Cornering The Truth:

A Defense Of Scientific Realism

by

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ABSTRACT

This is a study of scientific realism, and of the extent to which it is undermined by objections that have been raised by advocates of various forms of antirealism. I seek to develop and present a version of scientific realism that improves on past formulations, and then to show that standard antirealist arguments against it do not succeed. In this paper, I will first present my formulation of scientific realism, which conceives of theories as model-based and as fundamentally non-linguistic. I advocate an epistemic position that accords with indirect realism, and I review and assess the threat posed by theses of underdetermination. Next, I review and discuss three important views: the antirealist constructivist view of Thomas Kuhn, the realist view of Norwood Hanson, and the antirealist constructive empiricist view of Bas van Fraassen. I find merits and flaws in all three views. In the course of those discussions, I develop the theme that antirealists' arguments generally depend on assumptions that are open to question, especially from the perspective of the version of realism I advocate. I further argue that these antirealist views are undermined by their own tacit appeals to realism.

DEDICATION

I dedicate this dissertation to the most supportive, patient, and loving friend I have in this world, my beautiful and intelligent wife, Dan Li. I also dedicate this work to my astonishingly brainy and sweet children, the lights of my life, Levi and Elijah. Life has no meaning without my family.

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TABLE OF CONTENTS

CHAPTER ONE: REALISM, ABDUCTION, AND UNDERDETERMINATION

Formulating a Realist Thesis

Realism is a topic that permeates across many areas of philosophical investigation, including morality, color, mathematics, fiction, possible worlds, and science. What I will discuss in this paper only addresses philosophy of science and will have little to no bearing on those other interesting fields of study. When I speak of 'realism' throughout the remainder of this paper, it will be shorthand for 'scientific realism.' – The first order of business is to formulate a clear thesis of scientific realism; and this is not easy to do, for there are a variety of positions within this camp. Brock and Mares (2007:2) suggest all forms of realism share at least these two basic sub-theses: (i) the existence thesis that there exists facts or entities distinctive of the particular domain (in our case, the scientific domain); and (ii) the independence thesis that the existence and nature of these facts/entities is "in some important sense objective and mind-independent" (*ibid*.). This is a good start, though I observe that many, if not most, forms of scientific antirealism also share these two sub-theses, so more discrimination is required. Both the existence and independence theses are metaphysical in nature. Epistemological concerns comprise a good deal of the discussion in scientific realism, and so, a further refinement in this direction is needed. Brock and Mares propose two additional sub-theses: (iii) the confidence thesis that "although it may in some circumstances be difficult, we are always capable of coming to know about the existence and nature of the domain we are realists about. That domain is epistemically accessible to us...” (*ibid*.); and (iv) the insecurity thesis that “It is possible to be in ignorance or error about the domain we are realists about. In order to avoid such mistakes, one must make appropriate contact with the domain in question, and there is no guarantee that anyone will succeed in doing that” (2007:6). These two sub-theses are separable and non-exclusive; that is, some realists may adopt one or the other or both. I would disagree that "always capable of coming to know" and "there is no guarantee" are generally reconcilable.[1] I also point out that the insecurity thesis is rarely a feature of scientific realism.[2] Finally, I observe again, both sub-theses are also shared by most forms of scientific antirealism. In the scientific realism debate, I suggest, the central dispute regards the choice of proper domain of discourse for science (whether the class of all entities of the physical universe or just observable entities or just sense-data, and so on), but once chosen, realist and antirealist accounts alike generally satisfy three or more of the above four sub-theses. Nevertheless, I commend Brock and Mares in moving the attempt at a formulation in a generally good direction.

Another approach is to begin with scientific antirealism and limn the outlines of realism by contrast. One category of antirealism is *prefix fictionalism* according to which statements that *prima facie* appear to be asserting the existence of a fact or entity or asserting the truth of a certain scientific law are to be, instead, construed as though the following was prefixed to the statement: 'according to such-and-such theory...' (*ibid* 28).

[1] Of course, there are feats we are capable of doing but which we fail to achieve. However, the realist cannot tolerate the case of always capable but never achieving, for it opens the door to antirealism again and arguably undermines the basis of the alleged capability.

[2] Dr.Creath points out that Putnam is a realist who accepts the insecurity thesis.

In other words, as van Fraassen (1980:35) puts it, to the claims of, e.g., Rutherford's atomic theory (which appear to assert things like 'atoms exist'), the fictionalist will construe these claims as: "the observable world is nevertheless exactly as if Rutherford's theory were true..."[3] Thus, the fictionalist treats the *prima facie* claimed entities to be, rather, useful fictions. Another standard antirealist position is instrumentalism. Instrumentalism and fictionalism are often equated, since instrumentalists treat *prima facie* claims of existence as merely a means to a non-realist end and both positions evaluate theories by measure of usefulness. However, one[4] could dispute this equation, arguing that the instrumentalist differs from fictionalist in the instrumentalist's construing the *prima facie* existence claims literally, but then opting only to *accept* such claims and not *believe* them, where 'acceptance' only obligates one to regard the claim as empirically adequate but not (necessarily) true, and 'belief' obligates one to regard the claim as true. The distinction between belief and acceptance is one famously advocated by van Fraassen (1980:12). However, van Fraassen would quarrel with this distinction as a basis for defining *instrumentalism*. van Fraassen rejects instrumentalism (as he differently defines it) on the grounds that it does *not* give a literal construal of existence claims. This clash in terminology may be the basis for some confusion among authors, many of whom regard van Fraassen as an instrumentalist while van Fraassen himself explicitly denies he is an instrumentalist. We could use *constructive empiricism* for the position that a scientific theory is to be construed literally but only accepted and not believed, and *genuine instrumentalism* for the position that withholds existence claims and merely treats the theory for its outward effects. A third standard type of antirealism is

[3] This is not van Fraassen's position, just his well put characterization of fictionalism.
[4] Brock and Mares (2007:29)

constructivism, e.g. the sort propounded by Kuhn. This position holds that *prima facie* claims of the existence of, e.g., unobservable entities are to be construed literally and believed; however, the referents of the referring terms in these claims are understood not to be things in the world, but rather, items within a (social) construction.

So, to sum up this non-exhaustive but classic set of antirealist positions, an antirealist may treat *prima facie* existence claims: (a) as proper to construe non-literally and to be believed; or, (b) as proper to construe literally but only to be accepted, not believed; or, (c) as proper only to treat instrumentally (without regard for content); or, (d) as proper to construe literally and to be believed, while ruling-out anything other than items in a (social) construction as the referents for terms. Of course, there are other important forms of antirealism left off this list (notably, *structuralism*), but this list serves as an adequate indication of the negative space in which realism may occupy. – van Fraassen has devised a formulation of realism against this sort of antirealist foil, as follows:

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

Here, van Fraassen emphasizes that the realist construes certain *prima facie* existence claims literally, holds these as capable (in principle) of being fully believed, and regards the referents for terms to be items in the actual world. We can read van Fraassen as implying that the realist subscribes to the existence and independence theses. In stressing that the realist only “aims” at truth, van Fraassen is distinguishing the genuine realist from the *naive* realist who believes that any empirically successful theory is therefore true. So, van Fraassen’s formulation for genuine realism is weaker than the confidence thesis, but stronger than the insecurity thesis.

Of course, it is never wise for one holding a particular position to accept the formulation of one's position as given by the opposing and hostile camp. The above antirealist positions were all presented with a linguistic orientation, where science is understood to manufacture statements and where antirealists differ from one another in the way they treat those statements. This linguistic orientation is one dominating 20th century philosophy, and van Fraassen would certainly be correct in the way his formulation indeed describes the bulk of 20th century realists, who similarly adopted the linguistic orientation. Echoing van Fraassen formulation, Brock and Mares divide scientific realists into two camps: those who believe “the purpose of science is to provide us with true [or approximately true] postulates of laws and entities” (135) and those who more modestly hold the same, save just the entities, not laws, mentioned in the postulates are worthy of belief.[5] This formulation, like van Fraassen's, also holds that realism amounts to the sort of attitude one adopts towards a certain class of sentences.

Another commonly agreed aspect of realism, much discussed by van Fraassen (and Brock and Mares), is its advocacy and use of abductive reasoning. However, there is wide divergence, even among realists, over how to define abductive reasoning. It is

[5] Dr. Creath notes this is the position of Nancy Cartwright

usually understood as some form of *inference to the best explanation* (IBE), and explanations are linguistic artifacts. van Fraassen spends a third of his book, *The Scientific Image*, discussing the concept of explanation in order to counter realism, which indicates he regards abduction as essential to the realist position. Hanson, a proponent of realism, makes abduction (understood roughly as IBE, but which he calls ‘retroduction’) a centerpiece of his pro-realist arguments. The *no miracles argument*, advanced in one form or another by major realists (such as Smart, Putnam, Boyd, and Worrall), is regarded by most as abductive in character. Suffice to say, abduction should be included in the formulation of the realist position. To this end, we may modify ‘...involves the belief that it is true’ in van Fraassen’s above formulation of realism to read ‘involves abductive inference to the belief that it is true.’ One notable exception to this rule is Popper who respected only deduction as the lone, legitimate form of inference. – Conversely, it has also become a hallmark of antirealist positions to reconstrue, reduce, or discredit abduction, preferring instead to stick to probabilistic reasoning as the distinctive inferential method of science. The motivation for this will become clear when we study antirealist epistemological and metaphysical commitments.

I suggest that the realist need not adopt the linguistic orientation mentioned above to differentiate himself from his rivals (and even from other fellow realists). This would require, if subscription to abduction is retained, a non-linguistic rendering of abduction, which entails a departure from explanation. Work has already been done in this direction by those who view abduction as a kind of model-based reasoning. I propose to conceive of *models* as non-linguistic artifacts. If we denote as *target* the thing being modeled, then a crude notion of this sort of inference could be: Insofar as the model bears some similarity to the target, we have reason to believe the target is similar in further ways not yet observed to the model. I emphasize this is my own suggestion, and I do not pretend this version of model-based reasoning represents any other viewpoint. Entreating the reader's patience and charity in the use of my idiosyncratic reading of abduction as this sort of model-based reasoning, then I will press forward with framing a new formulation. But first, we should look at one more key ingredient to this debate, *viz. underdetermination*.

Antirealists have developed a wide variety of arguments to combat realism, but among the oldest and most potent are the ones which attempt to undermine credence, built on the basis of evidence *E*, in a belief *B* about the world on the grounds that this self-same evidence *E* would lend identical inferential support to some other beliefs *H*, *I*, *J*,..., which are all incompatible with *B*. This basic argument, in one form or another, has been the fountainhead of skepticism for millennia, and it has underwritten the less-than-skeptical scientific antirealism. So taken for granted is this argument that many prominent antirealists (like van Fraassen[6]) hardly bother to mention it, let alone defend it, while yet constructing their polemics on the back of it. In its more mature form, underdetermination argues that, for any given theory T which is both consistent with and relevant for a body of evidence *E*, there exists an infinite number of other theories, incompatible with T, which are equally consistent with and relevant for *E*. One would be safe in generally characterizing the scientific realism/antirealism debate in terms of either

[6] van Fraassen certain makes explicit and extensive use of underdetermination, but he almost never mentions it by name or characterizes it or mounts a particular defense.

party's response to underdetermination, with realists denying or downplaying the severity or inescapability of the threat and with antirealists emphasizing the threat (to realists) and motivated to find means of escape by re-assigning our commitments from the world to something more local, accessible and knowable (and presumably safe from the underdetermination threat). If this characterization is correct, then I urge that we modify the above language-oriented formulation of realism as follows (with the newer modification italicized):

> Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves abductive inference to the belief that it is true, *such that it provides grounds for coping effectively with underdetermination in preserving confidence in that belief.* (language-oriented formulation of realism (LR))

Of course, there will be exceptions among realists who give little consideration to abduction or underdetermination. However, I am intending my formulation of the language-oriented version of the formulation of scientific realism to be more normative than descriptive, especially with respect to underdetermination. My opinion is, foremost, that any realist argument which fails to confront underdetermination is not worth its salt. The attempt to cope by way of abduction is, I submit, a fair description of what many realists have historically done.

van Fraassen himself offers an alternative to the language-oriented formulation of realism based on his 'semantic view of theories' in terms of models. While he is sometimes less than clear about which sorts of things are to count as models, he certainly is clear that it could include non-linguistic artifacts. He also is quite straightforward in equating with *truth* the isomorphic correspondence between the structure of a model and the target in the world (1980:197). A scientific theory, on the semantic view, is fundamentally *not* a set of sentences (i.e., theorems), but rather, a theory is a collection of models. van Fraassen lauds this approach as superior, for (a) the fact that a model is obtainable already guarantees its consistency; (b) the same set of models could be described in any number of radically different ways by any number of different languages, each language with its own limitations (1980:44). Of course, van Fraassen goes on to fold the semantic view of theories into his antirealist account, but in delivering this basic starting point, he presents the possibility and virtues to the realist of an alternative formulation of realism.

Let's understand by *taking a model seriously* both (i) using the model for the purpose of telling us something about its target, and (ii) regarding the model-target relationship as one of similarity or resemblance. The alternative, non-language-oriented formulation of realism can now be given:

> Science aims to give us a set of models of the world; and evaluating the worth of a model involves abductive inference to the conclusion that it indeed resembles the target, such that it provides grounds for coping effectively with underdetermination in preserving confidence in that conclusion.[7] (NLR)

[7] Dr. Creath notes that NLR allows room for an antirealist like van Fraassen to pass as a realist. I respond that insofar as van Fraassen holds that models can tell us about parts of the observable world that are empirically remote to us, he would indeed count as a realist. van Fraassen, however, attempts to utilize underdetermination to limit our judgments of resemblance just to empirical substructure, whereas if we effectively cope with underdetermination, then such an antirealist move would be blocked.

As with the language-oriented formulation, this one commits us to the existence and independence theses and falls somewhere in between the confidence and insecurity theses. This formulation also gives a non-language-oriented equivalent of literal construal and belief. I note that evaluating the worth of a model entails taking the model seriously.

Now that we have two formulations of realism and the beginning of an understanding of the variety of issues within the realism/antirealism debate, I propose to provide a fuller sketch of the sort of position NLR would have to be to satisfy the terms of the formulation, I will also expound upon some related issues and defend NLR.

Abductive Inference, a Beginning

It is something of a controversy whether or not abduction is legitimate or whether there is *any* other type of inference beyond deduction and induction. Realists generally favor some version of abduction (Worrall would be an example of a realist who thinks abduction (as IBE) is illegitimate), and antirealists generally resist ratifying abduction, even if they agree that something like abduction has currency with practicing scientists.

Let's consider how abduction may be differentiated from deduction and induction. Think of deduction as inferring from the state of affairs in the domain described by the premises to that same state of affairs, or a part of it, described now by the conclusion, in which all that is entailed in the premises and conclusion is open to view. As I used to explain to my introductory logic students after illustrating by Venn-diagram just the premises of a valid syllogism (and dramatically capping the marker and placing it down on my desk), 'Without adding anything further, what I've just now done is *also* illustrate the truth of the conclusion.'

Induction (generally and crudely), however, infers *from* a revealed portion, described by the premises, of an otherwise opaque domain, *to* the opaque part. From the part of a set that is open to view, we make a guess about the part of that same set but that is closed from view. In some cases of induction, the portion that is open to view may be general aspects of the whole of the domain while the hidden part is an unknown particular piece of that original whole. In other cases, the open-to-view part is a known subset of the whole, and we infer to a hidden and unknown different subset of the same domain. Usually, people have in mind the case of inferring from the open-to-view and known subset to general aspects of the whole domain. It makes no sense, inductively, to talk about inferring from some known portion of a domain to an entirely different domain.

Because inductive inference is always *to* a portion closed from the reach of the premises, the inference contains a risk of failure. As Peirce pointed out in calling inductive inferences *ampliative*, this is a good thing because, despite the risk, it enables an enlargement of our corpus.

I contend that an important limitation on induction is that i*nduction cannot introduce new conceptual categories.* Like blind men puzzling out what is an elephant (assuming they had no prior concept), no iteration of inductive inference could ever put the disparate pieces together into a completely new, unified concept, despite induction's report on the correlation or suggestion of a common cause. Categories of both entities and relations and the character of these (stability, repetition, and so on) must be set in place as a precondition on induction. Inductivists usually hide this in the small print.

Before introducing abduction, we should first elaborate some on structures and models. The idea of *structure* is a very general one, and sociologists, crystallographers, engineers, business management people, mathematicians, and others make good use of the term, though each field has additional ideas pertaining to *structure*, specific to their respective discipline. Very generally, *structure* is used to denote a set of relations holding between a set of things.[8] It is the set of relations that constitutes the structure, so that the same structure can be duplicated among different sets of things. For the present consideration, one sort of structure of interest is the structure embodied in (parts of) the world. Homomorphisms may obtain between structures of many sorts. I'm not using *homomorphism* in its usual sense, tied to mappings of algebraic structures. I'm concerned to utilize homomorphism to capture the notion of resemblance, conceived generally as a mapping between two structures A and B, such that structural relations in A are consistently mapped to structural relations in B. Let's call *operational resemblance* a homomorphism that is rich enough so that an operation on the elements of the structure A

[8] Note that *structure*, as I use it here, denotes structures with specific, determinate relations obtaining between elements, to be contrasted with syntactic structures as of the sort advocated by structuralists.

consistently maps onto an operation on the elements of the structure B. An *M-model*[9] is a structure which bears an operational resemblance to another structure. Let's call the other structure the *target* of the M-model. So, an M-model is always a model *of* something. Note that two M-models may each bear a different operational resemblance to the same target. An M-model is not the same thing as a logical model which is more general. An M-model may serve to satisfy a set of statements, but its purpose with respect to abduction is relative to another structure, not relative to statements. An M-model is a distinct structure that may be embodied in a number of different things, some concrete and some not. M-model structures are independent of us and whether or not we think about them. I have in mind, for the purpose of clarifying my position in the realism/antirealism debate, that the target structure is embodied in a (part of) the world, and the M-model structure is embodied either in some sort of mental space or also in a part of the world.[10]

We can't always know whether a structure is an M-model or know to what degree the M-model operationally resembles the target. In order to ground such judgments, I propose to understand *abduction* as a type of inference *from* the structure taken to be an M-model and the available evidence *to* taking the target structure to be thus and so.[11] Abduction, on this view, is *not* an inference *to* a theory or *to* a representation. Abduction

[9] I chose this terminology, thinking of the 'M' standing for *mimetic*. In this paper I will frequently just use 'model' to mean 'M-model.'

[10] Many thanks to Dr.Armendt for some long discussions helping me get clarity on the ideas discussed in this paragraph.

[11] I phrase the conclusion portion of the inference in such a way as to make room for the interpretation of a practical inference.

is ampliative in that it infers to a conclusion not contained in the premises, where 'premises' is understood to be the information embodied in the M-model together with the evidence. A characteristic mark of abduction is that it can yield judgments about features of the world that are unfamiliar or remote from us, meaning that an M-model may predict in the target a feature for which very little inductive support exists. If the prediction bears out, then the model is regarded a *fruitful* one.[12]

So, in terms of domains, abduction might be thought of as an inference from one domain that is open-to-view to another, distinct domain possibly closed-from-view except in the intersection. I intend the common elements in the intersection to count as the current evidence, but the evidence that the open-domain receives here about the other domain is (i) regarded differently than it would be in the case of induction (i.e. not as grounds for a pattern to be extended), and (ii) is expressed entirely in terms of elements of the open-domain (so, the remainder of the closed-domain may be composed of different stuff). Whereas induction proceeds on the assumption of uniformity and stability within the domain such that, e.g., the entire domain will probably continue the pattern of the revealed portion, abduction advances on the notion that elements in the open-domain can operationally resemble elements in the closed-domain.

12 I emphasize that my usage and special treatment of the word and development of the concept are not intended to describe the common usage and understanding of the concept of a 'model,' even among scientists. I'm hatching and evolving a position that not everyone agrees with but which, I argue, will present a coherent view of models as representational structures, a view that attends to the epistemological position of the model-maker to the world.

In the case of induction, our confidence about the nature of the concealed portion is increased as the revealed portion is increased. With abduction, our confidence about the structure in the closed domain is increased (a) the more of the M-model that coincides with the evidence at the intersection under all available circumstances, and (b) the greater the number of common elements in the intersection of the domains (e.g., by increasing precision or by taking actions that increase the presence of such elements). Though, there are important qualifications to this confidence which I will present shortly.

In our having control over the M-model structure, we are afforded, I suggest, the capacity to introduce new conceptual categories, and this move is welcome in the pursuit of determining resemblance to the target. If induction is conceived as the search for patterns (which are extended into the concealed portion of the domain), then abduction may be conceived as a sort of world-building activity. However, the 'world' of the model is purposefully abridged and stereotypical. The goal of science is not to describe the biography of each and every boson and quark. On the other hand, it may be a (lofty) goal to capture each and every aspect of a target.[13]

Some care should be taken to distinguish between (a) properties the model possesses independently of context and application and (b) properties the model has by virtue of context and application. van Fraassen argues that there are no (important) properties a model/representation has independently of context (so that it fails even to be

13 I discuss *aspect* towards the end of this chapter, in the discussion of underdetermination. Roughly, I understand *aspect* to be a subset of elements in an individual model which bear a relation to something else in the wider, world-model.

a representation outside the appropriate context), but I will argue in the chapter on van Fraassen that his view is both unorthodox and mistaken. As well, I contend that a model is not an analogy, though there may be some overlap between the two ideas. While a model may be embodied in, e.g., balsa wood, whereas the target is not similarly embodied, the embodiment is not taken to be identical with the model. An analogy, on my understanding, includes essentially both the part that relates to the target and the part that does not, and the part that is unrelated to the target is important to the overall character of the analogy.[14] In the case of a model, if we included the embodiment, then it might greatly detract from the resemblance it has would otherwise have to the target. Perhaps, this trial and error method leads us to discriminate carefully the model from its embodiment.[15]

A Rough Example and Some Initial Objections

Let's take an uncomplicated example, where both the model and target are open to view. Suppose I'm a Wright brother. I'd like to know which wing design is best before climbing into an airplane and risking my life (or embarrassing myself by not getting off the ground). I build small-scale wing models and measure their lift and stability (I recall reading the brothers did this by mounting wings on articulated metal arms with weighted counterbalances and spinning these about a pole). Then, I would build medium-scale models of airplanes, test-flying those in real conditions. Finally, after much testing, I'd

[14] E.g., the 'bear' part of 'I'm angry as a bear'

[15] A point along these lines was made by BonJour (2007) with respect to Locke's indirect realism and the distinction between primary and secondary qualities.

put myself in a full-scale airplane. If my testing demonstrated intermediate abductive success (the wing-models showed themselves operationally resembling the medium-scale airplanes), then on the basis of iterated abduction[16], I have grounds to conclude my full-scale airplane should fly just as the model did. I would further abductively conclude that the rejected models of wing-design showing poor performance likewise correctly predict a full-scale airplane of similar poor performance. Very roughly, my verbal reasoning would be something like: Just as the model has features F1, F2, etc. which behaved by measures *a, b, c*, etc., so I infer that the target's features F1, F2, etc. will behave by measures *Ma, Mb, Mc,* etc. (where *M* is some coefficient that adjusts for the difference (if any) in scale). – To emphasize the difference between induction and abduction, one could make the case that, while the inductive inference is weak, there may be strong abductive inference (a) from facts about models to facts about airplanes which are really a different species of thing than the models, and (b) from the fact of successful prediction from one part of the model to the expectation that another, less-explored part of the model will therefore successfully predict behavior in the target[17]. The history of science is rich with accounts of scientists who formulate stories of how the world might really be, stories with great internal integrity and cohesion and built from familiar relations but applied in unfamiliar ways (as Einstein imagined 'riding on a beam of light'). I suggest this kind of thinking is abductive in character.

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16 (*akin* to transitivity, such that if *A* resembles *B* (by some aspect *z*) and if *B* resembles *C* (by aspect *z*), then *A* resembles *C* (by aspect *z*) – though, I am not asserting true transitivity)

17 (e.g., if I've found a sketch of Margaret Thatcher to successfully resemble her in respect of her hair, nose, eyes, etc., and then I discover the sketch portrays her having a small mole, abductively I now have good reason to expect a mole on the real Margaret Thatcher.)

Returning to the Wright brothers, suppose they start-up an airline company and hire a marketing executive to help them sell their new flying service to the world. This marketing person paints logos and splashy designs on various scale-models of airplanes in different dioramic settings and presents them to focus-marketing groups for feedback. This person is not interested in the aerodynamic aspects of the model, but rather in the artistic/marketing relevant aspects. It is no less a model, though, than the one used by the Wright brothers, and the abductive value is no less either. (Though, obviously, the marketing person is using induction to conclude the larger population will respond as the focus group did).

The key points here are (i) that the defining relationship between model and target is essentially one of (operational) resemblance, (ii) the model, target, and relationship between them are all fundamentally non-linguistic, (iii) the model's working in a certain way was necessary for me to draw conclusions from it, and (iv) insofar as the model evidences similarity to the target, we have reason to believe the target is similar (in ways not yet observed) to the model.

But, are these points correct? Objections can be raised. (a) "'Resemblance' is a vague and relative notion. What is resemblance in one context will fail to be in another. We would have to agree on some standard of resemblance and that requires convention and pragmatics." I have at least two responses to this: (a1) While I'd agree that there are such things as convention-based and context-relative instances of 'resemblance' (where you and I agree to say '*x* resembles *y*' for no other reason than we've agreed to speak this way), the model-theoretic idea of context-independent homomorphism is defensible. (a2) I will argue that abductive models are epistemologically fundamental, so states-of-affairs in the world, like contexts, cannot be a requirement on models if models are prior to knowing about contexts.

Another objection: (b) "Many models that we use to predict larger-scale and more complex phenomena nevertheless bear no resemblance to them. E.g., I could use a system of weights and gears to model economic phenomena, but there is certainly nothing about the weights, etc. that resembles the complex social phenomenon of an economy." I have two responses to this challenging critique: (b1) I completely agree with the critique, if the point is that no resemblance entails no abduction. In the example, we could think of the weights and gears as just an awkward calculating machine or a didactic illustration, but not a model in the abductive sense. (b2) Or, depending on how we draw inferences from it, one might make the case that, indeed, we are making abductive sorts of inferences, in which case the weight-gear system is standing proxy for a truly world-resembling M-model. Or, another possibility is that the weight-gear system is a second-order model, the first-order model being the one properly aiming at world-resemblance, of people exchanging money in an economy.[18] But, the essential feature of an abductive model is its resembling its target. By this criteria, abduction fails if the ‘model’ is a statistical chart or a set of equations or a collection of statements, and so on.[19]

[18] Higher-order models are discussed in Appendix I, Note 1.
[19] Though, it may be understood these things point to a true M-model.

Another objection: (c) "You are making unwarranted metaphysical assumptions in your use of *resemblance*. In fact, resemblance can only proceed by way of a (linguistic) conceptual framework which carves up both world and model in the same way. Outside this framework, no independent sense can be made of 'resemblance.'" This is a most powerful challenge. For the most part, I agree wholeheartedly. My example above and the normal way we think about resemblance does require a conceptual framework. But, there's the problem of a foundation, of starting somewhere, of ground-zero. We aren't born with a conceptual framework (and, even if we were, we could hold it suspect until foundational grounds are provided). The account I'm attempting to develop contends that the conceptual framework itself can only have emerged by way of fundamentally non-linguistic abduction (and that much of the scaffolding of that framework is itself an M-model), and this argues for (by indispensability) an inferential path to judgments of resemblance outside and prior to conceptual frameworks. This is not to deny anything of the powers and import of deduction or induction, except the fact they fail to provide the foundational inference needed to establish a basic world-theory. In the next subsection, I present the rough plan for this inferential path to judgments of resemblance.[20]

Inferring to Resemblance: Blackboxes, Elimination, and the Form of the Idea of the World

So far, no grounds have yet been given to infer that a constructed model bears resemblance to a target. If anything, the suggestion of underdetermination would appear

20 If the reader desires a more serious and detailed illustration of abduction that involves ground-zero epistemology, please see Appendix I, Note 1.

to undermine any attempt to draw such a conclusion. I also have ignored the obvious skeptical objections, such as the objection that it could well be the case that anything counted as evidence is not the result of some extant thing or process (i.e., there really is no target) or the objection that the pool of possible models may well really be just a pool of falsehoods (some of which are empirically successful). Skeptical objections are legitimate and, even, welcome, but they are indiscriminate between realists and antirealists. If the skeptics are correct, it would make science illusory. The dispute between realists and antirealists, however, begins with the assumption, however unwarranted, that science is not illusory, that the world exists, that there are grounds to hold that other minds exist, that languages exist, and so on. It is by a parliamentary decision between realists and antirealists, not by a philosophical demonstration, that we assume some minimal basis on which science becomes feasible despite skeptical possibilities. Thus, when I ignore the obvious skeptical objections it is by appeal to the parliamentary agreement, between all disputants of the realist debate, to ignore the global skeptic.

