasoning will take us. If it is critical to be safe in one's beliefs, then it is safest to ascribe to the VCB, which asks us to accept the safest theory – the one that will make the least false claims (in light of the fact that we <u>cannot</u> be sure which of the empirically equivalent rival theories is true, unless we have independent motivation for virtue-realism, which we don't). If we employ the VCB, then we only believe a theory to be empirically adequate (its observational entailments are true), and we suspend belief about the rest of the theory (its non-observational claims). This methodology would put us in the safest position with respect to error avoidance. The only error we can make if we employ VCB is to fail to believe claims that are true. Let's say we've selected T_3, an empirically adequate version of both T_1 and T_2, on the basis of VCB. However, T_1 makes only true claims; in believing T_3, we have made the error of not believing T_1's claims that are beyond the observable level. However, we have avoided selecting T_2, or another of the many empirically equivalent rival, which makes many errors. Thus, it seems, we have obeyed the epistemic rule of safety in belief ascription to the best of our abilities if we employ VCB.

It seems to me, then, that the fence sitter, or the leap-of-faith-realist, may be swayed to accept VCB rather than one, or more, of the SEVs by an appeal to a fundamental goal of epistemology. If they are swayed by this, or some other motivation for VCB, then the Underdetermination Argument can be levied against them.

3.2 A problem for the safety motivation

It could be argued that the demand that we hold only safe, an ambiguous term at best, beliefs, has been erroneously analyzed above. Safety could be analyzed in terms of the greatest probability of being true, rather than error avoidance. If this were the case, it is certainly possible that in some cases of theory selection T3, the empirically adequate version of several empirically equivalent rivals, is not the most likely to be true. Thus, my offered motivation for the VCB depends on a lengthy debate about the goals of epistemology and an analysis of the safety requirement, if it turns out to be a legitimate goal of epistemology. This is a debate that I will not carry out at this time; however, the preceding discussion does offer an interesting strategy for the vindication of the Underdetermination Argument.

4. The strategy for the vindication of the Underdetermination Argument

Recall that a place for the Underdetermination Argument has been offered: the argument could be levied against fence-sitters or leap-of-faith-realists if the VCB can be motivated. Above I offered a possible motivation for the VCB, I also showed that a good deal of argument is needed before my suggestion can be taken seriously. However, it opens an interesting avenue of argument for the anti-realist: provide reason to believe that there is reason to choose VCB over the SEVs for those who do not already accept one over the other. I will pursue this strategy no further here; however, there remains a criticism of the strategy in general.

4.1 The strategy must have already failed[134]

If it were possible to find some independent argument for the VCB over the SEVs, or vice versa, then, the realism/anti-realism debate would not be at an impasse, it would be resolved. With an independent motivation for either the VCB or some SEV, one position would enjoy a greater level of coherence than the other, and members of the other camp would be rationally compelled to switch sides. Since it is the case that neither camp has been abandoned, we have good reason to believe that there can be no independent motivation for the VCB, as I have suggested there might be.

4.2 A possible reply[135]

Rational decision making is not as obviously simply as the above objection makes out. It is unclear that we, qua humans, make theory switches/decisions on the grounds that one theory enjoys a greater coherence. It may be the case that there is an independent motivation for the VCB that will compel fence-sitters, but that will not compel those that believe the SEVs to have epistemic import.

Let us assign some numbers to see how there might be enough added coherence to compel some and not others might work. Say that anti-realism enjoys a coherence score of 99, with a well-analyzed version of VCB, and realism enjoys the same coherence score of 99, with a well-analyzed version of at least one SEV. Now, let us say that there is an independent argument for VCB over the SEVs and this adds to the coherence score of the position a total of 1 point. Now, we can see how the fence-sitter, or even the leap-of-faith realist, may be swayed to adopt anti-realism at this point, due to its better overall coherence; however, one could see how a stalwart defender of realism may well be unmoved. With such a small change in coherence, the realist may well want to hold out for more argument, or see no reason at all to switch camps. Also, consider this other possible case. Again the independent motivation for VCB again adds a single point to the overall coherence of anti-realism. However, the anti-realist project only enjoys a coherence score of 98 without the motivated VCB, and the realist's project already has a score of 99. In this case, we can see how the fence-sitter could be swayed by the underdetermination argument, properly motivated, but that the realist camp, would, of course, not have be abandoned by its proponents.

