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On Scientific Realism:

In Defence of a Deflationary Approach

PhD Dissertation

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Abstract

This thesis explores the question of scientific realism. It proceeds by first providing an historical examination of the history of the debate in recent decades that has led to scientific realism replacing logical empiricism as the received view of scientific theories. Van Fraassen's proposed replacement "constructive empiricism" is examined and found to be an inadequate replacement. However, it is made clear that his critique of realism is valid. Recent post-van Fraassen positions are then examined in order to develop a plausible solution to the realism debate. The thesis focuses in particular on "deflationary approaches", those positions that eschew global solutions to the debate that are supposed to apply to the whole of science.

A solution is proposed that draws features from recent "deflationary" approaches to the realism question. The normative methodological role of the deflationary approach is then defended from the claim that no interpretative, normative or methodological role is left for such a position. An illustration of the utility of the approach is demonstrated through a case study of the methodological role that the realism question has played in the field of quantum mechanics.

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Introduction

This thesis examines and points towards new elements of resolution in the debate over scientific realism. In recent philosophy of science this debate has been polarised around two contrasting theses: realism, which holds that when a theory is accepted, belief in the theory's postulated unobservable entities is implied; and anti-realism, which holds that belief is only warranted in what can be observed. The large majority of the philosophical literature that addresses the realism question defends one or the other of these two positions.

Bas van Fraassen's volume *The Scientific Image* (1980) marked a turning point in this debate in so far as its characterisations of realism and anti-realism have been generally undisputed and have served as the benchmark for subsequent debate. My thesis will agree with van Fraassen that realism, as he defines it, is philosophically untenable. Nevertheless, discussion of van Fraassen's work, and earlier critiques of anti-realism, have made it clear that anti-realist accounts (including his own) rest on arbitrary distinctions between the observable and unobservable. My thesis demonstrates that the realism question has thus far been approached from two equally untenable points of view.

A number of contributions to the realism debate have been published in recent years have proceeded from similar conclusions, notably Richard Miller's "piecemeal realism" and Arthur Fine's "Natural Ontological Attitude". Nevertheless, these positions (formulated subsequent to van Fraassen's contribution) differ greatly from one another and their exact relation to the positions of realism and anti-realism is sometimes unclear

As Miller and Fine and others exemplify it, most of the literature that addresses these positions does so from the point of view of realism. Very little has been written in terms of a general survey of these accounts, presented within the context of the recast parameters of the realism debate. This thesis fills this gap by delineating the new parameters of the realism debate and serves as an evaluative comparison of the more promising contemporary candidates that offer themselves as solutions to the realism debate.

In addition to providing a detailed and comprehensive survey of some recent turns in the realism debate, my thesis supplies a critical evaluation of these positions that offer themselves as candidates. This evaluation demonstrates that none of the competing positions is wholly satisfactory, although each one contains at least some worthwhile insights that point in the direction of the solution that I will sketch. Moreover, it will be found that the realism issue has a direct bearing on the relationship between the philosophy of science and current scientific practice. This point will be demonstrated in the final section of the thesis.

Before any detailed discussion of scientific realism can take place, the doctrine, and thus proper parameters of the discussion, must be delineated. That is to say, the doctrine must be distinguished from others of the same name in order to avoid misleading digression into other debates that are unhelpful to the resolution of the debate at hand. For instance, been quite extensive debates over Hilary Putnam's doctrine of "internal realism" and Michael Dummett's version of "anti-realism". Upon closer examination of these debates it becomes clear that they have little to offer to the resolution of the scientific realism debate. This is so because these doctrines are general epistemological conceptualisations concerning the relation of human knowledge to the world. In other words, they relate to general theses about the relation between human conceptual schemes and the concept of "truth". However, this thesis argues that these general epistemological debates offer little help in the philosophy of science, since the debate in that domain specifically concerns the status of unobservable entities, like electrons, that typically enter the specific theoretical frameworks of scientific theories. A person could, for example, accept an epistemological thesis like Putnam's "internal realism" with regard to the "conceptual scheme", but still suppose that particular unobservable entities like electrons are just tentative postulates. On the other hand, one could support Dummett's view of epistemological anti-realism and still think that unobservable entities propounded by the sciences deserve the same level of commitment as those that can be directly observed. The debate in the philosophy of science is a specific one over the nature of the degree of acceptance that ought to be granted to the various entities propounded by science that cannot be directly observed.

With this aim in mind, this thesis reserves a chapter for an historical treatment of the debate over the question of the status of unobservable entities. In particular, the current debate in scientific realism finds its origin in the context of the breakdown of the logical empiricist project of providing a general account of scientific theories that could delineate a clear distinction between the observable and unobservable constituents of science. Current versions of scientific realism typically proceed from the recognition of the failure of the logical empiricist project and propose belief in unobservable entities as the best replacement for logical empiricist views of scientific theories. The first sections of this thesis trace the reasons for that failure and the subsequent advancement of realism as the dominant view in the philosophy of science.

Realism, of course, suffers from its own problems and has been on the defensive since the publication of van Fraassen's seminal book *The Scientific Image*. Essentially, scientific realism trades on the idea that no distinction between the observable and unobservable is to be had. Thus, to accept a theory is to accept the whole theory. The 'acceptance' of a theory, however, need not imply belief in the theory, especially if alternative and quite different theoretical constructions are available to account for the scientific data in question. Moreover, just because there is no philosophically defined distinction between the observable and theoretical in all theories, this does not at all rule out the possibility that some scientists might want to reserve judgement on certain parts of their theories.

From this discussion of the recent history of scientific realism and the discussion of the debate over van Fraassen's critique, two very important conclusions emerge. The first main conclusion is that the resolution of the realism question has considerable methodological import in terms of what is conceived to be the proper goal of scientific activity. For the realist, science provides causal explanations in terms of the causal actions of theoretical entities on each other (observable or otherwise). For an anti-realist, while explanation may be seen as important, the goal is 'empirical adequacy', a theory must be consistent with the phenomena it deals with. In this view, "empirical adequacy", and not explanation is seen as the legitimate goal of science. If a successful theory provides empirical adequacy in the absence of causal explanation, a realist must see it as inadequate but this would not be so for the anti-realist.

The second main point that emerges is that both realism and anti-realism (either van Fraassen's or logical empiricism) suffer from a similar error for reasons related to the methodological import of the debate. In either case a general solution to the problem is sought that is supposed to account for, and have normative import for, the whole of science. This, however, overlooks the very real plurality of methods and approaches that exists across the sciences. The solution to the realism debate must rather be sought by looking at science and how it is practised, and not through the deployment of a general philosophical model.

The natural course to take is to look at specific instances of science for the realism question on a case by case basis. As already noted, a body of literature has developed that seeks a resolution of the realism question along such "deflationary" lines. These approaches, however, have thus far been quite diverse and little exists in the way of a general treatment of this "deflationary" turn. In order to clarify this situation, the latter sections of this thesis examine the various deflationary approaches to the realism debate with a view to highlighting the threads that run through this literature and identifying the differences. Two fully-fledged accounts of this type are represented by Miller and Fine's approaches, our discussion will show that each one (for quite different reasons) fails to overcome the problems that plague the general philosophical models of science that they each seek to replace. Miller's approach allows for topic-specific accounts of the status of unobservables, but retains a realist *a priori* account of scientific methodology since he regards causal explanation as the hallmark of a good theory. Fine's "Natural Ontological Attitude" purports to be a "compromise" between realism and anti-realism by rejecting any philosophical interpretation of theoretical posits. Ironically perhaps, this account also imposes a global philosophical account on scientific theories through its ban on any interpretation of the status of theoretical posits. It ignores the very real interpretative component of science and entirely fails to do justice to the methodological import of science's various interpretative stances.

In addition to the difficulties inherent in these two stances (and others that I shall also examine), one important aspect of science has been largely overlooked by the deflationary approached to the realism question. How should a properly conceived deflationary account deal with cases where competition exists within a theoretical domain between two different interpretations of the theoretical posits? Does philosophy of science have an adjudicatory or merely a descriptive role in these instances? This thesis provides a resolution of these questions by addressing the question of the adjudication of competing stances directly within a deflationary approach. Essentially, my thesis will show that given the methodological import of an interpretative stance on the realism question, the philosophy of science can help in the adjudication of the proper stance to the realism question on a case by case basis. This occurs through the evaluation of the relative success of the competing interpretations on the basis of the respective successes of the deployment of their methodological recommendations.

The one thread that runs through all of the deflationary approaches to the realism question is the idea that it is to be addressed not through general philosophical debate over the abstract structure of any scientific theory, but by looking to specific cases of science in practice. In addition to avoiding the difficulties that plague the other proposed solutions to the realism question (standard empiricist view, constructive empiricism, Fine and Miller's position etc.), the value of any such account will rest heavily on its ability to be deployed to account for specific case studies. Moreover, in so far as my approach lays claim to an adjudicative role, it must be able to account successfully for specific cases where competing interpretative stances exist. Thus, in order to make the case for the deflationary account that I favour, it is necessary to deploy it in a case study. An obvious source for such a demonstration is the debate over the realism question in Quantum Mechanics. This is so not only because all of the major approaches to the realism question that I deal with purport to be the best approach to Quantum Mechanics, but also because a fierce debate has taken place within Quantum Mechanics between competing realist and anti-realist approaches. Some theorists have argued that the standard anti-realist "Copenhagen" interpretation to Quantum Mechanics must be seen as incomplete because of its failure to provide causal explanations of quantum systems. Various causal models have been proposed to replace the Copenhagen view. These models are realist in nature because they argue for the existence of "hidden variables" that cause the phenomena under consideration.

It will become clear that the realist demand for causal explanation has been unsuccessful relative to the predictive approach of the rival Copenhagen interpretation. The empirical consequences of the deployment of the realist and anti-realist methodologies in Quantum Mechanics permit the adjudication of the stronger of the two approaches to the realism question in this instance. In this case, anti-realism is clearly the best stance (at least for the present) in Quantum Mechanics. The final sections of this thesis that deal with the Quantum Mechanics case study will also reveal that other major approaches to the realism question (NOA, realist and anti-realist) provide inferior analyses of Quantum Mechanics relative to the specific deflationary account that I propose.

Chapter 1: Historical Background to Realism

Scientific realism has become the predominant view in the philosophy of science and has been so for at least twenty years. I shall undertake to trace back the origins as well as the reasons of scientific realism's rise to prominence in contemporary philosophy of science, with a view to outlining the major arguments that have been put forth for scientific realism. At the same time, this will make explicit the features of realism that have come under criticism form Bas van Fraassen's conceptions which will be dealt with in subsequent chapters.

However, before going into the historical background and presenting the main arguments in favour of it, scientific realism must be distinguished from other forms of realism. This chapter will, thus, be divided into three main sections. The first section defines scientific realism and distinguishes it from other doctrines of the same name. It will also provide arguments as to why these other doctrines are not germane to the debate over realism within the philosophy of science. The second section outlines a brief history of realism's rise to prominence. This is done in the context of reviewing the reasons for the collapse of the view of scientific theories deriving from the logical empiricist view of scientific theories, since contemporary realism emerged for the most part against the background of the demise of the logical empiricist movement. Thirdly, an historical outlook provides a summary of the main reasons that have been advanced in favour of adopting realism as the best replacement for the "standard empiricist view"

The main goals of this chapter, then, are putting forth a definition of scientific realism, outlining the difficulties with its logical empiricist predecessor and reviewing the arguments that advance realism as the best replacement for logical empiricism. With this task completed, the stage will be set to begin the discussion that will occupy chapter two, the examination of the views of Bas van Fraassen.

The collapse of the standard empiricist view essentially gave rise to a general consensus in the philosophy of science concerning the validity of a realist interpretation of scientific theories. But van Fraassen's work stands out as a conspicuous exception to this rule. Van Fraassen rejects the arguments for realism summarised in this chapter. In addition, he opposes the logical empiricist views that realism seeks to replace.[1] To appreciate fully and evaluate his views, it is therefore necessary to examine the reasons why realism came to replace logical empiricism as the received view in the philosophy of science.

1-1 Delineating Scientific Realism:

In the context of this thesis scientific realism is identified with the doctrine defined as such by van Fraassen:

> 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.[2]

For the scientific realist, to believe in a theory is to believe that the theory, the whole theory, is true, including those parts that refer to entities that cannot be directly observed. This formulation has the virtue of being consistent with the multifarious arguments propounded by philosophers adhering to scientific realism. Indeed, in spite of the many fierce critiques that van Fraassen's work has received, no realist philosopher of science has opposed this characterisation of the basic features of the doctrine.

Nevertheless, prior to the wide dissemination of positions corresponding to the definition of realism just given, philosophers of science sometimes used the term in a different sense. An excellent example of this is the use of the term "realist" by Feigl to characterise the later work of Carnap. For Feigl, Carnap's work has "a greater affinity with critical realism than with phenomenalistic positivism".[3] This might seem confusing because Carnap's work is squarely within the tradition of logical empiricism. Of course, logical empiricism propounded a view of science for which realism advances itself as the replacement. However, Feigl is simply using the term in this instance to draw attention to the fact that Carnap's work bears no relation to phenomenalism. That is, it rejects the once fashionable view that scientific statements ultimately refer to sense data. He is not trying to commit Carnap to a view that implies belief in unobservable entities postulated by scientific theories. "Realism" as meant by Feigl is now rarely used within the philosophy of science.

It is also necessary to distinguish 'scientific realism' from Michael Dummett's use of the term 'epistemological realism'. For Dummett, realism is "the belief that statements possess an objective truth value, independently of our means of knowing it... The anti-realist opposes this view".[4] This is quite a wide definition of realism and excludes only those positions that would declare that a statement's truth-value rests only on something like conventional agreement or sensory perception. In this sense, a Dummett-style realist would oppose phenomenalistic positivism along with Carnap; certain forms of conceptual relativism (those that define truth as convention) would also be rejected by this brand of realist. While all realists falling under van Fraassen's definition are realists under Dummett's version, someone who opposes realism as defined by van Fraassen could be a realist according to Dummett's vocabulary. In so far as both Feigl and Dummett characterise realism as a doctrine generally opposed to phenomenalism and relativism, Carnap is a realist in Dummett's sense in exactly same sense that he a realist in Feigl's. In the next chapter it will be made clear that in so far as he is not committed to phenomenalism or conventionalism, van Fraassen is also a realist in Dummett's sense.

In general, then, Dummett's notion of 'realism' is too wide for use in the debate over scientific realism. Indeed, it leaves the central question of the scientific realism debate open. One could, for example, adopt the view that statements have independent truth-value while also holding that statements about certain postulated entities ought only to be provisionally accepted. The debate that divides scientific realists from anti-realists, in van Fraassen's terms, is over the validity of the realist defence of the truth or at least approximate truth of scientific statements about theoretical entities (unobservables). Within the philosophy of science, those who identify themselves as realists concern themselves with the more specific debate about the status of unobservables. Their concern is not with the purely metaphysical debate over whether or not any statements at all refer to entities independent of their speakers, or with epistemological concerns over whether or not knowledge claims are justified about the "external world". Therefore, when addressing the question about realism in the philosophy of science, it is preferable to frame the question in van Fraassen's terms. Of course, to be fair to Dummett it must be noted that he is concerned with a much more wide ranging set of metaphysical and epistemological issues than are germane to this thesis.

Nevertheless, it remains the case that scientific realists define and defend positions within the narrower framework identified by van Fraassen rather than the within the broader context of Dummett's "epistemological realism".

Thirdly, scientific realism must also be distinguished from Putnam's doctrine of "internal realism" which is considered by some to be a repudiation of his early arguments for scientific realism (considered later in this chapter) and which is not really a form of realism at all.[5] Indeed, with the potential confusion associated with the multiplicity of senses of 'realism' in mind, Putnam, for a time, advised that the term be avoided altogether to avoid confusions of sense.[6] But it is not the case that "internal realism" necessitates a repudiation of scientific realism as it is defined in this thesis. Describing "internal realism", Putnam notes that for the statements formulated within a conceptual scheme, those which are true will depend on the scheme one has adopted. Nevertheless, given the concepts and categories of that scheme, what makes something true or false will be independent of the holder of that scheme.[7] For example, consider two different conceptual schemes S and S': scheme S holds that for each two particulars an object exists which is their sum while S' denies this. When confronted by a world containing three chairs an adherent of S will hold that the statement "there are seven objects" to be true and the statement "there are three objects to be false", while the devotee of S' will hold the opposite to be the case.[8] However, in each case it is not convention or sense perception that determines the truth-value of the statement in question, but the number of chairs that are present. Thus, Putnam's internal realism is not necessarily incompatible with his early scientific realism that urges belief in the truth of statements about unobservables. In the context of "internal realism", statements are still either true or false by virtue of facts independent of the scientists who utter the statement in question.

Therefore, Putnam, when speaking of any given scientific theory, can still defend his early arguments without alteration by simply adding the observation that given the "scheme" that includes the scientific theory in question and the relevant background theory, belief in that theory's unobservables is warranted. An anti-realist (taken in van Fraassen's sense) about the same theory could also support internal realism by arguing that given the scheme that includes the theory in question and the relevant background assumptions, belief in the theory's unobservables should only be tentative.[9]

Two conclusions follow from these observations. First, contrary to what some realist philosophers of science have argued, it is still legitimate to associate Putnam with his early arguments for scientific realism.[10] Secondly, with regards to the question the legitimacy of belief in unobservables Putnam's view is in roughly the same position as the sort of realism defined by Dummett. Internal realism excludes only one sort of scientific realism: the sort that not only argues for the belief in theoretical entities but also claims that current science provides the only viable scheme. But no philosopher claiming to be a scientific realist would support such an extreme version. Putnam's internal realism, then, can include both sides of the debate in the philosophy of science and thus the debate over the validity of internal realism is not germane to the questions that are of interest to this thesis.

1-2 The Standard Empiricist View of Science and its Problems:

It was already noted that realism has come to be more or less the mainstream position within the philosophy of science, insofar as the bulk of the literature published in the last twenty years either provides arguments in favour of the position or proceeds from a standpoint that more or less accepts its main points . This was not always the case. In the decades prior to 1960 philosophy of science was dominated by a position that can be termed the "standard empiricist view". To fully understand the arguments for scientific realism, it is necessary to examine the view that realism has largely replaced and the reasons for its abandonment. This examination will make it clear that the demise of the standard empiricist view was not the result of a realist critique. Rather, the realists made use of the logical empiricist's critical evaluations of their own positions in order to advance a wholly contrasting view.

The standard empiricist view reached its most mature form in the accounts outlined by Rudolf Carnap and Carl Hempel in their later writings. These formulations represent the strongest presentation of the standard view, immune to many of the problems that plagued earlier versions. Their views, while similar in many of the most salient areas, are not of course identical and were modified over time. In order to present the central features of this approach to scientific theories, I will first outline Carnap's formulation as it is presented in his later work. Secondly, I shall very briefly draw upon some of Hempel's very late writings to outline the areas in which their views differed in order to evaluate his attempts to work around the problems that plague Carnap's formulation.[11]

1-2 a) Carnap's Account

In "The Methodological Character of Theoretical Concepts", Carnap takes up the task of propounding a canonical model of a scientific language that is meant to meet all empiricist desiderata. Such a language is fundamentally construed as being a first order language with respect to its logical basis. Furthermore, it is considered that the theory is axiomatised by a set A of axioms and that it is closed under deduction. That is, the theory T is identified with its axioms and all their logical consequences (noted $Cn(A)$). Furthermore, it is assumed that a scientific theory will typically put into play concepts that are not definable in terms of "observational" vocabulary. "Observational terms" are characterised as terms that refer to a) observable objects and events, b) observable properties thereof, and c) observable relations holding between such objects and events.[12] If we note by v_o such an observational term and by V_o the observational vocabulary of the theory, the observational language (noted L_o) of a given scientific theory can be characterised as the sub-language of this theory that contains only members of V_o. Let any non-observational (i.e. "theoretical") term be noted v_t and let V_t stand for the theoretical vocabulary of a theory (including all the mathematical apparatus needed). We thus obtain a model of a theory's language L as comprising two parts, L_o and L_t, where L_o and L_t are disjunct. The part of a theory $T = Cn(A)$ that contains no theoretical terms represents the observational consequences of the theory, that is, all the claims of the theory that can be formulated in observational, non-theoretical terms. Carnap then introduces a lexical distinction within the vocabulary of scientific theory between V_o and V_t. This distinction can be readily generalised to the language of a theory's formulation. L_o contains only V_o as descriptive vocabulary. Any sentence containing a term from V_t belongs to L_t. T is of course formulated in the full language of the theory i.e. $L_o \cup L_t$. Given this, the exact relation between L_o and L_t, for Carnap, stands in need of clarification. Thus, the nature of scientific language stands in need of clarification in terms of an analysis of how meaning is assigned to theoretical terms and how they relate to L_o.

Carnap takes for granted that the division of the language of science into an observational and theoretical part is unproblematic. Observational statements refer to what can be, in principle, observed. A statement is to be included as part of the observation language just if it contains only observational terms in its non-logical vocabulary. There is, for Carnap, no philosophical difficulty associated with the meaning of observational terms. Observational terms refer to that which can be observed and their meaning is thus assigned.

The theory of meaning of L_o terms that is implied by Carnap derives from Tarski's logical semantics. In this view the meaning of an expression is determined by a function that assigns to a statement its denotation in a specified domain. Thus singular terms are related to singular objects, properties to sets of objects and binary relations to sets of binary relations and so on. According to the semantic conception of truth, the sentences of a given language (say L_o) are interpreted when their truth conditions are determined. For L_o, the assignation of observable entities' denotations to terms of L_o can be accomplished through empirical means, in other words through observation. The question of the truth-value of a sentence of L_o can be answered through observation. L_o then is empirically and fully interpreted in so far as its truth conditions are determined empirically. The truth of sentences in L_o is then unproblematic. The truth of a statement such as "roses are red" can be checked empirically with few problems because both roses and red are observable.

Carnap, then, restricts his discussion to the difficulties associated with the meaning of L_t terms and their relation to L_o.

As indicated above, prior to the later formulations of Carnap and Hempel, many philosophers sympathetic to the standard empiricist view held out the hope that L_t terms could be shown to take on their meaning solely by reference to observational terms. However, in addition to this, considerable discussion took place on the question as to whether or not theoretical terms could be eliminated altogether from the reconstructed language of scientific theories. Thus, before discussing the project aiming to show how theoretical terms take on their interpretation with reference to observational terms, it is worth digressing in order to examine the project to *eliminate* theoretical terms altogether and the reasons for its failure.