As to the grounds for concluding resemblance, it is important to emphasize that the process is one of elimination. Inferring that I am entitled a degree confidence that some present model resembles its target involves first showing the competitor models under review all failed to survive a test. The anti-global-skepticism premise also plays a role in capping the upper-limit of worse-case-scenarios for (i) how large the pool of possible competitors could be and (ii) how wrong we could be in the face of enduring empirical success. Finally, it needs to be argued that the remaining pool of possibilities is being reduced in such a way that weeding-out yields remaining possibilities along a narrower and narrower spectrum. I will touch on these shortly, in discussing underdetermination. First, though, it is instructive to look at an important piece of work in computer science from Edward Moore.

In 1956, Moore published a short but momentous paper, *Gedanken-Experiments on Sequential Machines*, wherein he proves some results relevant to abduction as conceived here. He considers an experimental situation in which we are guessing at the nature of a blackbox discrete-state machine which we can only test by feeding it inputs and examining outputs. Moore gives as an example of the situation he has in mind the case of capturing during wartime a cryptographic device which can't be opened for fear of self-destruction or boobytrap; Moore also notes that, while the analogy is imperfect between his gedanken-experiment and the situation of the scientist seeking to learn the nature of a system which he cannot view directly, his results may be 'of interest' (Moore 1956:133). The blackbox machine is assumed to have a finite number *n* of states, a finite number *m* of possible input symbols, and a finite number *p* of output symbols. This machine can only be in one state at a time, with transitions to each new state by discrete time intervals and with each state other than the initial state being rigidly determined by the previous state and previous input symbol. The goal of the experimenter is to distinguish among states of the machine by inputting a sequence of symbols and observing the outputs. A state q_i of machine S is indistinguishable from state q_j iff every experiment performed on S starting in state q_i produces the same outcome as it would staring in state q_j; and a pair of states is distinguishable iff they are not indistinguishable (i.e. if there exists some experiment the outcome of which depends on which of these states S was in at the beginning of the experiment) (1956:136).

Moore's first two theorems concede that a class of machines exists for which (a) any pair of states is distinguishable but there exists no simple experiment to determine the initial state, and (b) it will never be possible to perform experiments to distinguish it uniquely from the class of all discrete state machines. However, if the class of machines under consideration is restricted to just those of a certain size {*n*,*m*,*p*} of states, input symbols, and output symbols and restricted in certain other ways, then some interesting results are obtainable. Moore's Theorem 8, for instance, proves that an {*n*,*m*,*p*} machine any two of whose states are distinguishable will be such that there exists an experiment of length (n(n-1))/2 which determines the state of S at the end of the experiment. Another researcher, Karatsuba, proved, for a similar machine S, the length of the shortest and longest experiments to determine the end-state of S.[21]

It is beyond the scope of this paper to present a formal account of abduction; however, the vast literature on Moore machines (and its improved incarnations (such as the Mealy machine)) points the way, I believe, to developing a rigorous account. Among other things, an account along these lines would need to (a) treat models with greater granularity, with respect to objects and relations; and, (b) provide greater flexibility with respect to input and output sets (in terms of the number of inputs/outputs per state of the

[21] Dr.Creath correctly points out that appeal to Moore's work depends crucially on first establishing that state-machines are good analogies for targets in the world. I propose the idea only as a future research program, and arguing for the relevance of the program will be requisite.

model). The general idea is a good one. Treating the target (world) as a blackbox in the way Moore suggests, with simplifying assumptions in line with those necessary to permit science and ward-off the global skeptic. It tallies well with the scientific account of our perceptual/epistemic situation – *viz.* one of isolation with respect to the world. I highlight here that whether the blackbox is the state of the furniture in the room or the state of molecules in a chemical sample makes no material difference to one in a state of epistemic isolation.

Generally speaking, resemblance is established for some (unspecified) members in a pool of models by seeking out points of non-resemblance and eliminating from the pool classes of models sharing the non-resemblance. An invaluable tool in this process of elimination is the detector, the device appointed to provide us evidence.[22] At bottom, a detector is a measuring tool, though it is a matter of theory which objects we should decide to treat as detectors. The senses we are born with are, practically speaking, the set of detectors forced upon us, though, in time, we will come to treat these with impiety. As most antirealists stress, language is essential for the conduct of science, but before we can come to learn or use a language, we must first come to regard other people as having minds and as reliably signaling information to us, i.e. we regard other people as detectors. Much (or most) of our world-theory is a result of the output of people-*qua*-detectors. But, regarding people as detectors itself must rest on a theory that understands how people are with respect to us and to the world. It's debatable just where to draw the line on what is and is not a detector. Is the rippling surface of a pond a sort of detector (indicating

[22] Represented by elements in the intersection of the two domains in the earlier diagram.

something just thrown into it)? Does a series of glass lenses form a new kind of detector or are those lenses just a modification of an existing detector (*viz*., the eye)? As to the last question, it is a famously recalcitrant position of van Fraassen's to regard the output of microscopes not as a magnification of that which is not naked-eye observable (i.e. not as detector output), but merely as new empirical content (i.e. as just another artifact that is naked-eye observable) which must be made consistent with the theory. My position on microscopes is to agree with van Fraassen that they don’t extend observability, but then to disagree that there is such a thing as observability in the first place, in the sense that a direct-realist holds we can have first-hand knowledge of certain things in the world.[23] I hold the representational realist view and maintain that our sense-organs and certain other things we appoint in the world together serve as detectors. However, I would accept the convention to call 'observable' some collection of our most settled detections.

It is certainly a complex and advanced topic to argue, just how and on what basis we are justified to regard something a detector (or diminish or promote in rank one detector over another). But, it is easier to see that, once so regarded, the introduction of a new detector-type enriches the space on which we form our notions of model-parts and model-relations, such that (i) models gain in capacity (representational and otherwise) and (ii) there is an increase in the magnitude and nuance of evidence. It is for the historian of science to establish that the introduction of new measuring devices and detecting apparati precede important conceptual changes in science that supersede the

[23] Dr.Creath points out that we do have uninferred judgments of, e.g., what we’ve just seen. I deny that these judgments may be taken as true reports of states-of-affairs in the world. Moreover, merely being an inferred judgment is insufficient to establish it as an *observation*, for I have any number of uninferred judgments (e.g., ‘Caesar wore a toga’) which I’d hesitate to call *observations*.

theoretical capacities of the theory that gave rise to the devices (and which may eventually change the story how exactly the devices work, but not *that* they are still detecting devices), but a superficial review seems to corroborate generally such an opinion. The point I wish to emphasize is that an increase in the number and type of detectors brings with it an increase in the ways by which models may be eliminated.

One advocating a deductive view of the structure of scientific theories might, for instance, hold that a proper theory should provide a set of axioms each making a statement true of every member of the domain, such that, given some set of actual initial conditions and any actual event *E* true of the domain, *E* is deducible from the axioms given the initial conditions. On a reductionist view, axioms should concern the most elemental properties/relations/entities of the universe, such that all possible events in every field of science will be deducible given initial conditions on just the elemental items. Such views naturally place the greatest importance on the axioms and fashions a hierarchical arrangement of dependence whereby any error in the axioms spells doom for the entire system. It is no wonder that philosophers of science subscribing to such deductive views of theories have been apt to place special, if not singular, emphasis on physics, as opposed to the rest of science, for such views grant special status to the field of science whose job amounts to describing the ultimate axiom set. Yet, the curious tension with this perspective and the actual situation in science is that physics tends to be among the more speculative and less settled areas of science, whereas other fields, like biology, have enjoyed greater stability and a considerable degree of insensitivity to the upheavals in physics. Even more settled and less concerned with physics would be those items of belief which we fully take for granted, along the lines of 'here is a hand,' discussed by Wittgenstein in *On Certainty*. From an individual, developmental point of view, these pedestrian pieces of our world-theory must be firmly in place long before we can begin to think about science proper. – The model-based view I espouse does not share the singular, hierarchical structure of the deductive view and better captures, I submit, the inter-relations among levels, or domains treated-of by science (and common-sense).

I hold that the ordering that starts with facts of the sort 'here is a hand,' having greatest familiarity and reliability, and the ordering that starts with the most elementary and fundamental sorts of facts[24], are *both* necessary to a complete account of the structure of theories. More precisely, I suggest there are three axes along which science develops. The first axis runs from generality to specificity - i.e. from higher-order models (whose targets are lower-order models) to lower-order models (where the target of the lowest-order model is the world). The second axis runs from elementary or constituent bits to larger unions or composites of those bits. The third axis runs from more entrenched parts of the model to less entrenched. As I discuss in Appendix I, regarding orders of models, every level of order is important in its own way, with higher-order models providing regulative and heuristic assistance to lower-order models and with lower-order models providing stability in entrenchment. As well, the generalities afforded by higher-order models are also important in strategically coping with underdetermination, in the way such generalities point to features whose rejection would eliminate the greater number of models from the pool of possible contenders.

[24] i.e., those facts that are constituent to other facts, but which have no facts constituent to themselves.

Along the elementary/composite axis, it *is* important, as the deductive view of theories encourages, to understand how changes in elementary bits propagate through the structure. However, a model is not an axiomatic system, so there may be (if we so allow it) a modularity to the various chunks or regions of the model such that some chunk may turn out wrong and it need not devastate the remainder of the model. If the goal is resemblance, then a model can still satisfy that goal at some levels of composition while yet failing at some elementary level (as a painting portrait will capture the likeness of the subject in the parallel between relations of the splotches supposed to be eyes/ears/mouth and the relations among the subject's actual eyes/ears/mouth, but not in the parallel between the individual splotches of paint and those corresponding bits of flesh on the subject). This same point is also relevant to the third axis of entrenchment by which we are advised which chunks of the model should serve to 'anchor' or test those less settled chunks. In evaluating models along this axis, we consider the degree of confidence we have in some model-part. So, e.g., in some cases, we might evaluate the elementary part against the anchor of the more entrenched composite. In fact, I suspect that 'Eureka!' breakthroughs in science often arise when very successful and well-entrenched model-parts are applied in clever, new ways (e.g., as Democritus and Leucippus applied the *divisibility* relation). Finally, there is obviously a connection between entrenchment and the intuitions behind parsimony. I will discuss in the Kuhn section how my proposal here would yield consequences different than those advanced by Kuhn for the case of theory failure, for theoretical content carrying-over across theories, and for theory-ladenness of observation. In short, Kuhn's account follows from a deductive view of theories but would not necessarily follow from my model-based view of theories.

Inferring to Resemblance: Taming Underdetermination

Now that we have some sense about theory-generation, how do we cope with the threat of contrastive underdetermination? Parliamentary agreement (among other reasons to be discussed) reduces underdetermination possibilities. But, for any given theory/model, there would still remain an unsettlingly large number of empirically equivalent alternatives. Realists, in the past, have made appeals to various forms of the 'no miracles' (NM) argument in the face of underdetermination. The NM argument, generally, reasons that the spectacular empirical success of mature theories is too great to be a lucky coincidence, and since we deny both miracles and the presumption of sequential lucky coincidences, then our mature theories must be at least approximately true.[25] The entreaty to approximate truth is made in order to reconcile the fact of the predecessor theories all having turned out false with the fact of their being empirically successful, though antirealists have rightly challenged realists to produce the details behind 'approximate truth' which could account for this reconciliation. And, for some time, the debate had been revolving over just which account of approximate truth could effect this reconciliation, with nothing convincing so far forthcoming. However, especially where theories take mathematical form, it is easier for antirealists to argue that empirical success is just a matter of 'curve fitting' the mathematics to the empirical data,

[25] Dr.Creath points out that the argument as presented is a *non sequitur*. The argument given in terms of IBE is less of a *non sequitur*, but then depends on the guarantee of IBE.

that no metaphysical conclusions may be drawn from a theory's merely 'saving the phenomena.'[26] Realists (such as Worrall (1989:114)) have responded that mature theories don't just fit existing empirical data, but typically (Worrall argues *must by definition of 'maturity'*) predict novel phenomena, and such predictions typically provide confirmational vindication, and so have theoretical *fruitfulness*.[27] No inductive or Darwinian sort of account can derive the reason for this, for as Dawid (2009:3) points out, to claim (as van Fraassen and Kuhn both do) that fruitfulness is explained by fruitful theories being selected-for on that basis is just to beg the question.

Among some realists (not Worrall) who prefer an explanationist version of the NM argument, the explanatory quality of a theory is an essential factor accounting for its success in making novel and correct predictions. The claim is attacked by van Fraassen, as will be discussed later. Notwithstanding, it would seem that fruitfulness gives realists (explanationist or not) good grounds for optimism against underdetermination. – However, Hoyningen-Huene (2011) has devised an exceedingly clear rendering of the transient underdetermination argument, which makes use of the most charitable representation of *approximate truth* and, at the same time, sets-up a counterargument to the novel-prediction form of the NM argument. It is most helpful to trace carefully Hoyningen-Huene's argument, in order to appreciate fully the force of the underdetermination argument, both to realism and antirealism alike.

[26] I note here that calling mathematical descriptions of phenomena 'mathematical models' adds to the confusion of special terminologies; I've already acknowledged my use of 'model' (M-model) is a reconstruction and won't jibe with common usage (where scientists consider 'mathematical models' to be models), but I'm rejecting here that 'mathematical models' are first-order M-models

[27] I note that the realists' demand to explain the persistent re-occurrence of fruitful theories is not an argument *for* realism, but (in my view) an argument *against* the antirealist alternatives to realism and a challenge for which any account of science must properly account.

Hoyningen-Huene begins with the assumption that appropriate notions of truth and of approximate truth of theories have been defined and that true and approximately true theories exist.

> Let D_1 be a finite set of data.
> Let T_1 be the set of theories such that $T_1 := \{T, T$ is relevant for and consistent with $D_1\}$;
> We assume that $T_1 \neq \varnothing$.

Hoyningen-Huene next partitions T_1 into two subsets: those theories which are true or approximately true, and those which are radically false (not even approximately true).

> $T_1^{AT} := \{T \in T_1$, T is true or approximately true$\}$
> $T_1^{RF} := \{T \in T_1$, T is radically false$\}$
> with $T_1 = T_1^{AT} \cup T_1^{RF}$

The final preliminary detail to Hoyningen-Huene's transient underdetermination argument is the introduction of a measure μ which represents the size of a subset, the measure yielding a magnitude more general than (but similar to) that of Euclidean volume, in order to provide the basis for a judgment of probability supplied by differing relative sizes of the sets of T_1^{AT} and T_1^{RF} theories. It is assumed that there are far more false theories than there are approximately true theories, and realists generally do not dispute this assumption. – Thus, the argument for transient underdetermination may now be presented:

Argument 1 (Transient underdetermination)

$T_1 = T_1^{AT} \cup T_1^{RF}$ and $T_1^{AT} \cap T_1^{RF} = \emptyset$

$\mu(T_1^{AT}) << \mu(T_1^{RF})$.

Therefore for any $T \in T_1$, it is very probable that $T \in T_1^{RF}$.

That is, for any arbitrary theory T, chances are overwhelming that it is radically false and not even approximately true.

The NM argument can be formulated in the same terms that were used to present the transient underdetermination argument:

Argument 2 (NM Argument)

$T_1 = T_1^{AT} \cup T_1^{RF}$ and $T_1^{AT} \cap T_1^{RF} = \emptyset$

$\exists\ T* \in T_1$ such that $T*$ makes the novel prediction N

For any $T \in T_1^{RF}$, it is very improbable (or even impossible) to make prediction N.

Therefore, it is very probable (or even certain) that $T* \in T_1^{AT}$. I.e., contrary to the transient underdetermination argument just made, $\mu(T_1^{AT}) << \mu(T_1^{RF})$. That is, according to the NM argument, keeping fixed the same assumptions regarding the volumes of approximate true and radically false theories in T_1, the fact that some arbitrarily chosen theory T* bears the property of making novel predictions is grounds for regarding that theory as likely approximately true.

Transient underdetermination, however, can easily deal with this version of the NM Argument. For any theory T* which produces novel data *N*, we now define a new data-set D_2 and theory-set T_2, as follows:

Let $D_2 = D_1 \cup N$ is a finite set of data.
Let T_2 be the set of theories such that $T_2 := \{T, T$ is relevant for and consistent with $D_2\}$.

And, transient underdetermination is born anew, utilizing D_2 and partitioning T_2 just as we did D_1 and T_1 before:

$T_2^{AT} := \{T \in T_2$, T is true or approximately true$\}$
$T_2^{RF} := \{T \in T_2$, T is radically false$\}$
with $T_2 = T_2^{AT} \cup T_2^{RF}$.

The radically false theories contained in T_2^{RF} are (just as with T_1^{RF} with respect to D_1) relevant for and consistent with the data D_2. As well, per the undisputed assumption, the relative sizes of the sets T_2^{AT} and T_2^{RF} yields:

$$\mu(T_2^{AT}) << \mu(T_2^{RF})$$

Thus, most of the theories that manage to be relevant for and consistent with the old data D_1 *and* the novel data *N* are *not* even approximately true.

While Hoyningen-Huene's argument seems explicitly directed against realists (who are the ones defending the NM argument), in fact, it is indiscriminate between realists and antirealists. While antirealists, like van Fraassen, embrace underdetermination as lethal to realism, the truly fatal assumption is that antirealists are not equally susceptible to it. Only where language (and all the necessary and sufficient conditions for language) and direct perception of the world and its regularities can be taken for granted as 'givens' and shielded from underdetermination worries can the antirealist even begin to sport sanguinity at the prospect that underdetermination is his friend.[28] However, underdetermination just as easily threatens these 'givens,' as Quine and others have pointed out, and I certainly am not going to give the antirealists a free pass. Moreover, even if Hoyningen-Huene's argument removes confidence in the approximate truth of T* theories making novel predictions, it remains a challenge to antirealists to account for the regular occurrence of such exceedingly empirically successful T* theories. For, it will still be the case (where $\cancel{T}_2$ denotes the set of all theories that are *not* relevant for and consistent with D_2):

$$\mu(T_2) << \mu(T_1) << \mu(\cancel{T}_2)$$

That is, the probability of choosing an arbitrary T* from among the set of *all* possible theories is exceedingly tiny. So, the onus *still* remains on the antirealist to account for the sequential occurrence of fruitful theories making novel predictions (which, I repeat,

[28] As I will elaborate in this paper, on my analysis, the general antirealist tact is to reduce the methodology of science to a function of language and direct-observation, but Hoyningen-Huene’s argument doesn’t stop short of language and direct-perception. Especially if the view is adopted of epistemic isolation from the world, then the antirealist will then have to make very realist sorts of arguments to establish confidence in judgments of language and perception.

cannot accounted-for by 'curve fitting'). This challenge of accounting for the improbable selection of fruitful theories is made even stiffer when the situation is finding ourselves at epistemic ground-zero.[29]

Does Hoyningen-Huene's argument show all hope lost for the realist view of the aim and structure of science? Of course not. The move to an infinite collection of possible theories is itself made possible by a simple mathematical induction argument wherein theories are already presumed inexhaustible so that the only task remaining is just to find the right way to correspond them to the natural numbers. And, insofar as theories (commonly conceived) are linguistic or mathematical artifacts with limitless vocabularies and boundless combinations of terms and expressions, it certainly would follow there are an infinite collection of theories. The argument being pursued in this section, however, is that models, not linguistic/mathematical artifacts, are the fundamental and essential (but not solitary) means for inferring to the shape and character of the world. Are there an infinite number of possible model-parts, model-relations, combinations of these, and therefore, an infinite number of models? In the following arguments that I give in answer to this question, I assume, *per* the ‘parliamentary agreement’ mentioned above, that knowledge of the world is achievable and, to some degree, has been achieved.

The answer is not so obvious to this question of whether we should worry about an infinite number of possible models, especially if we insist that possibility be

[29] As I noted in footnote 27, this is not an argument *for* realism, but an argument *against* the antirealist alternative and a challenge for any account to have to meet.

predicated on practical constructibility. I assert that an unfeasibly constructible model is not an M-model. The skeptic will harp the possibility that the would-be correct model of some given target may be in-principle unconstructible, but that would imply the possibility that the world is in-principle not knowable (assuming models are the sole means to worldly-knowledge and the possibility exists that every correct model is unconstructible). I suggest that an in-principle unconstructible model is like an in-principle unfeasibly demonstrable proof; if you cannot in-principle demonstrate it, then you cannot call it a 'proof,' and if you cannot in-principle construct it, then it is not a model.[30] In order to permit science (or any knowledge of the world) to go forward, we rule-out by parliamentary agreement the threat of this possibility suggested by the global skeptic and accept that correct models are in-principle feasible to construct. I note, though, that this skeptical challenge is separate and unrelated to the underdetermination challenge which does not dispute that the correct model is practically available to us, but just that other, incorrect models, indistinguishable from the correct one, are also available.

So, we now ask: Are there an infinite number of practically constructible models? If infinitely large models are not practically constructible, then to arrive at infinitely many models, there would have to be an infinite number of different *kinds* of model-pieces out of which the infinite collection could be constructed. Are there infinitely many different kinds of model-pieces? The kinds would have to be qualitatively differentiable

[30] By 'in-principle' I don't mean 'in-principle by humans,' but 'in-principle' under any condition. Later, I address realist modesty and human limitations. On the other hand, *contra* skepticism, it would warrant holding that it can't be the case for *everything* we humans believe about the world, we are wrong.

from one another in an infinite number of ways, but I see this could only happen either (a) by qualitative degrees (as the color spectrum along a real number line), or (b) by an infinite number of different kinds of detectors[31]. As for (a), it is controversial that qualitative degrees can, in fact, be infinite, since it implies an actual infinity. But, assuming such a thing to be the case, then it would have to *make a difference* with respect to a model's relation to the world. In particular, it would have to spell the difference between resemblance and non-resemblance by that infinitesimal degree. If resemblance is established as the sort of property which will not fail on account of one real-numbered degree, but rather, which can be equally satisfied by a comfortably generous interval, then (a) is not a problem. Infinitesimal degrees could determine a difference in mathematical models, but I set these aside for now, except to note (i) mathematical models aren't going to be foundational, and (ii) they are not going to be first-order models (bearing direct resemblance to the world).

As for (b), we again can wonder whether an actual infinity of kinds of detectors is meaningful here. Assuming such a thing, one might think underdetermination could get a toe-hold. But, it would first have to be established that the availability of a boundless number of kinds of detectors makes for a boundless number of incompatible models.[32]

When are two (or more) distinct models to be regarded as belonging of the same class of models? It is generally uncontroversial to treat all the possible states of a model as just being a set of static models (with a common, stable set of objects and relations) all

[31] As I discuss in Appendix I, Note 1, the ‘raw material’ for models is derived from detectors.
32 For additional discussion please see Appendix I, Note 2.

belonging to the same class, such that we would say of any of them, 'That's the same model but in a different state.' Relatedly, we may sometimes say of two distinct things, ‘These are just two different aspects of the same thing’ or of two distinct models, ‘These each show different aspects of the same target.’ A child may need some convincing that ice, liquid water, and steam are all the same thing, that a caterpillar and butterfly are the same animal, and so forth, but once convinced, this child will grant these disparate forms as having identity. Similarly, a town may be regarded from economic, historical, aesthetic, and political points of view (each with a distinct model), yet never be confused as more than one and the same town. For terminological convenience, let's call all models in the same identity class *aspects* (though we should distinguish state-aspects, developmental-aspects, trait-aspects, etc.). These models, though distinctly different from one another, are not necessarily incompatible or competitive.

How, then, do we decide which models are aspects and which are competitors, which are friends and which are foes? The answer, in rough form, has to be that only by virtue of an all-encompassing world-model can we determine which models are competitors *versus* aspects. Aspectual models are always about a thing *x* with respect to a (possibly empty) set of other things *a, b, c* (with the remainder of the universe *ceteris paribus*). For instance, modeling the economic aspect of a town involves representing that town with respect to the representations of production, distribution, and consumption of goods and services among people, whereas modeling the historic aspect of a town involves representing that town with respect to representations of past persons and events (and prior states of the town). Two competing economic models for town *x* would be such that each model contains the same town *x* and the same respective items that define that aspect. None of these distinctions are possible, however, unless we have first in-hand a world-model complete enough to make sense of which things are which. Thus, underdetermination for anything other than the world-model must be with respect to the world-model.[33]

As insolent and out-of-line as may be for me to say so, I believe van Fraassen and Carnap have a sense of the very same point being made here, but they err in assigning this role to language or conventions, which as I shall argue subsequently, must make appeal to the very same world-model to begin to get off the ground. van Fraassen, in particular, in his recent work, makes the argument that aspectual seeing is primary to resemblance. Since only a select subset of elements constituent of a thing (a photograph, say) is being employed in the resemblance relation, while the remainder is being ignored, and since the subset changes from context to context, it can only be context which enables and actualizes the resemblance. But, this is only robbing from Peter to pay Paul, since the context is itself just a subset of elements in the world, which on pain of regress, cannot appeal to yet another context. Moreover, van Fraassen never explains how we can know the subset elements in the first place in order to utilize them by way of context.[34]

For all its importance, a world-model is still just a model. But, except for that unified view, we couldn't make proper sense of individual models and concepts. This

[33] Dr.Creath notes that not all competing theories get at the same aspects. Perhaps so, but I am keeping this discussion mostly simple, reserving a more detailed discussion for a larger work.

34 These points will be discussed in the discussions on van Fraassen and pragmatics.

notion comports with the conceptual holism found in Hanson and others and is certainly not my original idea, except, perhaps, in the way it ties to model-abduction. Concerning underdetermination, the issue would now center on world-model *versus* world-model, since it follows that non-identical world-models will all be competitors with one another. The uppermost question we have been considering is whether there are an infinite number of such possible competitors. I already argued that models must be in-principle practically constructible (for, if not, then global skepticism wins). I also assume, for similar reasons, that the possibility space for models, provided by the particular detector-set, is only practically divisible in a finite number of ways.

Any realist would be foolish to suggest that we are nearing the final and true set of theories of everything. My view of realism only requires of the realist that he be able to diminish the pool of possibilities by n+1 steps at a time. If the possibility persists of infinitely many possible models, of a kind that bypasses the neutralizations I've so far suggested, then even a modest progress of n+1 would be meaningless. Or would it?

One kind of progress that may be immune even to the possibility of infinitely many incompatible models is that of diminishing the pool not by volume but by squeezing the bandwidth along certain parameters. Then, it could still spell victory for the realist. By analogy, suppose a police sketch-artist has spoken to witnesses and has limned a certain general idea of the suspect's face: the nose looks more or less like *x*, the mouth is more or less like *y*, and so on. As more details are filled-in, we should never (in Zenoic fashion) arrive at the final finished sketch, but at least, we should succeed in defining an interval such that there would still remain an infinite number of possibilities, though all those possibilities fall within a range we tolerate. This would perhaps involve ruling-out extremely odd possibilities by fiat on grounds that, otherwise, global skepticism wins. As mentioned above, the parameters along which squeezing is done are chosen by our highest-order models along the axes of generality and entrenchment (so, e.g., whichever world-model is the correct one, it will bear representations of hands, chairs, divisible things, the sun, frogs, and so on). This possible strategy for cornering the truth needs further attention, but at least, we can conclude that, to constitute a problem, underdetermination must disallow being squeezed by parameters and possess an infinite number of non-resembling yet equally workable models.