The preceding defense of the strategy I offer for the anti-realist is far from resolved; a resolution can only come with a good deal more work on how it is humans make rational switches/decisions with respect to theory selection – quite the project in its own right. However, I believe that it has been shown that the strategy is, at least, a viable suggestion for a place and defense of the VCB. And, of course, if the VCB can be motivated, so can the claim that EE does entail UD, at least to some.

6. Conclusion

In this chapter I believe that I have successfully defended the claim that underdetermination can follow from the ubiquity of empirical equivalence. Realist attacks on the claim include appeals to the following: Supra-Empirical Virtues, which claim that certain virtues of theories have evidential status; the flow of evidence, which is an appeal to the connection of theories as evidence for one rival over another; and the historical success of a theory, which amounts to the claim that we have reason to believe our best theories over their rivals, since they have done so well. I have shown that each of these fail to usurp the anti-realist's belief that underdetermination does follow from the ubiquity of empirical equivalence. However, the realist can argue that the anti-realist has provided no reason to believe that underdetermination <u>is</u> entailed. I have offered a strategy for the anti-realist to pursue in order to motivate the entailment; however, such a strategy includes abandoning the belief that the underdetermination argument could ever sway a virtue-realist, though it may well still be used against certain realists or fence-sitters.

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[1] Descartes' First Meditation, taken from The Rationalists (1960), pages 116 – 117.
[2] Clearly, the Cartesian Evil Genius oversteps the bounds of the underdetermination argument, since the anti-realists do not seek to challenge the justification of empiricism in general; rather, they challenge only our belief in the theoretical. Thus, the underdetermination thesis is a localized version of the genius.
[3] The use of the term "appropriately ubiquitous" is meant to capture the essence of the underdetermination of theory by evidence, which is intended as a formal, a priori matter. Underdetermination is intended to be a transcendent form of skepticism, not a merely a problem that arises in a few cases.
[4] EE may not even need to be this strong to generate the underdetermination problem. However, I believe this criterion can be satisfied as is, so I will not deal with other possibilities at this time.
[5] The general framework for the Underdetermination problem, as presented here, is from A. Kukla's book (1998).
[6] Earman, (1993).
[7] Van Fraassen notes that the term "literally" is vague; however, he believes, as do I, that it can be well enough understood to support an appropriate reading of what it means to read a scientific theory literally.
[8] Earman, (1993) page 19.
[9] The very least of the skeptical worries that the realist must avoid are of Putnam's disastrous induction.
[10] Thanks to R. Remillard, Dr. T. Schroeder, Dr. R. Martens, and Dr. C. Matheson for helpful discussions on this topic.
[11] Fodor believes that the best explanation for our language acquisition is that we are born with a "language of thought." Such a language exists in our minds at birth and is the basis for all understanding of language. If perception is modular (I will define this shortly), then it should be easy to see that Fodor would hold that such perceptions are on par with our "language of thought," in that they are pre-theoretic in nature. The combination of these two theory-neutral (they are hard-wired in some sense) abilities is sufficient to produce some theory-neutral language.
[12] I hope that this terminology is not too arcane. Direct (realistic) perception is entailed by our perceptual mechanisms giving us access to the external world that requires no intermediary (in this case, no inference). Indirect (realistic) perception is entailed by our perceptual mechanisms giving us only mediated access to the external world. I include 'realistic' in brackets because neither the scientific realist nor scientific anti-realist will be compelled by phenomenalistic accounts of perception.
[13] For a more comprehensive argument and exegesis for the modularity of perception and an outline of Fodor's observation-theory distinction, see Fodor, J. (1984) Observation Reconsidered. Philosophy of Science v. 51. p. 23 – 43. See especially pages 36 – 38 for a discussion of modular perception.
[14] Bruner, J. (1957) On Perceptual Readiness. Psychology Review v. 64, p. 123-152.
[15] This is not Fodor's example, he uses the Muller-Lyer lines, but I find it more illustrative.
[16] It is also possible that the anti-realist could claim that the theory that states that observation is modular has empirically equivalent rivals and that these rivals also entail that observation is safely modular. This conjunction would allow them to avoid having to admit that there are some theories that have no empirically equivalent rival. However, since I believe that there is a version of the evidence-theory distinction that does not face this worry, there is no reason to pursue this line of defense.
[17] Van Fraassen, B. (1980) The Scientific Image. Clarendon Press, Oxford, p. 14.
[18] Ibid. p. 16.
[19] Ibid. p. 17.
[20] Ibid. p. 17.
[21] Maxwell, G. (1962) "The Ontological Status Of Theoretical Entities." In H. Feigl and G. Maxwell (eds.) Scientific Explanation, Space, and Time. University of Minnesota Press. p. 3 – 17.
[22] Van Fraassen, B. (1980) The Scientific Image. Clarendon Press, Oxford, p. 19.
[23] Dr. Carl Matheson disagrees. He believes that Maxwell's charge is truly a simple charge of arbitrariness. Of course, if this is the case, then the problem is one that Van Fraassen simply does not answer. I must admit, that if this is the case, then I have no answer either – so just ignore the following argument and move on to the third section of this chapter.
[24] Van Fraassen, B. (1980) The Scientific Image. Clarendon Press, Oxford, p. 18.