Various mechanisms for doing so were proposed: two of the more discussed being Craig's theorem and the Ramsey sentence. The Ramsey sentence purports to facilitate the elimination of theoretical terms by trading on a distinction in logic between a "valuation" and "interpretation". "Valuation" is the more general of the two terms. A valuation is a function that assigns to any expression of a formal language a value in the domain of discourse. If it is a singular term, it assigns the term an object as its denotation. If the term in question is a monadic predicate (i.e. expressing a property of objects), it assigns the predicate a set of objects as its denotation. If it's a dyadic predicate (expressing a binary relation), it assigns the predicate a set of ordered couples of objects (eg. Objects x and y stand in relation Rxy). If it's a triadic predicate the same applies except with an ordered triple of objects and so on with n-adic predicates being assigned an n-tuples of objects.

Formal languages aiming to encompass arithmetical theories will contain functors; in that case, a valuation will assign a determinate function to a functor. In all these cases (where the terms are closed terms, i.e. containing no free variables) a valuation is also an interpretation. For example in the case of a singular term, the term is assigned an object (and no other) as its denotation. Thus the term is given that object as its interpretation. However, not all valuations count as interpretations (although the reverse *is* true). Consider the expression $x + y = 10$. The function v that say assigns the value 4 for x and 6 for y is a valuation for x and y but not an interpretation (other values are possible for the variables, 5 and 5 for example or 9 and 1). A value is being assigned to the terms but the terms are not stipulated as having a fixed denotation. Given this understanding of valuation and interpretation, the notion of truth in formal languages can be defined. In the case of function v the values of x and y satisfy the expression $x + y = 10$ in that it renders the expression true in the intended domain of interpretation (under the interpretation of + as expressing the binary relation "addition").

An expression that is satisfiable by all values is called a *true* sentence. The sentence $x + y = 10$ is not a *true* sentence because there are possible values for x and y that do not satisfy the expression (such as 2 and 3). Given this, and using "satisfaction" as an auxiliary idea, *truth* can be defined for the language in question, following Tarski: a well-formed formula α of language L is true under a given interpretation σ of its terms, if and only if all valuations of the terms of α in the intended domain of discourse satisfy σ. The notion of truth is in this way defined for sentences of the forms Pa, $\forall xPx$ *or* $\exists xPx$ and then recursively for sentences of the form $\neg\alpha$, $\alpha \wedge \beta$, $\alpha \vee \beta$, $\alpha \supset \beta$, $\alpha \equiv \beta$.

The Ramsey sentence uses this distinction between valuation and interpretation in a very straightforward way. It simply replaces interpretations of theoretical terms with variables that are given a possible valuation, thus eliminating any terms that are not given empirical interpretation. Consider a theory *T* that contains two kinds of vocabulary: $T(t_1, t_2 \ldots t_n, o_1, o_2 \ldots o_n)$ where $(t_1, t_2 \ldots t_n)$ are theoretical terms. The resulting reduction from the Ramsey replacement is $\exists x_1, \exists x_2 \ldots \exists x_n (x_1, x_2 \ldots x_n, o_1, o_2 \ldots o_n)$ where the theoretical terms formerly enjoying an interpretation are replaced with variables that are provided with valuations. Previously a sentence containing *P* as a theoretical predicate looked like this: 'Pa or $\forall x Px$ or $\exists x Px$'. After the Ramsey reduction is performed the result is '$\exists Y Ya$ or $\exists Y \forall x Yx$ or $\exists Y \exists x Yx$'. This is a second order formula which defines the valuations for the variable that replaces the predicate *P* in the object language.[13]

The Ramsey sentence is straightforward in practice. Consider the following example: a sentence like "*a* is intelligent" becomes "There exists a theoretical property *Y* such that *a* has property *Y*". In this sense *Y* is left fully uninterpreted and no terms are left over in the language of a theory whose empirical interpretation is not fully determined. The problem with the Ramsey sentence is equally straightforward: as the example shows, the new sentence may not use the L_t term "intelligence" but it cannot avoid *reference* to the theoretical concept in question.[14]

Craig's theorem also purported to be able to eliminate all theoretical terms from the reconstructed language of science. It performs this function by demonstrating that all the deductive consequences of the language *L* of a given scientific theory can be derived using only the axioms of L_o, i.e that subset of *L* that contains only observational terms. A complete discussion of the details of Craig's theorem would entail a discussion is which is beyond the scope of this chapter, but it can be noted that it too is plagued by insurmountable difficulties. For example, every sentence that is logically to the theory's resulting axioms must be included after the theoretical terms are eliminated. These sentences will be infinite in number. This makes the procedure, other difficulties aside, unworkable in practice. However, the main difficulty with Craig's theorem is that in the resulting language L_{Craig} every well-formed formula becomes an axiom, and the logical connections between the sentences in the original theoretical language are not preserved. As Leroux notes, "The decidability of A* [the axioms that make up the language resulting from the application of Craig's theorem] is interesting from a metamathematical point of view, but since the Craigian transcription in no way preserves the logical structure of the original theory, it really doesn't cut much epistemological ice".[15]

Given the problems relating to the use of Craig's theorem or of the Ramsey sentence to show the syntactical or semantical dispensibility of theoretical terms, empiricist philosophy of science turned to discussion of L_t terms in the hope that they could be shown to take on their meaning solely by reference to observational terms. For example the theoretical L_t term 'mass' might be defined as referring to the observed results of measurements. If this were possible, neither the interpretation of L_t terms nor the truth of statements in L_t would be problematic, since their meaning could be fully interpreted in terms of L_o However, the meaning of L_t terms cannot be given in this direct way. This is so because some L_t terms will only connect with L_o terms by way of their relation to other L_t terms.[16] Consider the example of the term 'inertial force' in Newtonian physics. 'Force' is not a directly observable quantity. It is defined as the product of the acceleration of a body (which is observable) and its mass. Mass of course is a L_t term. The interpretation of the meaning of force in Newtonian physics rests on the interpretation of another L_t term, mass. The meaning, then, of some L_t terms cannot be interpreted without reference to other L_t terms.

The domain of L_t is much wider than the domain of L_o. Recall that the domain of L_o is restricted to what can be observed and L_o is interpreted by the fact that the truth conditions of its statements are determined empirically, but this is not so for L_t. L_t contains not only reference to entities that cannot be observed (because they are too small etc.) but also all the mathematical relations needed by the theory. And as I noted in the last paragraph, not all the terms of L_t are definable with reference to L_o terms. The empirical interpretation of L_t is then left open to a large extent. For an empiricist like Carnap, at least some L_t terms can have a fully empirical interpretation in so far as they appear in a law (statement) with fully interpreted L_o terms (such statements are referred to by Carnap as "Correspondence rules" or "C-rules"). L_t terms that do not appear in statements with L_o terms can still find a partially empirical interpretation in so far as they appear in statements with L_t terms that appear in the C-rules.

L_t terms then can only be given what he terms a "indirect and incomplete interpretation".[17] This is so because, as the previous paragraph points out, some theoretical terms can only be related to observational terms by way of their relation to other theoretical terms. Some L_t terms do relate to L_o without reference to other theoretical terms but this relation is not direct, but through the role of a set of correspondence rules C. The correspondence rules permit, "the derivation of L_o terms using the terms of L_t or vice versa".[18] For example, the L_o term "heavier than" might be connected to the L_t, term "mass" by the rule C, "if u is heavier than v... the mass of u' greater than the mass of v"".[19] However, this correlation using the rules C can, for Carnap, only be incomplete. Note that in the example just given the meaning of mass is not completely interpreted but is simply related to an observational quantity through a C-rule. Furthermore, sometimes the C-rule only connects L_t terms with L_o terms to a certain degree of probability. Such a "statistical correspondence rule" might, for example, state that "if a [space/time] region has a certain state, specified in theoretical terms, then there is a probability of 0.8 that a certain observable event occurs".[20]

In this construal, then, L_o *does* receive a fully empirical interpretation and hence its distinction from L_t. The viability of this construal hinges on the question as to whether observation and measurement can fulfil the semantical function bestowed upon them, that is, whether they can warrant an interpretation that would be direct and theory-free and that one can justifiably refer to as "empirical". If it is granted that observation plays a role in interpreting a part of the theory's terms, can it warrant the claim that within a full vocabulary, there are some terms whose interpretation proceeds entirely in a direct (through observation or measurement) and theory-independent way? In addition, empiricist constraints require that this interpretation provided by experience be given, i.e. fixed. If the interpretation of theoretical terms can vary according to changes made within the theory, there is no meaning change in the observational language when theoretical changes are made, if the interpretation of observational terms is indeed empirically fixed. Moreover, in this case the interpretation of an observational term should be invariant with respect to the different scientific theories within which it occurs.

It has become clear that the empiricist constraints on the interpretation of observational terms noted in the last paragraph are problematic. It is by no means clear that there is such a stock of terms in the vocabulary of science that bear such empirically fixed interpretations. This has been made clear in the works of Grover Maxwell and the other critics of logical empiricism. This work will be discussed in detail later in this chapter. Nevertheless, in order to further clarify Carnap's construal of the standard empiricist model it is worth a exploring, in a digression, how the empiricist model of a two-layer language for science evolved from considerations pertaining to the definability of scientific terms.

A fully-fledged empiricist standpoint would require that all scientific terms be entirely interpreted by empirical means. In the context of first-order axiomatic systems (which is the context adopted by the standard empiricist construal of scientific theories), this would impose the constraint of explicit definability on every scientific concept. Thus, all theoretical terms (L_T-terms) should be explicitly defined on the basis of observational terms (L_o-terms). Roughly speaking, an explicit definition of a term T on the basis of a term O has the following form: $\forall x(Tx \equiv Ox)$. However, this represents too strict a requirement for scientific terms.

Such an attempt to fully define all scientific concepts in terms of some basic vocabulary had been made by Carnap in his early book the *Aufbau*. In this work he made use of Russell's doctrine of abstraction and his hierarchy of types to attempt to construct all concepts in terms of a single given relation.[21] However, Carnap soon acknowledged the shortcomings of this sort of logical exercise. Many so-called "disposition terms" (like "soluble" or "aggressive") can only be partially defined on the basis of observation terms. For example let Sx denote the predicate "x is soluble", and let "x is placed in water" and "x dissolves" be designated by O_1x and O_2x respectively (where these are deemed to be observable). Sx can only be defined conditionally in the following way: $\forall x(O_1x \supset (Sx \equiv O_2x))$. This statement expresses that for all objects placed in water, this object has the property of being soluble if, and only if, it dissolves. Thus property S is only defined as being equivalent to O_2 for objects that have property O_1. By virtue of the truth-functional character of the material implication, the meaning of Sx is left entirely open for all objects that do not have property O_1. Since such a term proves to be semantically irreducible (or at least not fully reducible) to observational terms, it must be considered "theoretical". Therein lies the ground for distinguishing between observational and theoretical terms, and admitting at least some theoretical terms into the vocabulary of science.

Because all of the sciences are replete with such "disposition" terms, the requirement of full semantic analysability of all terms on the basis of observational concepts was abandoned. The empiricist criterion was first liberalised in a requirement that all scientific concepts be partially definable in terms of empirical concepts, that is, on the basis of observational terms whose interpretations are fully fixed from an empiricist point of view. This liberalised programme was quite in line with the then prominent operationalism associated with Bridgman and the behaviourism of Skinner. In operationalist terms, O_1 and O_2 are not to be taken as "properties" but as "operations" performed to measure a property, and "a term is empirically meaningful if an operational definition can be given to it".[22] That is, a term is meaningful if it can be defined in terms of operational concepts. In the behaviourism of Skinner, for example, the important terms "stimulus" and "response" were understood as operations. Concepts such as "aggressive" and "intelligent" were defined operationally in terms of particular sorts of responses given particular sorts of stimuli. Carnap notes the "healthful" affinity between logical empiricism, operationalism and psychological behaviourism, but ultimately concluded that behaviourism in particular and operationalism in general, "imposed too narrow restrictions".[23] As I have already noted, Carnap's final formulation of the standard empiricist construal admits into the scientific vocabulary theoretical terms that do not admit even operational definitions and must be given only partial interpretations in terms of the C-rules.

In spite of the necessary incompleteness of any relation to L_o. Carnap concludes that this account of theoretical terms in scientific theories does preserve the core of an empiricist view of science. This is the view that a clear distinction exists between statements that relate to what can be directly observed and theoretical statements and that the meaning and truth of scientific statements relates in some way to this observational language. Nevertheless, the interpretation of L_t terms in Carnap's formulation of the standard empiricist view is quite liberal given the partial interpretation of L_t. Therefore, with regard to truth, only the language of T restricted to L_o can be considered empirically true because only its truth conditions are empirically determined. For an empiricist of course it is better not to grant assent to the truth of a set of statements (say L_t) unless the truth can be determined empirically (barring analytic statements). Theoretical statements (while meaningful for the theory T) remain theoretical and the acceptance of theory T entails (for an empiricist) only the truth of the statements of L_o and not L_t because of L_t's incomplete and indirect relation to L_o.[24]

In sum Carnap's formulation of the standard empiricist view of scientific theories states that the language T of any scientific theory can be divided into two parts L_o and L_t. The descriptive terms of L_o refer to observable objects, observable properties of observable objects and observable relations that hold between observable objects. The vocabulary of L_t refers to unobservable objects, their unobservable properties and the unobservable relations that hold between them. The meaning of L_o terms is unproblematic given that they refer to what can be observed, but the meaning of L_t can only be given a partial interpretation by way of their relation to L_o given by the correspondence rules C.

The question immediately arises as to how exactly the distinction between what is observable and what is not is to be drawn. For Carnap the line between the observable and the unobservable must be drawn pragmatically depending on the needs of the situation. Some might consider that which can be directly detected using simple instruments, a philosopher with a stricter requirement might not, but "there is no question here of who is using "observable" in a right and proper way".[25] There is a "continuum" of observability that starts with "direct sensory observations and proceeds to enormously complex, indirect methods of observation... no sharp line can be drawn across this continuum it is a matter of degree... individual authors will draw the line where it is most convenient"[26]

Although Carnap does point out the impossibility of arriving at universally acceptable terms for what counts as "observable" this does not mean that he considers the distinction to be unimportant or that it cannot in practice always be drawn. Indeed in spite of the fact that the line varies from situation to situation, it is the central feature of the standard empiricist view that it can always in practice be drawn. In point of fact the standard empiricist view insists that the descriptive terms of the language of a given scientific theory have very different properties depending on which side of the line they occupy.

1-2 b) Some Difficulties with Carnap's Approach

Carnap of course acknowledges that the line between what counts as observable and what does not is impossible to fix to everyone's satisfaction. Nevertheless, Carnap's account of scientific theories rests on the claim that the line can be drawn in practice and that scientific theories are characterised by the relation between the resulting halves. Many philosophers, such as Grover Maxwell, have called attention to difficulties with this claim. Maxwell offers several lines of argumentation to the effect that the location any such distinction must be hopelessly arbitrary with the implication that no genuine difference in the characteristics of these terms can be distinguished.

Maxwell's critique first considers observational terms that refer only to objects observable with the naked eye. This formulation holds that images seen in telescopes are not to be considered as being referred to in the terms of the observational language and remain theoretical. The rationale for such a restricted observation language is straightforward. In these cases, what are seen are not objects themselves, but the images of objects formed by the viewing apparatus in question. The veracity of the image's correspondence with the purported physical object must rest on the validity of the arguments defending the reliability of the apparatus. In any event, by this rationale, when such data is referred to, what the terms correspond to are not physical objects but images, images that are only supposedly of physical objects.

For Maxwell, however, this argument leads to absurd consequences. If, he argues, this line of reasoning is taken to its logical conclusion then anything "observed" by dint of any lens would bc an instrumental artefact and not a physical object. This would include the images formed with opera glasses or even ordinary spectacles. Even objects viewed through air only slightly distorted by heat generated disturbances (a common phenomenon in the summer) would also fail to be "genuinely" observed. If such an implausible conclusion were to hold, a supporter of this distinction would have to maintain that only a person with 20-20 vision in clear room temperature air could be described as having, access to genuine observable entities. Those doomed to wear spectacles would be forced to refer to the same objects only theoretically.[27] Indeed, the consequences become even more implausible, something seen through a window is only "theoretical", but when the window is opened what is seen is suddenly "observed".

These objections to the observational/theoretical distinction are, for Maxwell, not exhaustive. Consider, for example, the case of the molecule. Consisting of nothing but electromagnetically bonded atoms, it is presumably a paradigm case of a theoretical entity. However, according to modern valency theory certain very large crystals like diamonds and salt are in fact large single molecules consisting of millions of bonded atoms (carbon atoms in the case of the diamond). Diamonds are, of course, clearly observable. Other large molecules can be observed in an optical microscope, still others can be viewed in a scanning electron microscope. The observability of molecules, then, falls on the same continuum that the results of various viewing apparatuses do. Given that the existence of the smallest molecules follow from exactly the same theory as the clearly observed existence of the largest does, no reason can bc given to deny the ontological status of one and to grant it to the other. As far as Maxwell is concerned, to believe in the modern scientific view of salt is to believe in molecules.

Maxwell also argues that the empiricists can find no solace in the contention that the observation/theoretical distinction can be made so as to count as theoretical only what cannot in principle be observed. For Maxwell, adherents of this position usually define "unobservability in principle as being entailed by the theory itself".[28] But such arguments lead to paradox. Consider the sentence, "electrons are unobservable", while this sentence obviously predicates unobservability to electrons, the question remains as to whether or not this sentence falls within the observation or the theoretical component of the scientific language. If it falls into the observable part then this creates a contradiction. How could one observe an electron's unobservability? However, in the case that the sentence falls under the theoretical part, then it can only be accepted tentatively and is open therefore to revision and lacks the cognitive import of observation statements and cannot, therefore, compel belief. Naturally, this undercuts its implication that one is compelled by theory to believe that electrons cannot in principal be observed. For Maxwell, these reflections indicate that not only is the distinction impossible to fix in practice but that the purported qualitative difference that exists between the two different types of descriptive terms is a chimera. For Maxwell it is impossible to fix a line between the observational and the non-observational. Furthermore no features that differentiate how theoretical and observational statements are assigned meaning can be distinguished.

1-2 c) Further Problems with Carnap's Account

In addition to the difficulties associated with drawing a meaningful distinction noted by Maxwell, further difficulties rest with the standard empiricist view's assertion to the effect that once it is drawn, the distinction identifies two classes of statements that have different, epistemologically significant, characteristics. In Carnap's formulation outlined above, the distinction is characterised by the observation that L_t terms are only partially interpreted with regard to their meaning through their connection to L_o through the C-rules. The issue of partial interpretation aside, the view contends that the C-rules guarantee a means by which statements in L_t can be tested. This rests on the notion that the meaning of non-analytic statements can be ultimately reduced to direct observations and that the truth-values of such statements can, at least in principle, be individually confirmed through observation. In the case of L_t, meaning can only be partially interpreted. However, insofar as it can be interpreted, the meaning for L_t is its observational entailments derived using the C-rules. Thus the virtue of the standard empirical account is that meaning of non-observational statements can be given an interpretation without giving up the presupposition that the meaning of a statement is defined in terms of direct observations.

Quine, of course, has called this presupposition into question. Quine's now well accepted arguments have shown that there is no non-question begging way of establishing a clear distinction between analytic and synthetic statements and, correspondingly, that no class of statements can be isolated who's meaning and truth value are exactly equivalent to their empirical entailments. While a full discussion of Quine's argument is not appropriate in this context a very brief digression is in order to outline the salient points that are germane to this discussion. Quine holds that the analytic-synthetic distinction is commonly defended on the basis that a wide variety of analytic statements can be shown to be such by identifying their subjects and predicates as synonymous.[29] However, Quine points out that synonymy cannot be defined without presupposing the notion. Abandoning the notion calls into question the analyticity of those statements supposed to be true by virtue of the synonymy of their terms. This conclusion calls into question the empiricist view of meaning. This is so because the lack of distinction between analytic and synthetic statements casts doubt on the conviction that a special class of statements, synthetic statements, can be isolated that can be independently confirmed or falsified. Without synonymy, then, the empiricist view of meaning is called into question. It follows from the impossibility of isolating a class of synthetic statements that can be individually confirmed that statements do not face the tribunal of experience in isolation, and their meaning is interpreted in terms of their role in an overall framework.[30]

Carnap would, of course, resist the notion that his view entails a theory of meaning which claims that two L_t statements with the same observational consequences would have identical meaning. Nevertheless his view does entail that a theoretical statement's meaning finds its interpretation, ultimately, in what is observable and that through observations it can be individually confirmed up to a degree. However, this view of meaning is ruled out if one takes Quine's arguments seriously. In the absence of an analytic/synthetic distinction for individual statements and with the untenability of the view that the meaning of statements is equivalent to their observational consequences, statements take on their meaning through their role in the entire scientific language. Meaning and confirmation, then, do not occur in isolation, the statements of a theory face the tribunal of sense experience as a "corporate body".[31] If this is so then there is no special problem of assigning meaning to a specific subset of terms L_t within the overall theoretical vocabulary and thus the philosophical point of dividing the theoretical language into the parts L_t and L_o disappears. Observation terms, in this picture, have their meaning impregnated with theory and vice versa.

1-3 Hempel's Reformulation of the Standard Empiricist View and its Problems:

The implications of Quine's position are well appreciated by Hempel who expresses doubt about the tenability of and the need for the theory/observation distinction. Hempel concedes that observational terms have their meaning impregnated with theory and recognises that observation terms are understood in terms of the whole theory, which includes theoretical terms, and that the distinction cannot be clearly drawn.[32] He proposes instead a reformulated distinction that, he argues, will preserve the core of the standard empiricist view of theories and avoid the difficulties of the older conception of the theory/observation split. Hempel proposes that instead of dividing the theoretical language up into observable and theoretical parts, it is better to divide it up into a theoretical part and a part that he refers to as the "antecedent vocabulary". These are connected together with "bridge principles" which are similar in some ways to the familiar correspondence rules of Carnap.[33]

Hempel's recourse to the antecedently available vocabulary provides quite a different characterisation of the L_o terms than Carnap's recourse to the notion of observability. They are terms whose meanings find their interpretation prior to their introduction into the theoretical vocabulary of the theoretical language in question and are unchanged in their meaning by it. In this sense, they are not unambiguously observational or theoretical but their meaning is antecedently understood without reference to any new terms that the theory may introduce. These terms are to be contrasted with the theoretical terms that the theory introduces. In this picture the constituents of the theoretical language of a reconstructed theory are not terms that do not refer to observable entities, but are "introduced specifically to characterise the theoretical scenario and its laws".[34] In this construal the meaning of terms in the antecedent vocabulary draws upon no untenable theories of meaning or untenable observation/theory distinctions and is thus not vulnerable to the sort of critique of the theory/observation split that Hempel accepts. However, for Hempel the question still remains open as to how the particular meanings are assigned to the terms of L_t once they are introduced in the new theory alongside the antecedent vocabulary in its new context. The answer Hempel provides is that they receive a partial interpretation through the bridge principles that connect them to the antecedent vocabulary.