A final candidate means to subduing underdetermination has already been broached. If the goal for realism is shrinking the pool of possible models, and if each subset in the class only has finitely[35] many possible models, then shrinking each one of the finite subsets would bring the overall pool, though still infinite, one step closer to the determinate model. Of course, that's only a fanciful conceit for a worst-case-scenario. The grim reality under any scenario is that we sentient beings can only ever acquire a finite number of detectors, and diminishing the pool of possible models available to us, still leaves untouched all the models for all the detectors we shall never have. The silver lining, it could be argued, is that (as I've presented the case) resemblance comes in degrees and can be achieved along different dimensions (e.g., by granularity, by parts

[35] I assume here that subsets are determined by a collection of detectors unique to that subset. So, assuming an infinite number of detectors for the whole class, each detector with only a finite number of possible outputs, this yields (I argue) a finite number of models for that subset.

matching, and so on) and can be achieved at least for some region or dimension of the world. The parliamentary agreement provides security in the conclusion that *some* measure of success is possible, given our means, in acquiring worldly knowledge. If, by such means, we can shrink the pool by some small measure, for some region or dimension, and progress in some way towards an improved resemblance, then a modest realism has triumphed.

CHAPTER TWO: HANSON AND KUHN

Norwood Hanson's landmark 1958 book, *Patterns of Discovery*, made an enormous impact on the realism/antirealism debate, though ultimately not in the way he likely intended. Hanson's account is singular in having been presented as a blistering realist attack on logical empiricism, yet having had the misfortune of its core arguments being hijacked by antirealists, most notably Thomas Kuhn who published just a few years later in 1962 *The Structure of Scientific Revolutions*. I am persuaded that Hanson's style of philosophy, clearly influenced by Wittgenstein, obscured his realist arguments to the degree that even a fellow, sympathetic realist can only guess at the details. Nevertheless, it is instructive to study Hanson's and Kuhn's accounts side-by-side in order that the contrast should sharpen what perhaps Hanson intended. I stress, though, that I am not attempting scholarship in Kuhnian or Hansonian studies, but rather, I am making an attempt to locate a strong Hansonian-style argument in defense of realism and comparing it with a strong Kuhnian-style argument. To that end, because Kuhn's accounts are the more well-known and because Hanson's are more nuanced, I propose to present these in reverse historical order, in order to show better just how Kuhn fails to measure up to Hanson by illuminating the differences within the exegesis on Hanson. In the end, I hope to show where both accounts fall short while yet recovering their strongest points for service in my own view.

Kuhn's Account

Kuhn's strategic approach to Philosophy of Science was to reposition the big questions from the framework of philosophy to the framework of history. Moreover, Kuhn makes a persuasive presentation of working science, and fashions his philosophical position from that context. Even his severest critics will concede at least that much philosophy of science pre-Kuhn had too often been ignorant or at least naïve about every day, in-the-trenches science as it is actually carried-out, as well as the effects of historical forces and conceptual shifts on the development of science.[36] All his merits notwithstanding, Kuhn, in my view, nevertheless depends too much on several weak assumptions, and in so doing, gives a flawed philosophy.

Kuhn's arguments in *Structure* are aimed to counter a variety of philosophical views, but most notably those of Carnap and Popper. With respect to Carnap, Kuhn more recently wrote: "...if I understand Carnap's position correctly, the cognitive importance of language change was for him merely pragmatic. One language might permit statements that could not be translated into another, but anything properly classified as scientific knowledge could be both stated and scrutinized in either language, using the same method and gaining the same result . . . Language change is cognitively significant for me as it was not for Carnap" (Kuhn 1993:313)(via Grunberg *et al* 1995). However, Kuhn's understanding of Carnap is clearly in error, and the two, in fact, have much in common. Carnap's Principle of Tolerance permits a choice of framework, with a categorical

[36] Dr.Creath correctly notes that the logical empiricists were not among the group of naïve philosophers.

difference between judgments made internally and those made externally to the framework. The latter are often nonsensical where they exceed pragmatic concerns, and the former are strictly governed by the rules of the framework's system. As a result, theoretical postulates within the framework cannot be challenged, but rather, if one does not like the performance of the framework, then it must be wholly rejected in favor of an entirely new one. Because theoretical terms get their meaning (partial-interpretation) from TC – theoretical postulates and C-postulates (correspondence rules) – it follows that a change in TC would produce a change in the meaning of the theoretical terms. C-postulates also impart influence on observation terms, and the framework determines which sorts of things are to count as observations in the first place (recall the protocol sentence debate). So, a kind of theory-ladenness of observation is at work in Carnap's framework scheme. These points add up to an incommensurability among sufficiently different frameworks, with no (guaranteed) means of translating from one framework to the next. It should then come as no surprise that Carnap himself would use the language of 'revolution':

> First of all, I should make a distinction between two kinds of readjustment in the case of a conflict with experience, namely, between a change in the language, and a mere change in or addition of, a truth-value ascribed to an indeterminate statement (i.e, a statement whose truth-value is not fixed by the rules of language, say by the postulates of logic, mathematics, and physics). A change of the first kind constitutes a radical alteration, sometimes a revolution, and it occurs only at certain historically decisive points in the development of science. On the other hand, changes of the second kind occur every minute. A change in the first kind constitutes, strictly speaking, a transition from a language Ln to a new language Ln + 1 (Carnap 1963b:921)(via Grunberg *et al* 1995)

Boyd (1983) points out that Kuhn relies on Carnap's law-cluster account for the meaning of scientific terms, and Kuhn's social conventionalism plays a role in theory-selection comparable to the role Carnap's linguistic conventionalism plays in framework-selection.

Of course, the view that Kuhn and Carnap were birds of a feather is not without exception, and notable dissent from philosophers like Earman (1993) appears in the literature. Earman notes that degree of confirmation in theory-choice plays no role for Kuhn. Earman also argues that, unlike Kuhn's picture of complete commitment among adherents in theoretical acceptance during the normal science period, Carnap views theory-choice being more circumspect, with theory-abandonment an easy and available choice. – Another obvious difference is that, whereas logical empiricism treated the history, sociology, and psychology of science as essentially irrelevant to questions of scientific justification, Kuhn argued they are inextricably wound together.

Remarks on Kuhn's Account

The most controversial aspects of Kuhn’s picture of science hinge on just one central thesis, to which I take objection, *viz.* the thesis of a monolithic, globally-holistic, meaning-determinative linguistic/theoretical construct that acts as a manifold through which one’s world-view is configured and filtered. I understand this to be a mostly Wittgensteinian inspired thesis that views social/conventional forces as behind the creation and securing of the construct. Thus, if a person is brought up and inculcated (by social forces) in a particular system of language, the consequence is a world-view and ordering of concepts and experiences which are strictly non-translatable between a (sufficiently) different world-view and concept-ordering. Or, to put it in a more simplified, Wittgensteinian way, a person using a language according to one set of grammars could not participate in any language-game with another person using a language according to a different set of grammars disjoint from the first person's set, though superficially both persons' words may look the same. The failure to recognize this difference in grammars causes philosophical confusion. Kuhn asserts that scientific training brings about a socio-linguistic construction of a world-view that marshals a specification and ordering of concepts, ways of observing, and standards and means of justification. The theses of incommensurability and non-cumulativity ensue from this. However, the basic assumption, that the constructs are categorical and independent, each resting separately on its own piece of bedrock, is undefended by Kuhn. I see no support for Kuhn's view in the works of Wittgenstein (as I read him). I suggest there are also independent reasons to deny Kuhn's thesis.

Let's first consider Hanson's and Wittgenstein's views on language and the basis for language.

In contrast to Kuhn's view, Hanson (as I read him) presents a stratified and modular view of conceptual-systems, with those systems peculiar to science residing at the top of many layers of more fundamental systems. This allows for an easy multilingualism among agents who then may explore and evaluate scientific conceptual-systems 'from the inside' (so to speak) as well as evaluatively 'from the outside' (so to speak) from the vantage point of other, usually more fundamental sub-systems. This stratified and modular conceptual-system view challenges the basic constructivist tenet that one scientific paradigm determines one world-view completely and exclusively, such that one is forbidden the option to 'try on the goggles' (so to speak) of various sub-systems without departing from the global system, as well as opt to adopt no scientific view at all. Presumably, the bottom 'strata' among the layers of sub-systems form the basis for the language. Hanson does not discuss this much, but to the extent I have described it correctly, I contend Hanson's view more correctly captures the later Wittgenstein's view.[37]

What is Wittgenstein's view of language? I certainly concede that Wittgenstein is notoriously difficult to interpret. Some passages from his writings will be construed differently by different people, for instance:

> Think of chemical investigations. Lavoisier makes experiments with substances in his laboratory and now he concludes that this and that takes place when there is burning. He does not say that it might happen otherwise another time. He has got hold of a definite world-picture - not of course one that he invented: he learned it as a child. I say world-picture and not hypothesis, because it is the matter-of-course foundation for his research and as such also goes unmentioned. (1969:#167)

Except for the second to last sentence, about the world-picture being learned as a child, this passage could have been read in a very Kuhnian manner. However, the parenthetical remark makes all the difference. Wittgenstein is here saying that Lavoisier had his world-picture (world-view) put together long before he ever took his first science class. By

[37] This is not to say I agree with Hanson in all respects.

Wittgenstein’s lights, Lavoisier’s groundbreaking scientific achievement is a mere ‘hypothesis,’ compared to his world-picture. Consider another passage:

> I learned an enormous amount and accepted it on human authority, and then I found some things confirmed or disconfirmed by my own experience. […] In general I take as true what is found in text-books, of geography for example. Why? I say: All these facts have been confirmed a hundred times over. But how do I know that? What is my evidence for it? I have a world-picture. Is it true or false? Above all it is the substratum of all my enquiring and asserting. The propositions describing it are not all equally subject to testing. (1969:#161-2)

Again, with selective inattention, one could find a Kuhnian sort of remark here. Yet, the passage is not saying that textbooks form our world-picture, but rather that the world-picture I already have in place, before I read a single textbook, is such that I will take as true what I find there. The suggestion is that a world-view is primary to and separable from scientific training.

In overview, I understand Wittgenstein as proposing (a) an individual language-game is governed by an internal grammar (which is not a set of explicit rules, but which is established on the fixedness of the usage of certain linguistic elements relative to other elements). (b) Language-games comprise modules in the language and form clusters with respect to one another, making for ‘family relationships.’ Certain regions of each cluster will share common, overlapping grammatical elements. (c) Some set of grammatical elements, also overlapping, nevertheless constitutes a logical axis for the entire language. The axis is rooted in a ‘form of life,’ which I take to be at least biological, but perhaps, also including some deep social/historical factors. As he writes in *On Certainty,* for instance: “I would like to regard this certainty, not as something akin to hastiness or superficiality, but as a form of life. That means I want to conceive it as something that lies beyond being justified or unjustified; as it were, as something animal” (1969:#358-359). This ‘root’ is determinative of certain basic cognitive and behavioral commonalities among humans which we cannot and do not deviate from, so these become fixed in language-use as well. Hence, as Wittgenstein famously said (1953:223), even if a lion could be made to talk, we would not understand him. I agree to an extent with those who read ‘form of life’ to include the *deepest* social/historical factors, such that, e.g., a feral child might fail to master a language for this reason, even though she is human. I would disagree that world-view is substantially affected by more recent or local differences among social groups. Because of the ‘atomism’ of Wittgenstein’s view. because of the rootedness in biological and deep socio-historical commonalities, and because holism is only local and not global, we may conclude that Wittgenstein’s view of language tolerates a fair degree of divergence between any two systems without this resulting in an incongruity, let alone an incommensurability. Differences in theories are only neighborhood differences, not system-wide.

In my reading of Wittgenstein, the seeing/seeing-as distinction, relevant to observation, is not *directly* pertinent to a world-view, but rather comes under the local governance of language-games. Suppose two people, A and B, each have a set of language-games with which they are familiar and the intersection of the two sets is rather large, but smaller than either original set. A is an architect with no knowledge of real estate, whereas B is a real estate agent with no knowledge of architecture. They each pass the same house and *see* it *as* something different[38], something *we* can say of them because *we* can talk about the two ways of seeing (assuming you, the reader, and I are each familiar with both real estate and architecture). This doesn't mean that the architect and real estate agent 'inhabit different worlds,' but only that they differ in the particular language-game being played. Each is taking different aspects of the house to be salient according to grammatical directives of the particular language-game that make relevant one feature but not another. That this is the case does not preclude one from being both a real estate agent and an architect and adopting both language-games simultaneously. A difference in world-views is vastly deeper than just a difference in sets of language-games. Undoubtedly, two individuals with different world-views see the world differently, but a difference in seeing does not necessitate a difference in world-views.

I cannot defend, in the space of this paper, my interpretation of Hanson's view of language or of Wittgenstein's account of language, world-view, or 'form of life,' nor will anything be settled regarding Kuhn's account merely by presenting Wittgenstein's view, even if there is no quarrel over my interpretation. Assuming I am right in my interpretations, then Kuhn's view is clearly at odds. Notwithstanding, I would like to place this view, regardless of authorship, as the alternative to Kuhn's view. I will add my own elaboration on this alternative view before returning to Kuhn.

Among the reasons to translate from one language into another, two are of special interest. One reason is to render an equivalent meaning of some concept in the one

[38] Which means, according to Wittgenstein's account, they also each have other ways of seeing it, too.

language into the concept(s) of other language. Another reason is to be able to say, from either language, that the other is speaking generally about such and such. Let’s call this second way the ‘rough translation.’ For instance, 'tuna' in English and 'maguro' in Japanese have dissimilar respective webs of associations and relations to other terms in each one's own language. Some concepts associated with 'maguro' in Japanese have no proper English counterpart. In this way, there is no strict translation into English of 'maguro' which carries the identical associations and relations. Even if one knows English and Japanese, one cannot know how to give a strict translation, for such is not possible. Could the bilingual person, however, give the rough translation, saying in English: "The Japanese word 'maguro' generally means 'tuna,' but Japanese people associate other ideas with 'maguro' that we do not in English"? I suggest an affirmative answer to this question. At the end of this section, I will discuss the Use-Meaning thesis and the Thought Theorist counterpoint which would deliver a deeper justification for my stance, but for now, let’s just consider that languages must be learned. On the Davidsonian model, for instance, verbal behaviors are executed under particular circumstances, and the language learner must ascertain how those behaviors and circumstances relate to the speaker’s intentions, where this understanding amounts to something like a charitable ascription of a T-sentence. Kuhn adopts a conventionalist view of language, so he should not find the Davidsonian idea terribly objectionable. This learner’s understanding certainly cannot require a language if he is acquiring his first language. My point, then, is that language learning requires an extra-linguistic understanding of verbal behaviors, speaker intentions, and circumstances, otherwise no language would ever be learned. I suggest that this understanding at least would enable something of a neutral view, though it is another question whether or how this understanding could be expressed. I will return to this line of thought momentarily. Now, let's return to Kuhn's view.

Kuhn's view of language, in contrast to the alternative view, subscribes to a global-holism, a non-modular architecture, and a very different conception of world-view that renders it more readily affected by proximate social-historical differences. Insofar as language constitutes a construction that determines (or even just profoundly influences) observation and standards of justification, then incommensurability indeed follows where two languages are sufficiently different. World-view for Kuhn, then, is not in terms of an anchoring foundation but in terms of theory-determined observation. Galileo sees a different pendulum than Aristotle. Because the seeing is entirely determined by a globally-holistic conceptual-system and that system manages everything we experience, it makes for a *world*-view. On Kuhn's account, the world-view is symptomatic of the language, not underwriting it. If anything anchors a language, it is convention.

On the issue of translation, Kuhn regarded *any* manner of translation impossible. Whereas Quine argued, in the indeterminacy of translation thesis, that translation is not only possible, but supernumerary, such that we can't decide which translation is the correct one, Kuhn argued we can't even succeed in the first attempt.[39] Kuhn (1982) criticized Quine's indeterminacy of translation thesis as being too tepid in arguing for mere uncertainty. Kuhn distinguished sharply reference from translation as well as distinguished acquiring a language from translating a language, arguing that, if the

[39] Thanks to Dr.Creath for clarifying this.

globally-holistic conceptual-systems are distinctly different between group A and group B, then even where a member of B also acquired the language of A and so knew the references of terms in A's language, translation between A and B is still impossible. This conclusion describes Kuhn's position of semantic incommensurability. The meaning-holism thesis holds that, e.g., because the term 'mass' in Newton's theory versus Einstein's theory is *used* in different ways and because each conceptual system is holistically interconnected, it follows that the different systems are *sui generis*, even if every other term besides 'mass' were to be used identically. If this is the case, there can be no theory-neutral system between any two theoretical systems, for that third system will just constitute another, new *sui generis* system.

Let's list some problems with Kuhn's view. (a) First, it is at odds with the alternative view, which I elaborated above from my reading of Hanson and Wittgenstein. Kuhn has not defended, but only assumes, the global-holism view. Unless he gives reason to think so, we are not compelled to accept his further conclusions based on the assumption. Is there a reason to prefer the alternative view over the global-holism one? I suggest some in the subsequent items. (b) It was noted above that learning a first language, on the Use-Meaning view that Kuhn apparently holds, requires understanding, in advance of knowing the language, which behaviors are verbal behaviors, types of circumstances and contexts relevant to those behaviors, and even speaker intentions. If we must understand these in order to learn a first language, and if all languages are similar insofar as they use terms with respect to circumstance and intention, then this same understanding would provide the language-neutral basis to compare languages. If this understanding is in terms of M-models, then it would preclude the Kuhnian objection that the understanding can't be expressed in some other language. However, without such a meta-linguistic basis, no language can be learned. (c) This also suggests that observing the world cannot be fully determined by the language, otherwise neither a first nor a second language could be learned. For the second language, how could we comprehend some other way of using a term in a context, if the first language already determines that we see the use as that first language dictates? In other words, insofar as the two languages coincide, the second language will be understood as matching the first. Insofar as the first and second language diverge, the first language will dictate a malapropism. So, this would appear to preclude learning a second language, implying there could be no theory-shifting. (d) On the other hand, if observation is fully determined by the language, then, as Boyd (1983) and others have pointed out, it follows that anomalies should never occur. (e) It is also implied from (c) that there could be no such thing as theoretical innovation. Any violation of the grammar or logic of one's language is intolerable and incomprehensible. Modification is undifferentiable from a violation. (f) What are the necessary and sufficient conditions for semantic incommensurability? If even the smallest divergence between any two systems makes (like a 'butterfly effect') for holistic difference and if holistic difference makes for incommensurability, then, unless any two language users is executing language use identically, everyone will be speaking a different language incommensurable to the next. If no one is speaking the same language, then there can be no conventions to constitute a language in the first place (on the Use-Meaning thesis). How could one tell if one is speaking the same language as the next person if no meta-linguistic view is possible? (g) Finally, semantic incommensurability, if taken seriously, does not account for exclusivity or competition between two theories. If two distinct theories are so estranged from one another that one could not regard both simultaneously or compare them (as a rough translation would allow us to say: “T1 is giving a different account of *mass* than T2”), then how can they be in competition? Sewing and cooking applesauce are wholly different and, so, not in competition. As I suggested in Chapter One, for two theories (models) to be in competition, they must be understood (meta-theoretically) to be talking about the same thing but proposing different accounts for that thing. Yet, such a meta-theoretic understanding is precisely what is ruled out under Kuhn’s view. The idea of the ‘rough translation’ discussed above is what I have in mind for determining the points of contention between two languages. If the ‘rough translation’ or some other basis for establishing competition cannot be fixed, then why should one theory exclude the other? Why have a scientific revolution?

Under the alternative view, second-language acquisition is explicable, since any two languages can still have a common basis (in ‘form of life’). Because the alternative view has identity conditions that tolerate divergence (of some modules, not the whole system), it therefore makes innovation is possible, and the innovation does not entail a failure of communication. As well, anomalies and theoretical competition are easily explicable, for one can evaluate a module’s performance, or difference from another module, from the vantage point of other, more fundamental modules. In just this way, a ‘rough translation’ is possible, either from the M-models or from the common subsets of modules between any two languages. At the same time, strict translations will still be impossible, as well they should be.

In this short section, I have given nothing approaching a rebuttal to Kuhn's view. I hope I have at least shed some doubt that Kuhn's incommensurability thesis rests on solid grounds. The vulnerability in his view, I have suggested, is in misapprehending the correct source of language and the nature of language. While the alternative view does not necessarily commit one to realism, at least there's a path from the alternative view to realism, whereas from the Kuhnian view, there is none. To begin to establish realism from the alternative view, the emphasis should also shift to explaining first-language acquisition (in terms of M-models), as well as to an account of progress towards truth, not by way of cumulativity, but by way of shrinking the pool of possible contender theories. As a final note, I observe that realism is not incompatible with the constructivist view that agents are encapsulated from the world by a construction which denies direct access. In fact, the orthodox view in cognitive science is both that our experience and conception of the world is entirely a construction (in terms of mental representations) and that this construction is (mostly) correct. The only difference between an antirealist constructivist and a realist constructivist is that the latter holds that we have good reasons to think the construction is correct.

Hanson's Realism

Hanson's arguments in *Patterns of Discovery* (1958) are like the fibers in a braid and, stylistically if not in deeper ways, obviously influenced by Wittgenstein's style of philosophy. Unfortunately, this style lends itself to a wide range of interpretations, and it is most unfortunate that antirealists, such as Kuhn and van Fraassen, managed such an egregious (in my opinion) misinterpretation of Hanson's realist philosophy of science. I do not claim to have the uniquely correct interpretation that Hanson intended for his own work, but I offer my interpretation and analysis of Hanson's account in *Patterns of Discovery* that aims to make a strong case for his sort of realist position.

I propose first to explore three major points in Hanson's book, then to try putting them together into a coherent whole, and finally to evaluate the result.

The Theory-Ladenness Thesis

It is in his first chapter, on observation, where Hanson first uses the phrase 'theory-ladenness.' It occurs in the context a motivating question by which he commences the book: 'Do Kepler and Tycho see the same thing in the east at dawn?' (5). The question is asked to impress upon us the distinction between a purely-optical, sense-data inspired notion of 'seeing' *versus* a conceptually-rich, systemic, patterned notion of 'seeing.' Hanson points out that little can be gleaned from the strictly optical kind of seeing, and that, but for a language and a conceptual system with which to pattern that seeing, our optical experience would be little more than a 'buzzing confusion' a 'kaleidoscopy' and of little relevance. Sense-data proponents would respond that we *interpret* the purely optical elements of our seeing and that this is how we arrive at the more conceptually rich experience. However, Hanson points out two flaws in this picture: (i) we are aware of no procedure or activity of interpreting when we, for example, see that this thing is a table; there's no 'squeezing into a box' process that occurs. (ii) Secondly, this picture implies that we can directly refer to the sense-data, but this also is not something we can really do.

Hanson concludes that 'observation is theory-laden.' Hanson does not here explicitly define what is to count as a 'theory,' but from his examples (which include both scientific and non-scientific kinds of observation), it is safe to interpret him as meaning something like the conceptual-subsystem modules of language that I described above for the alternative view to Kuhn. Says Hanson, our observation of X is shaped by our knowledge of X. The conceptually-rich notion of seeing, Hanson calls 'seeing that,' and attempts to show by examples that 'seeing that' bridges the gap between pictures (purely optical) and knowledge (fundamentally linguistic). Pictures cannot assert truth or falsity, cannot characterize, and cannot include non-optical (e.g., auditory) aspects in its representation. A picture's capacity to represent owes to its possessing features in common with the subject represented. All representing elements of a picture work in the same way.[40] But, language is different. Hanson disagrees with those who, like (the early) Wittgenstein, held that language represents in the way pictures do. Concepts in language do not all work the same way, do not represent by similarity of form, and because of this (and not despite this), language has the power to talk about the world, to merge or purge

[40] Hanson's use of 'representation' should be understood in the narrow sense of an aspectual reproduction, not in a broader sense to include things like maps which related to the target in more complex ways.

different sensory concepts, to make category-mistake errors, to assert truth and falsity, and so on. In short, the utility and versatility of language is in part due to its being free of representational capacities. Language and the picture-like optical experience are two very different kinds of thing, but 'seeing that' (observation) closes the gap and ‘threads’ knowledge into seeing. Our observing or 'seeing that' something is an X entails all of X's attendant possibilities and relations. I understand Hanson to be asserting that ‘seeing that’ involves the participation of sensory elements into the language, just the way a word (thought of as a set of phonemes) gains meaning when it joins a pattern of linguistic usage (on the Use-Meaning notion of meaning).

It is tempting to read Hanson as promoting the more forceful view that observation is being dictated by a monolithic non-modular conceptual system, but this interpretation does not jibe with the rest of the book. This erroneous interpretation is fueled by Kantian instincts that import into Hanson's theory-ladenness thesis the claim that the ‘objects of the senses’ must conform to our cognition. However, as Hanson argues in his second chapter (and elsewhere), there are many conceptual systems, and we may willfully move from one to the next, though force of habit or convenience may discourage migration. Hanson asks us to consider some alternate ways of expressing mundane facts, ways that will not be familiar to us. Instead of the adjectival idioms "The sun is round" or "The sun is yellow," Hanson has us try on for size the verbal idiom forms: "The sun yellows" (as in radiates color) and "The sun rounds" (as in incessantly pulls itself together into a sphere). The shift in expression, says Hanson, carries with it a shift in the ‘logic,’ which shapes our sense of the fact, changing it from one about a passive property to one about an active property. The exercise of putting down one conceptual-subsystem and picking up another to 'try on' indicates that Hanson is promoting conceptual multilingualism that does not entail a full departure from the global system and that does not assert a single monopolistic conceptual system that precludes ‘external’ (to the subsystem) comparisons and contrasts, while yet remaining inside the total system. Notice that, in Hanson's example, (i) the mediation of observation by conceptual systems begins at a very basic level, at the point of ordinary language and folk-theories, and (ii) Hanson does not claim the impossibility of there being a purely optical, sensory registration on our minds, apart from the conceptual system's patterning. About point (ii), I mean to emphasize that Hanson is not claiming the conceptual-system is prior to and the gate-keeper of *any* sort of purely sensory experience, but rather, he leaves open the possibility that the conceptual-system must work in concert with the purely sensory experience. By this, I mean that the purely optical has a status independent of the conceptual system and so can act as a kind of check on that system, insofar as the system may predict something of the purely optical and the purely optical may genuinely falsify the prediction. I understand Kuhn’s view to preclude the possibility that the purely optical could falsify a theory.

Kuhn would certainly not agree with the availability of a multiplicity of conceptual-subsystems (being that he is a global-holist), that we can and do practice a conceptual multilingualism (among the sub-systems), or that the control a scientific theory exerts over us is marginal and usurpable by the mundane-level conceptual systems we must developmentally master first. If these are the claims Hanson is making in the text, then Hanson is, so far, no ally of Kuhn.

Patterns, Facts, and Theory-Breaking

Hanson poses an important question: "What is an in-principle inexpressible fact?" This question leads to one of several case-studies (two historical and one imagined) of scientists who each push a scientific theory to its breaking point for the reason, one would say in retrospect, that the theory was incapable of expressing a fact of interest. The antirealist might see in these examples a different lesson: *All* theories possess inbuilt limitations and incapacities which (i) indicate that no theory could tell the whole truth, and (ii) determine an inherent eventual obsolescence of any theory. However, Hanson's case-studies tell a far more optimistic story, one that underscores progress, rather than the opposite.

The case study of Galileo is intended to elucidate (i) the difficulty of superseding a conceptual system that both enables and hinders explanation, and (ii) the degree to which a conceptual system spells-out and coheres with the facts. Galileo, says Hanson, sought not merely a descriptive and predictive formula for the data, but much more, *viz.* an explanation of the data, intelligently systematized, reasoning back to more fundamental principles. He did not seek a cause; that was Descartes' program. Galileo first tried to build a rigorous physics on the Aristotelian notion of impetus, but failed. Then, he tentatively substituted for 'internal motive power' the idea of 'repeated external shocks,' which was his “march out of the wildernesses of contradiction” that had beset his predecessors who were always seeking after “a constant cause… to produce a variable effect” (41). “By allowing for an increase in acceleration while a body was under the effect of a constant cause, the impetus theorists were admitting creation *ex nihilo*” (41). In Padua, Galileo developed the notion of 'moment' (the product of weight and velocity) which was a turning point in his thought. Motion could be regarded as brute, and no longer a “perplexing explicandum” (42).