[25] I believe that goal relativism is a very serious epistemological problem. It is my particular belief that no resolution to this problem will be found. However, this is an argument for another time. The import of this comment is merely to point out that it is my feeling that no particular goal can be satisfactorily justified.
[26] As noted in endnote 10, this goal is no more or less arbitrary than any other.
[27] This section is mostly a survey of the problems Andre Kukla points out in Studies in Scientific Realism, 1998, Oxford University Press, Oxford, Ch. 10.
[28] I have taken this citation from A. Kukla's 1998 book: Churchland, P. M. (1985) "The Ontological Status Of Observables: In Praise Of Superempirical Virtues." In P. M. Churchland & C. A. Hooker (eds.), Images of Science. University of Chicago Press. p. 35 –47.
[29] Van Fraassen, B. (1985) Empiricism in the philosophy of science. In P.M. Churchland & C.A. Hooker (eds.), Images of Science. University of Chicago Press. p. 256 –257.
[30] Providing an empirically adequate theory involves providing a description of the world where the observable objects and events of the world are covered.
[31] Van Fraassen points out that 'scientific community' requires further analysis, but claims that this task is quite large, and is a burden that does not fall solely on proponents of the underdetermination argument.
[32] As Kukla points out, this would mean that our blind scientists should treat their sighted colleagues as members of a different scientific community. This seems overstating the bounds of Van Fraassen's suggestion. The blind scientists ought merely not take the visual data of their colleagues to be a part of the scientific data for their scientific community: it is not the case that the sighted scientists are a radically different community. However, unless a distinction can be made, the proponent of a van Fraassian type distinction may well have to do some bullet biting.
[33] Friedman, M. (1982) Review of van Fraassen (1980). Journal of Philosophy v. 71, p. 278. Thanks to Kukla (1998), which contains this quotation.
[34] Here this question is merely rhetorical, though there is some serious debate over the need for language in the formulation of beliefs.
[35] In the next chapter, I will argue at length that empirical equivalence is ubiquitous. If empirical equivalence is ubiquitous, then the proponent of the observable-unobservable distinction has at least one other theory in which to couch her beliefs about observable consequences.
[36] Musgrave, A. (1985) *Realism versus Constructive Empiricism* in Philosophy of Science: The Central Issues, Curd, M. & Cover, J.A. (Ed.s), 1998, W. W. Norton and Co., London. p. 1088 – 1112.
[37] Kukla, A. (1998) Studies in Scientific Realism. Oxford University Press, Oxford, p. 138-9.
[38] Ibid. p. 139.
[39] Throughout this section, I speak of scientist's attitudes and behaviors. I do not take it to be the case that actual scientists would speak and act the way I suggest; I do, however, take my comments about the actions and attitudes of scientists to be a reasonable rational reconstruction of the operation of science.
[40] Since I intend to offer only a best explanation argument for externalism in the smallest portion of science, I will not be exploring externalism in general. However, it is safe to say that if the externalism project is a failure, then my argument collapses. Also, I externalism 'spills over' and captures all of science then the realism/anti-realism argument is of little import and underdetermination is irrelevant. I argue that externalism applies only in one small facet of science.
[41] I will return to the question of the coherence option shortly.
[42] My familiarity with Entitlement comes from work by Tyler Burge, but I will leave the details of this work to others. See Burge, Tyler & Peacocke, Christopher (1995) 'Our Entitlement of Self-Knowledge,' in The Proceedings of the Aristotelian Society. p. 91- 150. These two argue for entitlement in the case of self-knowledge, I argue for perceptual entitlement, so their work is only of tangential interest – though they are the ones who introduced me to the idea of entitlement.
[43] I am grateful to Dr. R. Martens for her helpful questions and interest in discussion of this section in particular.
[44] Here I say 'we' to refer to our current scientific community.
[45] Rowe, W. (1992) Philosophy of Religion: An Introduction. 2nd Ed. p. 60.
[46] Kepler took it that Brahe's observations of the heavens were veridically experienced.
[47] The reason that this must hold will be advanced in Chapter 2.
[48] This is only true if the community in question does not presuppose scientific realism.
[49] Admittedly, this is more of an intuition pump for my claim than an argument.