Hempel's reformulation of the standard empiricist view does move away from Carnap's contention that the meaning of scientific terms is ultimately related to observation. Nevertheless, his construal does approach scientific theories within the context of the standard empiricist problematic: the problem of how theoretical terms are assigned meaning. In this sense, and in the similarity of the general structure of theories that is envisaged, Hempel's view amounts to a construal that is in many respects consistent with the mature views of Carnap. Moreover, it also purports to avoid the difficulties that plague that view with regard to its untenable distinction between observational and theoretical entities. Be this as it may, on closer analysis Hempel's reformulated distinction fails. As I will show in what follows, it fails for reasons that are similar to the reasons why Carnap's distinction failed.

1-3 a) Problems with Hempel's Account and their Implications

The discussion in the last section relating to the failure of the observational/theoretical distinction made it clear that the statements of a scientific theory acquire their meaning in relation to each other and as a group. This implies that the meaning of observational statements is impregnated with theoretical concepts and that no clear distinction between them can thus be drawn. Hempel accepts this and seeks instead to distinguish the antecedent vocabulary, whose meanings are understood prior to the theory in question separate from the theoretical vocabulary that is introduced in the context of the theory. The meaning of terms in the antecedent vocabulary is supposed to be understood without reference to the theoretical vocabulary. The antecedent vocabulary, then, is not supposed to be theory-laden as the observational vocabulary in the older distinction turned out to be. However, this claim cannot be sustained. The reason for this is straightforward. The antecedently available vocabulary, while not necessarily interpreted observationally is supposed to find its interpretation independently of the theory. However, the meanings of at least some of the terms in the antecedent vocabulary will be changed in the context of the new theory. That is to say, a term in the antecedent vocabulary will not necessarily have exactly the same meaning in the context of the new theory than it had in the old. This new meaning will have to be understood in the context of the new theory and cannot, therefore, be divorced from theoretical concepts.

Consider, for example, the meanings associated with a term like gravity in Newton's theory of universal gravitation and in Einstein's general relativity. In Newtonian universal gravitation "gravity" is defined as a field the strength of which drops in a relation that is inversely proportional to the square of the distance from its centre. But this is not what gravity means in Einstein's theory of general relativity, it is defined in this context as a curvature of space-time. In this new context, the meaning of "gravity" is permeated with the theoretical terms introduced in the new theory. In this case, the theoretical concepts relate to the curvature of the non-Euclidean space-time metric introduced by general relativity. Equally telling examples abound in atomic theory. Nuclear particles, in Bohr's day were considered to be fundamental. In current theory they are defined as amalgamations of various exotic particles, including "quarks" of various sorts, introduced by current theories. Furthermore, electrons were understood in Bohr's day to be "located" in "orbital shells" around the nucleus, but this idea of location has been replaced by the idea of quantum energy levels. The change has also taken place in the meaning of "location" within the nucleus itself. What is understood by "gravity" is not the same as was understood in Newton's day and the same applies to protons and electrons and their respective locations. Their meaning cannot be understood outside the theoretical context in which they now function.

It must be noted that the two examples given in the last paragraph hardly amount to what Carnap would characterise as constituents of L_o. However, in each of the examples, gravity and electrons, the terms in question were what Hempel would consider part of the antecedently available vocabulary. In other words, the interpretation of the meaning of statements including terms like "electrons", and no new terms, would fall under L_o in Hempel's reconstruction of the theory. In this sense the similarities and differences between Carnap and Hempel's construals can be brought into sharp relief. Each draws a lexical distinction in the terms of the theoretical vocabulary of the reconstructed theory such that the theoretical language can be divided into L_o, the interpretation of which is unproblematic, and L_t, the statements of which receive only a partial interpretation based on their relationship with L_o. However, for Hempel, the distinction simply delineates the differences between the way the statements of L_o and L_t are interpreted. L_o, for Hempel, contains much that is clearly not ultimately interpreted through observation, his point is only that the meanings of the constituents of L_o are not theory-laden with the theoretical concepts introduced by the theory in question. However, as we have already seen, this last claim cannot be sustained.

While Hempel's distinction is somewhat different than Carnap's, the reason for the failure of Hempel's distinction is comparable to the failure of Carnap's in some significant respects. In each case the difficulty lies in the impossibility of providing a construal of the L_o, L_t distinction where the interpretations of the statements of L_o are not theory-leaden. In Carnap's case difficulties arose with his notion that there exists a class of statements whose interpretation could be purely empirical. For Hempel problems arise with his notion that the meaning of the terms of the antecedently available vocabulary do not change.

The implications of Carnap's and Hempel's respective failures demonstrate that the central features of the standard empiricist view of theories are not adequate to characterise the nature and structure of scientific theories. Realists, such as Grover Maxwell, interpret this failure as implying the adoption of a scientific realism. For them the untenability of the distinction between a theoretical and non-theoretical language implies that that the acceptance of a given theory implies the acceptance and belief in the truth of that theory *en bloc*. This conclusion follows not only from the contention that the theory/observation split is untenable but from idea that no real difference of any sort separates the way one sort of scientific statement is assigned meaning from another. In this case no reason at all exists to be more sceptical about the existence of one specific set of entities referred to by the theory if the theory as a whole is accepted as sound.

1-4 Scientific Realism's Claim to be the Best Alternative to the Standard Empiricist View:

By the 1970's a general consensus had emerged within the philosophy of science that the standard empiricist view is not adequate to account for the nature of scientific theories.[35] Scientific realism at this time began to replace the standard view as the received view of scientific theories. However, scientific realism is not the only account of scientific theories that rejects the problematic empiricist linguistic distinctions. Kuhn's early socio-psychological view of science also rejects the theoretical/observational distinction.

However, the majority of writers in the field came to regard these views as inferior to realism. Realism finds wide appeal because of its claim to accounts for the nature of scientific theories, avoiding the difficulties with the empiricist view. Another appealing feature is its claim to preserve a special epistemological status for science. This feature is central to why it is held by many to be superior to the socio-psychological interpretation. This view, while widely divergent from anti-realism (in that it does not share its conception of a theoretical and observational divide), seems to rob science of any features that make it empirical. This is, for most realist philosophers of science, an unacceptable state of affairs.

A lengthy treatment of the socio-psychological interpretation is impossible in the context of this chapter whose main purpose is to outline the reasons for the attraction realism holds for many philosophers of science. However, because realism is presented as a superior alternative to this view as a replacement for logical empiricism, a brief discussion of the salient features common to this view that robs science of its special empirical status is in order. The feature that realist philosophers most object to is the doctrine of incommensurability between theories (associated with Kuhn and Feyerabend), the thesis that theories amount to such radically different world views that they cannot be compared. In this context, theory change is not described by means of normative methodology but through socio-psychological accounts of scientific group behaviour. The question in this context is what special epistemological status can science be given since it is, in this view, seen entirely as a product of group psychology and not as a method that provides at least some veridical access to the world?

However, the thesis of incommensurability is plagued with the same difficulties that attend most forms of conceptual relativism and these difficulties are fairly obvious. The thesis that one world-view (or theory of choice) is so different from another that no comparison can be made implies that one can step "outside" of one's theory and compare its features with another. In other words the claim can only be convincing if the theories can be critically juxtaposed, but this is exactly what the incommensurability thesis claims cannot be done. A paradox seems to attend any thesis that argues that all scientific claims emerge from points of view that cannot be compared. Either this thesis only holds for the theory from which it itself emerges or it must argue that some claims hold in all theories. However, the second alternative is ruled out by the central thesis. The idea of total incommensurability between theories, then, seems impossible to sustain. Indeed, Kuhn acknowledges these difficulties in his later works.[36]

With the untenability of socio-psychological views of science one appealing feature of scientific realism is its claim to preserve the special status of science as a distinct and uniquely successful epistemological practice. In the view of many realists, science's great success was something an account such as Kuhn's would have to consider to be just an accident. However, realism's characterisation of science as explaining nature through proposing and demonstrating the existence of theoretical entities is held by many who defend realism to explain science's success. This apparent virtue, along with its rejection of the untenable linguistic distinctions associated with logical empiricism are held to be compelling reasons for adopting the realist characterisation of scientific theories. These arguments and others of a related nature are characteristic of many of the influential defences of scientific realism of Sellars, Salmon, Smart, Boyd and Putnam and will be outlined below.

1-4 a) Arguments for Scientific Realism

One important formulation of this sort of argument for realism comes from J. Smart. In Smart's view a realist view of science is necessary because only realism can sustain a distinction between correct and merely useful theories. Smart accepts that it is true that scientists make use of theories that they do not necessarily believe to be true and simply deem to be useful. Nevertheless, the whole idea of a theory being "not true but just useful" makes no sense unless the "merely useful" can be juxtaposed against theories that are accepted to be true. Indeed, Smart notes that the explanation for the usefulness of theories not deemed true often rests on a juxtaposition of its predictions with those of a true theory whose predictions are similar. To illustrate this point, Smart considers the example of an astronomer who accepts the truth of the Copernican heliocentric model of the solar system who, nevertheless, considers the outmoded Ptolemaic model to be useful on the basis of the similarity of its predictions.[37] There was, of course, a period in history, shortly after the general acceptance of the Copernican model was achieved, when the vast bulk of astronomers would have assented to the observation that while the Ptolemaic model might not be true, its usefulness is beyond doubt. Such a scientist would, in Smart's view, explain the usefulness of the Ptolemaic model on the basis of the similarity of its predictions to those made by the heliocentric mode. The justification of the continued use of the geocentric model in these circumstances might rest on several of its features: elegance, and ease of use and so on. Early on, the Copernican model was in fact more difficult to use and its predictions were no better, if not worse than the geocentric model, belief in its truth rested on its intuitive plausibility and its much less *ad hoc* sounding explanations. Given that an opponent of realism restricts belief only to observable consequences, she is not in a position to account for the useful/true distinction. For a non-realist two theories with similar predictions would be similarly useful and nothing more, but this simply does not do justice to the distinctions that scientists do in fact make.

In addition to drawing attention to the realist's ability to account for the useful/true distinction, Smart advances what van Fraassen refers to as the "cosmic coincidences" argument. Smart argues that if a successful theory were not true, there would be no explanation for its success other than by an implausible appeal to a lucky accident.[38]

The following example will illustrate Smart's line of reasoning. Consider a theory *T* that makes successful predictions and explains them by reference to unobservable entities. Suppose that this theory is reformulated such that the resulting new theory *T'* makes the same predictions as *T*. For Smart the truth of *T* would serve as the only explanation for the similarity of the predictions. And such similarities, for Smart, do stand in need of explanation. Realism, therefore, must follow from the demand for an explanation for regularities like these.

Similarly, the case of "not true but, nevertheless, useful theories", in this picture, need not cause trouble for the argument that truth is the explanation for predictive success. Useful but not true theories derive their usefulness from their similarity to true theories and their predictive success is explained by virtue of this similarity. Differences in predictive accuracy between them are, naturally, easy to explain. True theories in the long run make better (if occasionally more inelegant) predictions than their more easy to use rivals because they are true. Exactly such a situation could be said, in this view, to exist between the useful predictions of Newton's planetary mechanics and the very complex but true theories of Einstein.

A related argument for realism, proposed by Salmon and outlined by van Fraassen, follows from Reichenbach's principle of "common cause".[39] The nature of this claim will be made clear by a brief discussion of Reichenbach's principle and its implications. The principle of common cause relates to different kinds of events that are generally seen to occur together.

In Reichenbach's view, such correlations stand in need of explanation in terms of causation. Van Fraassen provides an excellent elucidation of this demand by drawing attention to the constantly observed correlation between smoking and lung illness. Such a regular correlation stands in need of explanation. However, (as tobacco companies are fond of pointing out) in this case, no observed causal factor is present directly linking the two correlated events. For a realist, the demand for causal explanation of the correlation demands an inference to the existence of unobservable processes.

It must be noted that the principle of explanation in terms of common cause makes no demand for explanation in terms of strict deterministic laws governing the causal correlation. For Reichenbach, causal correlations are often probabilistic. Nevertheless, even without a requirement for complete determinism, merely probabilistic explanations of causal correlations, like explanations of the link between smoking and lung illness, do make reference to the actions of unobservables. For a realist of Salmon's stripe, to accept the validity of such explanations is to accept the existence of the concomitant unobservables. Thus, in so far as the principle of common cause demands casual explanation of statistical correlations, the principle of common cause implies scientific realism.

Further arguments for realism, similar in some respects to the arguments from "common cause", are advanced by Wilfred Sellars in his book *Science, Perception and Reality*. For Sellars not only do observed positive correlations stand in need of explanation, but science itself is incomplete if it cannot explain, "why the objects of the domain in question (of any given scientific theory) obey the laws that they do to the extent that they do".[40] Sellars contends that such a demand for explanation implies the following realist motto for any given scientific theory: "molar objects of such and such kinds obey (approximately) such and such inductive generalisations because they are configurations of such and such theoretical entities".[41] Sellars concludes, in effect, that unless an explanation for the behaviour of observations can be observed, the task of science is incomplete without reference to "hidden" or theoretical structures.

Sellars considers an imaginary case study to support this conclusion. Suppose that a series of apparently pure samples of gold are placed in *aqua regia*, the gold dissolves now at one rate now at another in spite of the fact no apparent difference between the samples can be discerned. For Sellars, any explanation of the dissolving of the samples of gold would be incomplete if it failed to explain the reason for the differences in the observed rate of dissolution and simply established the observational range of the speed at which the gold dissolves. If, for example, current chemical theory permitted the "simple modification to the effect that there are two structures of microentities each of which 'corresponds' to gold as an observational construct such that pure samples of one... dissolve at a different rate from samples of the other" then this modification ought to be adopted in order to complete the explanation of the way gold dissolves.[42] In other words, the differences that exist between rates of dissolution of apparently identical samples imply that the differences result from hidden or unobservable factors. If an addition to current theory, such as the one considered above provides an explanation without conflicting with other well established principles, then the modifications *ceteris paribus* ought to be accepted. Indeed, the quest for such an explanation is, in Sellars view, required. In Sellars' view, then, the demand for a complete explanation of observable phenomena entails belief in purely theoretical (unobservable) entities.[43]

The previous arguments have all been based on the general claim that only realism can account for the demand for explanation that is so central to scientific practice. Richard Boyd argues for scientific realism along a slightly different track. He contends that realism is the only position that can account for the experimental practises of science. For Boyd, theory plays an extensive role in the design of scientific experiments and realism is the only position that can be made consistent with all the implications of this observation. That is to say, the design of experiments implies belief in the truth of the theories and the success of the experimental method can only be explained on the basis that theories are not just instrumentally useful but approximately true of the world. Boyd notes, "what explanation [for the success of experiments] besides scientific realism is possible?".[44] Boyd argues that this follows from the claim, "for any successful experiment whose design was based on principles derived from theory... then it is the business of scientific epistemology to explain the reliability of that principle".[45] As far as Boyd is concerned this explanation can only rest on realism. Thus, Boyd's argument for scientific realism stems from its idea that the reliability of the principles governing scientific experimental techniques stand in need of explanation and only realism can provide this explanation. The explanation provided by realism is, of course, that success of experiments rests on the fact that the theoretical principles upon which they are based are true.

Boyd defends this contention by providing imaginary examples of cases where theory might guide experimental design. In one set of such examples Boyd considers the case of a theory which holds that the cell walls of a particular sort of bacteria are vulnerable to a certain drug because of a specific chemical reaction that it triggers. From this theory an equation is derived such that the population of the bacteria is a function of the dose of the drug and the length of the time of the exposure to it. When considering the nature of the experiments to test this equation, Boyd contends that a good test will take place in conditions where the theory, if it is to fail at all, it is most likely to do so.[46] In this case, tests of its validity ought to be structured so that conditions are created where the equation is at the highest probability of failing to predict the observed outcome. To determine what those conditions are, the scientist draws on other well-confirmed theories.

Boyd imagines that his scientists are aware of a drug similar to the one under consideration but that kills the bacteria in a slightly different way. It does not destroy the cell walls but prevents the development of new ones after the bacterial cells divide. To test whether or not the theory in question is correct in its contention that the drug in question kills the bacteria in a slightly different way than the second drug, one way to proceed would bc to verify its ability to kill off a given population of bacteria before it can divide, given that a big enough dose is administered. If the time needed for the bacteria to reproduce is known, a successful test of the equation implies that, given a large enough dose of the drug, the bacteria ought to be killed off in less than this time. A drug that prevents the development of new walls, of course, could not do this no matter what the size of the dose might be.

For Boyd, the causal claims made by the theory are tested against "alternative causal mechanism[s] and other known facts which might inhibit the proposed causal mechanism.".[47] The validity of the experiment considered above rests on the believed truth of the causal mechanisms associated with the second drug, otherwise it could not stand as a reliable control against which to test the equation associated with the action of the first drug. In other words the equation for the validity of the experimental design rests on the "realistic understanding of the relevant collateral theories".[48] In the case of a successful experimental test of the equation in question, the success of the experiment would have to be explained in the same way. Boyd's arguments boil down to the claim that not only do 'the causal correlations identified by science' stand in need of explanation so does the nature of experimental design and its successful deployment. For Boyd experiments are based on the assumed truth of collateral background theories and in the case of successful experiments, their success is explained in virtue of the truth of the same background theories. Thus, the role that the belief in the truth of background theories plays in the design of successful experiments implies acceptance of scientific realism.

In Putnam's early writings arguments are advanced for realism that are related in some respects to Boyd's. While Boyd argues that the reliability of the role of theory in scientific practice (i.e. in experimental design) stands in need of explanation and that realism provides this explanation, Putnam argues that the steady progress and success of science itself stands in need of explanation. For Putnam, realism also can provide this explanation. Although similar in some respects to Smart's argument that realism is needed to explain the success of theories that account for regularities in nature, it is not exactly the same argument. As van Fraassen notes, Putnam is not just seeking to explain why successful explanations are so, he is seeking to explain why science in general is successful in any respect and why, indeed, it is so successful in so many respects. Putnam, then, is not just calling on realism to explain the successes of particular explanations but that of scientific progress in general.

Putnam argues that the success of science stands in need of scientific explanation. In this sense, when the successful applications of scientific theories are considered, two explanations present themselves. Either the success of science is an accident or successful scientific theories are at least approximately true of the world. For Putnam, the overall successes of natural sciences like physics or evolutionary biology are just too great for the accident hypothesis to be plausible. Putnam concludes that, "realism is the only philosophy that doesn't make the success of science a miracle"[49] For the realist, then, according to Putnam, the view that the theories of the mature sciences are approximately true (and that the entities that they refer to exist, even if they cannot be observed) is not a necessary truth. It is the best scientific description of science. The view that the entities referred to by theories exist (in a way which approximately corresponds to the theory's description), then, is "part of any adequate scientific description of science and its relation to its objects".[50]

1-4 b) Conclusions about the Realist Consensus

To conclude, then, the philosophical climate that preceded the publication of van Fraassen's *The Scientific Image* gave rise to a robust realist consensus. That is to say, the dominant position in the philosophy of science essentially holds that to accept a theory implies belief that it is at least an approximately true description of the world and that the entities referred to by theory, directly observable or not, exist. As the discussion of this chapter shows, this position rests on the strength of two main lines of argumentation. The first line of reasoning argues for the failure of the standard empiricist view on the basis that it relies on an untenable distinction between observational and theoretical statements. The second general line of reasoning supports realism directly on the basis of realism's own merits, its ability to account for science's success and its ability to avoid the relativist implications of the socio-psychological approaches to science. Smart, for example, argues that only realism can account for the difference between true and merely useful theories, a distinction he identifies as being important to science. Salmon and Sellars argue variously to the effect that realism is necessary to account for the scientific demand that regularities observed in nature be explained. Boyd holds that only realism can explain the role of theory in the design of experiments and Putnam contends that the overall success of science stands in need of an explanation and only realism can provide it.

While the various positive arguments for scientific realism each focus on different aspects of the scientific enterprise, explanation, experimental design etc., nevertheless, they all converge on one related claim. Each individual argument contends that the non-realist positions are less consistent with actual science either in terms of science's structure or practice than realism is. Essentially the scientific realist holds that to accept science on its own terms implies that one must be a realist. The other views of scientific theories are, for the realist, confronted with a counter example, science itself.

While Fraassen takes issue with both planks of the realist platform, he accepts realism's rejection of the standard empiricist view. Nevertheless, he rejects the untenability of the distinction between observable and unobservable parts of scientific theories and therefore resists the inference to realism on the basis of the failure of the standard empiricist view. He also rejects the positive arguments for realism that argue to the effect that only realism is compatible with science. Van Fraassen's critique of realism and the merits and implications of his own alternative position will be given close consideration in the next chapter.

Notes

[1] The standard empiricist construal of scientific theories evolved from the doctrines of the philosophers in the Vienna Circle in the 1920's. With the exception of Schlick, who favoured realism, the Vienna Circle philosophers came to reject realism and developed a view that was much closer to anti-realim or instrumentalism at least with regard to the theoretical posits of science. However, the abandonment of the early "positivist" doctrines of the Vienna Circle did not immediately imply a general acceptance of realism. As this chapter will show, a number of logical studies investigating the possibility of eliminating theoretical posits from the reconstructed language of science held sway for many decades.

[2] Bas C. van Fraassen, *The Scientific Image*. Oxford: Clarendon Press, 1980, p. 8.

[3] H. Feigl, "Some Major Issues and Developments in the Philosophy of Science of Logical Empiricism", in H. Feigl and G. Maxwell (eds.), *Minnesota Studies in the Philosophy of Science, Vol.I*, Minneapolis: Minnesota University Press, 1956, p. 16.

[4] M. Dummett, *Truth and Other Enigma's*. London: Duckworth, 1978, P. 146. Dummett is a well-known proponent of anti-realism.

[5] Many realist philosophers like Michael Devit and Ian Hacking have tried to show that Putnam's internal realism is not compatible with scientific realism; as my arguments show however, they are mistaken.