By this and adopting a geometrical representation of motion, Galileo was able to ignore impetus and its causal/time orientation in favor of a spatial orientation. It is ironic that the new logic would free him of one thing but trap him from another.

In a geometric representation, time had no prominence. In Galileo's earlier geometric proofs, velocity is the sum of instantaneous velocities acquired at each point along a trajectory, representable by triangles as a linear function (43). But, velocity is also the sum of instantaneous velocities acquired at each moment in time, which cannot be plotted this way and which only allows for a uniform increase in time (43). There was no 'logical space' for a time parameter (43). Eventually, after enormous intellectual struggle, in 1604, Galileo made the necessary modifications, but representing time geometrically was unwieldy. As Hanson puts it, “thinking thoughts in a conceptual system not designed to express them [required of Galileo] unprecedented physical insights” (46). Says Hanson: “The task of the few has been to find means of saying what is for others unsayable" (46). Despite not having calculus, Galileo still succeeded in capturing the unifying idea of constant acceleration.

The case-study exhibits facts unwelcome to a Kuhnian account of theory-change. Galileo's story is *not* one of a communally intolerable anomaly that impels the community to dispose of the old theory in favor of a new one. Galileo's story is that of a lone genius, struggling with and against the conceptual system and logic available to him, locating its limitations, and taking rational steps to repair. It's a story of conceptual and logical *progress*, finding a means to rationally attune the available logic with other theory-independent elements: the data, 'physical ideas,' and so on. To put this another way, Kuhn's scientists work to find a pragmatic limitation in the theory and then move to trash it. Hanson's scientist works to understand *why* there exists a limitation in the system and then moves to *repair* it.

Theories are explanatory systems, meaning that theories provide very general systems of patterning from which facts and observations follow 'as a matter of course.' Facts and observations have a good degree of independence from the scientific theory, which is precisely the reason Galileo found frustration in the failed versions of his theories. As noted above, observation is largely influenced by mundane-level conceptual systems, though, owing to the co-existence of many conceptual systems, influence may be exerted from many directions at once (including the scientific theory). The monolithic conceptual system described in Kuhn's account has been long criticized as preempting and not guaranteeing the discovery of anomalies. Hanson's account, as I understand it, provides a more plausible picture for the appearance of anomalies where theory-ladenness is in effect.

Nevertheless, the antirealist can challenge Hanson's picture of progress. Just because a pattern is found by which the facts follow 'as a matter of course,' it does not follow that we have convergence to the truth. Indeed, Hanson’s forceful rejection of the preservationist ‘correspondence principle’ (as advocated by Weyl) can be read as an embrace of the history of science viewed as a series of solid rejections of old theories and acceptances of entirely new theories logically incompatible with the old ones. If truth were being approximated, then one would think Hanson would have adopted the preservationist stance on theory change, but he spends the final section of his book making a full-throated repudiation of such a view. Yet, as I shall discuss next, Hanson is equally insistent that theories that best explain the facts provide good grounds for regarding those theories true. This is quite a puzzle in light of the rejection as false of explanatorily successful theories in history.

Hansonian Theories and Hansonian Abduction

To unravel the puzzle requires first paying close attention to Hanson's idea of the structure of scientific theories. At bottom, he rejects the deductive view in favor of a collaborative view of theories embedded in a wider system of modules. The deductive view stems from a misapprehension (by philosophers) of the nature of causal relations. Philosophers, Hanson says, focused on causal chains as representative of causality in general, but that misrepresents the more integrated and decentralized understanding scientists actually have of causal relations. Scientists rarely use the word 'cause' and even less 'causal chain' in the lab or in journals. Instead, they treat causality “less like the links of a chain and more like the legs of a table” (52). Causal chain examples are loaded with implicit assumptions and theoretical presuppositions, without which they would be unintelligible. For instance, in the example of one billiard ball bumping to the next which bumps into the third, contiguity, propinquity and asymmetry are insufficient to a complete causal account. Much more is needed. The geometrical properties of balls, the material properties of the balls and their surfaces, the dynamics of elastic bodies, and so on, must all be included into an organic understanding and must be in place before we form the expectations we have of billiard ball behavior. Hanson says: "We have... an explanation of X only when we can set it into an interlocking pattern of concepts about other things Y and Z” (54). So, causality for X only becomes intelligible after we see X’s place in relation to other things, i.e. its conceptual role in the greater system. This implies that “a *completely* novel explanation is a logical impossibility” (54), which is not to rule-out conceptual innovation but only to recognize limits on innovation. For Kuhn, *any* novelty in explanation (that exceed the strict bounds of the theory) is a logical impossibility.

Hanson charges that the hypothetico-deductive (HD) and enumerative-induction (EI) accounts of theorizing are entirely unfit to account for what scientists do. The EI view is simply false; scientists do not derive theories from instances. And, continues Hanson, HD does not tell us how theories are arrived at, but is only relevant to completely finished theories. Hanson insists that explanation is the actual goal for all theories and that this can't be done by building-up from isolated facts. Explicans cannot be derived from explicanda by summarizing over particulars, as he puts it, the way an actuary squeezes functional relationships from columns of data. For instance, that spectra are produced when sunlight strikes a beveled mirror is not explained by pointing out that all such mirrors in such circumstances do this. Explanations are not produced by searching for deductive systems out of which the data appear as consequences.

Hanson's positive account of explanation (abduction) is forthright, but delivered with only implicit supporting argument. According to Hanson, the heart of explanation can be found in the Peircean account of retroduction (abduction), which has us begin with data, search for hypotheses, study the facts, and devise theories to pattern the data. The only justification for retroductive inference, says Hanson, is an appeal to indispensability: If we are ever to understand anything at all, it must be in this way. Peirce described retroduction as “perception of the world of ideas” (86). Hanson reads this to mean that abductive inference and perceptual judgment are “opposite sides of the same epistemological coin” (86). In other words, just as perceptual elements are utilized in conceptual-systems on the simpler level of observation, they also play a role in forming abductive judgments. However, as I understand Hanson, unlike the case of observation where perceptual elements play a more passive role with respect to the conceptual system which adopts them, I understand ‘perceptual judgments’ at the level of abduction to be somewhat authoritative. I think this is at least part of what Hanson indicates in recounting the role of ‘physical insights’ in the Galileo case study. The general form of abductive inference is as follows: (1) Surprising, puzzling phenomena P is presented. (2) If hypothesis H is true, then P follows as a matter of course. (3) Thus, we have grounds for holding H true. Hanson emphasizes that we cannot retroductively infer H unless its content is already present in the premise (2). Perceiving the pattern in the phenomena is essential for its being explicable it 'as a matter of course.' The hypothesis' providing intelligibility to the data constitutes a 'conceptual gestalt' that now patterns the phenomena. Hypotheses are *not* pieced together from the phenomena, but rather what make it possible for us to observe the phenomena as being of a certain sort and as related to other phenomena.[41]

Hanson asks: "What is it to supply a theory? It is to offer an intelligible, systematic, conceptual pattern for the observed data. The value of this pattern lies in its capacity to unite phenomena which, without the theory, are either surprising, anomalous, or wholly unnoticed" (121) Why should we accept a theory? "You should accept it because if you do a comprehensive and systematic explanation of these diverse and apparently incompatible microphysical phenomena will follow as a matter of course. What could be a better reason?" (109). That question crystalizes his basic indispensability argument for realism. At the point where our explanations no longer lead to unsound inferences, intelligibility demands that we regard the entities/relations invoked by our theories as no doubt real. Hanson says these entities, like elementary particles, are not just logical fictions or mathematically divined hypotheses spirited from nowhere to serve

[41] Dr.Creath points out this is a 'fully Kantian point.' I think, though, Hanson would deviate from Kant in Hanson's conceiving us as having much more individual control over the conceptual system that fashions perceptual judgments.

as summary descriptions for large-scale observations or as deductive scaffolding. He says (somewhat cryptically) that we must learn that such knowledge derives from means more complex than philosophical accounts would suggest. I take him to be intimating that the relationships between a person, the conceptual systems available to him, other people and forces in the world involved in shaping that system, and the actual targets of investigation altogether make for an extremely complex collection which ordinary epistemological accounts don't even begin to address. Nevertheless, he is vigorously affirming that knowledge is achievable.

Assuming for the moment that retroduction is a legitimate category of inference, then there is nothing wrong with holding to its ampliative conclusions, even if such conclusions turn out wrong. Just as, in the case of induction, it is right to hold to an expectation of approximately 50% of flips of a fair coin showing heads over the long run, even if it should turn out 99% of flips for that fair coin happen to show tails. In this one regard, I would say Kuhn and Hanson might be on the same page, since Kuhn regards it the proper attitude for a scientist in the 'normal science' mode to treat the claims of science as beyond reproach, even though the history of science should prompt pessimism.

I think it is important to emphasize, in judging Hanson's account, that the composition and configuration of a conceptual-system (whether a language-game, or a folk-theory, or a scientific hypothesis, or a scientific theory) is largely outside of our individual control. One lesson from the case-studies may be that only the rarest of humans in the rarest of times and with the greatest efforts could exert some small individual influence over a conceptual system. Hanson does not say so directly, but one might be led by the text to think that the relative independence of conceptual-subsystems and their roles in retroductive inference suggest some possibilities in the direction of truth-regulation: (a) the world exerting an impelling external force on conceptual-system formation (as Boyd (1983) suggests in appeal to the Putnam/Kripke causal theory of reference); (b) an economy of minds (and sensory organs and interactions) collectively cobbling together conceptual-systems and giving them more/less currency; (c) some combination of the economy of minds and the world itself imparting degrees of entrenchment to a conceptual-system. In general, these possibilities suggest that conceptual systems are not our personal creations, are not within our control, are not obedient to our wills, but instead, conceptual systems are fashioned by complex interactions between social forces and worldly forces in such a way that, as Boyd puts it, for some type term *t* and some real entity *e*, "what is said of *t*, generally speaking and over time, is reliably regulated by the real features of *e*" (1983:209). Thus, despite our basic epistemic isolation from the world, conceptual systems are available intermediaries causally regulated to bring us closer to truth. As attractive of an idea this is, it amounts to little more than a hypothesis and the skeptic can demand non-circular justification for it. So, none of these possibilities would necessitate that a successful conceptual-system is more likely true, but in these sorts of ideas of truth-regulation is the beginning of a weak argument. At the very least, the emphasis on conceptual-system independence bolsters the indispensability argument, which is really the only one Hanson directly gives.

In the last two chapters (1958), Hanson makes the clearest statements of the rational demand that we accede to the reality of the entities described by our most successful theories. He dwells at length on the breathtaking precision of our current conceptual-systems in science and the fantastic intricacy of complex usages of their elements which all must hold together in the most intimate way. He asserts that this symphony of exceedingly precisely calibrated coordination must be just so. Otherwise, even the smallest alterations would result in dramatic shifts or chaos. If some group of scientists made even a miniscule change to the rankings of usages, it would ultimately lead to a dramatic divergence in problem selection, research programs, and ‘frontier’ science (119).

It is safe, I think, to conclude that Hanson's central argument for realism boils down to indispensability and inevitability. He attempts to make the case that there just is no alternative to accepting as true our most successful theories. We cannot, as antirealists counsel, hold our theories at arm's length and pretend to adopt agnostic or skeptical attitudes towards these, for scientific theories are not severable from the rest of our conceptual systems; they are not in our control to change or ignore. By what other conceptual-system could the antirealist think his thoughts? There are no alternatives; therefore, the antirealist is either confused or pernicious. – This points to an inconsistency in Kuhn's account, which argues along similar lines for indispensability of the conceptual-system to the working scientist, but yet somehow makes an exception for the historian of science who can occupy an point of view outside the bubble in order to draw antirealist conclusions. If anything, Hanson is consistent whereas Kuhn is not.

Concluding Thoughts on Hanson

I would nevertheless agree with antirealists who find Hanson's stated arguments for realism ultimately weak, while granting that sub-theses along the way (e.g., the theory-ladenness of observation) are powerful and persuasive. If we are at the point of self-consciousness of indispensability and can make sense of the thought that we are compelled to pronounce a theory 'true' and/or treat as 'real' the entities mentioned in those theories, then it hollows to emptiness the substance of the pronouncement of 'truth' or treatment as 'real.' It would make such gestures as meaningless as a gunpoint 'confession.' Hanson has not, in the end, provided us with adequate justification for his assertion that we must regard successful theories as true. He has not provided enough detail in his account to, e.g., determine the degree of theoretical success sufficient to compel belief.[42] I disagree with some of Hanson's points, such as that linguistic-systems are the necessary and only means by which adequately to pattern experience, that certain areas of science are in-principle unpicturable (if this is taken to mean: in-principle not able to be modeled), that predecessor theories in science are *never* embeddable and *always* logically discontinuous with successor theories. More importantly, I do not accept his retroduction form of abduction, principally because I do not see what requires or motivates us to accept its conclusions.

[42] Thanks to Dr.Creath for making this point.

However, I have taken great inspiration from Hanson's work, and his other central theses I embrace. Pure sensory data would be useless to us without an entire system by which to make sense of that data, as I also tried to argue in Chapter One. I agree with Hanson that we cannot separate our most rarefied scientific theories from our most mundane ideas about the world, for (as I argued in Chapter One) the mundane ideas anchor the theoretical ones, as well as provide the practical pathways for testing and confirmation. I agree that scientific theories must be explanatory (at least in the model-based sense I have argued), and we at least should consider seriously theories based on their ability to pattern the data (which is how I understand M-models to perform with respect to the data). – I also would agree that the realist's best hope of achieving dominance over antirealist lies with *some* kind of indispensability argument, but the one Hanson chose is not foundational enough to refute the antirealist.

The Use-Meaning Thesis, the Thought Theorist View, and Realism

I find it interesting that both Hanson and Kuhn, whom I view as intellectual rivals, each make appeal to the Use-Meaning thesis, no doubt each being under the sway of Wittgenstein and the Oxonian school of ordinary language philosophy. While Hanson found a way to bend this view into the service of realism, the Use-Meaning thesis generally is only favored by antirealists. I agree that the Use-Meaning thesis is pernicious to realists, where it is construed as positing that language fundamentally constitutes meaning and explanatorily precedes thought. On my view, the realist must establish that thought has priority to language, in order to preclude the antirealist maneuvers that deflate realist claims about the world (re-casting them instead as some manner of *flatus vocis*). So, with an aim to addressing Kuhn's antirealism and to anticipating van Fraassen's antirealism, I think it important to undertake a review and critique of the Use-Meaning thesis at this point in the paper.

The Use-Meaning thesis is a view that held great sway by the middle of the twentieth century. Beginning with Grice's carefully distinguishing semantic meaning from pragmatic, the Use-Meaning thesis and Ordinary Language philosophy in general began its descent. The last great defender of the Use-Meaning view was Michael Dummett who argued (the Priority Thesis) that language is explanatorily primary to thought, that language use 'encompasses the contents expressed by the utterances' (Heck 2007), that T-sentence semantic theories are best understood as descriptions of a language user's *practical ability*, not as a basis for language use. Dummett writes:

> [T]he philosophical task of explaining in what a mastery of a language consists is not completed when we have set out the theory of meaning for the language. . . . [W]e have to go on to give an account of what it is to have such knowledge. This account can only be given in terms of the practical ability which the speaker displays in using sentences of the language. (Dummett 1993:101).

Dummett points out that, contrary to the Though Theorist's view that non-linguistic thought supplies semantic content into language-use, if this were so, then why can't we refer to these thoughts directly *sans* language? Why do we structure our thoughts in language? How do we establish that our non-linguistic thoughts are correct?

Contra Dummett’s arguments, John McDowell (1998) dockets two horns of a dilemma facing the Use-Meaning theorist as he attempts to clarify the thesis: (i) If the use of a sentence is taken to be what that sentence is used to *say*, then its meaning is just the thought it is used to express (i.e., the use is 'meaning-laden' by virtue of the thought). This clearly contradicts the Priority Thesis. (ii) If use is understood in Quinean terms (noises people make in certain circumstances), then the Use-Meaning thesis commits one to a 'behavioristic reduction of meaning' (*ibid*.), which few will wish to commit to. Richard Heck, Jr. (2007) gives a careful partial-defense of the Use-Meaning thesis, propounds a hybrid notion, with content dividing into two sets, the Gricean one being (presumably more elementary) thoughts bearing content, while the other Dummettian set gains content from use, in particular being use-determined constructions out of the simpler Gricean components (along the lines Davidson suggests). Heck also distinguishes 'content' from 'meaning' in a significant way: *content* belongs to a theory of truth (is associated with components of the T-sentence), whereas *meaning* is a meta-semantic concept (belonging to a theory of meaning) and treats of content in the determination, by rational judgment, of correctness of a T-sentence. In making this distinction, the Use-Meaning theorist can have his cake and eat it, too, by permitting content of thought to instill content into basic expressions, but denying that this amounts to meaning which belongs to the realm of reason in requiring the capacity to judge the thought-contents and expression as suitably related with respect to use/convention, such that use indwells in meaning. Since thought-content has been integrated, the one, Quinean horn of McDowell's dilemma is avoided, and since *meaning* is made out to be a kind of second-order relation, separate on that plane from content, Heck believes he has avoided the meaning-ladenness horn as well.

I disagree that Heck has saved the day for the Use-Meaning thesis. Let’s sum-up how Heck has differentiated the Use-Meaning view from the Thought Theorist view. I understand his proposal as follows: Let Y be either thought-content and/or usage-content. Let "*x*" be an expression in language; then, <"*x*", Y> is a pairing (of the sort, say, representable by a T-sentence). Let Z1 stand for some pattern of conventional language usage in a context for expression "*x*". Then, *meaning* will be the triple <<"*x*",Y>,Z1> where "*x*" is used in accord with Z1.[43] Perhaps surprisingly, the Thought Theorist of a Gricean bent would propose almost exactly the same formula for the meaning of the expression "*x*", since the priority-of-thought-friendly Gricean program full-well recognizes that utterance-expressions are conventionally selected encodings for thoughts. So, the full-description of an expression's semantic import must include, first, the thought-content and, then, the proper coordination with convention/usage. The singular difference is that, for Heck, Y will sometimes have pure usage-content, with no admixture of thought-content. So, I challenge this singular difference.

What is the epistemological dissimilarity between knowledge of states-of-affairs in the world and knowledge of conventions? How aren't conventions also just states-of-affairs in the world? Why would knowing one set of states-of-affairs in the world, in order to ascertain correctness of a pairing of *“x”* and Y, be different in kind from

[43] Of course, I'm running roughshod here, but my purpose is a fast-and-dirty bottom line on Heck's proposal

knowing another set of states-of-affairs in the world in ascertaining the correctness of a pairing of “snow is white” with that very state-of-affairs? I suggest that linguistic conventions are not different from other artifacts in the world. The epistemological challenges in coming to know a language or a convention are the same challenges that must be overcome in coming to know other things and relations in the world.

Keeping in mind the point just made, let's return to the questions of the role of thought-content and the nature of thought-content. To this end, I introduce another example. Suppose a particularly brilliant parrot, Polly, having been trained to converse linguistically in just the way a (less than brilliant) person would and to do so correctly, by the conventions of usage. There is, we presume, an elementary thought-content in Polly's head, involving perhaps thoughts of certain squawks and facial contortions from his human and thoughts of sugar treats. But, we can be confident, the thought-content does *not* include a parrot's notions of snow being white when Polly is using the words 'snow is white.' So, the parrot conforms to Heck's account that has the language agent utilizing elementary thought-content sufficient to acquire the conventions of language, but then depend only on the conventions of language for meaning thereafter. The Use-Meaning advocate might say that Polly's words, because they *are* correct with respect to convention, *do* have meaning. Let us now suppose Polly's sister Molly is equally trained to converse by correct conventions of usage. Polly and Molly carry a conversation for quite some time, even when no humans are around. In fact, a researcher could describe a by a T-sentence theory the know-how capability, in Dummettian terms, of these parrots to use language that would match that of the parrots' human counterparts. Would the Use-

Meaning proponent, however, still insist that the words have 'meaning'? If so, then the question becomes: *To whom* do the words have meaning? Certainly, there is no meaning in these words to Polly or Molly. Perhaps, the Use-Meaning proponent would argue that the conventions are not really parrot conventions but human conventions and that this is the reason why Polly's words are meaningless to Polly. I respond that this objection misses the point of the example, which is intended to show that mere conventions and mere elementary thought-contents only associated with the proximal features of those conventions cannot amount to semantic content of the sort that would account for the truth of 'snow is white' standing or falling on whether snow is white (vs. grass being green), no matter how much more we complicated the conventions.

The parrot example is supposed to illuminate the need for a fuller sort of thought-content in the determination of meaning. Recently, I suggested that the Use-Meaning advocate wishes to replace the issue of the relation of actual states-of-affairs to correctness of pairing with an account of convention instead. To the contrary, the parrot example shows that correctness of pairing can be achieved between an expression and a convention, but if the thought-content has no relation whatsoever to the actual states-of-affairs (that a realist would say are described by the expression), then we really can question whether meaning was achieved after all – that is, whether Heck's peculiar definition of 'meaning' is correct. This is the heart of the realist view, with respect to language, that our judgment of an expression's truth depends intimately on what we think is the actual state-of-affairs described by that expression.

Dummett (1973:255) has raised doubts about the role of the actual state-of-affairs to a determination of truth and has given examples such as colors changing in fact but this fact being unrecognized by us or our measuring apparati. In such a case, our use of language would be unchanged, including our assignment of truth-values. The conclusion Dummett draws is that the actual state-of-affairs must not be relevant to the meaning of our expressions if the meanings fail to change when the states-of-affairs do. I have two responses to this. First, as stated above, conventions of use are also states-of-affairs, so it can't *generally* be the case that states-of-affairs are irrelevant to meaning without that also undermining the convention basis, also. Secondly, even waiving this first response, Dummett's challenge portrays a simplistic view of the relationship between our beliefs (or thought-content) about states-of-affairs and the actual states-of-affairs. It describes the naïve realist. The mature realist has no problem with aiming for a target and missing the mark, so long as there are grounds to hold that something was aimed at, that we can come to a meaningful judgment about more or less distance from our guess and the target, that progress in accuracy is a realizable goal.

The case in which it makes sense to speak of aiming and missing should be contrasted with the case in which it does not make sense, i.e. the case in which there is no connection whatsoever between our thoughts of states-of-affairs and actual states-of-affairs. The question, however, for both cases is this: What is it that we have in mind when we think about the states-of-affairs such that can still meaningfully talk about those states-of-affairs (and their truth conditions), even where we are in fact wrong? The realist could say that we have in mind a world-model, under which truth-conditions relative to that model, for some state-of-affairs, can be specified. What makes those truth-conditions genuine is that we have grounds for thinking the world-model correct, such that even if we turn out to be wrong on occasion, we can still claim justification for having made that judgment in the first place. The mature realist does not have to be correct in every instance or to every degree to maintain his position. It is only necessary that progress be substantiated. And, the argument for progress can't be defeated by the antirealist without also defeating grounds for holding all states-of-affairs suspect, including those that comprise conventions.

The antirealist, on the other hand, will answer the above question, saying we only need have in mind that which is necessary for following the conventions of usage, even where this includes some elementary thought-content. Expressions will have meaning where conventions for the use of those expressions (and paired content) are correctly followed. I challenged already exactly this claim with my parrot example. Dummett's rebuttal to the realist assumption contained in the parrot example, that there must be in meaningful language a connection between thought-content and states-of-affairs, has now just brought us back to the antirealist making the same claim all over again!

I think this argument loop can be broken if we consider more carefully what we must have in mind when we have knowledge-how, whether Dummett's kind of knowledge-how (practical ability) or Heck's more though-content inclusive kind of knowledge-how. First, contemplate the extreme case of a thoughtless practical ability, such as a worm's ability to wriggle. One should hesitate to say of a worm that it *knows*-

how to wriggle. It twitches muscles in a patterned way, starting and stopping in response to certain cues. Compare that to a fish's ability to swim (the example used contraventionally earlier). A tuna, e.g., appears to do more than merely twitch muscles. It swims sometimes quite strategically and in response to novel circumstances that cannot be regarded as mere environmental cues. One could safely presume that a tuna is not self-aware of its own swimming, that it has that ability without needing to think about it. However, it is more difficult to claim that the tuna is not conscious of anything at all. In fact, one is given to say that, but for a somewhat comprehensive modeling of its environment, of predator and prey, of complex behaviors of other tuna, and so on, it should not swim as it does. The more intelligent is the particular animal, the more we are willing to say that its know-how ability is impossible except for a prerequisite, non-linguistic, and largely correct (if perhaps crude) modeling of the world. This is not to say that the animal's model isn't possibly wrong in the details. The point is that what the animal has in mind when it executes a knowledge-how is a model which it takes to be the world, and that model should bear a relation to the world such that the animal can successfully perform its feats by taking the model so. Success is tantamount to correctness here, if we regard the way tunas perform their feats as a convention in the sense of a social pattern of behavior. The realist can make further arguments linking empirical success to progress towards accurate representation of the model to the world.

What I am putting forward so far is that knowledge-how for thoughtful animals does require a prior knowledge-that, though not a linguistic knowledge-that. For humans, we have both linguistic knowledge-that and non-linguistic knowledge-that. Clearly, a linguistic knowledge-that can't be a prior requirement for a linguistic knowledge-how. But, I do assert that a non-linguistic knowledge-that is required for linguistic knowledge-how, and not just a knowledge-that of the conventions of usage. I am asserting a non-linguistic knowledge-that of a world-model as a prerequisite for knowledge-how of the use of expressions which we take to be meaningfully about the world. To assert that the knowledge-that required for language ability is limited to the worldly elements making up conventions of usage is just like asserting that strategic animal behavior is merely muscle twitching in response to proximal cues. There are too many novel arrangements of things and their relations to formulate a single set of rules that would warrant success in all (or even most) cases. The knowledge-that must be more than knowledge-that of proximal environmental cues and patterns of linguistic behavior. Even if some pattern were rich enough to capture correct linguistic behavior, it would be too intractable for human brains to learn or retain.

When I hear, "There's a banana on that table," what comes before my mind is not a convention, but rather, my non-linguistic notions of bananas and tables and their relationships to each other and to other things in my world-model. The challenge Dummett makes to this intuitive idea is that, if non-linguistic thoughts were to be available which matched the structure of language, then why don't we just think them? Why do we in fact always structure our thoughts in language? This is a good challenge from Dummett.

The standard example that is supposed to show the superiority of language to bear certain content the incapacity of non-linguistic thought to bear that same content is the case of the past-tense. Wittgenstein's example is of one's dog being able to be happy to see one today, but not (today) being able to be happy about having seen one yesterday. This would be correct if all that we had, in the way of resources for non-linguistic thought-content, were static pictures or images. If you show me a photo of a fellow and his dog, I couldn't tell you which day it was, without some other clues. If such a picture were all that comprised a single thought-content, then indeed Wittgenstein and Dummett are right. But, this is wrong. We don't have a photo-album conception of the world. We have a robust world-model and the passage of time is part of that model. I quibble with the suggestion that, say, a calendar or other tool for measuring time, while certainly conventional in the particulars of form and unit, therefore belongs to language. Tools are tools; they extend our abilities and help us better avoid errors. Someone with a prodigious memory could, with effort, construct a sequence of experienced events and mark the passage of time this way.

If our concept of the past is fundamentally a linguistic one, and if language embodies the idea, then how do we learn the convention for this in the first place? Every teaching and learning of something takes place in the present. Which way do we point to point a language-learner to the past? Pointing to a day last month on the calendar is only to point to a spot on the calendar today. In order to learn the convention of past-tense (or to use a calendar for that matter), we must already have in-place a notion of the past in our world-model. For example, the calendar teacher would point to a place on the calendar and say, 'Remember that day, when you lost your tooth?' or refer to some other past event, where what comes to mind in the mind of the student is not another piece of language or a convention of usage but a memory of a string being placed around the tooth, and other related people and things surrounding that event (including a special marking of the calendar), and the relation of this event to other events, forming sequences of events. The resultant historical model is what shapes our understanding of the past. I suggest that the non-linguistic notions must be primary before we can properly learn the linguistic conventions of past-tense, not *vice versa*. So, to revisit Wittgenstein's dog example, the dog could, in fact, be happy today to have seen us yesterday, but *we* can't know this is what he's happy about. The dog, lacking a language, cannot communicate in a Gricean fashion what's on his mind.