[50] I would like to thank Dr. J. A. Bailey, C. Derksen, Dr. R. Martens, Dr. C. Matheson, and Dr. T. Schroeder for their helpful comments and criticism on a plethora of versions of this paper.
[51] In discussion with Dr. C. Matheson, it has become clear that even this feature of empirical equivalence may well not be necessary for the equivalence itself.
[52] This section is a synopsis of the defeasibility argument as found in Laudan and Leplin's 1991 paper.
[53] By 'advances' in science, we need to mean nothing more than science changes over time.
[54] By 'properly' here I mean satisfied in such a way that will be able to produce the "preordained and transcendent skepticism" that the anti-realist propounds.
[55] Laudan and Leplin note that it is unlikely the "line" will move back; however, it is, in principle, possible that it will.
[56] Optics is a specific example, apt for modern science. A theory of perception is, of course, required to determine the observational entailments of a given theory.
[57] When I use the phrase 'the best set of available auxiliaries,' I am using the language of the realists for the sake of the reconstruction; the anti-realist would likely be happier with 'a set of acceptable auxiliaries,' since the use of 'best set' assumes that there is a single set of auxiliaries which is better than the others in some real way.
[58] Laudan and Leplin, (1991) page 454.
[59] Laudan and Leplin, (1991) page 452.
[60] Boyd, R. (1984) The Current Status Of Scientific Realism. In J. Leplin (ed.), Scientific Realism, University of California Press. p. 41-82.
[61] A total science is the combination of a theory and its auxiliaries.
[62] Kukla (1998).
[63] I am happy to pluralize this statement if both theories remain empirically adequate after the evolution.
[64] Of course, Laudan and Leplin are unable to concede that auxiliaries can be confirmed independently, as such a concession would violate their belief in the 'uncontroversial' NAP.
[65] Laudan and Leplin, (1993) page 9.
[66] Laudan, L. and Leplin, J. (1993) Determination Underdeterred: Reply to Kukla." In Analysis, 53.1, January, 1993
[67] Laudan and Leplin, (1993) page 10.
[68] Of course, Laudan and Leplin will point out that we have reason to believe that this new rival will also be rejected in further evolutions. This justifies our belief that our preferred theory is in some way better than its guaranteed rival. However, this gloss on the argument does nothing to harm the anti-realist's belief that the EE criterion is satisfied, since it merely challenges the believability of the rival theory (an attack on the 'EE entails UD' criterion).
[69] Laudan and Leplin, (1991) page 456.
[70] Laudan and Leplin, (1991) page 457.
[71] Kukla, (1998) footnote 2, page 166.
[72] Recall that I claimed that a reasonable analysis of empirical indistinguishability was the claim that two theories had identical empirical consequence classes. Kukla would need a different analysis, which, simply, he has not provided. I will leave this debate here; however, I am concerned that there may be trouble for an analysis of empirical equivalence that differs from the one I have given.
[73] While the debate with respect to the observable/unobservable distinction continues, I have do admit I find the Van Fraassian accounts far more compelling. I will take it on faith that the debate will turn out in my favor. I believe that versions of the distinction, such as Laudan and Leplin's, characterize observables too broadly. If the debate does not turn out in favor of some Van Fraassian version, then I am willing to fall back to Kukla's claim that T and T! do have divergent empirical consequence classes but remain empirically indistinguishable.
[74] Or that appear in the Manifest Image if one accepts my account of the distinction.
[75] Of course, this is still a somewhat vague story about what it is to be observable, since it is unclear what really counts as a detection device. Do artifacts such as telescopes count as detection devices, or not? If they do, then corrective eyewear seems to be a detection device. I believe the distinction between detection devices and perceptual aids is as follows: perceptual aids merely allow people with lesser observational capacities to observe what the average human could already observe. I have by no means added anything to the debate, nor do I hope to resolve it. However, the reason I note some of the problems that arise for the