[6] *Cf.* H. Putnam, "A Philosopher Looks at Quantum Mechanics", in H. Putnam, *Mathematics Matter and method.* London: Cambridge University Press, 1979, p. 132. Putnam of course was not long able to follow his own advice.

[7] *Cf.* H. Putnam, *The Many Faces of Realism*. LaSalle, Ill.: Open Court Press, 1987, p. 20.

[8] *Cf. ibid.*, p. 20.

[9] *Cf.* Van Fraassen, 1980, p. 219*ff.*

[10] Putnam has in fact never denied this and has even modified his internal realism somewhat to make it more friendly to other forms of realism. *Cf.* "Replies", in *Philosophical Topics*, 20, 1992, pp. 383-98.

[11] To characterise the mature form of the standard empiricist view as it is formulated by Carnap and Hempel I will draw chiefly on Carnap's "The Methodological Character of Theoretical Concepts" in H. Feigl and G. Maxwell (eds.) *Minnisota Studies in the Philosophy of Science. Vol. I.* Minneapolis: University of Minnesota Press, 1956 and from Carl Hempel's "The Theoretician's Dilemma" in C. Hempel, *Aspects of Scientific Explanation*. Toronto: Collier-Macmillan, 1965.

[12] Carnap acknowledges the controversial nature of the concept of observability, but makes the point that if one wished to draw the line one can. The pragmatic nature of the observable/non-observable divide does not mean that there is no divide, it means that such a divide can always be drawn (I will take up this argument in more detail at a later point).

[13] *Cf.*. J. Leroux, *La Sémantique des théories physiques*. Ottawa: Les Presses de l'Université d'Ottawa, 1988, pp. 60-61 [my translation].

[14] Maxwell, "The Ontological Status of Theoretical Entities", in *Minnesota Studies in Philosophy of Science. Vol. III.* Feigl and Maxwell (eds.). Minneapolis: University of Minnesota Press, 1962, pp. 17-19. *Cf.* also W. Rozeboom, "The Factual Content of Theoretical Concepts", in *Minnesota Studies in Philosophy of Science. Vol. III.* Feigl and Maxwell (eds.). Minneapolis: University of Minnesota Press, 1962, pp. 293-299.

[15] Leroux, *op cit.*, p. 60. *Cf.* also Hempel, "The Theoretician's Dilemma" in C. Hempel, *Aspects of Scientific Explanation*. Toronto: Collier-Macmillan, 1965, pp.173-229 and I. Scheffler, "Prospects for a Modest Empiricism", *Review of Metaphysics*, 10 1957, pp. 383-400.
[16] Carnap, *op cit.*, p. 47.
[17] *Ibid.*, p. 47.
[18] *Ibid.*, p. 47.
[19] *Ibid.*, p. 48.
[20] *Ibid.*, p. 49.
[21] Feigel, *op cit.*, p. 16.
[22] Carnap, *op cit.*, p. 65.
[23] *Ibid.*, p. 70.
[24] It is in the restriction of truth to observable parts of the theory that Carnap's account draws closest to van Fraassen's "constructive empiricism". They differ, however, in their account of what is to be taken as a theory. In the standard empiricist view a theory is a collection of syntactic objects, for van Fraassen it is a collection of models which realise a set of sentences. I will explore this difference and its implications on the next chapter.
[25] R. Carnap & R. Jeffrey (eds.), *Philosophical Foundations of Physics*. New York: Basic Books, 1966, pp. 225-226.
[26] *Ibid.* p. 226.
[27] G. Maxwell, "The Ontologocal Status of Theoretical Entities" in H. Feigl and G. Maxwell (eds.), *Minnesota Studies in the Philosophy of Science*. Minneapolis: University of Minnesota Press, 1962, pp. 7-8.
[28] *Ibid.*, p. 9.
[29] Quine does of course acknowledge that the analyticity of some statements is unproblematic. $A \supset A$ is true by virtue of its form. In this case there is only one term A. Quine's concern over the analytic synthetic distinction rests with statements like "Bachelors are unmarried". This case is more controversial than the first because for this statement to be analytic, "bachelor" and "unmarried" need to be synonymous. Thus to show such statements to be synonymous, the concept of synonymy needs to be defined.
[30] *Cf.* W. Quine, "Two Dogma's of Empiricism", *Philosophical Review*, 60, 1953, pp. 20-43.
[31] *Ibid.*, p. 40.
[32] *Cf.* Hempel, 1965 and Hempel, "Formulation and Formalisation of Scientific Theories: A Summery-Abstract" in F. Suppe (ed.), *The Structure of Scientific Theories*. Illinois: University of Illinois Press, 1977 pp. 244-255.
[33] *Cf. Ibid.*, pp. 244-255. The reformulation of the theoretical/observational divide is not the only difference between Carnap's and Hempel's views. Carnap, for example, in his 1956 paper maintained a loose requirement for cognitive significance, namely that L_t make some difference for L_o. However, "loose" that requirement may be. Hempel abandons the distinction completely. However, given the small role this requirement plays in the mature received view and the wide contemporary consensus from all positions within the philosophy of science that it is unnecessary further discussion of this point would be digressive. For a complete review of the reasons for the abandonment of the requirement for cognitive significance *cf.*, Hempel, C., "Concepts of cognitive significance" in Hempel, C., *Aspects of Scientific Explanation*. Toronto: Collier-Macmillan, 1965, pp. 99-123.
[34] *Ibid.*, p. 245.
[35] *Cf.* Suppe, 1977 p. 3-5.

[36] *Cf.* T. Kuhn, "Second Thoughts on Paradigms", in Suppe, 1977, pp. 455-483.
[37] J.C. Smart, *Between Science and Reality*. New York: Random House, 1968, p. 151.
[38] *Ibid.*, p. 105 f.
[39] It must be noted that Reichenbach did not formulate his principle of "common cause" with the specific aim in mind of arguing for scientific realism. Salmon merely argues that scientific realism follows from Reichenbach's work. *Cf.*, W. Salmon, "Theoretical Explanation", in S. Körner (ed.), *Explanation*. Oxford: Blackwell, 1975, pp. 118-45. *Cf.*, also van Fraassen, 1980, pp. 25-29.
[40] W. Sellars, *Science Perception and Reality*. London: Routlege and Kegan Paul, 1963, p. 123.
[41] *Ibid.*. p. 123.
[42] *Ibid.*, p. 121.
[43] *Ibid.*, p. 121-123.
[44] R. Boyd, "Realism, Underdetermination, and a Causal Theory of Evidence", *Noûs*, 7, 1973, p. 12.
[45] *Ibid.*, p. 3.
[46] *Cf. Ibid.*. p. 10.
[47] *Ibid.*, p. 11.
[48] *Ibid.*, p. 11.
[49] H. Putnam, "What is Mathematical Truth" in H. Putnam, *Mathematics Matter and Method*. Cambridge: Cambridge University Press, 1975, p. 73.
[50] *Ibid.*, p. 73.

Chapter 2: Van Fraassen's Constructive Empiricism and its Aftermath

We outlined in the last chapter the reasons for the widespread acceptance of realism and emphasised that realism is purported by its supporters to be the best alternative to the problematic standard empiricist view. However, the work of van Fraassen offers a robust alternative to realism to replace the standard empiricist view. Van Fraassen dubs his position, which he advances in the context of a thoroughgoing critique of its realist rivals, "constructive empiricism". In this discussion I provide a critical analysis of constructive empiricism and the responses it has elicited from realist critics. I shall argue that van Fraassen's critique of realism is on the whole valid. However, it will be made clear that constructive empiricism is not ultimately an adequate resolution to the question of scientific realism.

The arguments supporting the conclusions mentioned above will revolve around the observation that van Fraassen's contention that theories only compel belief in their observable components rests on arbitrary distinctions. That is, van Fraassen draws problematic distinctions between observable and unobservable entities and empirical and super-empirical (pragmatic) aspects of theories. First, I shall outline van Fraassen's constructive empiricism in the context of his critique of realism. Subsequently, I shall consider some of the criticisms that van Fraassen's work has elicited. I contend that, while most of the criticism of van Fraassen's misses its mark, a more adequate look at van Fraassen's position reveals that it is, in sum, an unsatisfactory replacement for realism. These conclusions will be presented with the aim of presenting a survey of what pitfalls a satisfactory account of the scientific realism question should avoid.

Van Fraassen's strategy is twofold. His constructive empiricism seeks to re-establish the validity of the essential tenets of an empiricist approach to science while taking into account the criticism of traditional empiricism provided by realist philosophers of science. To explore this strategy it is necessary to state explicitly the basic precepts of what he defines as realism and what he defines as empiricism.

Recall the main differences between realist and anti-realist pictures of scientific activity. When a scientist advances a new theory, the realist sees him as asserting the truth of the postulates. But the anti-realist [constructive empiricist] sees him as displaying this theory, holding it up to view, as it were, and claiming certain virtues for it.[1]

This theory draws a picture of the world. Science itself designates certain areas of this picture as observable. The scientist, in accepting the theory, is asserting the picture to be accurate in only those areas. This is, according to the anti-realist, the only virtue claimed which concerns the relation of theory to world alone.[2]

Acceptance of a theory thus compels belief only in as much as it is "empirically adequate".[3] That is to say, the part of the world that the theory designates as observable is comprised of entities that are (or at least can be) observed. Constructive empiricism holds that the empirical adequacy of a theory can only lead to the belief in the reality of the observable portion of the theory. It does not lead to the belief in the overall reality of the whole theoretical picture offered by the theory. For the realist, the opposite is the case. Acceptance of the theory (for the realist) implies belief in the truth of the picture of the world the theory implies, including belief in the entities that are not observed and are even unobservable in principle.

It is important to note that, for the empiricist, acceptance of a theory is not just a matter of its empirical adequacy. Two incompatible theories can be equally empirically adequate. Thus acceptance can rest on non-empirical or "pragmatic" considerations (simplicity, explanatory power, ease of use, etc.). Nevertheless, these considerations only factor in theory choice and carry no ontological warrant with regard to unobservable entities. For the constructive empiricist, they do not imply the truth of any part of the theory, they only relate to a theory's pragmatic value or weakness. Only a theory's empirical content can warrant belief, and this belief relates only to empirical adequacy.

At first sight, empiricism does seem more plausible than realism. If all we can ever have access to is observed phenomena, how could we ever know if we were correct about its underlying structures? This becomes especially acute if more than one theory is equally good at accounting for the phenomena in question. But the picture is not so simple. Indeed, traditional versions of empiricism have been roundly attacked by realists and, as I have indicated above, van Fraassen finds himself in agreement with some of this criticism. Much of this criticism relates to the empiricist distinction between the theoretical and observational.

2-1 Van Fraassen's Critique of Realism

Van Fraassen identifies Grover Maxwell's 1962 article "The Ontological Status of Theoretical Entities" as the starting point for most recent accounts of scientific realism. In this article Maxwell attacks Carnap's idea that the language of a theory can be logically reconstructed as having a theoretical part and an observational part devoid of so called theoretical terms.[4] For Maxwell, the idea that language can be divided into a theoretical and non-theoretical part is a false one. Van Fraassen finds himself in total agreement with Maxwell on this point, "The way we talk and the way scientists talk is guided by the pictures provided by previously accepted theories... [h]ygenic reconstructions of language... are simply not on".[5] As is outlined in the previous chapter, realists, following Maxwell's arguments, generally hold that in the absence of a theory-free observation language, empiricist agnosticism about theoretical entities is incoherent.

In this last respect van Fraassen and the realists could not be farther apart; but it is important to note that van Fraassen does support the main plank in the realist platform. It becomes important to consider the inadequacies van Fraassen finds in scientific realism. Van Fraassen argues that Maxwell's argument becomes muddled between two distinctions: the theory-laden/theory-free language distinction and the observable/unobservable distinction. For van Fraassen, the theory-ladenness of language does not entail the collapse of the second distinction.[6] A theory-laden language can still define something that is observable in principle, and something that is not, "a flying horse is observable in principle, that is why we are so sure there aren't any... and a calculation of the mass of a particle from the deflection of its trajectory in a known force field is not an observation of the mass".[7] In any event, even if the theory-ladenness of scientific language did imply the invalidity of the distinction in question, this need not imply realist belief in the whole world structure of a scientific theory. Language that is impregnated with theories that are known to be false is used all the time, "when we speak of the sun coming up in the morning and going down at night, we are guided by a picture that is now explicitly disavowed".[8]

In addition to arguments relating to the theory/observation distinction, van Fraassen treats several other arguments that are advanced in order to support scientific realism. Van Fraassen finds examples of these in the writings of Sellars, Smart, Salmon and Harmann. All of these proceed from or bolster the essential realist point that to accept a theory is accept the whole theory as true. One such argument associated with Harmann, is dubbed by van Fraassen the "inference to the best explanation." This argument essentially holds that the canons of rational inference require scientific realism.[9] Of course, the central rule in play here is one calling for inference to the best explanation. This rule essentially states that given two hypotheses that each explain a certain body of evidence, we should infer the hypothesis which provides the best explanation of that evidence. This implies complete acceptance of the hypothesis (and therefore realism) because this rule is ubiquitous not only in science but in everyday practical reasoning. Van Fraassen has two objections to this line of reasoning.

The argument from inference to the best explanation is predicated on the ubiquitous nature of this rule, but van Fraassen argues that the claim is ambiguous. Does this claim imply that the rule is a logical rule that everyone knows and consciously applies or is it a psychological theory, and therefore an empirical hypothesis in its own right? The first possibility is clearly wrong because many people simply do not consciously employ logical principles in their everyday life and indeed cannot even formulate such rules properly. Thus, the claim seems to be a psychological thesis concerning the way people solve problems. Van Fraassen proposes an alternative theory (equally psychological) to the effect that people make inferences to the most empirically adequate explanation. While either idea could be incorrect, in van Fraassen's view, his proposal shows that such ideas are just as compatible with empiricism as they are with realism.

Van Fraassen's second objection is somewhat more telling. Granted that inference to the best explanation is valid, this applies only to those situations where one has to choose between hypotheses that try to explain the same evidence. What if the hypotheses are each empirically consistent with the evidence but do not try to explain it? The realist needs to show that hypotheses must explain observational regularities and not just accurately describe them. For example, an hypothesis that draws a correlation between two factors but offered no explanation of this correlation might be empirically adequate in van Fraassen's sense and therefore worthy of acceptance. This hypothesis would be inadequate from the point of view of the realist demand for inference to the best explanation. Van Fraassen notes, then, that the realist argument from the rule of inference to the best explanation carries an undefended hidden premise that states that regularities in nature must be explained and that empirical adequacy is not enough to accept an hypothesis.[10]

Van Fraassen's identification of this realist assumption starkly illuminates a central feature of the divide between the realist and constructive empiricist epistemic stance. Empiricists do recognise explanatory power as a virtue. All things being equal, it can serve as a way of making a pragmatic distinction between two competing hypotheses. However, for the empiricist, explanation of phenomena can differ from accounting for them empirically. That is, an explanation of phenomena in terms of their theoretical (unobservable) processes may well be useful but no belief is warranted in these processes. For the realist, to accept an explanation is to accept it as true (with its unobservable component intact). In this sense, explanation is intimately tied with realist notions of truth and the goals of science. For the realist, science seeks and provides true explanations for what we observe in nature, namely regularities that stand in need of explanation. For an empiricist, the goals of science are characterised quite differently. Science seeks hypotheses that are consistent with what we observe, anything else is tentative, if often quite useful, speculation. It is in precisely this sense that van Fraassen characterises explanation as a super-empirical value and as having no implications for truth. Moving from this position, then, van Fraassen criticises realist arguments that demand explanations for all observed regularities in nature.

As we have seen, the realist holds that it is the central business of science to provide explanations for the regularities that are observed in nature. One way of thinking about this demand is to see it as claiming that the task of science is incomplete if it leaves coincidences unexplained. Van Fraassen quotes the realist philosopher Smart's formulation; science must explain the regularities in observed phenomena in terms of "deeper" structures, otherwise we must believe in "coincidences on a cosmic scale".[11]

This, van Fraassen argues, is not only a mistake, but also leads to absurdity when fully formulated. There are, of course, lots of complete coincidences that stand in no need of explanation. For example, if two people were to go to the market for unrelated reasons and meet, this would be such a case. That is, each person would have individual (and unrelated) reasons for going. Thus, their meeting, while fully explicable, would be a complete and irreducible coincidence.[12] While the search for "common causes" in cases like this may be unnecessary, such cases hardly amount to the regularities seen in nature that Smart has in mind. However, van Fraassen notes that even this weaker demand leads to confusion.

The implications of the demand for the explanation of coincidences can be exemplified by a quick look at Reichenbach's treatment of statistically correlated events. Reichenbach was one of the first philosophers of science to appreciate the probabilistic turn in modern physics. He recognised that deterministic laws were inadequate for the descriptions of the causes behind purely statistical regularities. Nevertheless, he urged that scientists can still formulate statistical (probabilistic) laws lying behind these regularities. Reichenbach argued that two positively correlated factors share a common cause C such that no correlation would be seen to exist in instances where C is absent.[13]

An example of such a relation exists between heavy smoking and lung cancer. A positive correlation exists between the two such that the product of the probabilities of being a heavy smoker (without cancer) and a cancer victim (without heavy smoking) is less than the probability of being a heavily smoking cancer victim. This correlation requires a common cause (in this case, long term heavy smoking in the past) such that the same correlation is not noticed between lung cancer victims and short term smokers. For the realist, all such correlations stand in need of common cause explanation. In the cases where no common causes are observed, unobserved ones must be inferred.[14]

However, van Fraassen argues that this last point is incorrect, pointing out that there are statistical correlations that do not have common causes. To make this point he borrows an example from modern physics, the so-called "Einstein-Podolsky-Rosen Paradox" (EPR). While EPR will be considered in considerable detail in the next two chapters, its use by van Fraassen to respond to the realist demand for explanation warrants a brief digression to roughly describe the basic structure of the paradox in order to illuminate van Fraassen's argument. Van Fraassen points out that the correlations exhibited in the paradox have been observed in many actual experiments. In the EPR situation an initial state S undergoes a change into a new state characterised by attributes F and G. F and G are not individual attributes but sets of attributes such that if the F attribute is F_1 then the G attribute will be G_1, F_2 will correspond with G_2 and so on. However, when the transition occurs the particular F_n/G_n correlation that results will be generated indeterministically. Thus, n will not be caused, and the correlation $F_n G_n$ will not have a common cause if S is a complete description of the initial state.[15]

Given that science must deal with cases like the EPR paradox, the realist must either accuse science of describing such situations incorrectly or give up the demand for common cause. Some realists have indeed tried to formulate hidden cause explanations of the EPR paradox, but these have proved experimentally inferior to their indeterministic non-explanatory rivals. Therefore, van Fraassen concludes that the demand for a common cause for all statistical coincidences is invalid. In cases like the EPR paradox, then, explanation plays no role at all, only empirical adequacy factors in.

None of this is to say that van Fraassen totally rejects the demand for common cause in all cases, only the demand that all regularities be so explained is rejected. For the constructive empiricist scientific activity is, "aimed at greater knowledge of what is observable".[16] In cases like the smoking example, common cause explanations do contribute to the accumulation of such knowledge. Van Fraassen concludes that while common cause explanation might have some value as a "tactical maxim" on a case by case basis, it cannot serve as a regulatory principal, "for all scientific activity".[17] And above all, it cannot support a realist argument for the belief in unobservable structures.

The last standard argument for realism that van Fraassen deals with is the "ultimate argument" which he attributes to Putnam.[18] This argument bears some resemblance to the cosmic coincidence argument of Smart. It states that realism is the only philosophy of science that does not make the success of science a mystery. That is, this success is a regularity which stands in need of an explanation. However, it is not, as van Fraassen points out, exactly the same as the demand that all coincidences be explained because it demands an explanation for why there is any successful science at all. For Putnam, the reason for this is simply that unless scientific theories are true (or at least approximately so) science's success would be inexplicable.

To this line of reasoning van Fraassen responds with a "Darwinian" view of scientific success. Briefly, van Fraassen argues that if the responses to nature advocated by science had not turned out to be true, then they would have long ago proved useless and would have been discarded. Naturally, very many hypotheses have suffered exactly this fate. This is to say, only the fittest theories survive. No particular explanation is thus required to explain why one theory proved to be more fit than another. For a Darwinist, species that are not fit do not survive, for van Fraassen theories that are not true do not last and nothing further need be said.[19]

2-2 Constructive Empiricism as an Alternative to Realism

Van Fraassen's critique of the arguments for realism compel rejection of the central thesis of realism; accepted theories must be accepted as true (unobservable entities included). However, as we have seen, van Fraassen does accept much of the realist criticism of traditional empiricism. Most importantly, he accepts Maxwell's thesis that a theory-free observation language is impossible. Thus, constructive empiricism must provide a viable alternative for realism without falling prey to the realist criticism that it accepts. Therefore, before considering objections to constructive empiricism it is worth looking closely at the tenets of constructive empiricism that permit these criticisms to be avoided. These tenets of constructive empiricism revolve around van Fraassen's defence of the observable/unobservable distinction and his delineation of the super-empirical merits (or liabilities) of theories.

The central feature of constructive empiricism that differentiates it from traditional empiricism lies in its replacement of "syntactical" constructions of scientific theories with "semantic" constructions.[20] The essential difference between the two is that in the syntactic approach the theory is entirely a linguistic entity, it views a theory as a set of related sentences. On the other hand, the semantic approach sees a theory as a conceptual, extralinguistic entity. In the semantic view a theory is construed as a propounded abstract entity serving as a model for the set of interpreted sentences that constitutes its linguistic formulation. That is to say, a theory is the set of models for which a set of related sentences is true. This amounts to more than just a shift in terminology because "the class of structures (models) could well be [linguistically] described in radically different ways".[21]

One example of this might be the various linguistic formulations that the models of classical particle mechanics admit: the Newtonian formulation, the Lagrangian formulation and so on. This change from language to models amounts to a major shift in emphasis. While the syntactic approach views theories as entirely linguistic entities (sets of sentences), the semantic approach sees theories as models, i.e. structures which are (and such is the claim of scientific theories) empirically realised. From a syntactic point of view different theoretical languages amount to different theories, this is not so from a semantic point of view as long as the different theoretical languages share models.