The confusion, perhaps, is in conflating the tools of thought, which certainly include language, with language itself (conceived in isolation). Language, as a tool, helps us do things we couldn't do very well ourselves, not unlike the way shovels and jackhammers help us. But, the capacity gained in the use of a tool is in our knowing how to use it, not in the tool itself. Language, as a tool, requires us to coordinate with convention. But, our knowing how to use language, I suggest, is knowing how to extend thought by means of it. Polly the parrot knows-how to coordinate with the conventions of language but does not know-how to extend thought by means of language. Only the latter makes for meaningful language.

So, to return to the evaluation of Heck's account, he builds his case on the compartmentalization of content and meaning, but I have argued here that such a segregation is tantamount to a category mistake. This becomes particularly acute when we point out that the convention itself is just another set of state-of-affairs in the world. Correctness with respect to knowledge-that of subtle facts of context, utterance, behavior, and so on, is no different than correctness with respect to knowledge-that of other states-of-affairs in the world. The Use-Meaning proponent cannot undermine the one without undermining the other.

CHAPTER THREE: VAN FRAASSEN

Bas van Fraassen has crafted a strategic position within the realist/antirealist debate that involves rejecting realist arguments and that aims at garnering all the benefits that realists had believed they had all to themselves to enjoy, while avoiding the pitfalls that beset his antirealist predecessors. I do not believe he succeeds. In my opinion, van Fraassen's most serious strategic error, in countering realists, is leaving himself vulnerable to the same sort of skepticism he applies to realists. van Fraassen assumes that whatever arguments would harm the constructive empiricist will just harm the realist far worse, and whatever arguments would help the realist will make the case far better for the constructive empiricist. But, this is a mistake.

van Fraassen's polemical tactics best make sense in light of his semantical view of theories. If we pair his semantic view of theories with the pragmatic view underlying his constructive empiricist account of explanation, we have the central planks of his position. In this chapter, I propose to present both his semantical view and the pragmatic view, then to appraise these, weighing the degree each has accomplish his antirealist goals.

van Fraassen's Account of Theories

The syntactic view of theories (1980:54) is one that van Fraassen rejects. It identifies a scientific theory with a deductive theory T, in a specified language L, with statements being axioms or theorems. Suppose the terms of L are partitioned into observational and theoretic terms. We designate the former as E, the observational sub-vocabulary. The empirical import of a theory is just its deductive observational consequences, represented as T/E, the theorems of T expressed in E. Two theories T and T' are empirically equivalent iff T/E is identical with T'/E. On van Fraassen's reading, this account led into a quagmire of logical difficulties, such that the important philosophical questions were lost in the fray, and much time was wasted (in van Fraassen's opinion) following up technical issues of no philosophical import. The chief questions of philosophical import surround the distinction between observational terms and theoretical terms. As it turns out, T/E can express everything that T does, albeit in a 'hobbled and hamstrung' fashion (55).[44] For instance, 'there exists a thing which both has position and does not' expresses a highly theoretical assertion, but uses just observational terms (54). Thus, says van Fraassen, the distinction "reduces to triviality or absurdity, it is hard to say which" (55). Several attempts were made to solve the difficulty, e.g. by narrowing the definition of empirical equivalence by constraints on axiomatic extensions. However, these attempts failed. van Fraassen says, even if a solution could be found, it would not matter anyway, since (a) we still would not be able to extract from T the observational information we seek and (b) the statements of the theory would not be expressible in a natural language. – Because realists made great hay over these problems of the syntactic view, and for other philosophically strategically motivated reasons, van Fraassen urges instead a semantic view of theories.[45]

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[44] Dr.Creath notes van Fraassen's claim exceeds what he has actually demonstrated.

[45] Dr.Creath observes that van Fraassen has here adopted the realist critique of logical empiricism in an attempt to inoculate himself against the charge that the 'failures' of logical empiricism are his failures.

Under the semantic view of theories, rather than a theory being identified with a set of statements in a language, a scientific theory is to be identified with a set of models. This set of models can be described in any of an endless number of different languages, each with its own potential limitations, and while such limitations stunt the capacities of a language, no such problem exists for the models. The collection of models that define the theory are (in principle) self-sufficient with or without a language to associate with. We designate a part of each model to serve "as candidate for the direct representation of observable phenomena" (65), calling this part its *empirical substructure*. Observable phenomena van Fraassen calls 'appearances,' being structures describable in measurement and experimental reports. The remainder of the model, not included in the empirical substructure, van Fraassen calls *internal structure* (what realists would take as candidate for corresponding to some trans-observational part of the world). A theory is empirically adequate (EA) just in case it has at least one model such that the empirical substructure of that model is isomorphic to all appearances. If for every model M of T there is a model M' of T', such that the empirical substructure of M is isomorphic to the empirical substructure of M', then we say that T' is *empirically at least as strong as* T. If T' is empirically at least as strong as T and T is empirically at least as strong as T', then T and T' are empirically equivalent (EE).

van Fraassen is less than clear on the precise nature of his conception of *model*, describing models at times as along the lines of a possible world, at other times in the sense of a logical model, and at other times along the lines of a mathematical model.[46] In

[46] For an extended discussion of this, please see Appendix II, note 1.

his clearest enunciation, it still remains open to interpretation: “I will continue to use the word 'model' to refer to specific structures, in which all relevant parameters have specific values” (1980:44).[47] Given the shift in his views from 1980 to 2006 (to be discussed), I suggest that, in *Scientific Image*, van Fraassen regards models as essentially representational and qualitatively definite. Later in this chapter, we will follow his transition from the semantic view of theories to a form of structuralism, in which a theory is taken to be an abstract mathematical structure.

Given van Fraassen's semantical view of theories, it is not difficult to see how he might exploit underdetermination to subdue realism while yet availing antirealists of the philosophical and practical benefits of a literal construal of theories. First, class together all EE and EA theories whatever their differing internal structures. In virtue of being EE, each will be empirically functionally the same. That is, for all members of this class of theories, for any empirical 'input,' the empirical 'output' will be identical.[48] van Fraassen intends this manner of underdetermination to be disabling to the realist, who is unable to ground belief in any one of this class of theories. For the constructive empiricist, however, this situation is most welcome, for he may accept whichever theory he pleases, immersing himself in the 'world-picture' of that theory. From an antirealist point of view, so long as the practitioner appropriately 'ontologically brackets' his statements, then he is free to indulge the theory nearly as if he were a realist. It won't matter that the theory might be, in fact, false, just as it wouldn't matter if it were true (since how would we

[47] Contrast this to van Fraassen's characterization of the use of models in science where certain parameters are left unspecified, and so are more properly model-types, as he puts it.

48 However, there will be important pragmatic differences among these theories, and this will form an important part of van Fraassen's pragmatic view.

know anyway?). All that matters is that the theory is EA and useful. That is the chief aim of science under constructive empiricism, *viz.* to find and apply EA theories. In fact, van Fraassen stresses that empirical minimality (i.e., having no 'metaphysical baggage') is *not* a virtue for a theory. 'Metaphysical baggage,' says van Fraassen, provides detours, via theoretical variables, to useful, manageable descriptions of observable phenomena (31). So, van Fraassen says to antirealists (as it were): "Don't worry! Have fun! Act like realists! Answer questions *ex cathedra*, let your language be guided by the theory, immerse in the world-picture, chase down robust causal explanations, but at the end of the day, make sure you acquit yourselves of all realist commitments by pronouncing a vow of agnosticism."

Under the syntactic view, the individual statements of a theory may be true, taken separately from the rest of the theory, and the truth of all constituent statements makes for the truth of the whole theory. But, that is not the case under the semantic view, where empirical adequacy is a strictly a global property of theories and is not applicable to constituent statements (representing classes of models). Each statement that can be called a proposition of a theory will be true for every model of the theory, and a statement that cannot be called a proposition of the theory will be false in at least one model. But, the empirical import of a theory cannot be isolated syntactically. Thus, it is nonsense to ask about (i) the EA of a single statement, or (ii) a logic of syntactic functions from premise to conclusion that preserves EA. Some statement *S* may be regarded as EA insofar as it is part of one theory T, but regarded as failing EA insofar as it is part of a different theory T'.

On van Fraassen’s semantic view, a singular statement *S* cannot by itself be EA, even if we understand that there may exist a set of models that satisfies *S*. Of course, if *S* is not well-defined or self-contradictory, then no models will satisfy it, and EA will fail. More interestingly, *S* cannot, by itself, determine what is or is not to count as empirical substructure. When merged to a theory T, *S* may achieve or fail EA depending on the content of *S*, the structure of the models in question, and whether T determines a larger or smaller portion of the structure of each model to count as empirical substructure. Also of potential import to the EA of *S* are the internal structures of the models of *S* that may conflict with, or be limited by, the internal structures of the models of T.

As an example for case in which EA for *S* is affected by the degree of empirical substructure allowed by T, suppose that S is 'Bill has a bacterial infection' and that T only allows naked-eye observations as permissible among the possible appearances. Further, suppose T regards red, inflamed tissue as evidence for bacterial infections. Under such conditions, *S* may be regarded EA with respect to T. But, if conjoined to a different theory T’ which dictates that microscopic data are to be included among possible appearances, *S* may now fail EA with respect to T’, where the microscope shows viruses and not bacteria on Bill’s tissue.[49] For van Fraassen, as I understand him, the case of EA for *S* ‘by itself' is unfathomable. There must be some larger theory present at least for fixing the limits of empirical substructure. So, perhaps, we are supposed to read any empirically evaluable, well-defined individual statement as always tacitly merged to some background/collateral

[49] Note that van Fraassen regards the phenomena of microscopes as consistent with empiricism (as a class of phenomena for which the theory must be consistent) but not as belonging to the class of 'observables.'

theory.[50] It should be noted that no inherent limits constrain the degree to which a theory chooses the range of empirical substructure. van Fraassen gives separate arguments why these limits should be fixed where empiricists want them, but in principle, a theory could be such that empirical substructure includes microscopic things. On the other hand, van Fraassen is happy to include the observable outputs of scientific apparatus as among the empirical. So, while he will regard the term *virus* to name the internal structure of a certain biological model (and we are to remain agnostic about the things in the world which may or may not correspond to that model), the microscope's image is to be taken as empirical data (about which we are *not* to be agnostic) and the theory can now stand or fall on what it predicts of the image.

What happens when a theory is extended, or merged with another theory?[51] Given what has just been presented, one would naturally conclude theoretical extensions or mergers might have an important effect on the EA of the involved theories. If an extension causes EA to fail for some previously EE contenders of a theory T, but preserves EA for T, then this would be a powerful tool in coping at least with local underdetermination. The constructive empiricist does not see things the same way. On van Fraassen's view, if theory T is EA, then extending (or conjoining) T with T' only means that T' must now 'find a home' (51) among the models of T, meaning that T' must now try to form a non-empty intersection with T. van Fraassen asserts that, since the models and resources of T are unchanged, T must still be EA. The extension of T to T' is

[50] See Appendix II, note 2.

[51] Realists see extensions/mergers as significant in that it renders previously EE and EA theories now discriminable, a potentially useful tool in coping with underdetermination.

'victorious' if the intersection is EA. The extension is a 'total defeat' if (T & T') is phenomena are such that no model in the intersection has empirical substructure matching the phenomena. van Fraassen's account of theories has hidden assumptions which I will straightaway begin to question.

Comments on van Fraassen's Semantic View of Theories

The theory-as-a-basket-of-models idea, while philosophically captivating, is hard to comprehend in practice, unless it's just a cunning philosophical device to evade the pitfalls that beset the syntactic view while still, in effect, using the syntactic treatment of theories. Suppose we are entirely in control of the choice of models, and suppose by 'models' van Fraassen means something along the lines of a definite representation whereby we establish EA by comparing the empirical substructures of the models directly to the set of all in-principle observations, searching for a perfect match. Then, the specification of the details of this match must be made explicit in the same way our derivations must be made explicit. Such a task would be daunting for even the simplest of theories. Where our specification of models and observations is less than perfect, we should worry always about the hidden saboteurs in the bunch. This can't be what van Fraassen intends.

On the other hand, still assuming models as definite representations, if model-selection is not fully determinate, then most of the particular details of each model will be beyond our control. In other words, if models are appointed in a way analogous to the way possible worlds are selected, as when we assert ‘Let *J* be the set of possible models at which *S* is true,’ then all we know about the appointed set of model is that, e.g., it does not falsify *S*. But, then, there is the epistemological problem of coming to know these models in other ways and the particulars of their empirical substructures in the first place, which is certainly important for establishing EA. Such knowledge and the means of model-selection in this case, seem to depend on language in a way that re-installs the syntactic view of theories as primary. Hence, I don’t see this interpretation compatible with van Fraassen’s semantic view of theories.

Finally, to the extent van Fraassen takes 'model' to mean a mathematical structure of a certain sort, then the sense of 'empirical substructure' becomes hard to capture, especially in light of van Fraassen's own rejection of a realist, descriptive notion of structuralism. It is not obvious how a collection of functions mapping numbers to numbers should establish facts about the non-numerical world.[52] Indeed, van Fraassen's recent work rejects mathematics as descriptive and instead proposes that mathematics be embedded within a pragmatic system in order to relate mathematical results to the world. At any rate, it is clear that coming to a judgment about EA involves far more than simply noticing a straightforward correspondence between a part of the model and a part of the observable world.

So far, we have been unable to locate a clear answer to the question: How could scientists utilize the semantic view of theories as a practical method for doing science? I

[52] van Fraassen will address this problem in his answer to Reichenbach's coordination problem, discussed below.

will now raise some further challenges to answering this question. (i) Epistemological: As just stated, van Fraassen does not detail the epistemological project involved in undertaking discovery of a match between empirical substructures and the observable world so as to judge a theory's EA. Direct-realism is assumed, as is the clear dividing-line between what can and cannot be directly perceived. On the assumption of direct-realism and a theory that determines empirical substructure, van Fraassen can uncontroversially prescribe agnosticism about whether a match might obtain between a model's internal structure and the imperceptible but full belief about a match between the empirical substructure of some model and the perceptible. However, this epistemological view is undefended, not just by van Fraassen, but by any advocate of direct-realism, as BonJour (2004) points out. If the epistemological project of establishing a match between empirical substructure and the world turns out to be a more complicated process, involving gaps that must be bridged by means other than direct-perception, then it undermines the rationale for the boundary line in the model and our corresponding attitudes. (ii) Metaphysical: There are also some undefended assumptions that the world is a certain way, yet this way is not ascertainable by empirical study. The scientist only ever observes slices of the world, and it would commit the fallacy of composition to conclude that the slices ever add up to a whole. Similarly, there are metaphysical assumptions involving, e.g., kind membership. The realist would hold that the internal structure of the models play a role in explaining how distinct things can belong to the same kind or how the world as a whole hangs together, and we cannot simply be agnostic about it. This point is especially important for the notion of 'regularities,' of which van Fraassen makes great use. Without somehow grounding these serious metaphysical assumptions, one cannot declare some set of things to constitute a *regularity*. (iii) Skeptical: Are other minds to be counted among the observable things in the world? Or, are we to be agnostic towards these and the observation reports they produce? Why isn't underdetermination (holistic or contrastive) a problem for deciding things about the observable world?

The challenges above are all too easy make, and if van Fraassen were worried about such challenges, he would have bothered defending against them. One important reason he does not, I think, is his assumption that whatever damages the constructive empiricist's position will damage the realist's position that much more. His position was designed to be the more conservative option, such that any realist confidence in the truth of a theory would be still greater confidence in the EA of that theory, but the constructive empiricist option, of mere acceptance but not belief, involves fewer metaphysical commitments and therefore less risk.

But, is this right? We should first appreciate the extreme difficulty of establishing the EA of a theory. EA is a very high bar to cross, as van Fraassen himself acknowledges. Even making simplifying assumptions, there are no grounds (short of assuming a tremendous degree of homogeneity in regularities for the universe and across time) for declaring a theory EA unless we can also establish that the ratio of actual observations that match empirical substructure to all possible observations that match empirical substructure approaches unity. Can this be taken seriously? – I will argue that, in our actual use of theories, grounding belief in the EA of an individual theory only proceeds by appeal to background theory, which implies a practical interdependence among theories. van Fraassen's semantic account mostly conceals this interdependence among theories, an interdependence which, when recognized, pushes us towards realism.

van Fraassen's example of theory extension is the case where a settled theory T, with EE alternatives, is attempted to be conjoined to a less settled theory T', and the conjunction *appears* to eliminate all the EE alternatives to T.[53] However, van Fraassen denies the appearance that the EE alternatives to T were actually eliminated by the conjunction, arguing that the new, conjoined theory (T&T') has itself an infinite number of EE alternatives (T&T')', (T&T')'', and so on. However, I counterargue, the example is not representative of the general case, and the fact that the conjoined theory is underdetermined does not refute the point that the original conjunction did indeed end EE for the alternatives to T.[54] While it is acknowledged that, indeed, sometimes, a less settled, interloper theory T' may be rejected on the grounds that it has obvious EE competitors, regardless the effects it would have in conjunction with T, in other cases, rejection of T' is not an easy option. In that case, the conjoining theory T' may be one that we place in high regard and to which we may lack reasonable EE alternatives. If T' is just as settled as T, is consistent with T, but is inconsistent with every EE alternative to T, then the rational choice is accept T&T' and reject the EE alternatives to T. In such cases, contrary to van Fraassen's assertion, theoretical extensions can positively be deployed as

53 For a careful and extended discussion of theory conjunction ending EE among a set of theories and a treatment of van Fraassen's counterargument to this point, please see Appendix II, note 3.

54 I show this Appendix II, note 3.

a weapon in the arsenal against underdetermination.[55] While this does not force van Fraassen to adopt realism, it forces consequences on theory acceptance that restrict subsequent choice, and it forces the constructive empiricist to take seriously the 'internal structure' of accepted theories.[56] Moreover, if T' is the background theory, then this feature of theory extension to discriminate among otherwise EE theories gives special status to the background theory.

Let's explore further how commitment to a background theory comes to force the antirealist into a more realist-like position. In general, van Fraassen's initial presentation of the semantic view suggests an epistemic independence among different models and theories, which belies the practical interrelations among them. He will come to acknowledge this point in a different way as he introduces his pragmatic views, but I think it is important to explore the way van Fraassen's official presentation of the semantic view courts inconsistency. On the one hand, van Fraassen says making changes to the collection of models constituting a theory can effect changes in global EA, while on the other hand, in the case of extensions, conjoining the models of T with the models of T' is expected always to preserve EA (except under the *ad hoc* constraint that T' is not to have any effect on T).

Let's consider a simple example. Suppose Holmes is trying to discover who murdered Smith who met his demise at the base of a cliff by the sea. Messieurs X, Y and

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55 at least local underdetermination.

56 The constructive empiricist must take seriously the internal structure in such cases because that is the very thing generating inconsistency with the EE alternative theories.

Z each have alibis except Mr.X for 2-3pm, Mr.Y for 4-5pm, and Mr.Z for 7-9pm. Given just the relevant evidence that Holmes has collected (and some body of background/collateral theory K_H), it appears that each suspect looks equally likely to be guilty of the crime. Then, the local constable suggests the circumstances of the tide may shed light on the case. After some investigation, it is determined that Smith would not have died in the manner he did had he fallen into the water (at high tide) but only if he had fallen onto the rocks (at low tide) which, on the date of his death, would only have occurred at 2-3pm, making Mr.X the guilty party. – True enough, the theory of the tides did not disturb the initial models, *per se,* but certainly did render new observational consequences, at least in making formerly irrelevant or unnoticed phenomena now decisive (a Hansonian point). Isn't this the normal sort of case?

van Fraassen might respond: 'The initial theories were not EE after all, but only incompletely investigated.' But, that is not correct (or else it assumes more about the models and background knowledge than is legitimate). The initial theories are EE given K_H and the scope of observability peculiar to those theories. Conjoining each of them with tide-theory spells defeat for two of the murder-theories; tide-theory models of water covering the rocks is inconsistent with whichever of the murder-theory models has Smith dying on the rocks. But, there's more to the picture than just that. Tide-theory changed the lighting, so to speak, by making certain phenomena salient, whereas, prior to the conjunction with tide-theory, that same phenomena was not worth noticing.[57] Suppose the set of models for T only addresses a range of appearances A. Those models will be

57 This is the 'Hansonian point' just mentioned above.

insensitive to some other appearances A', such that EA for T is unaffected by A'. If the conjoining theory T', however, finds these other appearances A' decisive, then (T&T') now finds the wider class of appearances (A or A') decisive.

Another possible response from van Fraassen: ‘I object to partitioning the background knowledge as you do it; there certainly is tide-theory (or the physics behind tide-theory) implicitly in Holmes' K_H.’ I reply: (i) we can always perforce arrange Holmes' K_H to fit the example, but it is unreasonable to expect complete sets of background knowledge. (ii) Going the other direction and enlarging K_H just makes my point. In order to navigate the world as we do, we don't or can't fashion models to have such autonomy with respect to one another or to the rest of the world. Without an ample background theory to tie things together, most scientific theories are truly disjoint (each internal structure treating of different categories of thing and each set of 'appearances' treating of different categories of measurement). Consider Paleontology and the important methodological use it makes of radiocarbon dating and geology. Then, consider the methodological use it makes of common sense assumptions (ontological stability of things, other minds, and so on). There is nothing original in the observation that, strictly speaking, most scientific fields have little genuinely in common, yet in practice, scientists draw from a unified theory-of-everything. In this way, van Fraassen's ‘clean’ portrayal of models is plainly misleading. On the ‘clean’ semantic view of theories, we would almost never have the rationale for extensions in the first place since internal structures and specific observational measures rarely overlap among theories of different fields. Yet, scientists do, with easy conscience, draw from a pool of theories, conjoined in some sense differently than van Fraassen has depicted it, with the result that, more often than not, EE hypotheses can after all be differentiated. Because a robust background theory can severely limit choices, the easy agnosticism van Fraassen describes for the constructive realist is actually not so available. In other words, believing in a wide array of theories that refer only to the observable world still places great constraints on acceptance of theories that refer to trans-observational entities. So, e.g., even mere acceptance of our contemporary scientific theories rules out also accepting goblin theory. If I do not accept goblin theory, then am I still agnostic about it?

I make one final remark before moving-on to van Fraassen's account of explanation. For the realist, generally, the expectations formed on the predictions of a theory are grounded on the belief that there is good reason to hold the theory true. If we know already that a theory is EA, then we would have a different, non-realist reason for forming expectations on theoretical predictions. However, van Fraassen never shows that we have good reason for holding a theory as EA. van Fraassen assumes correctly that any realist who holds a theory true *ipso facto* holds the theory EA, but he does not account for what constitutes good reasons for holding a theory EA *simpliciter*. And, for the realist, removing the belief that a theory is true equally removes the grounds for thinking it EA. Since, as noted earlier, no amount of actually collected evidence could ever practically begin to establish EA, the constructive empiricist is left with no reason to expect success from a theory. EA, it turns out, is not the default to realism, and if not, then constructive empiricism is not either.

When van Fraassen pivots to his pragmatic account, he will give different reasons for theory acceptance. But, pragmatic reasons for accepting a theory are not sufficient grounds for holding the theory to be EA. It is proper to demand of van Fraassen, on the basis of his semantic view, grounds independent of pragmatics for holding a theory EA. From the antirealist's point of view, why would (illegitimately) thinking a theory true provide grounds for thinking it EA? Unless solid grounds for thinking a theory EA are provided for, the antirealist has no right to take theoretical prediction seriously. If scientific antirealism is to be more than just skepticism, it must provide an independent, workable alternative to realism. As I said, antirealism doesn't win by default.

Let's take stock of where we are at the end of this subsection. Constructive empiricism hinges on a particular version of the semantic view of theories that defines theories in terms of models, but when the view is examined more closely, we discover that it is not clear what models actually are and that models are not practically useable. For those reasons, practically pursuing EA by means of models is elusive. But, even if EA were obtainable by comparing models to the world, in the absence of any reason other than the ratio of observed appearances to all possible appearances reaching unity, there is no reason to hold a theory EA. In effect, this means no theory will ever be judged EA. Since belief that a theory is EA is the rationale for antirealists to behave with respect to that theory like realists and perform science the way most scientists do, then that rationale is unavailable. Also, recalling a point from chapter one, constructive empiricism cannot account for the sequential fruitfulness of mature scientific theories, which just adds to its embarrassment. As we looked more closely at van Fraassen's account of theoretical extensions, which he had intended to show of no avail to the realist, we uncovered the very opposite to be the typical case, *viz*. conjoining two settled theories eliminates EE contender theories. Where one of the conjuncts is the background theory, tremendous constraints are placed on subsequent theory-choice, forcing even the antirealist to respect the internal structure of theories. Background theory plays an ineliminable role in actual science, tying together otherwise disparate theories and creating a global interdependence among theories. How do these points add up? As discussed in chapter one, I advocate a realist position under which the realist does not fully subscribe to any theory but does take theories seriously with the goal of diminishing the pool of possible contenders and bringing us just that much closer to the correct model of the world. The role that background theory and other settled theories play in eliminating contenders and constraining subsequent theory-choice exemplifies the realist approach of diminishing the pool of contender theories and thereby cornering the truth. I noted earlier that the realist approach is more of a methodological course than a credo that one endorses. Hence, if van Fraassen's account has led to the result that the constructive empiricist has no basis for holding his antirealist view but, unwittingly, follows the methodological course of the realist, then the constructive empiricist is a realist after all, despite whatever agnosticism he professes.

Explanationism

As many realists hold, that a scientific theory referring to the trans-observational world explains the evidence well provides the best grounds for thinking that theory true.

van Fraassen, however, rejects realist accounts of explanation, and he gives some oft-repeated counter-arguments that are easy to summarize. Against the realist's demand that every universal regularity requires an explanation (which leads inevitably to trans-observational explanations), van Fraassen replies (i) What is the warrant for such a demand? It can't be discovery of the truth, because trans-observational claims will forever be empirically frustrated in demonstrating this claim to truth. (ii) The demand cannot be applied universally, for it will lead to contradictions, such as the demand for hidden variables in quantum physics. (iii) The demand leads to an unhelpful regress, since if satisfying the demand (momentarily) just means supplying an explicans (about, e.g., microstructure) that is itself another brute universal regularity, then why not just accept the first (observable) one as brute? And, even if we were to accept the explanation, the explanationist would demand yet another, new explicans for the previous explanation, *ad infinitum*. A second general line of counter-argument wants to know whether there is any difference in empirical import between 'the theory which best explains is true' and 'the theory that best explains is EA.' If not, then the latter is the more philosophically prudent choice.[58] Dr.Creath points out that one could go further and argue that ‘the theory that best explains is EA’ is more *likely* the case, since it will hold even where the explanation is, in fact, false.