distinction, is so that the reader will be aware that the particular defense I am about to advance on Kukla's behalf requires a greater amount work than it may seem and than is provided here.

[76] Kukla, A., (1998) page 166.

[77] Laudan and Leplin, (1993) page 12.

[78] Ibid. p. 12.

[79] Kukla, A., (1998) page 68.

[80] Here, again, I am using Van Fraassen's account of observable.

[81] It is interesting to note that Kukla is interested not only in launching an attack on "warranted belief in a particular set of theoretical entities," but also one on "warranted belief in theoretical entities tout court." (Kukla, 1998) Such an attack does not only require that his method produce empirically equivalent theories, but that the instrumentalist's algorithm also produces genuine rivals, in order to usurp belief in theoretical entities tout court. Thus, his focus on semantic parasitism is strategic: he uses it to motivate a defense of the instrumentalists' algorithm.

[82] Of course, 'the proper characteristics of a theory' is vague; however, I will not undertake a defense of this claim, since it is unclear in the texts and it is unimportant to the reply that I will offer.

[83] I still need to obtain this reference from Dr. Martens, who was kind enough to point it out to me in discussion. (It is either in Lattis 1994, or Jardine 1979).

[84] Kukla, also, offers an example of this sort in his book, Studies in Scientific Realism, p. 70. His, however, demonstrates that the realists may want to hang on to theories that resemble those generated by the instrumentalists' version of the algorithm using an example from psychology.

[85] It may even be the case that the planets must jump in anticipation of heavenward glances, so that the light has time to reach us.

[86] Dr. Schroeder has helpfully pointed out that while we may never observe electrons, there seems to be a place for them in commonsense and our intuitions. Certainly, I can agree that electrons do not seem counter-intuitive, since they are part of our best scientific theories. However, it still seems to me that we would be shocked by anything which we could have no contact with at all, either by common scientific theorizing or by observing.

[87] Laudan and Leplin, (1993) page 11.

[88] Laudan and Leplin, (1991) page 450. Laudan and Leplin merely suggest that Hume's de facto constant conjunction may be an empirically equivalent rival to our best theories of causation.

[89] An evaluation of arguments from scientific practice may be a good starting point for such a strategy.

[90] Kukla offers argument with regard to the value of this sort of deferral in his book. He concludes that the fact that the experts disregard theories such as T! is otiose in the argument for EE. I will not concern myself with this argument in this paper, since Laudan and Leplin are happy to concede this move will not be accepted by many theorists and offer a criterion for genuine theories.

[91] Laudan and Leplin, (1993) page 13.

[92] Ibid. p. 13.

[93] I am indebted to Dr. T. Schroeder for pointing this attack out to me in discussion.

[94] Of course, realists can make use of pragmatic values, however, it is difficult to see that such values can be used to rule on the truth of a theory.

[95] Just a point: it is still possible to believe in the truth of our evidence, without believing in any given theory.

[96] Alternatively, one could simply drop the physical causation auxiliary hypothesis altogether – as I suggested in the previous chapter.

[97] There are at least two senses of believability: believability understood in terms of being psychologically compelling, and believability understood in terms of being probable (more likely true). So far I have taken believability to be taken in the first sense - any other analysis would require greater exegesis on the part of the realist and, I believe, would likely turn out to be either one of the SEVs in disguise or a question begging demand for realism.