It is worth digressing briefly at this point to highlight how van Fraassen's semantic view of theories casts the difference between constructive empiricism and the standard empiricist model into sharp relief. Recall that in Carnap's formulation of the standard empiricist view, scientific theories admits a division into a part that is observable and another that is not. In the standard model, direct confirmation can only come for the observable portion of theories. This implies that for an accepted theory only the observable portion of the theory can be known to be true, the unobservable part can only be tentatively accepted. Constructive empiricism mirrors these features of Carnap's version of the standard empiricist view exactly. In fact the only real difference between Carnap and van Fraassen lies in their view of what constitutes a theory: for Carnap, of course, a theory is a set of related sentences, it is a purely linguistic thing. It is just this aspect of the standard view that van Fraassen rejects, for him the theory is a set of models for which a set of sentences are true. This difference amounts to more than a reversal of what counts as a theory precisely because the location of the partition between the observable and unobservable partition shifts in constructive empiricism from the language to the models.

From van Fraassen's point of view, the syntactic approach's preoccupation with language lies at the root of its difficulties. Consider the distinction between theory and observation criticised by realists like Maxwell. The syntactic view of a theory's delineation of its observable content is vulnerable precisely because the distinction is presented as a distinction between different parts of language. Van Fraassen, of course, rejects this on the basis of the theory-ladenness of all language. He also points out that purely theoretical entities can be described in terms that, from a syntactic point of view, would reside in the observational part of a theory's language.[22] For example, in modern quantum mechanics there are theoretical entities which can be described as having sometimes a position in space and sometimes not.[23] The syntactic view of theories simply cannot sustain the distinction in question. In the end it degenerates into, "a hobbled and hamstrung... description by T (theory) of everything".[24]

According to van Fraassen, the observable/unobservable distinction must be drawn in the models themselves and not in language. Within the context of the model, observations would be delineated from theoretical structures and from the connections between themselves and purely theoretical structures. This is so because the distinction is not an *a priori* distinction but a distinction drawn by science itself. What can and what cannot be observed is delineated by the scientific theories themselves. Thus, scientific theory determines what is observable, but not in terms of an *a priori* distinction between parts of the theoretical vocabulary but by modelling the limits of human sensory ability.

Any model would define its parts in terms of what science dictates as possible to observe.[25] Assuming the empirical adequacy of these theories, then, the models contain the distinction as a structural feature. In this sense, constructive empiricism maintains its commitment to the core of empiricism while accepting the realist critique of the theory/observation split. This critique is aimed, for van Fraassen, squarely at language-fettered syntactic constructions and not at the semantic structures of constructive empiricism. "Empirical adequacy", constructive empiricism's hallmark of a good theory, relates to the parts of models that are delineated as observable.

A model, of course, can have other virtues such as simplicity, ease of use, explanatory fruitfulness, etc. but these are clearly delineated by the model as extra-empirical considerations. They may be important in deciding between equally empirically adequate models but they cannot compel belief because the structure of models stipulates what is observable and "explanatory power" is clearly not observable. Indeed, it is not a thing at all but simply relates to the deployment of theories for the solution of "why questions". In so far as explanation relates to the deployment of theories, explanation, to be successful, relies on the deployment of good theories. For van Fraassen the search for explanations is a search for empirically adequate, empirically strong theories.[26]

2-3 Problems of Constructive Empiricism

Critics of van Fraassen have chiefly attacked his distinction between observable and unobservable entities. But many of these criticisms miss their mark through a failure to appreciate the role of van Fraassen's view of theories. A characteristic example is Ian Hacking's discussion of how we "see" through a microscope although others like Schlagel, Dobson, Forest, Thorten and Melchert make closely related points. In this discussion, I will not treat each critic of van Fraassen individually. The large majority of criticisms proceed along closely related lines and a very large quantity of material arguing for the same conclusions has been published. Instead, I will deal with this literature by addressing characteristic examples of the main lines of criticisms of van Fraassen's position that have emerged.

Hacking argues that we do not properly "see" through a microscope at all, noting that a microscope (even a light microscope) does not operate on the same principles as either the eye or the magnifying glass. In this he is quite correct. However, he goes on to point out that confidence in the microscopically viewed arises from our ability to interact successfully with the viewed and indeed this principle of interaction has a corollary with the ordinarily experienced visual world of the eye. He notes that even with our binocular vision, the world only appears to us in three dimensions when we move around in it. Thus, our confidence in ordinary vision arises from the same principle of interaction that gives confidence in microscopic viewing. And Van Fraassen's epistemological distinction is thereby invalid.[27]

Hacking's points about the structure of microscopic "seeing" and its difference from ordinary viewing are, of course, quite in keeping with van Fraassen's demand that science, not philosophy, model what can and cannot in principle be observed. In addition, van Fraassen's semantic analysis is hardly one that characterises observation as a purely passive activity. Experimental interactions, measurements and all sorts of other activities are all part and parcel of the models in question. Further, the facilitation of successful interaction does not make a theory true. Newton's theories permit many a successful "doing" but eventually breaks down empirically. In this sense, "control power" of a theory must have no more epistemic warrant than explanatory power.[28]

Churchland seems to be making a similar error when he asks us to consider a community whose eyes are electron microscopes.[29] Are their perceptions to be considered empirically adequate or not? But this is to miss the essence of van Fraassen's position. What is observable, for him, is what is modelled by science as being capable of being seen (or heard etc.) by the epistemic community (ordinary human beings). If the microscope-eyed constituted the whole epistemic community then they would very likely model the limits of observation differently than science currently does. However, if they were just ordinary people fitted with special equipment, the validity of their "perceptions" would depend on acceptance of theories covering the reliability of their equipment. For van Fraassen, this acceptance would entail the theory's empirical adequacy not its complete truth.

Van Fraassen's distinction between empirical and super-empirical values has also attracted criticism. Hooker, for example, has argued that van Fraassen's contention that "super-empirical" or pragmatic values have no cognitive warrant is false. For Hooker, empirical adequacy can be seen as just as much of a poor a reason to believe a theory as explanatory power is, given our fallibility as observers. Given that van Fraassen acknowledges this fallibility, Hooker cannot see how van Fraassen can argue that observations provide greater cognitive warrant than pragmatic considerations (like explanatory power). The case for van Fraassen becomes worse when one considers van Fraassen's reliance on semantics to establish his notion of truth and empirical adequacy. Hooker notes that van Fraassen identifies semantics as an abstraction from pragmatics.[30] That is, semantic relations between an expression and the world really boil down to pragmatic relations between expressions and their users. If semantics are really just pragmatics, how can semantic notions be given greater cognitive force than pragmatic ones?

These arguments, like those directed at the observable/ unobservable distinction, miss their marks because of a failure to appreciate the significance and nature van Fraassen's semantic characterisation of theories. For van Fraassen it is true that language is fully theory-laden, and that to accept a theory is to accept its language and to use it.[31] However, as van Fraassen's semantic approach points out, many different theoretical languages could share models (that is a model can admit a variety of linguistic formulations). While van Fraassen accepts Hooker's point that all aspects of a theory's language ultimately rest on relations between language and its users, the language of a theory is, for constructive empiricism, adopted only after a model has been accepted as empirically adequate. While pragmatic considerations might play a role in the choice of a particular theoretical language, ontological belief is only warranted in the empirical adequacy of the models. This distinction between pragmatics and empirical considerations holds because a theory's empirically observable component is, in constructive empiricism, supposed to be delineated in the model and not in its linguistic formulation. The languageindependence of models, then, isolates empirical adequacy from the context-dependent and thoroughly pragmatic nature of theoretical language.

It has become clear at this point that van Fraassen's distinction between observables and unobservables and his distinction between empirical and super-empirical values lie at the root of constructive empiricism. Further, it has also become clear that much of the criticism levelled at these distinctions fails because of a failure to appreciate van Fraassen's semantic approach to science. Trenchant criticism of constructive empiricism and its characteristic distinctions, then, ought to take an axe to the root of the position and examine directly the merits of van Fraassen's use of the semantic approach.[32]

For Arthur Fine, difficulties arise in the manner that constructive empiricism isolates the observable component of a model. With regard to the delineation of the observable part of the model, "science is supposed to speak ... not philosophy".[33] Thus, scientific ideas establish what sort of things can be judged observable. For Fine, confusions arise when one closely examines the form of such judgements. Fine considers the example "carrots are mobile", for this claim to be considered observable and, therefore, to be a candidate for belief, mobility and carrots must both be judged observable. Now consider the claim "carrots are observable", is this a candidate for belief? If it is, observability must be scientifically determined to be observable. Fine points out that science simply has not provided such a judgement about observability and that van Fraassen's distinction is an *a priori* philosophical one.

Fine's arguments are not fully telling, van Fraassen could argue that Fine has become confused about the way constructive empiricism treats observability. Van Fraassen argues that models do not include "observability". For van Fraassen they delineate it. It is a linguistic artefact of theoretical language acquired after the theory is accepted because of the empirical adequacy of its models. In this sense, things like "mobility" and "observability" would be just ways of talking after the model has been accepted and are not themselves parts of the model. They are, therefore, not candidates for being designated observable. An entity *x*'s "ability to be observed" amounts to no more than the recognition that "*x* is observed in at least one model". Therefore the main question to ask of constructive empiricism then is how the difference between a detection or an inference is distinguished in constructive empiricism from an observation.

As I noted earlier, van Fraassen argues that scientific theories (theoretical models) whose intended domain is the nature of the human perceptual apparatus determine what ordinary humans can and cannot observe, hear and smell. Thus, in these models a pitch that is of a particular frequency would be modelled as unobservable and so also would be electromagnetic radiation of too high or too low a frequency. These distinctions would be based on the way the human ear and eye are modelled in the relevant theory of perception. For van Fraassen the existence of theoretical models that delineate the human perceptual capacity ensure that the observable/unobservable distinction is neither arbitrary nor variable from theory to theory. "I regard what is observable as a theory-independent question. It is a function of facts about us *qua* organisms in the world... there is not the sort of theory-dependence that could cause a logical catastrophe".[34] Thus, by delineating the distinction in this way, van Fraassen hopes to avoid accusations of argumentative circularity while maintaining his denial that the observable constituents of scientific models are theory-laden.

For van Fraassen, locating the distinction on the basis of the findings of theories of perception alleviates the possibility that his distinction relies on the argumentative circularity to the effect that a model determines the distinction on the basis that the distinction is a feature of that model. Of course, van Fraassen recognises that his distinction would fail if the argument supporting it were to presuppose some of its conclusions. Van Fraassen appreciates that this would indeed be the case if the distinction in any given model were to be based on features of that very model. For example, a distinction that placed electrons in the unobservable part of a model would hardly be convincing if this was justified on the basis that electrons are deemed unobservable in that model. Deriving the distinction in this way would also mean that no general account of observability and thus empirical adequacy could be given because the location of the distinction would vary from model to model. The observability of a particular entity like electrons might very well vary from model to model in this situation. Van Fraassen tries to avoid these problems by deriving the distinction from the results of theories that study the limits of the human perceptual capacity. That is to say every empirically adequate model would base its observable/theoretical distinction on the results of current theory about what humans can and cannot see, hear etc. This approach has the limitation of restricting the applicability of the distinction to human beings and raising the possibility that it might be modified with time in the event that theories of, for example, the eye and ear are revised. However, these concerns simply highlight the scientifically (not philosophically) based nature of the distinction. The distinction delineates what science can observe and it is humans, limited by their perceptual capacity, who practice science. Furthermore, revision of judgements about perceptual capacity would simply serve as the basis for the revision of the distinction in the various models of science, it would not mean that no distinction is there to be drawn. Constructive empiricism is hardly committed to the idea that the current models of science are the last word, merely that they are empirically adequate to current data.

However, when van Fraassen's strategy is closely examined it becomes clear that he has avoided one circularity only by falling into another. Theories about human perceptual capacities rely on other theories' unobservable structures in order to delineate what humans can and cannot observe. For example, a theory that determines what sort of light can be perceived by the human eye draws from contemporary theories of light that employ theoretical structures like wavelength. After all, what humans see is colour not wavelength and what humans hear is pitch not frequency. The case becomes worse with other sensory abilities like taste and smell because they rely on such obviously theoretical concepts like the presence of molecules of a particular sort or even of a particular shape. In other words, belief in the theoretical/observable distinction that van Fraassen proposes presupposes an acceptance of theories about theoretical structures like electromagnetic radiation, molecules and invisible vibrations in the atmosphere.

A constructive empiricist might well respond that this circle is not a vicious one on the basis that there is nothing illegitimate about the fact that theories rely on one another for mutual support. To draw an analogy to the linguistic formulations of scientific theories, just as the statements of science must be taken as a corporate body and not in isolation, so too must the various theories of science. The fact that a theoretical model about photons might draw a structural feature from a model about perception that in turn draws some of its constituents from the model of photons simply highlights the mutual interdependence of theories. It does not necessarily identify a circular argument. While there is no question that theories are indeed mutually supportive and that there is nothing necessarily problematic about this, the structure of particular theories must not rely on a straightforwardly circular argument. When the nature of the mutual reliance between theories that the observable/theoretical distinction is based upon is considered it becomes clear that this is indeed the case.

Consider the role of a theoretical entity like the photon in models about the eye and in models about the nature of light. Light of course is supposed to be composed of photons, and the eye is supposed to function by light (photons) striking its retina. Therefore, in the case of theories of light, the observable/theoretical distinction derives from theories about the eye, but this theory in turn makes use of theoretical entities from theories of light. Thus, the justification of the observable/theoretical distinction in theories of light is circular in so far as entities from theories of light are also entities in theories about the eye. Very similar arguments apply to other theories about human perceptual capacity and their role in the observable/theoretical distinction in the various theories of science. Similar arguments would apply, for example, to the relations between theories about vibrations travelling thought the atmosphere and theories about the workings of the ear, theories about smell and molecular structure and so on.

The case becomes worse when one considers the question as to how the observable/theoretical distinction is to be drawn in theories about human perception. According to constructive empiricism to accept any theory is to accept only that it is empirically adequate. That is to say that ontological warrant is only granted to belief in its observable portion. Naturally this also applies to theories about the limits of human perceptual capacities. Likewise, their delineation of the observable/theoretical distinction will rely on the findings of theories about the limits of human perception, as is the case with any theory. In this particular case, however, the distinction in theories about the limits of human perception must be based upon their own findings. The distinction in these theories, therefore, must be justified on the basis that it follows from exactly these theories. The distinction in this case, then, is necessary in order to determine the empirical adequacy of the particular theories in question. Yet the fact that the theories are empirically adequate is what justifies their acceptance and therefore the validity and location of the observable/theoretical distinction. This, of course, is exactly the sort of internal circularity that van Fraassen claims that constructive empiricism is supposed to avoid.

Van Fraassen is correct in his contention that the semantic approach to theories does avoid some of the difficulties that attend the lexical distinctions that the standard empiricist view seeks to establish. Furthermore, if the semantic view of theories is accepted then the linguistic formulation of a theory that one chooses to employ is simply only one of several possible linguistic formulations that the theory will admit. Van Fraassen is also correct in his contention that in the semantic view the theory is to some extent language-independent. However, trying to rely on modern theories about human perception to fix the theoretical/observational distinction presupposes the acceptance of the theoretical parts of other theories. Worse, the distinction in the case of the theories of perception themselves rests upon a vicious circularity, as I demonstrated above. In this sense, some of the difficulties with a lexical distinction between the observable and the theoretical are mirrored when an attempt is made to relocate the distinction in the theoretical models. Just as the whole of the linguistic formulation of a theory will be theory-laden, acceptance of the way the human limits of perception is modelled presupposes the acceptance of much that will make up the theoretical content of those and other models. Given this any model based distinction, like any linguistically based distinction, will ultimately need to be arbitrary.

The failure of constructive empiricism might lead one to ask; do these criticisms of van Fraassen force us back into Maxwell style realism? It must be pointed out that criticism of the "language free" account of models has no bearing on many of van Fraassen's attacks on the traditional arguments for realism. The demand for an explanation of science's success, for example, holds no more force if an observable/unobservable distinction is arbitrary than if it is sound. Of course, a realist might reply that if the observable/unobservable distinction is rejected, then to accept a theory is to accept it *en bloc*. After all, to reject van Fraassen's delineation of observability amounts to rejecting his condition that models are separable from the difficulties that attend the theoretical language. However, given that different linguistic formulations can share the same model, how is a realist to decide which theoretical language to accept as true?

The result of these reflections seems to be that van Fraassen is correct in rejecting realist metaphysics (i.e. belief in the truth of unobservables) but because he retains realist view of truth difficulties arise with his view. For van Fraassen, "a statement is true exactly if the actual world accords with this statement".[35] The project of constructive empiricism was to avoid the difficulties associated with realism by restricting access to this truth to only the observable parts of theories. If my arguments are correct, then the failure of constructive empiricism does not imply a return to realism but the need for a new account that rejects the notion that belief always entails belief in "correspondence with the actual world". This demonstrates very nicely the basic features of a more satisfactory "deflationary" approach. The central difficulty of constructive empiricism lies with its use of the semantic approach to seek a unified picture of science. That is to say, the claims that science provides a clear distinction between the observable and unobservable and takes as its main aim the construction of models that are empirically adequate cannot be sustained. However, van Fraassen's idea that the realism debate is to be solved by looking to science itself is sound. Difficulties arise with constructive empiricism when it tries to generalise its arbitrary observation/theory distinction and its goal of empirical adequacy to a general account of science. This too seems to fly in the face of established scientific practice because in some cases, explanation is the goal and in others, like quantum mechanics, it is not. The main feature then of a successful approach to the realism question is the rejection of *a priori* conceptions of science that are supposed to apply to all cases and the demand that science be analysed on a case by case basis in its own context. Given this, the subsequent chapter will outline two characteristic and fully-formed versions of what could be termed "deflationary philosophies of science".[36] The adjective "deflationary" refers to the fact that these accounts eschew general interpretations of whether or not belief in unobservables is warranted. In a related way they also typically eschew general philosophical interpretations of scientific truth.

Notes

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[1] Van Fraassen, *Scientific Image*, Oxford: Oxford University Press, 1980, p. 10.
[2] *Ibid.*, p. 10.
[3] *Ibid.*, p. 157.
[4] *Cf. Ibid* p. 14 Maxwell had in mind Carnap's argument in "The Methodological Status of Theoretical Concepts", Op cit., pp. 38-77. Maxwell's article is in *Minnesota Studies in the Philosophy of Science* Vol. III, Minneapolis, Minnesota University Press, 1960, pp. 3-28.
[5] *Op cit.*, p. 14. Van Fraassen is of course alluding to Otto Neurath's famous analogy of "washing the baby". Neurath held that science should be cleansed of metaphysical contamination and he was convinced that, in this case, there was no danger of throwing the "baby out with the bathwater." Van Fraassen, of course, denies this.
[6] Maxwell does offer some arguments that address the observable unobservable distinction directly. Briefly, he argues that a continuum exists between the two and thus no, distinction can be drawn. Van Fraassen responds that even if this is so there are still clear cases of both, which is all constructive empiricism requires. (*Cf. Scientific Image* pp. 15-17 and also *op cit* Maxwell p. 7) Maxwell's other arguments relating to this point are similar to those employed by van Fraassen's critics (chiefly Churchland and Hacking) and these will be addressed at a later point.
[7] *Ibid.*, p. 15.
[8] *Ibid.*, p. 14.
[9] *Ibid.*, p. 19.
[10] *Cf. Ibid.*, p. 21.
[11] *Ibid.*, p. 25.
[12] *Ibid.*, p. 25.
[13] Cf. H. Reichenbach. *Modern Philosophy of Science*. London: Routledge and Kegan Paul. 1959, ch. 3.
[14] *Cf. Ibid.*, ch. 3 and van Fraassen. *Scientific Image*. pp. 26-28.
[15] van Fraassen. *Scientific Image*. p. 29
[16] *Ibid.*, p. 31.
[17] *Ibid.*, p. 31.
[18] *Ibid.*, p. 39.
[19] *Ibid.*, pp. 39-40 In these passages van Fraassen's use of the term truth is very similar to the realist use. However, I think we can take him to mean that empirically adequate theories that prove useful and stand the test of time may well be in fact true (in the realist sense) but this requires no explanation. In any event, van Fraassen is clear enough elsewhere about knowing the truth of theories. he stipulates that only the empirical component could be known to be true.
[20] *Ibid.*, p. 47. It should be emphasised at this point that the semantic view of theories does not necessarily imply a version of anti-realism like that of van Fraassen. Van Fraassen's conviction that the theoretical models admit a partition between an observable and unobservable part serves as the basis of his arguments for his version of anti realism. Suppe who argues for a version of the semantic view draws no similar distinction and argues for a version of realism. Indeed the question over the general merits of the semantic view then is somewhat tangential to the realism question which is the main question of this thesis. Given this, I will deal with the semantic view only insofar as it is germane to the use that is made of it in van Fraassen's position.

[21] *Ibid.*, p. 44.
[22] That is, such observational terms relate to position, direction of movement, duration, etc.
[23] *Ibid.*, p. 54.
[24] *Ibid.*, p. 55.
[25] *Ibid.*, *Cf.* p. 58.
[26] *Cf. Ibid.*, p. 115.
[27] *Cf.*, Ian Hacking, "Do We See Through a Microscope" in *Images of Science*, P. Churchland & C.A. Hooker (eds.) Chicago: Chicago U.P., 1985, pp. 151-2.
[28] *Cf.*, *Op cit.*, p. 156.
[29] *Cf.*, P. Churchland, "The Ontological Status of Observables: In Praise of Superemperical Values" in Churchland & C.A. Hooker (eds.), 1985, pp. 35-46.
[30] van Fraassen, *Op cit.*, p. 89.
[31] *Ibid.*, p. 199.
[32] D. Hausman, for example, has argued that the advantages or merits of constructive empiricism rest in the use it makes of the semantic approach. *Cf.* Hausman, *The Inexact and Separate and Science of Economics*, Cambridge: Cambridge University Press, 1994, pp. 72-73.
[33] A. Fine, *The Shaky Game*, Chicago: Chicago University Press, 1986 p. 144.
[34] *Op cit.*. pp. 57-8
[35] van Fraassen, *Op cit.*. p. 90.
[36] A. Fine, *Op cit.*, p. 123 f.

Chapter 3: Piecemeal Realism, NOA and Deflationary Metaphysics

We have shown that while van Fraassen's critique of the standard arguments for realism is valid, his proposed replacement, constructive empiricism, shares the problems that plagued the standard empiricist view. What is needed to resolve the problems associated with the debate over scientific realism then is an account that succeeds in avoiding the problems that plague both van Fraassen's account and the version of realism that has been so far considered. Because most of the literature published in response to van Fraassen has attempted to defend realism from his critiques, there are comparatively few accounts available that proceed from these premises. However, there is a small and divergent body of literature that might be termed "deflationary" accounts of science.[1] These accounts warrant this description because they avoid interpretations of scientific practice that are supposed to apply *a priori* to all cases. They recommend that philosophy *add* nothing to science but find its various interpretations of science in science itself.