If explanation cannot guide us any closer to the truth, why, asks van Fraassen, is it being demanded? What impels us to want an explanation in the first place? To answer this question, van Fraassen has us look carefully at the relationships between theory, facts

[58] Note: 'total possible empirical import' and 'empirical adequacy' are synonymous; realists require EA of theories just as the constructive empiricist

in the world, descriptions, and explanations. He notes: “Traditionally, theories are said to bear two sorts of relation to the observable phenomena: *description* and *explanation*” (153). Some realist philosophers saw explanation as something 'over and above' description. Realists, says van Fraassen, were then emboldened and concluded that, therefore, explanation is an irreducible, special pathway to truth, these ideas giving rise to 'explanation-mysticism' (154) such as Aristotelian necessity or mysterious causal processes extending beyond the observable. Yet, if this is so, we should be able to say clearly how explanation differs from description. Yet, in the typical, real-life case, when we ask for an explanation from a scientist, all we actually receive, says van Fraassen, is a description of the facts, determined by the theory to be relevant to the context of our request.[59]

Digging deeper, van Fraassen asks: What is the relationship between a theory and an explanation? The paramount theory/world relations are truth, EA, and empirical strength. In fact, argues van Fraassen, explanation is *not* a theory/world relation and concerns the world little. Rather, explanatory power should be classed with the pragmatic virtues (along with simplicity, scope, mathematical elegance, etc.). The only *belief* involved in theory-acceptance is belief that the theory is EA. But, there are other non-belief factors ('human concerns') that are sometimes importantly involved in theory-acceptance: the commitment to account for all future phenomena, the commitment to a research program, the wager that the theory can confront future phenomena without our having to give up that theory, ..., and explanatory power. van Fraassen maintains that, far

[59] van Fraassen’s aim, in this line of argument, is to show the scientific realist confused, not to show scientific realism false.

from being a preeminent virtue, explanatory power is among the least important on the list, only becoming decisive in theory-selection when all other factors are evenly matched between contender theories. Since explanatory power is a pragmatic virtue, there is nothing peculiar to explanation *per se* that makes it *rational* to pursue. The epistemic merits a theory has in contributing to good explanation are just the ones it had in being EA, empirically strong, and so on. The 'name of the game,' van Fraassen famously says, is 'saving the phenomena,' and whatever pragmatic role explanation can serve to that end will be its *raison d'etre*.

"Theory T explains fact E" asserts only a relation between the particular theory and some particular facts, and the explanation asserts nothing about T's relation with the rest of the world. Explanation has no further theory/world features other than providing evidence that the theory is consistent with the selected facts, and so, it is impossible, on the basis of explanation alone, to establish anything further about the theory, such that it is true or *even EA or acceptable*. – At bottom, says van Fraassen, explanation has no *sui generis* mysterious powers, but it is rather, as van Fraassen will argue, that explanatory power is just a manifestation of the theory from which it issues. If an explanation has the qualities of empirical strength, and internal consistency, that is because the theory that issues it is empirically strong and internally consistent. A good explanation does not *lead* to consistency with the facts, but consistency with the facts is a precondition (on the issuing theory), in order to make possible a good explanation.

I reserve comment for now, except to point out that 'explanation' is an ambiguous term, and van Fraassen is seizing on a particular sense of that term that, I contend, is philosophically uninteresting to the realism debate and that has only to do with finished, secure theories. van Fraassen portrays 'explanation' as essentially 'theory-application to the facts,' and he is exactly right that a theory cannot be applied successfully unless the theory itself is in good working order. On the other hand, I see realists such as Hanson framing a different sense of 'explanation' wherein it is conceived as part of a meta-theoretical evaluation: How 'organically' do the theoretical concepts fit together to produce correct empirical results? How 'naturally' do the concepts derive from the wider extra-theoretical language? The term *explanation* (and related concepts), as Hanson used it, applies *especially* to incomplete theories, and so stands in stark contrast to van Fraassen's use of the same term.

The moral of the story from van Fraassen's analysis and critique of realist accounts of explanation is that an explanation cannot be untethered from a theory, for when it is, on van Fraassen's view, it becomes meaningless and leads to absurdities. However, he has yet to provide a constructive empiricist account of explanation, one that slakes the methodological, realist-like ‘desire’ (156)[60] to give and receive explanations, while still staying true to the constructive empiricist articles of faith.

[60] van Fraassen writes: “So, scientific explanation…is a use of science to satisfy certain of our desires; and these desires are quite specific in a specific context, but they are always desires for descriptive information.” He suggests that explanations are eliminable and replaceable by descriptions, but our (irrational though pragmatic) desires demand descriptions be served-up in the form of explanations.

van Fraassen's Positive Account of Explanation

van Fraassen begins his positive account of explanation with a motivating example of the extinction of the Irish Elk. Its disappearance was the effect of an entire net of causal factors, yet the one we find salient is its evolutionary development of unwieldy antlers, good for mating but bad for survival (a story I will tell my son when he becomes a teenager). van Fraassen also gives the example from Hanson of the poor fellow who met a singular demise, yet who received a great number of different explanations for his death (from the medical examiner, carriage mechanic, road engineer, etc.). Hanson says: There are as many causes of death as there are explanations (1958:54). van Fraassen takes Hanson to be asserting that, in order to locate the salient factors, look not to phenomena, but to the pragmatics of language, since explanations are linguistic artifacts whose meanings are determined by pragmatic factors.[61] Which factors become salient in answering the question 'why did this man perish?' will depend on who is asking the question and under what circumstances.

In van Fraassen's view, an explanation is, generally speaking, a certain sort of answer to a certain kind of why-question, guided by language, theory, context, and facts.[62] van Fraassen's account builds on the interrogative logic of Belnap and Hintikka. Consider the why-question: "Why did Adam eat the apple?" In order to answer this question correctly, we must first understand the precise sense of the question. The first

[61] I disagree this is Hanson's point.

62 van Fraassen's formal account of explanation is more general than just for causal explanations and should, he promises account for explanatory asymmetry, relevance, rejection, and salience.

component of this understanding is the *topic* which van Fraassen defines as: that which expresses the fact of the question. It would seem, in this case, that the topic is just the fact of Adam's eating the apple. However, depending on the context, there are numerous possible ways of reading the question, each one producing a different fact: Why Adam vs. Steve? Why eat vs. throw? Why apple vs. pear, and so on. – So, the *topic* will be just the one particular fact corresponding to the one particular reading of the question under the particular context, and the remaining facts for the other readings will now form the *contrast class*.

Having made these clarifications, what's needed now is a constraint on possible answers. This is the *relevance relation* (*R*), what van Fraassen describes as 'the respect in which the question is asked.' [63]

Explanations require that: (i) the question topic is true, (ii) all the contrast-class topics are false, and (iii) there exists at least one proposition which bears relation *R* to the topic/contrast-class pair. Thus, one feature of explanatory relevance important to van Fraassen, the rejection of why-questions, has clear conditions: If any of (i)-(iii) fail to obtain, then there does not exist a direct answer, and so, the proper response to such a request for explanation is rejection in the form of a corrective answer (correcting the falsified presupposition).

63 For the formal version of van Fraassen's explanationist system, please see Appendix II, Note 5.

The mystery of explanatory asymmetries[64] is handled by contrast-class and contextual relevance, and van Fraassen shows this by the following test: A change of contextual relevance (given a contrast-class) should produce a reversal in asymmetry. In an update on Aristotle's lantern example, van Fraassen has us imagine a father asking his son, “Why is the porch light on?” If the son answers: “Because the switch is permitting electricity to flow through the wiring to the bulb,” we might consider it impudent (where we have in mind an answer like “Because we are expecting company,”), but if the context is that father and son are doing electrical work on the house, the son's explanation would be correct (131). That is, whether *X explains the light's being on* or *the light's being on explains X*, depends on the specific sense of the question (in contrast to other readings) and the contextual relevance of X to the light or *vice versa*.

So far, van Fraassen's why-question logic does not have an evaluative component. van Fraassen introduces his account of what constitutes a 'telling answer' (i.e., a good explanation): the answer (a) must be probable in light of our background knowledge; (b) must probabilistically favor the topic over the other alternatives of the contrast-class (relative to background knowledge); and (c) must be comparatively better in these regards than other potential answers. Though he admits that the evaluative portion of his theory is least developed, van Fraassen nevertheless feels confident that, on the whole, the central issues of asymmetries and explanatory relevance have been dealt with, and an account of explanation has been given which meets the demands of constructive empiricism while yet being faithful to the aims of science. In the respects important to

[64] E.g., the flagpole explains the shadow, but the shadow does not explain the flagpole. Previous accounts of explanation had trouble showing why the shadow does not explain the flagpole.

him, van Fraassen has laid the groundwork for the antirealist to be able to immerse in the theory like a realist while yet knowing all along that immersion is being used strictly for pragmatic ends. Unlike other antirealists, the constructive empiricist can, with a guiltless heart and with good grounds, choose to prefer a causal explanation over a non-causal one, so long as the causal explanation meets these criteria of a 'telling answer' better than the rival non-causal explanation.

Some Comments on van Fraassen's Explanationist Account

Has van Fraassen really accomplished anything here? He has made it possible to accommodate the antirealist wishing to indulge causal explanations, but I note that nothing *compels* the use of causal explanation. A close reading of van Fraassen's account makes clear that bearing the marks of causal character is by itself irrelevant to an explanation's being 'telling.' Quite misleadingly, van Fraassen gives an example (112), much earlier in the chapter, of the Plains Indians, which appears to suggest that the causal explanation (whites killed-off the buffalo) of their ending up on reservations is intrinsically a better explanation than the citing of mere statistical facts (technologically superior invading peoples tend to displace indigenous peoples). But, by the end of the chapter, it is clear that the statistical explanation could equally trump the causal one, so long as van Fraassen's criteria are satisfied. So, with respect the why-question, 'Why did the Plains Indians end up on reservations?' suppose we have a *statistical* theory T (together with background knowledge K), a contrast class which lists the various alternatives we might be asking after (Plains Indians vs. Cherokee, reservations vs. cities, etc.) and a relevance relation *R* which obtains for the specific topic as opposed to the alternatives. If the answer "...because advanced civilizations nearly always displace less advanced ones" is true (given T&K), if it favors the topic vs. the alternatives, and if it is the best answer that T&K offers, then it is a *telling* explanation.

Thus, van Fraassen's earlier rejection of the statistical answer, over and against the causal one, is not because the causal one, and the satisfying story it gives is necessarily the better explanation, but rather because, where the speaker/audience *assume* a causal theory, then the causal explanation will be preferred. Even where T&K are rich enough that either a causal or a statistical explanation could be given, van Fraassen's why-question explanationist logic still perfectly allows for preference of the non-causal to the causal explanation, given that the appropriate conditions are met.

The important thing to note here is that the why-question logic only works to discriminate among contender explanations relative *TO THE SAME THEORY*. And, this is exactly as van Fraassen wants it to be. For, he does not view the request for explanation as any different in sum and substance than a request for description from a theory (as applied to some set of facts in a particular context). It is merely an exercise in theory application, executed within a question-and-answer format. If we don't like the results of the application, we have the theory to thank (or blame) for that, not the explanation (which is only a practical manifestation of the theory).

It is the inclusion of pragmatics that renders his account superior to the other reductive accounts of explanation, van Fraassen believes. However, I see his appeal to pragmatics as a back-door route to plunder the benefits of realism without having to commit to it. In short, the reason why asymmetries, relevance, and rejection are all available to van Fraassen but not to the reductive accounts is just that van Fraassen allows for the full, causal story to be made available, not because of the magic powers of pragmatics. There will be no mistake that the position of the sun caused the flagpole's shadow and not *vice versa*, e.g., because we have available to us the full causal story of all the elements and their roles with respect to one another. Pragmatics only seemed important because van Fraassen cast explanation in terms of question and answer, in terms of theory-application to specific instances (in certain contexts). But, I suggest, the power behind the explanation lies not with the clarification of the question or the re-processing of the theory to permit pragmatically-determined selection of answers. Rather, the power resides in the precise characterization of the relationships among entities and the full accounting of the natures of the entities involved, as tied to the bedrock of everything else we know or are committed to, such that, e.g., the causal asymmetry among a set of entities ‘follows as a matter of course,’ as Hanson would put it. This clearly involves more than theory-determined description. A theory may provide a context-specific description of phenomena, but if we lack the fuller story of the entities and their inter-relations and how this story connects to everything else in our corpus, we will find the description-cum-explanation unsatisfying.[65] If this is right, we will find the richer, more ‘organically-connected’ story, though with an insubstantial empirical track-record, preferable to, e.g., the statistical theory with a long track-record of empirical strength. This preference is not irrational or pragmatic, but, I contend, derives from the rational preference for a strong M-model over a weak one.[66] Indeed, pragmatics only seems like a feasible solution if we already have in hand the full causal story in the first place, where the only remaining task is to work backwards to the pragmatic proxy. But, in the absence of the rich causal story tied into the corpus, developing a pragmatic account which achieves the same results is thorny if not intractable.

So, we now ask: Is van Fraassen's victory over asymmetry due to deep insights into the pragmatics of explanation or simply due to the fact that the antirealist is now allowed to think like a realist? Hempel, Salmon, and others struggled to locate a reductive account in order to find more general principles and to *avoid* having to resort to blatant realist thinking. The realist, utilizing his fully fleshed-out theories, *never* has problems with asymmetry, even independently of the contexts of a particular application. The substance behind van Fraassen's account is not in the context, but in the relevance relation which plunders a realist theory for the specific causal relationships it discloses. Thus, if we switched away from the rich, realist theory to a purely statistical one, as we

[65] For a presentation of an additional critique of van Fraassen's explanationist account from Salmon and Kitcher, see Appendix II, Note 6.

[66] And, this implies inter-theoretic comparison of explanatory powers, where *explanation* is understood in terms of M-models. Also, this implies that we can gauge a story’s explanatory power *apart* from truth, as we do in gauging good fiction from bad fiction.

note van Fraassen's account of explanation perfectly well allows us to do, the asymmetry problems can creep right back in again.

In the case of choosing from among *theories*, under van Fraassen's account, there exists no explanation-specific framework for the relative comparisons of inter-theoretic explanations, so pursuing explanations for the purpose of theory-selection is a pronounced waste of time for all but the rarest of circumstances[67]. Stranger still to van Fraassen's account would be the pursuit of explanation in the appraisal of a single theory under review, as Galileo did in Hanson's case-study. By van Fraassen's lights, this is just topsy-turvy. Thus, van Fraassen demands the pre-existence of unproblematic, finished, well-worked theories as a prior condition on explanations when they are requested, for otherwise, a problematic theory will fail presupposition requirements, whereupon the appropriate response is to reject the request for explanation. Thank goodness Galileo and others did not subscribe to this line of thought.

If EA is the measure by which a theory is unproblematic and finished, then the kink in the plan is that no such theories are available, for by van Fraassen's own admission, EA is a never-ending quest. As I argued earlier, no positive account was given for judging the likelihood of EA. So, for constructive empiricist's explanation, the only remaining support is the pragmatic one.

[67] Circumstances that involve explanation only incidentally and *qua* pragmatic feature.

There is a point at which, one might say, pragmatic methodology and realist methodology are coincident, and whatever benefits may genuinely accrue to the one would incidentally accrue to the other. But, that is not to say both are necessarily capable of the same feats. If one is genuine and one is just imitating the other, then we must distinguish which is the impostor. So, I ask whether the pragmatics-version is just copy-catting and whether we could, after all, distinguish a difference from realism which shows it genuine in producing such golden eggs as empirically successful theories.[68] First, as pointed out above, van Fraassen's account is only such that it *accommodates* a realist-type 'immersion' in the theory, but it does not necessitate such immersion. The constructive empiricist, besides imitating the realist, also has the option to be a strict black-box functionalist. Yet, van Fraassen (in *Scientific Image* anyway) is oddly silent on this alternative. How would it go? Suppose a real black-box that tells us what phenomena to expect when we plug-in certain information. I say: 'Tell me what the per-share price of Apple stocks will be next week,' and my marvelous black-box tells me: 'On Friday of next week, at the close of market, the price will be $521.73.' Oh, how glorious that would be! Suppose this works for awhile, but after several months of successful stock picks, my black-box begins giving erroneous predictions. How do I fix it? What steps would I take? There’s not much that I could do; it is a black-box.

Even more elusive are the details about how a purely functional black-box theory is produced in the first place. As van Fraassen himself agrees (with Sellars), when he

[68] This pursuit is not intended, by itself, to show realism is correct in claiming the truth of theories or existence of unobservable entities, but rather is meant to show that pragmatics can’t replace a realist methodology.

discusses Sellars' demolition of the 'levels' picture of science, there really are no true empirical laws; everything has special exceptions: “On the level of the observable, we are liable to find only putative laws heavily subject to unwritten *ceteris paribus* qualifications...We do not really expect theories to 'save' our common everyday generalizations...” (32). This is one of several powerful arguments Sellars gives against antirealist construction of empirical or nomological generalizations from autobiographical observation. The import of Sellars’ arguments is that mere ‘curve-fitting’ of theory to phenomena, as a means to building black-box theories, is insufficient to account for all phenomena. Fruitful theories, as noted earlier, are that much more difficult to construct in this way, and as Dawid (2008) points out, van Fraassen cannot, without circularity, account for the availability of fruitful theories by claiming that they are selected-for on that basis. Hence, except for the exploitation of realist *methods* (whether correct or not) to develop empirically successful theories, the constructive empiricist appears to have no genuine alternative. If the constructive empiricist is to insist that his account provides all that realism does, but without the metaphysical commitments, then he must be able to show that it indeed supplies the promised goods, independently of adopting a realist methodology. I emphasize that I am not giving some sort of indispensability argument in favor of realism, nor am I specifically attacking the basis of constructive empiricism, but I am trying to make the case here for a differentiation of realist methodology and antirealist pragmatic methodology. My aim is to show that there is an important methodological difference that is important to a methodologically oriented account of realism.[69]

[69] A methodological-orientation is opposed to a credence-oriented account of realism.

So, here is one important difference between realist and pragmatic methods: When the pragmatic method ceases mimicking the realist one, it ceases to be workable. This is why van Fraassen, and antirealists generally, must always pretend empirically successful theories are in abundant supply, or that we or they evolve in short time-frames. Except for such easy spontaneous generation of successful theories, antirealists would then have to explain how their sterile methods should produce them, which they cannot.

van Fraassen's framing of realism and explanation is crucial to his case. For him, the only difference between the pragmaticist and the realist is the realist's declaration of belief that the theory aims at truth. Beyond that, on van Fraassen's view, the realist is letting his language be guided by the theory, appointing models, making observations, and so on, no differently than the pragmatic constructive empiricist. What more is there to a declaration of belief besides a little ceremony and pomp? And, if what remains after the declaration may equally be claimed and colonized by the constructive empiricist, then the latter has all the benefits and none of the risks. However, I have accepted none of these assumptions. Being a realist, I tried to argue in Chapter One, means following an abductive methodology and need not transpire in language or within a social context. Realists need not depend on (linguistic) explanation at all, and to the extent they would find explanations interesting would just be insofar as they reflect abductive modeling. So, I contend, contrary to van Fraassen's assumptions, the pragmaticist who behaves like a realist to the point that he is doing abduction (taking models seriously, performing experiments and discarding models on that basis, whittling the remaining pool of contender models, and so on), on my view, is therefore a realist, regardless of whether he crosses his fingers behind his back or refuses to make an official declaration. So, where constructive empiricist pragmatics and realism appear functionally equivalent, I see two possibilities: (a) the constructive empiricist just *is* a realist, because of what he is *doing*, though he will try to convincing himself otherwise, or (b) the constructive empiricist is lying in wait for the realist to utter his conclusions, then parrots those conclusions, telling himself it is merely a linguistic exercise that is being performed, not realism. However, unless constructive empiricism explains how *it* develops empirically successful theories in a way that doesn't just rely on the pre-existence of those theories and/or on an opulent background knowledge K being already in place, then it is just realism in denial.

Going the Limit: From Agnosticism to Constructive Empiricist Structuralism

In his most recent book, *Scientific Representation*, van Fraassen considerably deepens his development of constructive empiricist structuralism and its reliance on pragmatics. At the heart of this work are van Fraassen's attempts to tackle the challenge of representation that badgers other forms of structuralism. His account of representation understands it as a four-place relation: Z uses X to depict Y as F (21). For the scientific case, van Fraassen conceives of X as a representation, Z as a self-locatable person in a particular pragmatic context, Y as the target, and F as the particular aspect of interest (i.e., a predicate instantiated in Y).

As I read *Scientific Image* initially, van Fraassen presents his semantic view of theories officially and with fanfare, then by the end of the book somewhat inconspicuously pivots away from that view, collapsing it into a full-fledged pragmatic view. In *Scientific Representation,* he now heralds the preeminence of pragmatics stating: "There is no representation except in the sense that some things are used, made, or taken, to represent some things as thus or so" (2006:23). Whereas, before, the semantic view of theories left some ambiguity about the relationship between models and targets, offering room at least for an interpretation suggesting a model's inherent representational powers, now, he baldly states that representation is a capacity exclusively belonging to pragmatics. To support this view, he argues that representation *simpliciter* cannot be reduced to or defined in terms of something else, for it is a “cluster concept with multiple critical hallmarks...[and] only family resemblances among instances” (59). As with his account of explanation in *Scientific Image,* he argues that representation cannot be understood in itself or reduced, but has features that require contextual and pragmatic factors: “A representation is made with a purpose or goal in mind, governed by criteria of adequacy pertaining to that goal, which guide its means, medium, and selectivity. Hence there is even in those cases no general valid inference from *what the representation is like* to *what the represented is like* overall” (7).

van Fraassen argues that the concept of *resemblance* is important to understanding representation, but while he would allow some autonomy to the powers of one thing to resemble another, he observes that the resemblance is always selective and mixed together with other non-resembling elements and distortions that actually make the model more useful to us. Thus, it can only be in the intended use we make of the selected elements that representation occurs, for by itself, the sum-total of elements in the model do not even make a resemblance (come to "nothing" (25)). “Resemblance comes in, not when we are answering the question *What is representation*?, but rather when we address *How does this or that representation represent, and how does it succeed*?” (33). Thus, despite whatever degree of resemblance we may think belongs to a photograph or charts, they contain, in themselves, no representational powers, no meaning, unless and until we provide a context, a purpose, or other pragmatic parameters. Where we change the pragmatic parameters, the representations and meanings change along with them, while the physical objects themselves remain the same.

This heavy commitment to pragmatics therefore (a) rules out "the notion of mental images or mental representations, whether taken to be brain-states or something more ephemeral — for no such things, if they exist at all, are used or put to use, or taken in one way or another" (24), and (b) rules out "‘representation in nature’, in the sense of ‘naturally produced representations that have nothing to do with conscious or cognitive activity or communication" (*ibid*). Representations must be publicly traded commodities to be representations at all.[70]

van Fraassen argues for the role of representationality to be greatly enlarged, to contain even observation. Earlier, in *Scientific Image*, van Fraassen had distinguished observable phenomena (that which is in-principle observable) from *appearances* (that

[70] Obviously, this is another direct contradiction of my view

which actually is observed, taking the form in science of measurement and observation reports). In *Scientific Representation*, van Fraassen now elevates the role of measurement in science and adds representationality to its status, while fully subsuming observation into a function of pragmatics.

van Fraassen maintains that all observations, but particularly measurements, are perspectival. Perspectivity, like representationality, is an elusive concept, and it similarly cannot subsist in itself, but requires a pragmatic underpinning, especially indexical information. van Fraassen's prime example of perspectivity is cartography. A map, like a photograph, is nothing unless put to use as such. But, particularly in the case of a map, even assuming as unproblematic the map-supplied representational information of terrain markings, etc. and even assuming a 'you are here' pointer, an additional, essential piece of information that cannot be found in the map itself, *viz.* the information supplied by a user in the form of a self-ascriptive indexical (such as: 'I am here now').

What holds true of the map and user also holds true for theory and user. While theories and their models are 'officially' not perspectival descriptions (86), they amount to nothing until put to use, and application entails perspectivity. In science, perspectivity means measurement, and measurement is “...science’s main initial access to the phenomena” (87). Measurement yields the perspectival information that selectively represents the target, but at a carefully placed level of abstraction. Measurement does not show us what the target is like (actually), “but only what it 'looks like' in that measurement setup” (175). The data of the measurement are taken in an interaction within the phenomenal world, but are represented in a *logical space*, a representation initially taking the form of a *data model*.

The so-called *logical space* in which measurement representations occur is what might be called by others a conceptual-space or conceptual-framework. van Fraassen gives as example the warmth of the room, whereby a thermometer reading is a representation in one sort of logical space, while mean kinetic energy is a different measurement representation in a different logical space. Relative to a theory, these could be folded into a greater logical space and viewed as different perspectives on the same thing. The data model is further massaged ("'smooths' - in fact, 'idealizes'" (167)) into a *surface model*, which takes some liberties afforded by statistical methods. The surface model, in principle, can now be compared with the empirical substructure of the theoretical models, and if a match is found, embedded in the particular theoretical model (and if not found, the model is rejected).[71] van Fraassen writes: "So there is a 'matching' of structures involved; but is a 'matching' of two mathematical structures, namely the theoretical model and the data model" (6). And, the data/surface models connect the theory to phenomena by way of pragmatically enabled representation. Thus, the theory is the ultimate logical space within which these different representations can find a home with respect to one another and with respect to other things.

[71] I say 'in principle' because of the reductive pragmatic treatment given to such models (next paragraph)

van Fraassen is leading up to his constructive empiricist account of structuralism which must make sense of the connection between abstract mathematical statements and phenomena.

To that end, van Fraassen considers the riddle of Reichenbach's *coordination problem*, which is the problem of linking-up the objects of knowledge in mathematics with the objects of knowledge in physics. The theorems of mathematics are epistemically grounded on their internal coherence, while the truth of statements of physics is dependent on something external. Reichenbach argued we cannot just correlate an item of the one set with an item of the other, because, to do this properly, the items must themselves first be defined, yet we find that definitions presuppose the very theories whose elements we are trying to define in the first place. Reichenbach settled on the idea that the 'real' objects are in-themselves undefined, whereas the mathematical/theoretical items are uniquely defined by virtue of the system in which they participate, and so, physical objects (or perceptual objects) receive definition (at least initially) by being subsumed under the defining powers of the theoretical system in coordination with those theoretically defined elements. However, Reichenbach adds that, while the physical (perceptual) lack definition apart from the theory, it nevertheless constrains the theory in important ways so as to prevent willy-nilly correlations leading to absurd results.

van Fraassen takes the riddle of the coordination problem to be "How can an abstract entity, such as a mathematical space, represent something that is not abstract, something in nature?" (2006:2; also 1980:240). There is a shift from 'coordination' to 'representation,' but van Fraassen defends this, saying 'coordination' carries some baggage of mathematical ideas that might lull us (as he claims it did Reichenbach) not just into wrongly framing the actual problem in terms of mathematics itself, but in assuming a "presuppositionless starting point" (137). "But that makes sense only if the question of just what coordination can be, between something abstract and something concrete, has already been settled" (2). As he goes on to explain, if one (e.g. a realist) thinks of coordination in terms of a functional-mapping-to-the-world, then this requires we identify the domain, the range, and the coordinating relation between them. But, "if the target is not a mathematical object, then we do not have a well-defined range for the function" (3).

van Fraassen imagines a realist trying to defend the functional-mapping-to-the-world idea, by conceiving of the range of the physical system (say, a thunderstorm) as a set of parts in relation to one another; but, this is still to use the mathematical to relate to mathematics. If the realist says that, because the physical thing is real, the set-designated parts are real, and so, it is these real parts that get embedded in (or are isomorphically or homomorphically mappable into) a mathematical structure, then van Fraassen responds that this is fine except for the erroneous assumption of uniqueness, for we could have divided and grouped any number of different ways.

van Fraassen concludes Reichenbach made the fundamental mistake (and became 'puzzled' as a result) of neglecting context in his framing of the problem: "...how a specific mathematical object can be used to represent specific phenomena makes sense only in a context in which some description of the latter is at hand" (5).[72] Reichenbach misguidedly sought out theory-neutral, 'empiricistically hygienic' (6) description (a 'naïve' view, says van Fraassen), as if we could know the world directly and outside of context (and language and theory), which is to ignore the key fact that "there is no independent epistemic access to the parameters to be measured—no access independent of measurement" (138).

In fact, pragmatic conditions on representation (and so coordination) go further than mere necessity, but become sufficient (assuming the obvious qualifications): "...we can use any suitable entity, abstract or concrete, to represent something else, and represent it as thus or so, but only provided we [already] have a pertinent description of both items. The description must be in our own language, in our language-in-use" (5). So, by the prodigious powers of pragmatism, it would seem anything can be made to represent, that theory and world are successfully linked (in whatever ways pragmatic concerns deem permissible), and having re-assigned all problems that formerly puzzled great minds to pragmatism, there remains precious little else to do!

Finally, and important to present concerns, van Fraassen (giving a 'mea culpa') concedes that his initial presentation of the semantic view of theories in *Scientific Image* appeared to suggest just such a (metaphysically-oriented) position as the functional mapping one: "For empirical adequacy uses unquestioningly the idea that concrete observable entities (the appearances or phenomena) can be isomorphic to abstract ones

[72] Note that van Fraassen uses 'used' in the quote.