[98] Clark Glymour offers a good deal of argument with respect to the SEVs (especially simplicity) and their import as empirical evidence; unfortunately, time constraints have made it impossible for me to include an analysis of his work. Consider this section's conclusion as pending a proper analysis of his work.

[99] However, this is not simply a super-empirical virtue, but a necessity for human conception – i.e. there are no round squares.

[100] Dr. Matheson points out that there are many readings of 'general coherence.' This points to deeper problems for the realist than I even concede to them on this point.
[101] The anti-realist can accept that the SEVs have pragmatic value and still deny that they are evidence for one theory or another.
[102] This objection comes from Clendinnen, but my understanding of it comes from Kukla (1998).
[103] For a more detailed discussion of this strategy, see Kukla (1998) Chapter 6, p. 83 – 84.
[104] Laudan and Leplin (1991), p. 466.
[105] Ibid. p. 466.
[106] As far as I am aware, the H-D model is the only model of science that claims that the empirical consequences of a given theory are its only evidence.
[107] Laudan and Leplin (1991), p. 472.
[108] There may be some cases where EE will in fact entail UD, but a few, or even many, such cases are far from generating the ubiquitous and transcendent underdetermination in which the anti-realists and skeptics believe.
[109] It should be obvious that Laudan and Leplin's example of the coffee drinker is not an empirical equivalent to our best theories. While they do not need an empirical equivalent for their example, we should be wary of how their argument plays on our intuitions though. It may be the case that there are some data points that fail to support bad theories, but Laudan and Leplin need to show that this has an impact on empirical equivalent rivalries (i.e. they'll need an example where this happens in the case of empirical equivalents).
[110] Kukla, A. (1998) Studies in Scientific Realism. Oxford University Press, NewYork. p. 89.
[111] Ibid. p. 87.
[112] Boyd, R. (1984) The Current Status Of Scientific Realism. In J. Leplin (ed.), Scientific Realism, University of California Press. p. 41-82.
[113] No indirect confirmation can occur at the level of total sciences for the reason that there are no parent hypotheses for a total science.
[114] Hoefer, C. and Rosenberg, A. (1994) 'Empirical Equivalence, Underdetermination, and Systems of the World,' Philosophy of Science, 61. p. 592 – 607.
[115] With arguments about local theories in place either group could abduct to complete total sciences.
[116] Kukla, A. (1998) Studies in Scientific Realism. Oxford University Press, NewYork.
[117] Matheson, C. (1998) 'Why the no-miracles argument fails,' International Studies In The Philosophy Of Science, 12. p. 263.
[118] Progress (or success) is measured in terms of capturing the available observational phenomena.
[119] Matheson carefully notes that these names are to be used as shorthand only and should not be confused with the claim that such theories are examples of completed total sciences.
[120] Here we are talking about the truth, not approximate truth; since these are completed sciences, there is no longer any need for approximate truth talk.
[121] It is of little import which of the two is the actual world, if either.
[122] Of course, Matheson, correctly, notes that this assigned probability would decrease with each additional complete total science.
[123] This paragraph is essentially a direct paraphrase of and argument from Matheson, C. (1998) 'Why the no-miracles argument fails,' International Studies In The Philosophy Of Science, 12. p. 271.
[124] Ibid. p. 271.
[125] This example is found in Chapter 2, please refer back to said chapter if there is any confusion about this example.
[126] The careful reader will note that this strategy is virtually identical to the one Matheson presents with respect to wave theory versus particle theory.
[127] What follows comes from a discussion of this particular problem between Dr. C. Matheson and Dr. T. Schroeder. I am indebted to them for the strategic content of this final section.
[128] He also believes that this criterion underlies all of the anti-realist's arguments, not just the underdetermination argument.
[129] Kukla, A. (1998) Studies in Scientific Realism. Oxford University Press, NewYork. p. 98.
[130] Ibid. p. 105.
[131] Ibid. p. 105.
[132] Ibid. p. 105.

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[133] Ibid. p. 105.
[134] This objection was suggested by Dr. T. Schoeder, to whom I am indebted.
[135] This reply was suggested by Dr. C. Matheson, to whom I am indebted.