I want to outline two fully-fledged accounts of the scientific endeavour that proceed in this way: Arthur Fine's "Natural Ontological Attitude" and Richard Miller's "Piecemeal Realism". However, these accounts are not univocal in their approach to this goal although they share the conviction that the solution to the realism question is not to be found in general philosophical accounts that are supposed to apply to the whole of science. We shall thus consider the difficulties that face both of these positions respectively, with a view to highlighting what is plausible in the deflationary approaches and what difficulties are to be avoided.

This chapter will thus be divided into three main parts. Sections 3-1 and 3-2 will outline Miller's "Piecemeal Realism" and consider some objections to Miller's position and some possible responses to these objections. I will demonstrate that while there is, as in van Fraassen's case, much to be said for the position, it is not finally a satisfactory solution to the realism debate. Fine's "Natural Ontological Attitude" (henceforth abbreviated as NOA, according to Fine's habit) will be analysed in a similar manner in sections 3-3 and 3-4. I will show that this standpoint also is finally untenable. In the last sections 3-5 to 3-7 of the chapter I will draw on these reflections in order to outline a deflationary position that avoids the difficulties that plague Miller's and Fine's approaches.

It will be argued that the central appealing feature of a deflationary approach is its rejection of global interpretations that are supposed to apply to all the diverse practices and methods that fall under the rubric of science. Essentially, Miller's position and Fine's positions, while different in many respects, share the difficulty in not adopting fully this central deflationary insight even though they purport to do so. Miller, while rejecting the idea that general arguments for a realist interpretation of theoretical entities can be sustained, still adopts a global attitude with respect to the methodology of science. That is, he insists upon the central role of causal explanation as the main goal of science and this is the basis of his piecemeal realism. I will show that this is a generalisation that cannot be sustained. In Fine's case problems arise in several respects and especially in his global ban on philosophical interpretations of theoretical entities. This ban cannot be reconciled with the dictum that theories ought to be analysed in their own context. The position that I will propose as an alternative will contend like Miller and Fine that theories be analysed on a case by case basis. However, it will reject Miller's general methodological demand that causal explanation is the main goal of science and Fine's general ban on philosophical interpretation.

3-1 Piecemeal Realism

At the outset Miller rejects the standard empiricist view and proposes a replacement for the usual arguments for realism. He also rejects the possibility of a general philosophical justification for scientific realism. Nevertheless, Miller also wishes to retain, at least in part, what he sees as the central insights of scientific realism, namely the ideas that explanation is central to scientific practice and that good explanations are made in terms of theoretical entities (i.e. supposedly real structures with a causal role). What differentiates his views from realism is his conviction that no general argument for scientific realism follows from these insights. What does follow, with regard to realism, is that in some cases causal explanations are sufficiently sound to render disbelief in these explanations irrational. Miller takes the critiques of realism outlined above into account by assenting to the fact that there are situations where this is not the case and a realist view of theoretical entities is not to be recommended. The attenuated species of realism noted above, that Miller favours, is referred to by him as "piecemeal realism".

Miller's piecemeal defence for the belief in some theoretical entities rests on his theory of confirmation. Rival causal explanations are adjudicated in terms of "fair causal comparison".[2] That is, over time the strongest rival explanation proves superior to its rivals in terms of criteria that are "topic-specific" For Miller, each science has a range of accepted causal explanations that have proven pragmatically successful in that field, are easy to use, fit well with existing explanatory models, etc. These "topic-specific truisms" provide grounds against which new explanations are judged. For Miller the best explanation in a given case will cite causes that are actually present and sufficient to explain the phenomena in question and will fit the standard causal patterns accepted in the science in question. According to Miller there are situations when an explanation will fit these topic-specific criteria so well that it simply becomes irrational not to just accept it but to believe it as well.

One could well accept that Miller's topic-specific account of the adjudication of rival explanations is sound but doubt that it implies realism. The rival explanations might well be inferior to the preferred explanation in topic-specific ways but this might seem to say little, especially if the rivals shared empirical adequacy in van Fraassen's sense. How is one to know which (if any) of the rivals is "true" in the realist sense? That is how is one to know, given empirically adequate rivals, which set of unobersevable entities to consider worthy of belief. Simplicity or ease of use (as we have seen from the discussion of van Fraassen's views) might well give strong arguments for pragmatic superiority but they need not compel *belief*. Miller's answer to this question is based on his analysis of causation. For Miller, there is no general rule to which one can appeal to see if something counts as a cause. However, the fact remains, people use and understand the concept. Different uses of the term are related through their integrity to a general concept. In other words, different uses of the term are not separated by a complete difference in sense, as riverbanks and money banks are. But without a definitive set of characteristics to define what a cause is, exactly how are different uses related? Miller feels that the most promising clue lies in the uses and character of concepts like "number" and "work of art".[3]

Concepts like these are characterised as being made up of several different elementary varieties. People learn from experience that the various varieties deserve the same label (in this sense the various elementary varieties of cause deserve to be included under the same concept by virtue of their "family resemblances" in the Wittgensteinian use of the term). In the case of causes such elementary varieties would include such things as "a sting causing pain or a fear causing flight or the wind rustling the leaves".[4] Further non-elementary varieties are then included as causes as they come to be seen as related to elementary varieties. This process of identifying new types of causes is referred to by Miller as the "extension procedure".

All causal ascriptions, to one extent or another, then, rest on elementary varieties of causation. One understands these basic varieties by encountering them in life and learning how to apply them in language. Added causal agents are all couched ultimately in the understanding of basic causal concepts, and so are evaluations of causal explanations. In this sense sound causal explanations are compelling to any person with ordinary human experiences.

In terms of scientific practice, the admission of a new kind of cause into the causal repertoire might occur in the following way. An unexplained regularity or phenomenon is observed repeatedly, previously established beliefs and practices suggest that the regularity is not a coincidence. Repeated failures occur in explaining the regularity in terms of existing causal concepts. Eventually a novel explanation is proposed postulating a new sort of cause, this explanation, however, is distinct from existing concepts. The empirical success of this new sort of cause (and its superiority confirmed through fair causal comparison) could then allow that form to be admitted as a new sort of cause. Eventually, yet more new sorts of causes could be added to the causal repertoire on the basis of their resemblance to this new type of causal agent. An historical example of this sort of process is for Miller the advent of gravity as an explanation for planetary motion.

> The account of celestial trajectories as due to a force of attraction between all pieces of matter which is neither directed toward a single celestial body [the sun in this example] nor the pushes and pulls of matter in contact with matter.[5]

In this case, the familiar experience of objects falling to the ground was progressively extended to describe a universal force that acts between all bodies, including celestial objects, according to Newton's law of universal gravitation.

With such a theory of causation in mind Miller presents his case for realism. Causal explanations are introduced in terms of the topic-specific causes accepted in a given science. But because these causal patterns are accepted as results of the extension procedure from familiar causal concepts from ordinary experience like pushing or breaking, scientific causes can be accepted as just as true as our familiar causal intuitions. In a case like Einstein's explanation of Brownian motion, the movement of particles is explained in terms of the causal action of vibrating molecules that "hit" the particles suspended in a hot liquid. As this explanation is confirmed though the process of fair causal comparison, the belief in the "hitting" action of molecules is ultimately related to the familiar causal concept. Because this explanation is now so well confirmed, anyone who is familiar with the common concept of "hitting" can hardly doubt the existence of molecules.

Confirmation for Miller is fair causal comparison and the justification for accepting an explanation is arrived at through exactly the same process. However, justification comes with differing degrees of certainty, and the extension procedure can take one very far from the familiar causal concepts. For Miller, arguing for an ideal of justification that would equate with truth is a mistake that derives from a fascination with the general account of science his position rules out. Whether or not a confirmed explanation is believed to be true is a matter that is decided according to the local framework of explanation not global standards. Miller, like Fine, then resists providing a general definition of what must be the case for a scientist to be warranted in believing in a theoretical entity or explanation. Sometimes a confirmed explanation warrants belief, sometimes not. This is why realism, for Miller, can only be "piecemeal" and his account of fair causal comparison can only warrant belief in theoretical entities on a case by case basis.

3-2 Problems of Piecemeal Realism

Miller's claims that his realism is only "piecemeal" notwithstanding, his position does try to defend realist interpretation of theoretical entities such as the molecules used in explanations of Brownian motion. Thus, in order for Miller's position to be sustained, it must be able to defend itself from criticism to the effect that piecemeal realism has been drained of the ontological commitments that genuine realism implies. Furthermore, if this defence can be made, a supporter of Miller must be able to deflect criticism to the effect that his realism suffers from the same difficulties that plague the more usual arguments for realism that have been treated in the previous two chapters. While Miller's position can defend itself from the first line of criticism, it does fall victim to the second for the reasons alluded to above.

The defence of the claim that Miller's view is vulnerable to the same objections as the more usual arguments for realism will occupy the main argument of this section of the chapter. The soundness of this argument will be demonstrated in the following way: I shall first consider two representative examples of critics who argue to the effect that Miller's realism is realism in name only. I shall further provide some responses to these arguments that Miller might make to this line of criticism. Finally, it will be demonstrated that Miller's realism is vulnerable to the arguments that are commonly raised against realism. This will take place by examining the problems with Miller's view that explanation is always the goal of science.

An excellent and well articulated formulation of the contention that Miller's position is not really realism comes from Josef Rouse, his views then will be outlined first in order to bring to light the central features of this type of critique of Miller's realism.

3-2 a) Joseph Rouse on Piecemeal Realism

Rouse provides an excellent articulation of the view that Miller's position is not really realism. He finds himself in agreement with Miller's support for the realist critique of empiricism and the usual arguments for a Sellars-style realism. Nevertheless. Rouse finds Miller's arguments for piecemeal realism unconvincing. He argues that Miller's topic-specific point of view can be maintained only at the cost of eliminating "the robust conceptions of truth, reality and causality which originally attracted many realists to the view."[6] For Rouse, Miller's piecemeal realism is realism in name only. This, for him, is a happy outcome of Millers view.

Rouse observes that Miller's re-evaluation of the concept of causation and his arguments for a version of realism are related in an intimate way. Miller's realism stems in large part, then, from his rejection of empiricist accounts of causation offered in the tradition following Hume's influential account.[7] The general strategy of empiricist philosophers, for Miller, is to replace "causal stories" with accounts of cause that make extrapolations from observational regularities. A similar move by empiricist philosophers is to treat unobservables simply as tools or calculating devices for correlating data. Miller rejects this approach because of a commitment to take causal accounts seriously, "it does violence to our conceptual intuitions to say that *x* causes *y*, even though *x* does not exist."[8] Indeed, Miller notes, "What does not exist cannot cause".[9] Recall that Miller regards causation as a 'core concept' that is directly related to our everyday experience of things like pushing or breaking. For Miller, it is often irrational to disbelieve well-confirmed explanations precisely because doing so does violence to our understanding of these everyday experiences.

Miller proposes a model of explanation where "all explanation worthy of the name is causal."[10] In so far as this underlies Miller's realism, Rouse argues that Miller has conflated two uses of the concept of cause. One being the everyday use of the concept that equates the concept with explanation and the other is to identify a more specific kind of explanation, that is the sort used in the sciences.[11] That is to say while the employment of ordinary causal verbs may be central in the prosaic sorts of explanations used in everyday life, the more refined needs of scientific explanation might well call for something quite different. This conflation, in Rouse's view, creates a problem for Miller in that in every scientific situation the term 'cause' could be substituted for the term 'explanation' without cost. Rouse contends that this so is because if explanation were conceptualised in terms of its being a core concept (in Miller's sense), 'explanation' could be analogically extended to do precisely the same philosophical work as 'cause'.

Rouse notes that Miller could respond to these charges by pointing out that in the piecemeal realist's view of scientific explanation, cause is a "more general"[12] concept than explanation. In other words, Miller can argue that the causal account of explanation claims the ability to differentiate between causes that explain an event and causes that do not. For example, consider the explanations revolving around the reasons for Socrates' death. The immediate cause of his death was his walking about the jail cell. Miller notes that a proposed explanation that referred to this as the reason for Socrates death would hardly be explanatory. A better explanation of Socrates' death would appeal to deeper reasons, the reasons that he drank the hemlock in the first place, for example. For Miller, the first explanation would lack "causal depth", explanations, then, require such causal depth, and there are causes that do not form the basis of explanation.

For Rouse, such arguments still fail. In his view, the dictum that explanations possess causal depth does nothing to differentiate between the meaning of the two terms. It is not the case that identified causes that lack depth are not explanatory. They are explanatory, poorly so, but they are still explanatory. For Rouse, Miller's requirement for causal depth then amounts to nothing more than a general pragmatic preference for explanations that are not superficial and fully explicate the event in question. Miller's requirement for causal depth could be replaced with an identical requirement for, "explanatory depth".[13]

Rouse notes that a further way in which Miller might try to defend the clear distinction between his use of cause and use of explanation is by an appeal to his dictum that explanations need to appeal to causes not symptoms of causes. Miller, for example, contends that *a priori* formulations of explanations can not differentiate between symptoms and causes. Miller's causal theory of explanation, because of its topic-specific nature, addresses the distinction directly. Explanations made in terms of symptoms are not causal at all and therefore are completely lacking the causal depth required by Miller's account of explanation. Cause then, Miller could argue, has distinct philosophical content if it is to be distinguished from bare symptoms.

However, for Rouse, this argument fails for reasons that are more or less identical with the reasons for the failure of the appeal to non-explanatory causes. Attempted explanations made in terms of symptoms need not be ruled out by an appeal to a philosophical distinction between symptoms and causes. Explanations that rely only on symptoms could be seen as pragmatically abandoned in favour of better explanations in the context of evaluative comparison with rival explanations

That is to say, for any explanation that Miller would criticise as resting on symptoms and not causes, Rouse could reformulate that criticism and mean the same thing by using the term "explanation". Rouse might criticise the "explanation" as lacking depth, pragmatically preferring a superior explanation that "goes deeper" or "covers more ground" or "explains more of the picture". As far as Rouse is concerned then, Miller's requirement for "causal depth" amounts to nothing more than a pragmatic preference for detailed explanation.

Indeed, for Rouse, the pragmatic and field-specific conception of explanation that Miller holds is exactly what robs causation of any specific philosophical content. However, according to Rouse, Miller was well justified in emphasising the pragmatic and field-specific character of explanation. In the absence of a-historical, *a priori* models of confirmation, what counts as an adequate explanation will, "depend on the needs and character of the scientific field".[14] Rouse argues that all of these conclusions can be accepted without giving any special philosophical content to the concept of causation.

Rouse's point is this: if the evaluation of the validity of a given cause is pragmatic and field specific, and if the evaluation of the explanations that appeal to that cause proceed in exactly the same way, then the concept of cause (taken generally) has no specific content. Rouse argues that if this is true then there is nothing left to distinguish the concept and its role from the concept and role of explanation. Rouse is careful to point out that he does not seek to ban the use of the concept of causation from scientific and philosophical discussion. It is precisely causation's lack of philosophical content that makes its presence in discussion unobjectionable. Who, after all, would object to the inclusion of a term that served no philosophical function, except a philosopher who demands the use of an *a priori* model that should fit all the instances when the term is used? Rouse in essence seeks to divest piecemeal realism of all its ontological baggage that makes it a genuine realist account of scientific theories. In this sense Rouse's views approach those of Arthur Fine, whose general position will be considered later on. Fine's view of Miller's position is, not surprisingly, related to Rouse's. Fine's critique of Miller will be considered next.

3-2 b) Arthur Fine on "Piecemeal Realism"

Not surprisingly then, a similar critique of Miller's view comes from Fine himself. Fine's criticisms revolve around Miller's contention that there are cases when a theory is so well confirmed (according to topic-specific norms) that to accept it is to believe in its theoretical entities. For Fine it is Miller's insistence on the topic-specific nature of confirmation that drains his realist pretensions of content. Miller's case by case approach simply rules out the possibility of defining "accept" in a way which implies the "truth" of the causal stories that are accepted. What Fine wishes to say is that, while in some cases the process of fair causal comparison between explanations might produce a winner in such a clear way that failing to accept that winner would be irrational, the inference to the ontological implications of realism is not warranted.

> "[W]e still need to ask why the [topic-specific] truism is used to ground the necessity of belief in the *truth* of the causal covering story and hence in the reality of the causes. Were an instrumentalist to go Miller's way he would surely urge that the truism merely grounds belief in the reliability of the causal account."[15]

Put another way, in the case of the concept of 'acceptance', there is no reason given in Miller's account that prohibits a scientist from reading the topic-specific norms in a way that does not imply belief in the "truth" of the causal story, but instead emphasises its usefulness. For Fine there is an ambiguity in the way in which scientists employ the concept of acceptance. He holds that scientists employ the term in practice fully aware of the ambiguities that attend it. To debate the general epistemological implications of what the concept of "acceptance" implies, Fine argues, is to engage philosophy in a sterile debate. Science gets by perfectly well without wasting time in an insoluble debate, completely unrelated to the nature of its practice. In other words, Fine contends that to give the term "accept" the content it needs to imply realism genuinely, it requires a philosophical interpretation of the term "accept" that does violence to the way in which it functions in actual scientific use.

3-2 c) Is it Realism?

In the analyses of both Fine and Rouse the main thrust of the argument against Miller is that in order for his realism to stand he must provide and defend a general philosophical analysis of the key concepts that ground his realism. They contend that Miller has failed to do so. Rouse argues that Miller contends that his account of explanations as causal draws no general distinction between the concept of cause and explanation. Thus, each of his examples that are supposed to imply realism in that case (like the example of the explanation of Brownian motion) can be just as easily interpreted in anti-realist terms. Fine argues that Miller's realism does violence to the way the concept of acceptance functions in science. In a manner similar to Rouse, Fine argues that Miller's view could also accommodate an anti-realist interpretation of the way confirmed theories that employ theoretical entities are accepted. However, a supporter of Miller could respond to these charges by pointing out that critiques of this sort fail to appreciate fully the way in which the argument for a realist stance functions.

It is not the case that Miller is arguing that his examples of situations where a realist interpretation is implied infallibly entail that the scientist who accepts these theories interpret them in a realist way. What Miller is arguing is that these cases shift the burden of proof to the anti-realist on the basis of the fact that such an interpretation does violence to the way ordinary causal concepts function in both ordinary language and (through the extension procedure) in science. For Miller, the causal concepts used in science are localised refinements of the ordinary repertoire of causal concepts (like shoving and breaking) that function in ordinary language and that are learned from "ordinary human experiences". Following Elizabeth Anscombe, Miller argues that the basic causal concepts are directly experienced and in so far as these experienced are believed to be real, concepts deriving from these concepts can also be believed to be real with varying degrees of confidence.

Miller has not provided causation with a general metaphysical interpretation because for him there is no such interpretation to be sought, for him causation is a core concept and is nothing more than the everyday concepts that comprise the paradigm cases of the core concept. As far as Miller is concerned, there are cases in science where explanations invoke causes that can be so clearly related to the paradigm ordinary language cases and are so well confirmed (according to topic-specific norms) that it is less unreasonable to believe in those causes than to doubt them. In the Brownian motion case the causal concept invoked relates to the everyday experiences of hitting and jostling. Miller's argument for realism simply states that it is difficult to accept that a scientist who believes in these ordinary experiences and accepts the explanation of Brownian motion on this basis would doubt the veracity of their presence. Doubt in this case stands in more need of defence than belief.

This account of causal concepts and its role in explanation can serve as the basis of a response to Rouse's criticism that Miller cannot provide a philosophical analysis of the distinction between cause and explanation. Explanation in the sciences is causal for the piecemeal realist. This can be demonstrated by pointing to the way that causal concepts are used in explanations and how explanations are adjudicated. In Miller's view, explanations are adjudicated on the basis of fair causal comparison according to topic-specific norms. These norms are made up, in piecemeal realism, from a local repertoire of causal concepts that are derived from the paradigm examples of causation using the extension procedure. Thus, explanations in science are based ultimately on ordinary causal experiences and are judged for their success in terms of norms that are based ultimately on similar concepts.

Similar observations apply to Fine's accusations to the effect that Miller's position would support an interpretation of acceptance that is compatible with anti-realism. Of course, from the point of view of piecemeal realism there are explanations that, while superior to rivals, do not relate to core causal concepts closely enough for a realist interpretation to necessarily prevail. Across the sciences there are a wide variety of senses in which explanations are accepted. Miller's point is just that there are clear cases where the most rational approach to an accepted explanation is a realist interpretation to the causal action of its unobservables.

Essentially, if one accepts Miller's analysis of the nature of the basic causal concept's role in explanation and if one accepts Miller's contention that science's goal is scientific explanation, then, the piecemeal realism must be seen as unproblematic. Trenchant critique of piecemeal realism, then, must focus on Miller's analysis of how causal concepts are incorporated into scientific explanation. It must also focus on his twin arguments to the effect that all scientific explanation worthy of the name is causal and that explanation is the aim of scientific theories.

Nevertheless, the piecemeal nature of Miller's realism must be taken into account when his claims about the nature of the role of causal concepts in explanation are evaluated. It is not difficult, of course, to find examples in the sciences where the causal roles of theoretical entities are greatly different from the ordinary language paradigm cases that are supposed to ground all cases of causal explanation. In these instances belief cannot be unproblematically extrapolated from one to the other. For example, a piecemeal realist might try to argue for a realist interpretation of the Newtonian conception of gravity on the basis of its extension from the ordinary experience of things falling. However, Newtonian gravity differs greatly from this rough and ready concept derived from everyday life. Newtonian gravity is conceptualised as a field whose strength diminishes in an inverse relation to the square of the distance between the respective *centres of gravity*. Nothing in the ordinary experience of things falling corresponds to the concept of the centre of gravity nor is gravity experienced as diminishing with distance in ordinary life. The extension of gravity to include celestial bodies includes much that must be accepted on the basis of its explanatory and predictive success. An anti-realist (even one sympathetic with Miller's analysis of cause as a core concept) could well hold that the additional content added to the scientific concept's causal action is too tenuously related to ordinary experiences to warrant more than tentative acceptance on the basis of its instrumental utility. The failure of the piecemeal realist to defend realism in this case is no argument against the position because a piecemeal realist readily admits that there are situations where acceptance does not imply belief. Piecemeal realism only requires that there be at least some cases where the burden of proof rests strongly with the anti-realist interpretation. Miller clearly believes that this will be the case with most modern theories, but his position hardly entails this.