(substructures of models)” (3). Now, it should be clear what was meant (at the time or *ex post facto*), for “If we try to check a claim of empirical adequacy, then we will compare one representation or description with another, namely, the theoretical model and the data model” (8). There can be no such thing as a context-free checking for a match of the phenomena directly with the theory:

> ...the phenomenon, what it is like, taken by itself, does not determine which structures are data models for it. That depends on our selective attention to the phenomenon, and our decisions in attending to certain aspects, to represent them in certain ways and to a certain extent. (7)

Similarly, there is no context-free checking the representation of phenomena for a match with the theory:

> *There is nothing in an abstract structure itself that can determine that it is the relevant data model, to be matched by the theory.* A particular data model is relevant because it was constructed on the basis of results gathered in a certain way, selected by specific criteria of relevance, on certain occasions, in a practical experimental or observational setting, designed for that purpose. (253) [italics in the original]

Thus, for the empiricist, 'the theory is adequate to the phenomena' and 'the theory is adequate to the phenomena as represented' turn out to be the same.[73]

Now, the ground is properly set for van Fraassen's constructive empiricist version of structuralism (denote this CES). In *Scientific Representation*, he has taken

73 I have omitted van Fraassen's argument from history: "how can such coordinating definitions be meaningfully introduced except in a historical context where there are some prior coordinatings already in place? I submit that they cannot" (121).

structuralism into the core of his argument nexus to the point it supplants the widely received interpretation of his earlier views presented in SI. About this perceived shift in van Fraassen's fundamental view, Giere (2009) writes: “So, regarding unobservables, he was [in *Scientific Image*] a semantic realist but an epistemological agnostic,” whereas now, in *Scientific Representation*, van Fraassen has “abandoned standard semantics for a usage based view of scientific representation” whereby he is free to abandon agnosticism as well, with the result that “Empiricist structuralism is closer to skepticism than agnosticism” (2009:9). van Fraassen gives the essence of his structuralism in two points: “(1) Science represents the empirical phenomena as embeddable in certain abstract structures (theoretical models); (2) those abstract structures are describable only up to structural isomorphism” (238).

Point (1) issues directly from the considerations just discussed. Point (2) places an in-principle upper-bound on the resolving power of structuralist theories: “...mathematical structures, as Weyl so emphatically pointed out, are not distinguished beyond isomorphism—to know the structure of a mathematical object is to know all there is to know” (238). In other words, if two representations of the world (which we may, on a qualitative consideration, regard as inequivalent) nevertheless both equally fit a particular mathematical theoretical model, then that model declares them two different instances of the same thing. Giere gives, as an example of this, vibrations in a diatomic gas molecule versus vibrations in electromagnetic radiation such as visible light. Whereas we might regard these as distinctly different, the mathematical structure treats both as instances of harmonic motion, and “that is as far as our theoretical knowledge can go”

(2009:9). So, whereas Worrall would hold that the same equations could embrace both Fresnel's model of light and Maxwell's model of light but yet the models remain importantly differentiable, van Fraassen seems to be arguing that *all* theories are *essentially* structural and, moreover, *ought not* distinguish among equally embeddable though intensionally different measure-representations (except insofar as pragmatic factors would overrule).

I read Worrall as encouraging non-structural theories on the one hand while holding firm to the structure as a divining rod (so to speak) pointing us towards the truth. van Fraassen is boldly *discouraging* concrete theories and viewing the structural replacement as itself not even a *possible* world-representation but simply as a 'vehicle' for measurement-representations. To accommodate historically residual concrete theories within these assertions, van Fraassen views concrete theories as *pis aller*, their saving grace providing serviceable outputs for getting around in the world to those unlucky enough to be stuck with nothing better: "...the stories about nature, about what things are like, which spell out a way the world could possibly be like for such a theory to be true, take on a lesser role. They allow us to move around in the theory, to exercise the imagination, even to get to the point intellectually where we can draw qualitative consequences via the theory without actual calculation" (238). Notice the shift in van Fraassen's language here. Whereas, in *Scientific Image*, under the semantic view, theories are *identified* with those 'stories' (*qua* semantic models) in a way that aimed at preserving much of the semantic and epistemological content, by contrast, in *Scientific Representation* the 'stories' are fully eliminable, along with the semantic and epistemological content, where the genuine (or ideal) theory is now an abstract mathematical structure.

In the final chapters of his book, van Fraassen cements his commitment to structuralism and hardens his view that structural theories, without any 'internal' content (that is, without a story about unobservables etc.), manifests the enlightened goal of science. Because van Fraassen's structuralism depends so heavily on pragmatic factors, there is the obvious danger to his position that pragmatic factors, specifically propositions expressing indexicality, become themselves the subject of scientific representations and theories (i.e. become 'naturalized'), but van Fraassen strongly insists (261) such facts remain in-principle beyond the scope of science, since theories can only ever describe and there is an in-principle difference between describing a capacity and having the capacity.

van Fraassen's many examples seem designed to drive home the point that a great gulf hangs between the initial interaction with (phenomenal) things, through many intermediary steps and modifications, to the decision about what the data should be taken to 'represent,' and then, a further gulf, across further steps, from that point to the theory. If the theory is not structural, it ought to be, and so embedding is exchanged for empirical success without wanting or needing a realist tale of unobservables. van Fraassen uses every opportunity to argue for the contextual dependence of various key operations (measuring, representing, etc.), thereby further removing the sense that science is grasping at reality, and replacing it instead with the sense science is just supplying pragmatically determined outputs to certain highly contextualized inputs. It does not seem to occur to van Fraassen to defend this pragmatic basis as he charges realists must defend their basis. I see him sawing furiously, if elegantly, to cleave the bough from the tree, not realizing he is sitting on the doomed part. For, these very same gulfs that have been argued for also exist between the objects of pragmatics and the agent seeking to use them. Moreover, having blocked (for good reason) the trespass of science into pragmatic factors, it leaves him with substantially fewer resources to defend these factors.

Concluding Comments on van Fraassen

In *Scientific Image*, van Fraassen treated theories as models with essential representational powers. He there argues that the antirealist should defend antirealism by applying a clear epistemological line in the sand past which it is not acceptable to make commitments or to allow belief. In this scheme, the antirealist could still fully immerse in the theory and milk all the benefits of doing so but should understand it as a pragmatic exercise. I argued that van Fraassen's strategy of adopting the vehicle of realism but striving to avoid commitment by avoiding the mental acknowledgments just amounts to realism anyway. Once we clarify a true antirealist path, we see it is sterile. An antirealism that little more than parrots realism might just as well be called 'realism,' especially if realism is understood in terms of methodology not legal declarations.

In *Scientific Representation*, however, van Fraassen has tilted towards a more radical antirealism which now seeks to remove all realist trappings and to shift the notion of representation away from a capacity essential to the model and toward a purely pragmatic operation, where the representational is just an effect of context and convention. Whereas before, with an essentially representing model, one could adopt an agnostic stance and say 'maybe the model is telling us correctly about the trans-observational part of the world, but we must refrain from believing so,' now van Fraassen is arguing that our epistemological stance is irrelevant because there are only the pragmatic exercises of taking measures, building data models, embedding these in abstract mathematical structures and only up to isomorphism (which cannot distinguish intensionally different models), and evaluating the pragmatic results. At bottom, the actual world (whether observable or not) is being removed from consideration. This is van Fraassen's solution to Reichenbach's coordination problem (which, for Reichenbach, was the problem of how to bridge the mathematical and the actual world). For van Fraassen, the target of representation is not the world, but just another mathematical object (the surface model). This way of picturing things appears to solve many problems at once and yields a view of science in which scientists are not really investigating the nature of the world, but merely devising mathematical objects and plugging these into mathematical structures.

However, we can ask whether the world can so successfully be sidestepped in this way that the antirealist should never have to get his hands dirty. The central problems with van Fraassen's newer view are (i) rather than opting to mimic the realist, now he opts for the sterile method, which only works until it encounters the first bump in the road; and, (ii) all the eggs have been placed into one basket, *viz*. pragmatics, and, as I argued in the section that rejects the priority of the use-meaning thesis, pragmatics must be underwritten by realism. Whereas, in *Scientific Image*, van Fraassen's strategy was to divide the observable from that which is pragmatically determined (such that his account contained at least one set of things completely free of epistemologically worry), in *Scientific Representation*, his strategy is to render all of science into a pragmatic activity. He emphatically burns all bridges to an context-independent knowledge of the world, saying, "There is no independent epistemic access to the parameters to be measured—no access independent of measurement" (138).

Yet, if there is, now, nothing immune from epistemic worry and if all activity, that a realist would want to describe as 'coming to know what the world is really like,' is just so much qualitatively empty, pragmatic, numerical procedures, then where is the guarantee that pragmatic conditions indeed obtain in order to warrant the particular procedure? Such a guarantee would have to make appeal to actual circumstances in the world (the circumstances that comprise contexts). Yet, wouldn't this, by van Fraassen's account, require appeal to further contexts, in order to take those measurements and produce those data-models, and so on, required to establish the first round of contexts? As well, distinguishing that set of features of the world which comprise the context from those features, mixed together with the first set, which do not, also requires a context. This leads to regress. The problem is only exacerbated by the involvement of conventions, and other determiners of pragmatic value, that require a voluminous and enormously nuanced account of the world. Since no part of the new account sets aside a special reserve of knowledge of the world that may be taken for granted, van Fraassen's new account falters. Yet, if there is a way, outside of context, to know the contexts, then that same way may be utilized for knowing other things outside the context. This undermines van Fraassen's new account. Finally, as already stated, the new view, unless it can show a methodological pathway to empirically fruitfully successful theories, will come to a sterile dead-end. It is not enough to declare a dividing line between the context of justification and the context of discovery, for I assert, a key component of the meta-justification of the justification is precisely demonstrating its capacity to bear fruit. That is, if an inferential method does not yield mostly successful results, then it is legitimate to doubt the method.

CONCLUSION

My plan for this paper, in defending realism, could not be simpler or less original. Determine the fundamental vulnerabilities of antirealism and the basic lines of attack deployed against realism, and then, formulate an account of realism which both counterattacks the antirealist vulnerabilities and shields against the antirealist lines of attack. Where successful, this is the way of philosophical progress.

The fundamental vulnerabilities of antirealism, or almost any view for that matter, are found in its undefended assumptions. On my analysis, antirealists generally assume that language and direct-realism may be taken as givens. The strategy for many contemporary antirealists has been, in one form or another, to argue that realist claims about certain classes of purported things in the world are best understood as mere functions of language in concert with simple observation. Since the latter are taken by the antirealist as givens, then the judgments based on these are taken to be sound ones.

The main lines of attack made against realism have targeted either the grounds for its claims or the very medium of its claims (i.e. the language of realism). The most gentle of attacks only pitch realism as insecure. The most severe of attacks portrays the realist's claims as incoherent. Antirealists then argue their alternative is the better one, being neither risky nor unintelligible. Underdetermination has been the weapon of choice for antirealists, whether used explicitly or implicitly, in undermining realist's grounds for thinking claims of unobservables even approximately or probably true.

Naturally, the shrewd realist should respond by constructing a view of realism that challenges the antirealist givens and that presents realist claims and the grounds for those claims in terms that cannot be opposed by the antirealist without undermining antirealism as well. These grounds should plainly confront underdetermination.

The view of realism I have formulated requires that we begin at 'epistemic ground-zero,' an epistemic starting-place described by orthodox scientific theories which hold that humans are so biologically configured that our sensory output is, in fact, amorphous, that our minds lack direct access to the world, and that we are not born knowing a language or any other facts about the world. As a result, direct-observation and language are ruled-out as givens. I further argue that, in ascending from ground-zero to an epistemic position from which we may infer basic facts about the world, we must utilize a non-linguistic means of forming representations of the world and must appeal to abduction in justifying a representation as more or less correct.[74] Achieving this for even the simplest facts, of the sort even antirealists would require, means finding a route to successfully coping with underdetermination. This is accomplished by a combination of (i) parliamentary agreement with antirealists of minimal grounds for making possible science in the face of global skepticism, (ii) crafting a position of modesty and methodological emphasis for realist progress, and (iii) viewing success in 'cornering the

[74] I have phrased ('infer,' 'utilize,' 'appeal,' etc.) this description of ascent from ground-zero as though the agent were consciously in control of the process, but I wish to emphasize that the process could be automatic. Perhaps, it works better with a conscious and self-conscious agent.

truth' in terms of a diminishing pool of remaining contender theories, rather than in approximate truth in terms of the relationship of a current theory to the world.[75]

Kuhn's antirealist account is heavily invested in the 'given' of language as well as in unwarranted assumptions about the nature of language, the priority of language, the epistemology of language, and the agent-world-language relationship. Kuhn's attacks on realists are in terms of these very assumptions. Underdetermination is an element of this attack, especially in the way his account implies realists cannot discern investigating the nature of the world from fitting phenomena into the conceptual boxes of the socio-linguistic construction. Because of this overdependence on language, kicking out the pillar of support for his thesis also removes the force behind his offensive. I argued that the nature of language is such that it is not monolithic but stratified, that language cannot have priority since we must know the world sufficiently before acquiring a language in order to acquire it, that scientific language is just one influence (and a weak one) among many shaping our observation of the world, to list a few of the arguments aimed at Kuhn's central vulnerability. I agree with Kuhn that agents have no direct access to the world and that all we have is a construction; however, I disagree with Kuhn in that I hold this construction is fundamentally non-linguistic and that we have grounds for progress towards a correct view of the world.

Hanson is also heavily invested in language and makes considerable assumptions, like Kuhn, about the nature, priority, epistemology, and agent-world-language relation of

[75] Though, we can make a judgment (and find some degree of confidence) about the current theory with respect to the pool.

language. I defend many of Hanson's assumptions as more reasonable than Kuhn's. Hanson's account of stratified, modular conceptual systems, of theoretical progress, and of theory-ladenness are very much in league with my own view. However, the central dependence on language is ultimately Hanson's undoing. His main argument for realism is an indispensability argument: the very forces of language which pattern phenomena in concert with the other patterns and logics of the rest of the linguistic strata also compel us to accept-as-true whichever empirically successful theory is under current consideration. However, as antirealists have noted, we may simply view this sort of accepting-as-true as a function of language, rather than as a judgment about the world. Further, Hanson does not provide a prophylactic against underdetermination. I hope my own account has captured some of the best parts of Hanson's account but has avoided its pitfalls.

I tried to divide van Fraassen's general account into two different sorts of account, one that avoids dependence on language, in appealing to the semantic view of theories, and one that is heavily dependent on language, in appealing to pragmatics. The first account, as read straightforwardly and considered somewhat independently from van Fraassen's wider view, depends heavily on direct-realism and is specially designed to exploit the weakness of realism to underdetermination. van Fraassen argues we are able to check the match of the empirical substructures of models to our observations of the world, but we are not able to check the match of the internal structures of models to the world, which underdetermination guarantees will be infinitely many. van Fraassen's supporting arguments for his semantic view of theories was intended to bear out this view, as well as to supply an antirealist license for science to continue operating just as it does, even with scientists acting and speaking just like realists. However, I countered that this dependence on the 'given' of direct-realism, and a closer inspection of van Fraassen's semantic view of theories itself, reveal an antirealist vulnerability to underdetermination, a lacuna for practical application, a means for eliminating EE contender theories (that helps the realist cope with underdetermination), and a resultant methodological approach that coerces the antirealist into realism.

van Fraassen's language-dependent view is seen in his accounts of explanation and constructive empiricist structuralism. In his explanationist account, he advocates pursuit of causal, realist-like concrete theories, while his structuralist account leads him to advocate that those realist-like theories be avoided in favor of abstract mathematical ones. Nevertheless, his central argument is that the key operations involved in scientific judgments (explanation, representing, measuring, and so on) are fundamentally pragmatic operations, essentially dependent on context, indexicals, and other pragmatic parameters. These judgments are therefore pragmatic ones. Since those who utilize pragmatics are merely carrying out linguistic operations, realist aims are therefore precluded (or deflated). I noted two general liabilities of van Fraassen's appeal to pragmatics. First, if the constructive empiricist is doing something other than merely mimicking realism, then, not being required to use realist theories, the constructive empiricist should have an independent means to carry out science that works just as well. I argue he does not, and so, he offers no real alternative to realist methodology. If, as I try to argue, those who pursue realist methods just are (despite beliefs) realists, then the constructive empiricist is a realist. The second general liability is in accounting for the epistemological and even metaphysical requirements for carrying out pragmatic operations themselves. If the pragmaticist must appeal to realism to make possible pragmatics, then that undermines the constructive empiricist goals. In other words, I challenged the central 'given' of language for this part of van Fraassen's view.

For each of the philosophers studied in this paper, I have tried to show that the dependence on a 'given' of direct-realism or language always ends badly. I also attempted to uncover the fatal flaws in the antirealist attacks on realism. I have not fully demonstrated my own view, but I hope I have at least shown it does not suffer the inadequacies of the other views I have considered and that it holds promise for future research.

APPENDIX I

Note 1

A More Careful Illustration of Abduction Under Epistemic Restrictions

I emphasize that this illustration is merely figurative and that abduction may proceed by other routes. On the one hand, in Chapter One, I said that abduction is guessing about the state-of-affairs in an entirely different domain (the target domain) by way of indirect information composed of elements of the known (M-model) domain; then, on the other hand, I gave examples (toy in a giftbox, Wright brothers) where nothing at all is hidden or mysterious about the target (except perhaps that it doesn't materialize until some future point), where resemblance may be vindicated (ultimately) by an easy look-see. So, to clarify things, I will here sketch a more proper case of abduction, though, full disclosure, I am hardly equipped nor have I even begun to figure out the full details of abductive logic; this is just an attempt to make an attempt. As well, I make boatloads of assumptions. But, as the account develops (in this paper and beyond), these assumptions can find justification or be eliminated. We have to start somewhere.

So, let us assume a set of blip and bleep values (*b1, b2,...,bn*) delivered by a set of *n* vectors D1, D2,...,D*n*.[76] This makes an *n*-tuple of possible values. Let's say a particular assignment of values to each element of the *n*-tuple makes one *pixel.* We can imagine the *n* vectors generating an *n*-dimensional space (a possibility space for models under the

[76] 'Blips and bleeps' are the output of detectors. A detector could be a nose, eye, thermometer, etc.. Each detector's range of output makes for a vector in the space of empirical possibilities. I use 'blips and bleeps' to emphasize that there is no meaningful content assumed at epistemic ground-zero.

fixed constraints), and one coordinate in this space being a pixel. I am further assuming here that the realized values are involuntarily delivered to us, that we can faithfully recall past realized values, and other cognitive, etc., issues may be considered unproblematic for the time being. Motivationally, I'm also going to assume, for illustration, that the meta-situation is that we are building an M-model from a position concealed from the target (in the head of an animal), that the target is the physical world, that the vectors are sensory detectors, that the information is reliable, etc.. I will elect one sensor D*t* to be a time sensor (perhaps, measured in terms of strength of memory, etc.). Finally, I designate a set of *actions* (A1,...,A*n*) available to the agent. In terms of the meta-situation, this might correspond to things like 'turning head left,' 'reaching out,' etc.. But, for the homuncular model-builder, it will not be known what these actions or detectors actually are doing/detecting. We can imagine him having simply a row of buttons A1, A2, etc., and a set of outputting devices D1, D2, etc., and a grid that lights up. This model-builder, then, is a little like the guy in Searle's Chinese Room.

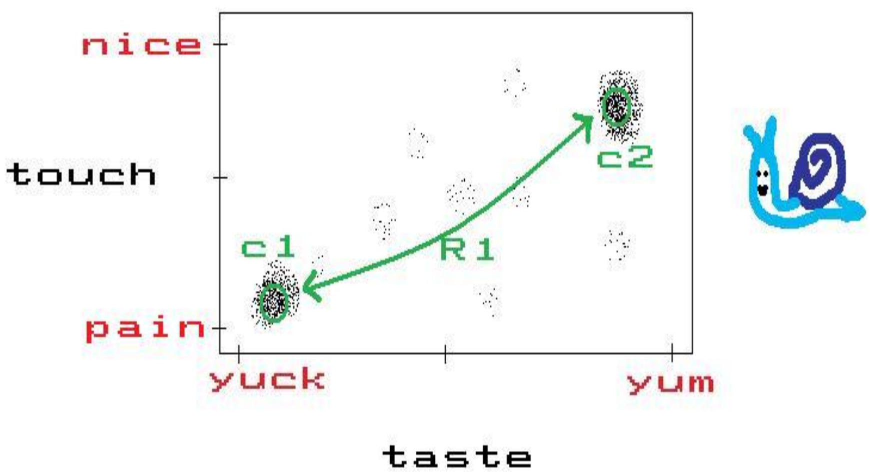
Let us imagine, next, that data streams in from the detectors and lights up the grid. There will be rough clusters of pixel activity that appear time after time (owing to the generous assumptions of the example). The model-builder draws lines around the clusters (i.e. classing certain bunches of *n*-tuple values) and remembers these as nodes or concepts or *objects* (c1, c2,..., c*n*). Next, he draws relational lines combinatorially connecting all the objects, together with the action-states, as for example the triple R1 = <c1,c3,<A1,..,An>> or 4-tuple R2 = <c1,c3,c4,<A1,...,An>> etc.. The *state* S of the model would be the n-tuple of all relations <R1, R2,..,Rn> which ultimately just amounts to a fancy classing of co-active pixels. I suggest that relations become characteristic over time, as the differences among the states take shape. The defining of such characteristics takes place in second-order models (models whose targets are first-order models). However, the first-order model is the metaphysically significant one, in that our aim is to make it resemble a worldly target.

The critic objects: “But, who's to say which collection of pixels is properly a 'cluster'? What grounds making this vs. that decision here?” And, it doubtless looks arbitrary and unmethodical. Yet, that's how it has to be done at the start. Abduction is unique, I assert, in its capacity to stipulate hypothetical categories, virtually *ex nihilo*. The methodology enters afterwards, in a way analogous to a Bayesian prior probability which will come to meet an updating procedure that hopes eventually to wash away those first priors (though, I stress, the Bayesian prior presupposes the categories).

In the second-order space, the model-builder will, e.g., look at the behavior of all *m*-tuple relations (indexing/plotting the relative *m*-tuple values with respect to one another), and in a fashion similar to how objects were defined in the first-order model, draw a circle around a cluster. It may turn out that, say, R5 and R6 exhibit very similar behavior. Or, R6 may be unique. Or, R9 may be so uncharacteristic and un-clustered (uncorrelated) that we decide to eliminate it as a contender. – Similarly, the model-builder can construct a second-order space on objects and draw similar conclusions. – The central purpose of the second-order model is regulative. We use these to help us define the 'vocabulary,' so to speak, for the first-order parts, to manage expectations on these parts.

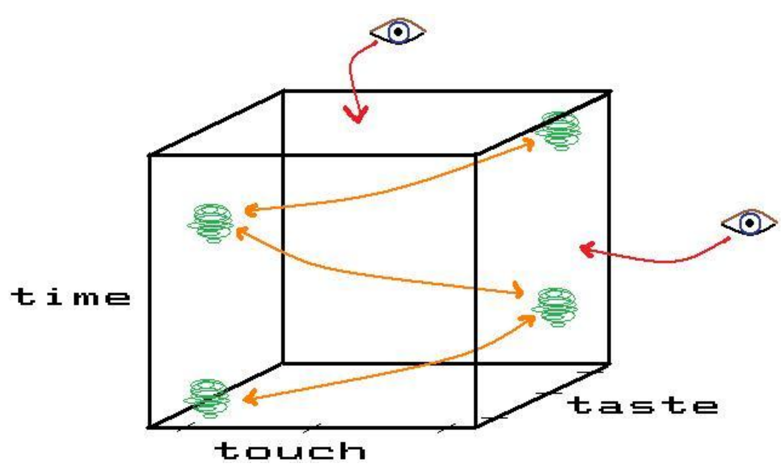
In the absence of more precise expectations, we may import and use the second-order object as a boilerplate object on the first-order model. There is no problem that I can see with this because what makes something first or second-order is the target, not the pieces. I must urge here, with special emphasis: *ONLY the first-order model is inferring to the world.* The higher-order models are *only* targeting lower-order models. From my point of view, much philosophical confusion arises from puzzling how something as abstract as physical laws should connect to the world, whereas such laws are typically and for the most part not speaking (directly) about the world at all. It follows from my position, perhaps problematically, that we can only assert *one* model of the world, whether that model includes tables and chairs or quarks. Of course, in practice, we have multitudes of models of the world, but philosophically, I view these as all being pieces of the same global-model. Where a theory or model becomes general, it ceases to be about the world any longer. Thus, van Fraassen's examples of representation in the form of generalized numerical charts and graphs strikes me as silly. These things are not resemblances of chart-like and graph-like things in the world, so of course, it will be perplexing to ponder how such things could 'represent' items in the world. In my view, such things are just models of models and say nothing (directly) about the world. Finally, I add that some models may *seem* to possess a general character in virtue of modeling some specific thing (e.g., a sort of molecule) and such a model will be asserted correct for all equally resembling individuals. But, I would argue this is not a case of generality in a strict sense (as it is possible to make a true general statement of a set of things, a statement that would be false for any element (e.g., 'the average family has 3.4 members')). In the case of models, resemblance can hold between the model and any number of equally resembling targets (e.g., a model of H_2O and every H_2O molecule in the universe).

Consider a fanciful case of abductive model-building, meant to show how, from barebones epistemological resources and epistemic ground-zero, a simple creature might come to have a representation of the world: Unbeknownst to a homuncular model-builder, he's in the clammy head of a Snedley the snail. Let's suppose two detectors D1 and D2 which are registering taste and touch (the model-builder knows none of this). This makes a simple vector-space. Let's further assume (for the meta-situation) only two items of interest in Snedley's environment: uncomfy rocks and delightful morsels of food. We can imagine his experience over some interval of time adding up to this:

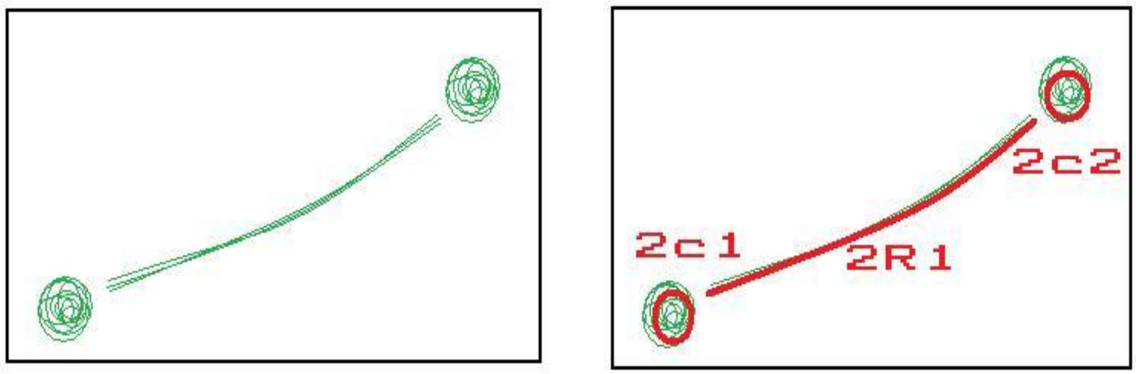


Object *c*1 is, *we* know, a rock, and *c*2 is a morsel. Let Snedley's actions be: A1= move forward tilt right; A2=move forward tilt left; A3=stop. We initially assume the relation R1 to hold between the two objects. Again, I emphasize that the homumcular model-builder knows nothing qualitative here, nothing of the target.