Much more serious problems rest with Miller's contention that explanation is the goal of science. Indeed, it is in his retention of this doctrine that Miller draws closest to the scientific realists such as Sellars and Maxwell. In this case Miller's piecemeal realism is vulnerable to the critique of the central role of explanation that van Fraassen directs at realists like Maxwell. While I have already discussed these criticisms in some detail, it is worthwhile to consider briefly how the salient features apply to Miller's piecemeal realism.

3-2 d) Piecemeal Realism and the Role of Explanation

Miller is indeed committed to the idea that the main goal of science is causal explanation and regards any science that fails to do so as incomplete. In this sense, Miller is committed to a methodological maxim to the effect that science ought to seek theories that provide causal explanations of phenomena. Consider Miller's views on quantum mechanics,

> For one thing it [Bohr's Copenhagen interpretation quantum mechanics] is incompatible with the version of confirmation that I have defended. If confirmation is fair causal comparison, then the approximate truth of quantum physics (which has certainly been confirmed) must be entailed by the best causal account of the statistical distributions in observations. The principles of quantum physics must describe the causal processes bringing about those distributions. However, if Bohr is right no such processes exist.[16]

Miller here is extending his view of explanation into a methodological demand for common cause explanations of statistical correlations. As we have already seen in the previous chapter, van Fraassen has shown that there are cases of such correlations that do not stand in need of explanation and have no common cause (this point will also be explored in greater detail in the next chapter). The example van Fraassen chooses to illustrate this point is in fact drawn from quantum mechanics, the EPR paradox. As was pointed out in the last chapter, common cause explanations of EPR have proved inferior to interpretations that simply seek empirical adequacy. Given the failure of interpretations of quantum mechanics that demand causal explanations then, this implies that Miller's causal account of confirmation cannot be generally applied.

The counterexample of quantum mechanics is a far more serious difficulty for piecemeal realism than examples of explanation that do not compel realist belief. This is so because the extension procedure from paradigm cases of cause leads to topic-specific norms that vary from situation to situation, thus realism is taken on a case by case basis. Piecemeal realism can admit counterexamples in these cases because it makes no general claims. The same is not true for explanation. Miller does claim that the goal of science is explanation and indeed identifies anti-realist quantum mechanics as a problem for his views on confirmation. Miller does try to produce a realist explanatory interpretation of quantum mechanics and this will be discussed in detail in the next chapter. Nevertheless, it is a fact that so far realist-inspired interpretations of quantum mechanics like the "local-hidden variables" interpretation of EPR have failed in experiments. Miller must, in order to sustain his general position on explanation, ally himself with scientific programmes that have so far failed. The problem becomes worse for Miller when one considers the fact that Miller admits the degree of confirmation that approaches inspired by anti-realism have received.

As I have noted, the next chapter will deal explicitly with the far ranging implications of quantum mechanics for the realism debate. Nevertheless the fact that Miller's position cannot be reconciled with at least one well-confirmed science suffices to cast serious doubt on his views on explanation. Miller's position, while appealing in many respects, is not a finally adequate solution to the realist debate. The main difficulty arises not with his arguments for a realist interpretation of some situations, but with his claims about the global role of explanation. Miller's realism amounts to the claim that in some situations the burden of proof shifts to the anti-realist. He does not dispute the presence of anti-realist doubt, he simply rejects the rationality of entertaining it in all cases. Piecemeal realism can deal with cases where only provisional acceptance of theoretical entities is implied. However, Miller does commit himself to the claim that a non-explanatory theory is incomplete and there are clear counterexamples to this claim.

3-2 e) Conclusions About Piecemeal Realism

Miller's piecemeal realism is a genuine form of realism. While critics like Fine and Rouse have claimed Miller's account of explanation cannot really sustain a genuinely realist ontological commitment (that would satisfy a follower of, say, Sellars), this comes from a failure to appreciate Miller's views about causation. For Miller, what does not exist cannot cause, and explanation is both causal and the main goal of science. Thus, a piecemeal realism can quite easily escape the charges of Fine or Rouse. However, by doing so, piecemeal realism becomes vulnerable to the sort of critique of realism that is provided by van Fraassen. While there are difficulties with Miller's account of the nature of causal concepts in scientific explanation, the most serious problem for Miller lies in his claim that explanation is the goal of science. However, it is by no means clear that this is always the case as the *EPR* experiments demonstrate. The piecemeal nature of Miller's realism does make his view invulnerable to rejection on the basis that there are sciences where researchers may be unwilling to confer belief on their theory's unobservables; but piecemeal realism is not piecemeal with regard to its stance on the role of explanation. Thus, a case like quantum mechanics that does not take explanation as its main goal does clearly refute Miller's claims to the centrality of explanation. It is for exactly this reason that piecemeal realism cannot serve as a satisfactory solution to the question of scientific realism.

3-3 The Natural Ontological Attitude

While Arthur Fine expresses sympathy with van Fraassen's critique of scientific realism, he adds some further arguments that strengthen the critique of realism's claims to account for the success of science. Fine notes that one of the main planks of realism is its claim that the progress of science cannot be accounted for unless the claims of science are at least approximately true. While the realist must admit the point that in any given situation more than one theory can account for an explanation, the realist could, however, fall back on what Fine calls the "small handful argument".[17] This argument is based on the observation that in almost all situations, rival theories and explanations are quite similar and that they typically retain (at least in part) the well-confirmed content of older theories. This older well-confirmed content when combined with the new features of the new theories permits new explanations and predictions. The successful outcome of these explanations and predictions is purportedly explained by the approximate truth of the new theories and by the truth of the well-confirmed content of the old. For Fine, this argument from approximate truth cannot be made sense of, for a variety of reasons.

First, the realist inference to approximate truth in this context is ultimately question-begging. From an anti-realist standpoint, the "small handful" argument can be responded to with just as must facility as the familiar realist argument from inference to the best explanation. The small number of rival approaches can be explained in anti-realist terms from the general methodological approach to the effect "If it has worked well in the past, try it again".[18] For the anti-realist what is meant by saying that the content of a superseded theory is in part retained by the new theory is that this content is used in combination with the content of the new theory to establish new explanations and predictions. With regard to the similarity of the theories in question, their small quantity and their retention of old well-confirmed content, no inference to approximate truth is made. Thus, the argument that the "small handful" argument implies approximate truth can only make this claim by assuming approximate truth.

When the history of science is taken into consideration the realist case for approximate truth becomes even worse. In the "small handful" argument the case for realism follows from the typical similarity of the rival explanation in light of their similar success. However, when the history of science is taken into account no such success is there to be found. In point of fact, Fine notes, this is a strategy that usually fails.[19] Most new explanations for observed phenomena turn out to be incapable of accounting for observations. Consider for example the eighteen years of failed explanations that followed the findings of the Michelson-Morley experiment. The main rival explanations that dominated this period were indeed similar i.e. that they retained as much of the content of older well-confirmed theories as possible in so far as they retained reference to ether and absolute location in reference to it. The explanation for the results that eventually came into acceptance rejects all of this content and approaches the problem in a radically new way. Given the usual failure of explanations in cases like the "small handful" experiment, the burden of explanation falls to the realist. It is not for the anti-realist to account for the successes of explanations it is for the realist to justify the appeal to approximate truth given the fact there is no pervasive success to be explained. Indeed, the "small handful" example is more consistent with anti-realism than it is with realism. Given this and the other arguments against realism that he finds acceptable, Fine comes to the conclusion that "realism is well and truly dead".[20]

Nevertheless, Fine does not consider himself as defending a form of anti-realism either. Indeed Fine is at pains to distance himself from anti-realism, of which he identifies two forms: "Truthmongering" and "empiricism". He identifies the term "truthmongers" with those philosophies of science that interpret scientific truth in terms of group consensus an approach he associates with the early views of Kuhn and with Rorty's epistemological behaviourism. The second group "empiricism" he associates with the works of van Fraassen. As was outlined in an earlier section, Fine finds van Fraassen's views flawed because of his reformulation of the theory/observation split. With regard to the "truthmongers", Fine's puts forward criticisms that closely resemble the arguments against such positions that were outlined in the last chapter. Briefly, such a position provides its interpretation of what counts as truth on the basis of how a given group of scientists behave. Naturally this implies an interpretation of how the group does behave. However, how is the reliability of this interpretation to be relied upon if truth is what is coheres with the standards of the group that is being interpreted? The answer seems to be that such accounts beg the question of their own validity, otherwise they must be seen as hopelessly circular.[21]

For Fine, the reasons for the failure of "truthmongering" versions of anti-realism and the failure of scientific realism occur for similar reasons.

> There is . . . a very close connection between these two conceptions. It is a typical dialectic that binds the metaphysics of realism with the metaphysics of behaviourism [Rorty's and Kuhn's positions]. Realism reaches out for

more than can be had. Behaviourism reacts by pulling back to the "secure ground" of human behaviour. In terms of that it tries to impose a limit, short of what realism has been reaching for. The limit imposed by behaviourism, however, is simply *less* than what we require. So realism reacts by positing something more, and then reaches out for it again. What we can learn from this cycle is just what makes it run and how to stop it.[22]

In either case both the realist and the anti-realist (of Rorty's sort) each approach science through the lens of a theory of truth. In the case of the realist, truth means correspondence with the world. In the case of the anti-realist, truth means consensus from the scientific group in question. An anti-realist like van Fraassen also runs into trouble because of the theory of truth that his approach espouses. In this case the theory is similar to the realist view, the difference between the two is the constructive empiricist attempt to restrict epistemological access to the truth of a certain class of entities, the observable constituents of scientific models.[23] In all these cases the difficulty is the same. By providing a substantive philosophical theory of the nature of truth, the philosopher moves well beyond the way the concept functions in science. In other words, if the task of the philosophy of science is to account for scientific practice it can hardy hope to do so if it imposes a theory of truth on science that is not to found in science itself. The solution to the difficulty, then, lies in approaching science on its own terms.

Fine attempts to elaborate such a solution that lies between realism and anti-realism that eschews their difficulties, a position he dubs "the Natural Ontological Attitude" which he usually refers to by the abbreviation "NOA". The Basic tenants of NOA are quite straightforward. NOA eschews both the realist and anti-realist push to attribute metaphysical interpretations to scientific truth claims. For the realist a belief in a claim like "there are electrons" implies that this claim corresponds to the "real world". For the anti-realist this statement means that the statement coheres with theoretical hypotheses and that no correspondence is implied. For NOA both claims are problematic because they both impose a philosophical theory of truth on the scientific statement. NOA adopts a much more modest approach to truth adopting Tarski's referential semantics which simply states that claiming that the sentence "the sentence 'snow is white' is true" is warranted in those situations when one is warranted in saying "snow is white". In Tarski's interpretation the term "truth" adds nothing to the statements that are true. Thus for NOA the statement "there are electrons" just means what it says, "there are electrons" and that there is "nothing more to say".[24]

NOA purports to leave science exactly as it finds it. That is, it warrants belief in the referents of a theory (observable or otherwise) if the theory is accepted and the theory sanctions such belief. In the case of a theory that does not make reference in terms of theoretical entities (like quantum mechanics) NOA simply accepts this theory on its on word. It can do this while still sanctioning belief in theoretical entities of other sciences because it imposes no *a priori* philosophical interpretation of the goal of science (like true explanations that correspond with the world). Fine argues that NOA not only puts the realism question to rest but it also accurately accounts for the way the science actually functions. For Fine, when a scientist believes in the truth of an unobservable entity like electrons, this belief simply means what it says, "there are electrons". The extra realist thesis, "our description of electrons corresponds (at least roughly) with what the world is really like", is not implied. NOA simply adopts truth in terms of the "standard rules of usage" of the term that are found in the sciences themselves. And this standard usage, according to NOA, comes with no philosophical theory.

3-4 Problems with NOA

Fine's NOA has attracted considerable attention in the philosophical literature, from both of those that find the position appealing and from those that do not. However, most of the commentators in either camp approach NOA from the point of view of a fairly straightforward realism. That is to say they either argue that NOA is a form of realism after all or reject it because it is not. The validity of these arguments is important to clarify because of my contention that realism is untenable. If NOA is realism then it is subject to the same difficulties that I have outlined in previous sections. If it is not, then it remains to be seen whether or not it is a valid replacement.

I have noted that NOA purports to be a "deflationary" view that opposes global ontological interpretations about the status of scientific claims about theoretical entities, and this is indeed how Fine sees his work. I will argue that the critiques of Fine's view that proceed from a realist standpoint are essentially misguided, and that NOA does in fact eschew the ontological commitments that realism requires and its problems do not ultimately derive from this rejection. Nevertheless, I will argue that NOA suffers from difficulties that are analogous with the various difficulties that plague the accounts that I have considered so far, the standard empiricist model, scientific realism, constructive empiricism and piecemeal realism. These difficulties all involve in one way or another taking a global position that generalises about the nature of scientific theories across the whole corpus of science. To make this case I will first consider the two sorts of approaches to NOA that derive from a realist standpoint that have been prominent in the literature. Finally, I will consider some arguments that neither take NOA to be realism nor reject it because it is not, but nevertheless find it unsatisfactory. This accomplished, I will analyse NOA from a general "deflationary" perspective that rejects the standard arguments for scientific realism. This will set the stage for the sketch of the features of a satisfactory "deflationary" approach to the realism question that will take up the last section of this chapter.

3-4 a) Musgrave: NOA as a Form of Realism

Allan Musgrave argues that "NOA's ark will after all be Fine (with a capital 'F') for realism."[25] Musgrave bases this contention on his analysis on NOA's so called core position. Fine, Musgrave observes, argues that both realists and anti-realists accept a basic core position. Realists and anti-realists differ over what they each add to this core position, and it is these additions that are supposed to be at the root of their difficulties, "what distinguishes realists from anti-realists, then, is what they add to this core position".[26] As far as Musgrave is concerned this basic core position *is* realism, at least as it is conceived of in any reasonable light. Musgrave then finds NOA quite accommodating for realism. While he has no troubles with NOA then Musgrave does take issue with Fine's characterisation of Realism and what it is supposed to add to this core position.

The core position is really just the natural ontological attitude itself. NOA is supposed to be just science without "additives". The core position is then simply that the findings of science are true. Musgrave notes that this characterisation may seem odd at first glance, given that the debate is supposed to be over the truth of statements about theoretical entities. Fine, Musgrave points out, argues that anti-realists and realists do accept the truth of scientific statements, they each just interpret them under different theories of truth. In the case of the realist, some sort of correspondence relation is implied between sentences and the real world. In the case of the anti-realist (in Maxwell's account of Fine's view) it is some different theory, for example truth as coherence or truth as pragmatic usefulness and so on.[27] For Fine, science does not concern itself with such philosophical questions and takes "truth" at face value.

Musgrave parts company with Fine over his contention that anti-realists support the core conception. As far as Musgrave is concerned, "adding to the core position a particular truth-theory for science actually demolishes the position".[28] For Musgrave, while realists and anti-realists might both assent to the wording of the core position, their different truth theories imply that they each, in fact support quite a different core position.[29] Fine argues that scientists see scientific truth on par with "homely" or everyday truths, but how are everyday truths to be seen? An anti-realist will see them in a different way than the realist. For Musgrave NOA does have content because there is no common core position and as far as he is concerned, NOA supports a core position that is more consistent with scientific realism than with anti-realism.

Musgrave notes with approval that Fine restricts his conception of truth to Tarski's condition T. Tarski's condition T states essentially that 'the statement "x is true" is true if and only if x'. In this picture the various true sentences are not interpreted in terms of an essential non-linguistic essence that they all share, what they all share is that they are true and nothing more. For Musgrave, convention T implies that what is true are "meaningful linguistic items...Languages are largely conventional human inventions, suited to different human purposes. Why suppose that the bewildering variety of truths languages contain form a *natural kind*?".[30] As far as Musgrave is concerned, no special metaphysical interpretation of truth is needed to be a realist, truth need only imply that, "a sentence is true just in case the entities referred to stand in the referred to relations".[31] But this is exactly what NOA's minimal core position on truth is. For Musgrave, even this has already gone too far for the anti-realist. Doubt about the veracity of statements about unobservables is doubt exactly over whether or not the entities stand in the referred to relations. All of this leads Musgrave to conclude that most realists accept NOA's core position and most anti-realists do not. While Musgrave does acknowledge that Fine does not interpret NOA this way, this is because of his mistake to the effect that anti-realists could live with the core position and that realists need something more than convention T to be realists.

While Musgrave is correct in his contention that both NOA and scientific realism share a general "common sense" approach to the objects familiar to everyday life such as tables and chairs, it is not the case that NOA and scientific realism amount to the same thing.

It is also possible that a realist might demand no more from an account of truth than is given by condition T, but this still does not equate with NOA. As I discussed in the first chapter, the realism debate in the philosophy of science is over the degree of belief in the truth of certain (but not all) sentences that is warranted. For the anti-realist, sentences that do not refer to what can be observed do not warrant such belief. But an anti-realist can agree with the realist over the truth status of observables and even the theory of truth under which these sentences are interpreted. Recall that Dummett's characterisation of realism can accommodate scientific realists and anti-realists like van Fraassen alike. Van Fraassen as we have also seen readily assents to the claim that the sentences of a theory are either true or false and what makes them true is external. That is, it is not coherence or the structure of our minds and so on that makes them true.

For Musgrave, NOA and realism share the conviction that a sentence is true if its referents stand in the referred to relations. But this does not make NOA realism. Scientific realism is not characterised by its position on truth. Indeed there is no reason to suppose that all realists even share the same theory of truth. Sellars for example adheres to a correspondence theory of truth and Putnam, as I noted in the first chapter, holds his own theory, "internal realism". Both are scientific realists in that they deny the validity of anti-realist doubt over unobservables and the anti-realist project of drawing a distinction between observables and unobservables. However, they do not share the same theory of truth. Scientific realism is characterised by its claim that epistemological scepticism about certain classes of statements is not warranted. Realists defend this claim with several lines of arguments, for example, they argue that only the truth of a theory can explain the predictive success of science and that only realism can account for science's goal of explanation. For the scientific realist, of course, the success of science stands in need of explanation and explanation is central to the goals of science. Anti-realists such as van Fraassen, as we have seen, have been able to cast doubt on these positions. NOA takes as one of its central features that the anti-realists have been successful in this. Given this NOA cannot be reconciled with realism because it rejects some of the main planks of the argument for scientific realism. NOA may well cleave to a theory of truth that many scientific realists find congenial. However, the arguments against scepticism about unobservables on the basis of the realist characterisation of the goals and success of science is what is central to scientific realism, not a view of truth. What scientific realism opposes in anti-realism is not so much the theories of truth that are common to anti-realists but their arguments to the effect that sentences referring to unobservables warrant special doubt. Indeed, given the failure of the arguments for realism that I have discussed, NOA's inability to be reconciled with realism is a happy outcome for the supporter of NOA.

3-4 b) Realists Who View NOA as Anti-Realism

A larger body of philosophers accepts the conclusion stated in the last paragraph, however, they do not consider NOA's inability to be reconciled with realism a happy outcome. They oppose NOA precisely because it cannot be reconciled with realism. Richard Schlagel, for example, takes issue with Fine's claim that NOA does not require that theory change be interpreted as progressive, a position anathema to the realist. NOA avoids metaphysical additions to science, thus it makes no claim to the effect that the entities referred to by a new theory are really the same entities referred to by the old. In this sense, NOA holds open the possibility that new theories are just different and not necessarily progressive over the old.[32] Although NOA is hardly committed to the notion that new theories are just different (this too would be a philosophical additive), a realist does demand progress in the form of closer and closer approximations of the truth. It is for this reason that NOA cannot be married with realism. Schlagel also argues that this consequence is fatal for NOA because the view that new theories are not necessarily progressive has been ruled out by the realist observation that "scientific inquiry is *anchored in physical reality*... Without reference to independent physical domains whose properties (e.g. mass, charge, spin, colour) are manifested to some extent under experimental probing, the success and progressions of science will appear inexplicable and have to be accepted on trust".[33]

There are a number of other objections to Fine deriving from a standpoint that argues that only realism can account for the success of science. John Hawthorne, for example, attacking Fine's article, "Unnatural Attitudes: Realist and Anti-Realists Attitudes to Science" argues that Fine's claim that for every realist explanation there exists a corresponding superior anti-realist explanation cannot be sustained. Fine argues in "Unnatural Attitudes" that explanations can be accepted in terms of their instrumental success, and that this avoids that problematic inference to truth and that the use of "truth" is therefore superfluous. Hawthorne finds the claim objectionable on the basis of the fact that it overlooks the widely accepted view that, "instrumentalism leaves the success of science a mystery".[34] Robert Klee also argues against NOA that the increasing success of predictions lends more support to realism than to its rivals that make no effort to account for it. Alison

Gopnik argues that it is an "empirical fact" that scientifically realistic cognition is a universal human trait, and this makes any rejection of realism untenable given science's success.[35]

While it is true that NOA cannot be reconciled with realism, critiques of NOA can hardly be convincing if they beg the question of realism. Arguments against NOA that proceed on the basis that realism can explain the success of science presuppose a problematic realist premise that Fine and others have addressed. It is, for example, not at all clear that scientific success stands in need of explanation, given the fact that the vast majority of new theories are eventually rejected as false. In point of fact realists (e.g. Miller) also emphasise this point. Miller indeed considers his rejection of the appeal to science's success one of the most appealing features of his brand of realism. Furthermore as I have already pointed out, the similarity of newer successful theories to successful old ones can be accounted for in anti-realist terms by adopting the instrumentalist maxim that what worked in the past can be profitably tried again in the future. With regard to Schlagel's objections, even if it is granted that science had progressed, this need not be understood in realist terms. It is quite clear that progress within a given science could be interpreted in anti-realist terms like including broader domains of phenomena or increasing predictive refinements. Schlagel's idea that only realism can account for progress presupposes realism in just the same way as the arguments from science's success do. Given all of this, it must be concluded that the arguments against NOA that proceed from a realist standpoint suffer from all the difficulties that plague the arguments for realism outlined in previous chapters. A Realist critique of NOA can therefore be ruled out.