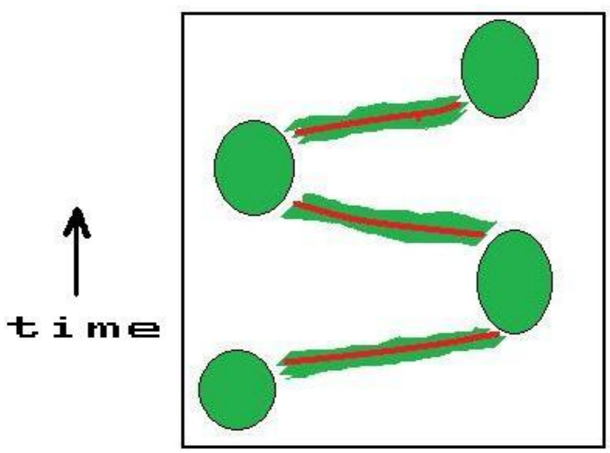
Next, we construct a second-order model which, we could imagine, is built-up as a stack of first-order models, with the *z*-axis being time, as a kind of representation of memory (I picture this as a stack of photo-transparency snapshots illuminated from the side opposite observation and observed from the top or side):



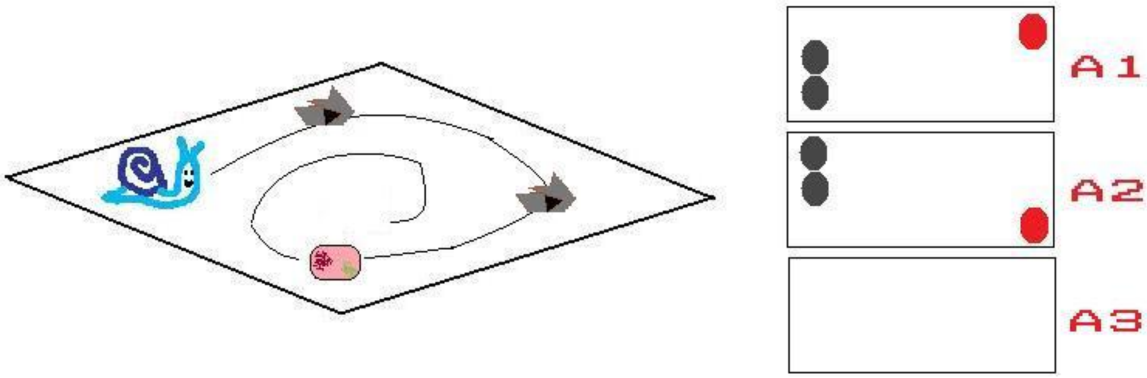
The top-down view would show a montage of superimposed clusters and relations.



We would see a kind of averaging of the objects and relations (left-side figure), but it would be an abductive decision where to draw the second-order lines representing the second-order conceptualization (right-side figure). The side view of the second-order stack would also yield some interesting subject matter for us:



Let's call this side-view the 'time view' for the sake of this example, though I stress that the second-order models could be constructed along various dimensions. I have neglected so far to include the coordinating elements of the actions A1, A2,..., which are an integral part of the relation, because this would complicate the illustration too much. But, we can consider it (in a simplified form) now:



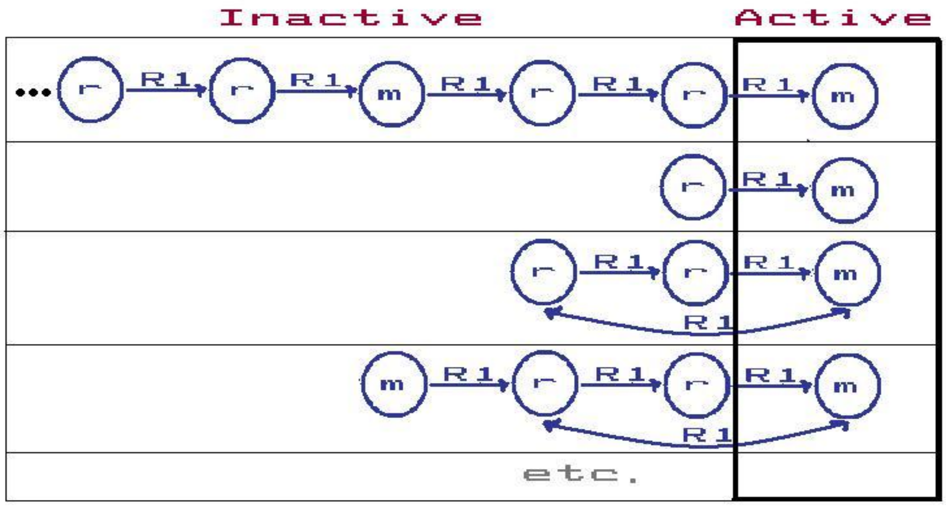
Suppose Snedley lives in a simple environment as pictured above (2 rocks, 1 morsel). If he maintains a sustained action over a period of time, then the 'time view' of the 2nd-order model would appear as shown above on the right (I represent the rocks in gray and the morsel in red). This table summarizes:

	t1	t2	t3
A1 (fwd-R)	Rock	Rock	Food
A2 (fwd-L)	Food	Rock	Rock
A3 (stop)	Nothing	Nothing	Nothing

The model-building homunculus is oblivious of all but the 'blips and bleeps' of the grid and has only the model-making tools at his disposal. In the table above, he would have *c*1, *c*2, etc. rather than 'rock' or 'food,' and R1 is just an interesting link rather than, say, a 'spatial relation.' Nevertheless, after a while, following abductive procedures, these grid-objects and relations will take characteristic shape. E.g., because the homunculus is tying the actions to the objects with respect to their interplay over time, the 2nd-order relation 2R1 will eventually germinate a little personality. Because rocks stay put and morsels tend to disappear and reappear in different places, our homunculus may come to label as R2 the relation between rock and morsel encounters, but retain R1 for the relation between rocks encounters. For the homunculus, R1, personality or not, is just R1, and even if the homunculus has his own separate knowledge of spatial, causal, temporal, etc. relations, those ideas have no applicability here.

Our homunculus should build a model such that the behavior of the model coincides with the activity of the grid. Obviously, many models would work (have empirical success) just fine here.[77] For now, let us imagine some possibilities, with *c*1 (rock-like experience) relabeled *r* and with *c*2 (morsel-like experience) relabled *m*. For simplicity, I will continue to use the single relation R1. I represent below a portion of each model outside the active grid and a portion superimposed atop the active grid. In each case, the model is matching-up with the activity of the grid:

[77] Underdetermination!



There's no reason to assume that any given model-object mapping onto experience is one-of-a-kind, one of a small number of the same kind, or one of a long series. The first model above supposes the latter. The second model supposes the former. The third model is, *we* know, the correct one for Snedley's actual environment. All such models collectively form only a subset of the total pool of possibilities, and it is certainly beyond the ken of any animal (snail or human) to know all the members of this set.

Of course, the example of Snedly the snail and his homuncular model-builder is a fairy-tale. Snails certainly operate by different means. I don't pretend to be doing molluscoid epistemology here. However, it is my hope that the morality-play of Snedly's working towards epistemological salvation is philosophically instructive. And, my approach is not entirely divorced from work being done in cognitive science and biology (not explored in this paper). I intend only that my reader should witness how, by way of philosophical reconstruction, I envision that abduction might operate under the epistemic strictures of ground-zero.

Note 2:

One imagines different sets of vectors, the output of each set orthogonal to the next, *ad infinitum*, as an old science fiction movie portrayed an endless number of distinctly different universes all sharing the same location but each existing at a different unique frequency and oblivious to one another. Certainly, treating vectors formally (D*1, D2,..., Dn,...*) and assuming an infinite supply, one may infinitely partition on the set and produce an infinite number of models, even though each cell can only produce a finite number of models. As an analogy, we could imagine each cell of the partition corresponding to one person forming opinions about his world based on his unique group of detectors (with the supply of vectors/detectors such that no amount of acquisition of additional ones would ever threaten that uniqueness). This, however, still does not settle whether there are going to be an infinite number of *sui generis* models, only that there are an infinite number of models. Yet, if only a finite number (however large) of *sui generis* models are possible, then the infinite number of repetitions is of no interest or worry. One might think it easy enough to attempt some sort of diagonalization procedure whereby each model includes a point-on-a-real-number-line, corresponding to a real number such that we could diagonalize and produce a new point-on-a-real-number-line for each new member, *ad infinitum.* But this misconstrues the representational purpose of a model (just as if one were to try a similar diagonalization on observation-reports): (i) M-model constructions must be grounded on the data, so such point-on-a-number-line chunks are disallowed unless demanded by the data; (ii) pointless (non-resembling) chunks would be eliminated.

Within the bounds of non-global-skepticism feasibility, any given finite vector-set will cap possibility to a finite number of possible models. Thus, the remaining avenue open to underdetermination would be the worst-case scenario of an infinite number of available vectors, whereby we could infinitely partition (as described above) and produce an infinite number of competitor world-models. There are independent reasons not to take seriously the worry of an actual infinity of possible vector/detectors, especially given the start-point assumptions that antirealists make about the world and our capacity to glean facts from it. Nevertheless, let's persist further in exploring the ramifications of this worry. First, it's worth mentioning that a cardinal component of the epistemic/social account of the practice and viability of science is, in terms of my view, the acquisition of new vectors/detectors over time (e.g. people-detectors). Some detectors, such as those for detecting electromagnetic energies, are regarded as treating-of a whole continuum of would-be vectors (such that a would-be infinite collection is collapsed into a single detector/vector). So, for the underdetermination worry under consideration to really bite at us, it would need to demand that the partitioned sets of vectors are neither (i) extendible into one another nor (ii) collapsible into one another, in either case in a way that would reduce the number of possibilities to a finite set. It is worth noting that antirealists, especially those who base their accounts on the assumption of sharing a common understanding (as in, a common language) among people, would regard the possibility of non-intersecting, dislocated conceptualizations of the world as an impossibility (being that linguistic concepts are public artifacts to begin with (by the views popular among antirealists)).

A second clarification has to do with the scope of aim in selecting our target. It is assumed that the universe is a fairly homogenous place within certain limits of time and space, otherwise fragility would preclude any process of empirical discovery, and then, global skepticism would win. Our goals in acquiring knowledge are modest, at least in where we place the lower threshold. Science should at least tell us what's going on in our own neighborhood. If the possible infinity of vectors/detectors is due to the possibility of there being an infinite space throughout which detectors may be distributed, or is due to there being an infinity of different levels or neighborhoods throughout which detectors would be distributed, then we defuse this particular underdetermination bomb by sticking to the modest aim of only asking our science to conquer one region at a time. It would still be a science that which only could determine the first ten levels of our local neighborhood of the universe. So, for underdetermination to proceed, it would have to insist that the possibility of infinite vectors/detectors should threaten for any scope of target(s), irrespective of the size or breadth of level or neighborhood.

Thirdly, there's another sort of modesty that realists are content with. Realism (on my account) does not insist to locate the true or even approximately true theory/model of the world or part/level/aspect of the world. Realists recognize, in despite of underdetermination, there are limits to what may be discerned. Cognitively limited animals can only do so much with the resources they have, and the human epistemic position is only somewhat better. The modest realist only aims to make substantive progress in shrinking the pool of possible contender theories, and if only so much progress can be made, then *c'est la vie*. But, progress is progress, and the realist succeeds thereby.

I add another small point of clarification. Typically, the underdetermination situation is framed (as above) as some number of theories, the data for which are equally fitting/confirming. But, under my scheme, the data of the vector-space can only be made consistent with the models formed in that very same vector space, and only a finite number of such models are possible for each set of vectors. So, for some finite set or sets of vectors, the set of possible competitor models consistent with the vector-specific data would only ever yield underdetermination in a non-threatening mode. The data for a particular vector-space is irrelevant to models of a different vector space. Underdetermination, of the worrisome sort, would have to be in terms of separate vector-spaces each producing their own separate data sets for which (and out of which) models are constructed. So, the meta-situation would be that some real event *e* is occurring; and the respective detector-sets are reporting whatever they report (which we presume reliable); and the models for each vector-space are constructed accordingly. This raises the question whether the different models are genuinely comparable, apples to apples. The argument could be made that they are not, in which case underdetermination ceases to be a threat. If such a route is pursued, then an account is begged for by which a model may be correct (or true) peculiar to its vector-space. This notion of vector-relative correctness does make a certain kind of sense. Painters will produce a resemblance of the subject in the medium of pigmented oils and canvas, and this mode of representation will be correct in a way distinct from, say, a sculptor's representation in the medium of marble. If the resemblance to the subject of each piece of art is successful, then it should also be the case that the painting will resemble the statue. By parity, for some model relative to vector set A and a different model relative to vector set B, should some way exist so that the model of A could register to B and vice versa, then it would be the case that, in principle, an agent by the lights of B infers B's model to resemble A's model, and vice versa. Of course, adding one more link in the transitive chain of resemblance, supposing A's and B's models are mental ones, a common touchstone could be provided by a public model accessible to an agent holding an A-model and a separate agent holding a B-model. At any rate, another possible avenue to quelling underdetermination is now suggested.

APPENDIX II

Note 1:

I observe that van Fraassen neglects a few details. What is the model made of? Where is it located? Who manufactures them? These questions would just provide a little comedy if van Fraassen were talking strictly about logical models, under which *anything*, real or imagined, known or unknown, occupying any universe of discourse, which does not falsify a set of statements is therefore a model. Such models are useless to science. Clearly, van Fraassen has in mind some special subset of those logical models. The domain of discourse is obviously not the world itself. The models must be known (or knowable) by the scientists who use them. – So, the questions above are not so funny anymore. What is the nature of these models? – Because van Fraassen vehemently eschews any kind of modal realism, the models are not possible words or possibilities in some ontologically similar, modal realism sense. It seems clear, models, in van Fraassen's sense, are not (at least essentially) physical things that we could keep in a bank vault. van Fraassen would not accept their being mental entities, since (a) he strongly rejects other accounts that have a great reliance on mental entities; (b) he rejects IBE, in part, because he construes it as a psychological rule, so I'd guess he wouldn't want his account of models to depend on our actually having to think of each and every one (or conclusions being drawn from what we imagine about such models); and (c) in his subsequent book *Scientific Representation* (2006), he urges that representations (appearances) and theories must be public things, that there is “no room for the notion of mental images or mental representations" (2006:24). In fact, he gives (or, one can read from the text) three sorts of (non-exclusive) characterizations of models: (1) In his initial introduction of models, he concludes with: “I will continue to use the word 'model' to refer to specific structures, in which all relevant parameters have specific values” (1980:44) (in contrast to van Fraassen's characterization of the use of models in science where certain parameters are left unspecified, and so are more properly model-types). 'Structure' is still ambiguous between mathematical structures and concrete (though not necessarily material) structures. Because van Fraassen's first example is an actual, drawn geometric figure (and to form a contrasting interpretation with my next point), I will regard this passage to indicate the concrete structure. (2) In his discussion of probability, van Fraassen applies his semantic view to solve the puzzle of precisely detailing what relative frequencies are. He says: “Such a [probability] space, and the model as a whole if it involves more, is a mathematical entity” (196). And, indeed, there are other passages, earlier, where his examples are ambiguous between mathematical and concrete objects. So, in this case, let's read this passage as indicating a mathematical object. (3) At the conclusion to the book, short of the epilogue, van Fraassen performs an astonishing feat (or shell-game) with modality. He asserts (openly acknowledging it is “at first blush inconsistent”) that “probability is a modality,” that “science includes irreducibly probabilistic theories,” and that “there is no modality in the scientific description of the world” (198). This apparent inconsistency gets resolved when modality is collapsed to a linguistic convention adopted by science for pragmatic ends. (van Fraassen is perhaps overly fond of this technique: identify a context-dependent concept in the opponent's assertion which cannot be deployed univocally; then, charge the asserted thing is therefore conventionally based). Says van Fraassen (I take the space for this quote because it encapsulates a boilerplate approach for van Fraassen, including his treatment of explanation as this chapter canvasses):

> [The correct diagnosis of the problem] is that modality appears in science only in that the language naturally used once a theory has been accepted is modal language. This relocates the problem in philosophy of language. So if anyone asks: 'What more is there to look at in science besides the models, the actual phenomena, and the relationships between them?' we can answer 'The structure of the language used in a context where a scientific theory has been accepted.' And the problem of doing justice to modality will have been solved to an empiricist's satisfaction if we can explicate the use and structure of language without concluding that anyone who does use it is committed to some sort of metaphysical beliefs such as that alternative possible worlds are real. (198)

A page earlier, van Fraassen discusses how modal realism and theoretical entity realism are more or less the same thing and dealt with in similar ways, *viz*. by understanding such realist talk as actually being about the non-empirical-substructures (aka 'internal structures') of models. – So, how does this all this add-up? On the surface, it seems safe to read van Fraassen as allowing any model to take the form of either a concrete object or a mathematical object, though he overwhelmingly favors the mathematical model for science. (1) Yet, neither of these objects, especially the mathematical one, has an empirical substructure of the sort described earlier. So, clearly, it must be the case that the mathematical object is only appointing models that *do* have the required substructures. Yet, this only brings us back to our original questions: Where are these models and what are they made of? (2) Supposing the actual models to exist in some fashion, and some theory (in some language) which these models satisfy, the next question is: What role is the actual world (observable or otherwise) to play in this picture? Typically, theory and models complete that picture, without dragging in any further players. What is the relation of model to world? Representative? van Fraassen will speak much about representation in SR, but not in this respect. In fact, he argues in SR for the (beatified) theory itself to have (or ought to have) no representational capacities at all. So, even assuming a match-up between model and world, what makes the model *about* the world? (3) Finally, we are told it is very important that at least one model have an empirical substructure isomorphic to all appearances. Why is this important, or any more important than just saying: 'We would like to have a complete accounting of every observable thing'? Why drag-in all this complex machinery if it amounts to little more than than this wished-for accounting? And, there's nothing here to indicate an incomplete accounting will produce any further match-ups. Yes, van Fraassen will argue for a frequentist basis to probability as inductive grounds on which to support judgments of regularities in phenomena, but *that* is a relationship between probabilities and the world, not model (in van Fraassen's sense) and world. – So, as we look past van Fraassen's rhetoric, making sense of the details becomes an exasperating task. And, all in vain, apparently, since it is all slated for absorption into van Fraassen's grand pragmatics scheme.

Note 2:

Does a theory's determining the limits of its own empirical substructure (i.e. what is or is not to count as empirical for that theory) introduce any serious issues, such as (i) Is it a problem that a theory should determine its own evidence?, or (ii) What role does background/collateral theory play in placing selection/other constraints on some scientific theory T under consideration?, or (iii) If each theory carries with it its own system, criteria, features for (self) appraisal, then does this commit van Fraassen to the incommensurability thesis (and, if so, is that a problem for van Fraassen)?

Note 3:

In order to tease out the interdependence among theories, let us consider more carefully a claim that T would remain EA after its extension with T'. I counter that this will only be the case under *ideal* conditions, not generally. I do not dispute the set-theoretic guarantee of EA for T as van Fraassen has characterized it, but rather, I dispute that this characterization correctly captures what it purports to. In the case of explaining the failure of EA for some statement *S*, we saw that a set of models consistent with *S*, that would be EA with respect to T, would no longer be EA if with respect to T', where T', e.g., conflicts with the internal structure of *S'*s models. Now, in this discussion about extension, van Fraassen has us focus on T, has us suppose it EA, has us notice that nothing about T is added or taken away in the attempted extension with T', and then has us conclude that, if T is unchanged, then it must still be EA (and all EE alternatives to T would remain likewise EA). We would thus naturally conclude, in the circumstance of a 'total defeat,' that we should just dispose of the interloper T' (and van Fraassen's example does not discourage this conclusion). As in all cases of magician misdirection, we are counseled to 'watch the other hand,' and in this instance, that means we should think about T', about background theory, and about the internal structures of the models involved. But, what if T' and T are both *equally* supported by the background theory such that we cannot dispose of either? We *must* make them work together (that is, if we hold a unificationist view). Holding T' fixed, if the internal structures of its models conflicts with all other EE alternatives to T, but is consistent with T, then we would be compelled to select T and eliminate all the EE alternatives to T. – This is exactly the imagined scenario that van Fraassen considers when offering an apparent counterexample to his view (1980:49). Let TN(v_i) be a version of Newtonian physics that also posits the velocity of the center of the universe is v_i , and so, under the right circumstances, EE with all the other TN(v_k). If TN(v_i) is conjoined with Maxwell's theory of electromagnetism E(0), whose internal structure dictates that two charged particles in motion will attract with absolute force F, this will render all but one of the TN(v_k) theories no longer EA. That is, by E(0), if the two particles attract with a force greater than F (*ceteris paribus*), it would follow that the center of the universe is traveling at velocity greater than 0. van Fraassen argues that we may reject this apparent counterexample once we consider that E is underdetermined, since any E(v_k) conjoined with TN(v_k) will be EE to TN(0) with E(0). – While I would agree that underdetermination is a genuine problem, it is distinct from the issue of whether a theoretical extension/conjunction can eliminate contender theories from among a series of EE alternatives. In the example, the internal structure of E(0) is such that it sweeps clean the intersection between itself and every other TN($v_i \neq 0$). By van Fraassen's own criteria, only (E(0) & TN(0)) is a victory whereas every other (E(0)&TN($v_i \neq 0$)) is a defeat. – We could have saved all this trouble had we simply extended any of the TN(v) theories to the theory satisfying: 'Any velocity other than 0 is impossible for the center of the universe.' Not a very compelling extension, but it illustrates the point (once we unmoor from the separate issue of underdetermination). – The important point here is that van Fraassen can't generalize from the example.

Note 4:

Prior Attempts at Reductive Accounts of Explanation

In building towards his own explanationist account, van Fraassen first reviews some history of the attempts by (mostly) empiricists to account for explanation by attempting to reduce to 'those features and resources of a theory that make it informative (that is, allow it to give better descriptions)' (154). Hempel defined '*A* explains *B*' as: knowing *A* gives good grounds for believing *B* (where 'giving good grounds' can mean P(*B*|*A*) is better than 50%). However, counterexamples are easy to come by. Giving good grounds does not always indicate a cause, e.g. as a falling barometer gives good grounds for an impending storm but does not explain the storm. Conversely, we can have a good explanation that does not indicate having given good grounds: P(having paresis | having latent syphilis) is a very small probability yet latent syphilis explains the paresis. – Salmon diagnosed the problem as a failure to involve *relevance*, which, when it is absent, renders a non-explanation, regardless how high the probability, as e.g. P(not getting pregnant | being a man and taking birth-control) = 100%. Salmon instead suggests identifying 'giving explanation' with 'giving sets of statistically relevant factors,' maintaining that the factors' imparting high probability is not necessarily important. However, Cartwright's famous counterexample challenges Salmon's view. Suppose we spray defoliant on a set of plants and 90% perish. It will then be the case that P(this plant's being alive) ≠ P(this plant's being alive | it was sprayed with defoliant), yet we'd never say that being sprayed with defoliant *explains* the plant's being alive. – van Fraassen concludes from a long review of similar attempts that these views all fall short in two important respects: (a) they have trouble accounting for asymmetries; (b) they have trouble accounting for relevance and rejection[78].

Along the lines of (b) relevance, van Fraassen undertakes a study of causal explanation, searching for a general account that solves the puzzle of 'salience' (being that particular cause, within a network of causes, which we take to be the relevant explanation for a particular fact (even though the salient cause is just one of many causes, the other causes not being able to similarly explain)). van Fraassen reviews some history in the philosophy of causality. The 'cause' of an event was traditionally thought to be the *conditio sine qua non* (the indispensable (essential) condition) leading up to and necessary for the effect. Yet, not every necessary condition is a cause (e.g. the existence of a knife is a necessary condition for its rusting, but is not the cause), and a cause may not be necessary (i.e. alternative causes could have had same result). – Mackie's INUS conditions (insufficient (non-redundant) but necessary part of an unnecessary but sufficient condition) also fall short of the mark: (1) even the necessary part of INUS may not constitute a cause (as, again, the existence of a knife for its rusting); (2) there may be no sufficient preceding conditions to some phenomena (e.g., the existence of radium is what caused the Geiger counter to click, but there's a better-than-zero probability that the Geiger counter will not click at all). – David Lewis' counterfactual account of causation

78 *rejection* meaning 'having warrant to reject a request to explain'

meets especially fierce resistance from van Fraassen. In particular, van Fraassen objects (1) that counterfactual causation carries implicit *ceteris paribus* conditions for which no invariant-exact content can be specified (for, argues van Fraassen, which conditions obtain depends on context) (e.g. counterfactuals do not in general obey weakening [(A>B) entails ((A&C)>B)], so <striking a match caused it to light> does not entail <(striking a match & dunking it in coffee) caused it to light>). (2) Lewis asserts: Whenever 'A causes B' is true, then also: 'if A had not happened, then B would have not happened.' van Fraassen responds that this is an insufficient criterion, giving a counter-example: <D's alarm goes off> is the cause of <D's waking up>; but also: if D had not gone to sleep the night before, he would not have woken up. We do not, per criterion, want to say that going to sleep caused him to wake up. – So, three attempts at answering salience have failed, and salience remains a puzzle.

van Fraassen construes these failures as due to the omission of a key element: “The discussion of explanation went wrong at the very beginning when explanation was conceived of as a relationship like description: a relation between theory and fact. Really it is a three-term relation, between theory, fact, and context” (156). van Fraassen goes on to assert that an explanation must be understood as essentially relative, relative to specific questions asked in a certain context and with respect to a certain theory. van Fraassen exclaims: “No wonder that no single relation between theory and fact ever managed to fit more than a few examples!” (*ibid*.).

Note 5:

In the case of a causal theory, for example, the relevance relation picks out (maps to) exactly those salient factors relevant for the topic. So, if a theory is rich enough to have, say, 10 salient factors, then that same richness will determine 10 'respects in which the question is asked.' Thus, for any why-question with the proper theory-determined specificity (meeting the constraints of this interrogative logic), it will be a foregone conclusion that there shall exist a theory-determined specified answer (or explanation). (Another way to think of this: The answers determine the questions, not the other way around).

The formalities: why-question Q is a triple <P*k*,X,R> consisting of topic P*k*, contrast class X={P1, P2.,...P*k*,...} and relevance relation R. A proposition *A* is said to be 'relevant to Q' just in the case that *A* bears relevance relation R to <P*k*,X>. A direct answer *B* to why-question Q is as follows:

B is a direct answer to Q exactly where there exists a proposition *A* which is relevant to Q and there exists a proposition *B* which is true exactly if {A, P*k*, (for all P*i* ≠ P*k*) not-P*i*} is true.

Note 6:

Salmon and Kitcher (1987) have objected to van Fraassen's account of explanation, arguing that the relevance relation is too liberal and would permit just about any answer to any why-question. They imagine the following counter-example: Suppose a theory of astrology that predicted the day on which JFK would die. The why-question is: 'Why did JFK die on 11/22/63?' Pk is 'JFK died on 11/22/63.' The contrast class *X* = {JFK died 1/1/63,..., JFK died 12/31/63}. The explanation is: 'Pk in contrast to *X* because *A*,' where *A* is a true description of the configuration of the stars and planets on JFK's birthdate. *A* therefore bears relevance relation R to the pair <Pk,*X*>. Salmon and Kitcher contend that this explanation meets van Fraassen's standards for a good explanation, since *A* is true; Pk is true; the other members of the contrast-class are false, and *A* counts as an answer to the why-question, relative to *X*, in picking-out those theory-directed salient factors relevant to the topic. Finally, on the evaluative side, assuming the particular astrological theory in question deliberately chose 11/22 vs. any other day that year, and assuming (for astrologers anyway) their background *K* is in cahoots with astrological theory, then that same theory and *K* (i) would probabilistically favor *A*; (ii) would probabilistically less favor other possible 'astrologically relevant' answers; and (iii) because *A* is an “astrologically complete” set of “initial conditions” which theoretically determines the facts to be explained, no other 'astrologically relevant' answer can render *A* irrelevant (Salmon & Kitcher, 1987). Thus, argue Salmon and Kitcher, van Fraassen's account comes to triviality since any suitably configured theory and background *K* can be made to 'explain' in accord with van Fraassen's requirements. – Many have critiqued

Salmon and Kitcher's critique, most of the defenses centering on the probabilistic side of van Fraassen's account, though I find these defenses just make maximum use of van Fraassen's parasitic exploitation of realism. Once the realist method delivers the goods, then a probabilistic rendering can as easily be milked out of it as a pragmatic one. The challenge to the antirealist is finding some way to accomplish what realists do (i.e. arriving at fruitful, empirically successful theories) in some way independent of a realist attitude.

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