3-4 c) Brandon and Kukla: Non-Realist reaction to NOA

In spite of the fact that NOA is neither equivalent to realism nor vulnerable to realist critique it is not a wholly trouble free view. Musgrave notes that the main thrust of NOA lies in its conception of the core position, which is supposed to be shared by scientists, realists and the various sorts of anti-realists alike. Although Musgrave is incorrect that the core position is most consistent with realism, trenchant critique of NOA ought to analyse this core position that is supposed to be both universally accepted and unproblematic. The failure of either of these two claims leaves the appeal of NOA in serious doubt. This is so because the appeal of NOA is based on the idea that all NOA does is identify an unproblematic core position that everyone already accepts and then demonstrate that nothing more than this core position is needed to settle the realism question in science. This section will analyse NOA and the core position in light of these observations. I shall draw from the works of various writers to show that the core position is neither unproblematic nor universally accepted. I conclude in fact that NOA's difficulties rest exactly in its attempt to take the core position as a global interpretation that characterises what is essential to scientific theories.

E.P. Brandon argues that NOA may well characterise the attitudes of at least some scientists in their day-to-day work. That is, they may not "be too precise about issues that worry philosophers".[36] However, these issues are hardly unimportant and indeed concern scientists, one such issue is "why science?" That is, some justification for the special epistemological status that is usually granted to the findings of well-confirmed science is needed. Why prefer science to shamanism or astrology, and what indeed separates these practices from each other? Brandon notes that in spite of their faults, both realism and anti-realism do offer fairly clear answers to these questions. The realist offers reasons why doubt about claims about theoretical entities is irrational and the various forms of anti-realism offer reasons why scientific theories are provisionally accepted even though reason for doubt exists. NOA on the other hand simply relies on a basic unreflective trust in the "truth" of science and in so doing begs the question of science. For Brandon "NOA arrives after all the serious selections have been made".[37]

Andre Kukla develops exactly the same set of concerns in his critique of NOA,

> Why should we restrict our beliefs to those that belong to the intersection of the pro-science attitudes of realists or anti-realists? Why not let some anti-science points of view into the circle- say the views of biblical fundamentalists? The Natural Ontological Attitude might just as well be defined as the core position common to all those who have any epistemological stance whatsoever. Alternatively, why not make the club more exclusive? Maybe we should restrict our belief to the core position shared by all *physical* scientists.[38]

In a manner quite similar to Brandon's, Kukla notes that realism and anti-realism each offer answers to the question "why science?". Kukla notes that NOA is supposed to offer everything that science needs and that the additions of realism and anti-realism are just superfluous, however, given NOA failure to address the question of "why science?" Kukla finds NOA's claims to be all that is needed less than convincing.

In all fairness to Fine, in his formulation of NOA he explicit that his main concern is to show that NOA is consistent with the attitudes of working scientists and the initial "core position" of philosophers of science. It is hardly unreasonable, then, for Fine not to address this question explicitly. Nevertheless Brandon is justified in pointing out that perfectly clear answers to this question can be found in the other doctrines that compete for acceptance as a solution to the realism problem and that NOA's failure to do so might be a weakness for this position. Although most would agree that science has little to fear from the sort of pseudo-scientific rivals like astrology, the point might be dismissed as almost trivial, and possibly avoidable through minor modifications to NOA. Nevertheless, when an attempt is made to do so it becomes clear that Brandon's and Kukla's point reveals serious failings in NOA.[39]

NOA claims to represent the attitude that characterises working science, and indeed it has some initial appeal when one considers the point that many scientists to not explicitly draw on philosophical principles in their day to day work. Furthermore many scientists would rarely have the occasion to consider what the justification for science is. However, the question is not one that scientists would consider not to have an answer.[40] When questioned on the issue a scientist could give one of several answers, for example a scientist might respond to the question by arguing that although dubitable, the current theories of science are at least consistent with the available data and nothing else is to the same degree. Another might respond that current science explains the reasons behind what we observe with consequences that allow us to control what will happen in the future, and this gives good reason to believe that science is grounded in truth. Another might answer that science is pragmatically useful and its rivals are not. However, when each of these answers is considered closely, each one comes with an implied answer to the realism question. In the first case an empiricist view close to van Fraassen's is implied, in the second a realist approach is clearly implied and in the last a robust pragmatism is in force. Each answer commits one to an interpretative position with regard to the ontological status of the constituents of scientific theories. In so doing, the question "why science?", which ordinary scientists would claim to be able to answer, moves scientists into a position that contains "philosophical additives" ruled out by NOA. Given this, it becomes clear that NOA's claim to characterise the ontological attitude of actual science can be called into question.

NOA's proven failure to answer basic questions about the interpretation of science leaves its universal appeal in considerable doubt. In addition to these problems NOA's ban on philosophical interpretation suffers from much graver problems still. Kukla not only argues against NOA's ability to answer the question "why science?", he also points out that serious problems exist with its ban on philosophical interpretation. NOA, Kukla notes, "suffers from a critical vagueness stemming from our inability to say where science leaves off and philosophy begins".[41] Indeed, Fine nowhere provides a clear demarcation between science and philosophy but his claim that NOA avoids philosophical additives does presuppose one.

The problem of isolating a demarcation between science and metaphysics was a preoccupation of the earlier phases of the logical empiricists. The lengthy quest for a criterion for cognitive significance was precisely the quest to define the line between science and metaphysics. However, as was discussed the previous chapters, the later phases of logical empiricism pay little attention to the problem. In point of fact, Hempel's work on the subject rules out the possibility of defining such a sustainable line. Furthermore, as we have also seen, one of the consequences of the failure of the analytic/synthetic distinction is the blurring of the line between metaphysical and empirical content, that is it rules out a clear distinction between science and metaphysics. Given this, a distinction between a basic scientific core position and philosophical additives seems difficult if not impossible to establish.

The consequences of NOA's failure to establish a distinction between the basic core position and philosophical additives are serious indeed. Fine's idea of the core position is of a basic metaphysics-free unreflective attitude to science. But, as I have demonstrated, NOA fails to take account of metaphysical questions that are important to science (such as the question of "why science?"). Worse, NOA cannot even define where the core position ends and metaphysics begins. In fact, it is quite clear that no such determination can be made. Given all of this, it must be concluded that NOA's unproblematic and universally accepted core position is a chimera. Given that scientists do take positions on metaphysical issues and that no sharp distinction exists between science and metaphysics anyway, it seems that the most reasonable course to take is to reject Fine's conception of the core position and with it NOA.

3-4 d) Conclusions about Fine: Deflationary Philosophy after NOA

NOA, of course, succeeds in avoiding some of the pitfalls that dogged piecemeal realism. It does not, for example, commit science to a problematic global demand for causal explanation. However, it does have some global demands of its own. These come in the form of a global ban on any metaphysical interpretations of scientific theories and practice. This global demand too proved unworkable. Another critic of Fine, Sergio Sismondo has observed, "some attention to practice can take us away from metaphysics, but only a little more can lead us right back".[42] One way of looking at Fine's NOA then is to consider it to have failed to take its own advice. NOA counsels the philosopher to avoid global interpretations and to consider science in its own context. However, the actual analysis provided by Fine inverts this dictum and ignores the very real metaphysical content of science and imposes a global interpretation that relies on an unworkable distinction between science and metaphysics.

The failures of both piecemeal realism and NOA provide some important guidelines to guide the construction of a more suitable deflationary position. From Miller we learn that confirmation takes place in topic-specific contexts according to local norms that do not extend across the whole domain of science. From Miller we also learn that this situation leaves room for different positions on the interpretation of theories with regard to the realism question. However, Miller violates his own advice against generalising across the whole domain of science by illegitimately elevating the demand for explanation in terms of the causal efficacy of theoretical entities to the global aim of science. Fine too violates his own advice with his global ban on philosophical additives.

Clearly a satisfactory deflationary account of science that purports to address the realism question in a satisfactory way must avoid both of these pitfalls. I propose then that a satisfactory view will eschew both global interpretations of the goals of science and global interpretations of the metaphysical stance implied by science. I contend that the realism question is best solved not by adopting a non-metaphysical but a pluralist stance, one that permits the most suitable approach to the realism question to emerge from the content of science itself on a case by case basis. I also propose that a pluralism be adopted with regard to the goals of science. Just as the most suitable metaphysical stance must emerge on a case by case basis, so too must the most suitable view of what the goals of science are. Both of these claims stand in need of clarification in terms of how metaphysical questions can and do in fact relate to the empirical practices of science and in terms of how they affect what the proper goals of science are understood to be. In addition, the pluralism I am advocating stands in some need of justification. Because Miller and Fine's views have failed, it does not automatically follow that no global position on the nature of science is possible. In the last section of this chapter I shall offer an account of how a pluralist deflationary position on the realism question should be formulated and I shall offer some defence of the core of the deflationary view that urges the abandonment of global positions.

3-5 Sketch of a Plausible Deflationary Position:

Scientific realism implies belief in the entities postulated by a scientific theory. It is a metaphysical position in that realist belief implies the belief in the existence of the entities in question. An anti-realism such as van Fraassen's also takes a position on metaphysical issues. We have seen that constructive empiricism views theories as collection of models that are partitioned into observable and unobservable sections, the metaphysical stance taken about the unobservables differs from the realist view although the position taken on observables is similar. Although both of these positions cannot serve as the basis for a satisfactory position on the realism question, we have seen that NOA's tactic of rejecting metaphysical interpretations also fails. This section will take Sergio Sismondo's observation (noted in the precious section) that attention to scientific practice can lead us back to metaphysics as its guiding morale and will provide an analysis of the role of the metaphysical content of science in an attempt to resolve the realism question. I shall argue in this section that the solution is to be found on a case by case basis by analysing the role of metaphysical interpretation in scientific practice. I contend that the stance on realism taken by different theories differs from case to case and the relative success of the particular stance in question can be assessed in terms of the success or failure of its deployment in practice.

3-5 a) Metaphysics and Practice

Answers to very broad questions like "why science?" proceed from metaphysical positions. Any reasonable position on the realism debate then must take into account the very real metaphysical content of science. Scientific theories then come hand in hand with general metaphysical principles that permit scientists to address such questions. In addition to addressing such broad questions about the nature of science in general, they have import for scientific practice. The idea that nothing can travel faster than the speed of light is one such principle. This idea is of course a consequence of the empirical problems addressed by the special theory of relativity, but it is also a metaphysical principle in that it dictates what can and cannot possibly exist in nature. It has implications that go well beyond what has been observed and it places constraints on what a scientist can give ontological assent to. There can, for example, be no faster than light signals or influences in the picture of the universe implied by this principle, and the special theory of relativity rules out any theory or explanation that invokes such signals or influences. A scientist who accepts the principles that nothing can travel faster than the speed of light could never grant assent to the ontological status of any entity that violated the principle. Such a scientist would in fact actively seek to eliminate the need to appeal to any such entity in an explanation or new theory.

Thus, not only do such general meta-principles follow from scientific theories, they serve as guides and impose constraints for the formulation of new ones. One example of this, in the case of the ban on travel faster than the speed of light, is the approach taken by astronomers attempting to account for the so-called superluminal velocities of the jets of gas that are emitted from certain exotic celestial objects like quasars. In recounting the problem of superluminal velocities, the Astronomer J.M. Pasachoff observes, "Since the special theory of relativity tells us that *superluminal velocities*- velocities greater than the speed of light- could not be real, other theoretical explanations of the data have been sought".[43] The investigation of this phenomenon proceeded from taking a metaphysical assumption at face value not the initial empirical data.

It must be noted that the example of the metaphysical principle alluded to above hardly implies a realist interpretation of empirical studies it applies to. The dictum "nothing can travel faster than the speed of light" of course tells the scientists what cannot exist and what should not be believed in. The resolution of the problem of superluminal velocities did not appeal to the existence any new entity, and indeed appealed only to existing data in concert with special relativity, none of this need embarrass an anti-realist. On the other hand, the metaphysical content of this example from current astronomy can hardly be generalised across the whole domain of science. The ban on faster-than-light signals does not serve as an *a priori* principle of rationality. It is a metaphysical implication that emerges from a theory devised to deal with empirical data. While scientists certainly do employ the principle when dealing with strange data, it does not rule out the possibility that the repeated failure of its deployment might make it reasonable for a scientist to someday consider its abandonment.[44]

These reflections about the metaphysical commitments that are found in science illustrate several of the ways in which the metaphysical content of science relates to scientific practice as it actually takes place. First, the metaphysical principles adhered to in science are consistent with the results of the best science. In the case of the deployment of the metaphysical implications of the special theory of relativity, scientists would hardly have adopted the principle in the first place if special relativity had not succeeded previously on its own terms. The relevance of the principle in new contexts derives exactly from its empirical success in its original context not on it's *a priori* conceptual plausibility. Secondly, when the metaphysical content of science is considered in this light, it becomes clear that scientific theories have metaphysical implications and the resulting metaphysics can only be made consistent with particular types of empirical data. The absence of this sort of data or contradictory of data means that in some cases a metaphysical principle can be ruled out by empirical investigation. Indeed, had the special relativity inspired approaches failed to deal adequately with the observed superluminal velocities, then both special relativity and its attendant ban on faster than light velocities would have been cast into doubt.

It could of course be argued that the ban on faster-than-light signals that comprises the example of a metaphysical principle is not a metaphysical principle at all but an empirical one and that this is so because certain data could rule the principle out. However, the function of the principle in practice makes its status as a purely empirical claim ambiguous. For example it does not simply serve as a predictor of what will be observed, it serves to limit what can and cannot exist in nature and not just of what will and will not be observed in nature. As the case of superluminal velocities demonstrates, the investigation proceeds on the assumption that the jets are not going faster than the speed of light even though they are seemingly observed to do so. The principle, in point of fact, urges scientists not to take at least some of their observations at face value. Astronomers derive corrected speeds by constructing calculations that accommodate the observations by taking the sub-light velocity of the jets for granted. The explanations that derive from these corrected assumptions are not actually more empirically adequate for that data than an interpretation that takes the superluminal velocities at face value. The principle then is an excellent example of the blurry line that exists between empirical and metaphysical content. However, in the example I have given of its use, it clearly functions in a metaphysical role in that it dictates what can and cannot possibly exist in nature.

One way to understand these conclusions is in terms of the methodological implications that follow from the various metaphysical principles latent in scientific theories. That is to say, metaphysical principles imply methodological maxims that serve as guides to both the formulation of new theories and can also play a role in dictating what is to be taken as the goals of that research. In the case of the phenomenon of superluminal velocities, the methodological principle that derives from the metaphysical implications of special relativity is the dictum "do not employ any faster than light signals in an explanation of a new phenomenon". In this case not only did the deployment of this methodological dictum permit a swift resolution of the problem consistent with well confirmed current theory in the field, its successful deployment added empirical confidence in the metaphysical principle in question. In this case, the dictum in question was applied to explanation and implies that the goal of its deployment is explanation. However, there are cases where metaphysically derived methodological maxims construct the goals of their deployment in a different way.

An excellent example of this is the way their respective approaches to the realism question had a direct bearing on the general methodological principles adhered to by the rival sides in the long-standing debate over the interpretation of quantum mechanics. Although the detailed discussion of quantum mechanics will occupy the subject matter of the next chapter, it is worthwhile to consider it very briefly in order to illustrate the role of metaphysical stances in the construction of the methodological approaches current in this body of theory.

Quantum theory essentially provides results that specify the probability of the dynamical quantities in a quantum system (like "spin") having particular values when measured (according to specific rules). However, the interpretation of what this amounts to metaphysically has been a matter of considerable controversy. For example, does quantum mechanics specify the probability of a quantity having a specific value or does it specify that the quantity acquires its value on measurement or does it simply relate to the probability that the measurement apparatus will record a particular value? Further questions arise from the nature of quantum mechanical description of the phenomena that it deals with. Does quantum mechanics describe the micro-systems that it deals with? That is, does it describe (however, abstractly) and explain the causal interactions of the micro world or does it simply restrict itself to the description (and prediction) of what is observed in experimental situations?[45]

Each of these understandings of quantum mechanics corresponds to a metaphysical interpretation of the relation between quantum theory and the "reality" that underlies the experiments. If the measurement process of a quantum system is taken to be descriptive of the underlying causal processes then it is clear that a straightforwardly realist interpretation is being invoked. An alternative approach might take quantum mechanics to simply be a calculating device for predicting measurement outcomes and to have nothing to say about underlying causal mechanisms. The latter is a position that is obviously anti-realist in that it claims that quantum theory, while empirically adequate to deal with and predict the outcome of operations, really makes no comment on what lies beyond what is observed. In quantum mechanics, the metaphysical interpretation that one takes, as in the case of the ban on superluminal velocities, comes with methodological implications.

The way in which the stances on the realism question play a methodological role in this case can be easily summarised by looking again briefly at the EPR paradox. Recall that EPR describes a situation where two variables (Fn and Gn) are constantly observed to be in conjunction such that if $Fn=F_1$ then $Gn=G_1$ and so on, although the value of n occurs indeterministically. For the realist, the observation of constant conjunction stands in need of explanation and for an anti-realist like van Fraassen this is not necessarily the case given that empirical adequacy is taken as the broad goal of science. For an anti-realist with no commitment to the idea that science must and does describe what is really there, the correlation need not be accounted for in terms of causal explanation provided the resulting account is empirically adequate. This could for example come in the form of accurate predictions about the experimental system in question. This approach is favoured by most scientists who, following Bohr, adopt an anti-realist stance to quantum mechanics. This view of quantum mechanics is usually referred to as the "Copenhagen interpretation". For the realist, science describes underlying realities and confirms their existence. So a theory that left EPR unexplained would be incomplete. This is the reason why the $FnGn$ correlation must be explained in terms of unseen causes. This approach to EPR is usually referred to as the "hidden variables" approach.

The EPR thought-experiment has not remained hypothetical and has been reproduced in a large and well-documented series of experiments conducted by John Bell and Alain Aspect and others. These will be discussed in some detail in the next chapter.

Because I deal with EPR here only to illustrate how local approaches to the realism question affect the methodological approaches of scientists, I shall restrict my comments on EPR in this chapter to the role of the rival metaphysical stances in formulating methodology and the outcome of the resulting experiments.

The hidden variable interpretations of EPR have been devised in order to fulfil the realist demand for causal explanation. The hidden variables approaches have the great virtue of deriving somewhat different predictions of the experimental outcomes than the Copenhagen interpretation does. This difference in prediction and the existence of experiments that test these predictions permit in the case of EPR the adjudication between the Copenhagen and hidden variable approaches to quantum mechanics. In addition, because the views imply stances to the realism question, the correct stance on the realism question in the EPR case is decided by Bell's and Aspect's experiments. Suffice to say at this point that the results of the experiments are unambiguously predicted by the Copenhagen interpretation and have violated the predictions of the realist approach. It is true of course that Miller, Fine and a number of other writers have contested the view of EPR just expressed. However, none would deny that the scientific adjudication of the EPR case has clear philosophical implications. My point in this chapter is not to defend the Copenhagen interpretation (although this will occupy much of the next chapter) but to show how metaphysics not only has implications for practice but that practice has metaphysical implications and that the correct metaphysical stance can be thus adjudicated in practice.

In the case of the realist interpretations we can see the methodological maxim "seek causal explanations for observed regularities" at work. In the anti-realist interpretation it is easy to see a methodological maxim, "don't worry about hidden causes, focus on what can be observed" at work. This, of course, is in line with van Fraassen's idea that the quest for empirical adequacy is primary and not the quest for causal explanation. The adjudication of the correct stance in the case of quantum mechanics comes in the form of the success of the latter interpretation relative to its rivals. While this example does not settle either the realism question or the proper methodological stance for every science it shows how the debate has methodological import in terms of formulating philosophically derived maxims that guide research. The realism debate is not to be settled for the whole of science. It is to be settled on a case by case basis in terms of the relative success or failure of the deployment of the methodological maxims that are recommended by the various sides.

It must be noted that the deflationary approached argued for here is arguing that the realism question must be addressed in context and that a global solution that proceeds from strictly philosophical arguments will fail to account for science as it is practised. While this is so, it should not be taken that the sort of deflationary approach defended here simply moves the debate from the preserve of philosophy into the preserve of empirical science. Rather, I am arguing that the blurring of the line between science and philosophy moves philosophical considerations into the domain of empirical science. It can fall to the philosopher, for example, to make explicit the metaphysical stance of a science in order to elucidate the foundations of its general methodology. In this sense the philosophy of science can still be of considerable utility in helping to make explicit the conceptual foundations of science.

3-5 b) Sismondo on Deflationary Metaphysics

Although the elucidation of the conceptual foundations of science is one of the traditional goals of the philosophy of science, the deflationary approach to the understanding of scientific theories that I propose implies a considerable shift in how philosophers should understand this project. In the standard empiricist view, scientific theories were to be evaluated philosophically in terms of the interpretation of a theory's referents based on the syntactical structure of theories outlined in the standard model. In the realist picture, theories are to be understood in terms of their approximate truth and in their role as casually explanatory devices. In either case, scientific theories are to be understood individually in terms of their partaking of a general structure meant to apply across the whole domain of science. In the case of the deflationary picture advocated here, no such general structure is implied and the philosophy of science must approach science on a case by case basis. In order to illustrate this point, it worthwhile to provide an example of how a philosophical analysis of the conceptual foundations of a research programme proceeds in light of elucidating its approach to the realism question.

In his article, "Deflationary Metaphysics and the Construction of Laboratory Mice", Sismondo has provided a useful example of such an analysis in his argument to the effect that some research programmes can be understood in terms of their metaphysical commitments. Sismondo illustrates this claim with his treatment of John Paul Scott's research programme that studied the social behaviour of laboratory mice. Before outlining the essence of Sismondo's characterisation of Scott's work it must be noted that my aim in presenting this example is to show how such an analysis can have bearing on the realism question within the context of the analysis (in this case, Scott's research programme). While this aim is not inconsistent with the stated goal of Sismondo's arguments, his explicit goal in analysing Scott's research was to show that science can be usefully analysed in context in a way that goes well beyond the austere philosophy of Fine's NOA. I shall expand on his arguments somewhat in order to highlight how the relative success of the methodological role of the metaphysical stance of a science that I have argued for can be measured against the overall success of the programme in question.

Sismondo engages in a conceptual analysis of the foundations of John Paul Scott's research programme that studied the social behaviour in laboratory mice. Scott was particularly concerned with the social characteristic of aggression and examined his mice in laboratory conditions that would permit the examination and control of this characteristic. Scott, Sismondo observes, minimised the behavioural effects of genetic anomaly in his mice populations by using mice that had been bred to be more or less genetically identical. Moreover, in order to isolate the social variables that he was interested in, Scott placed his mice in artificial laboratory environments that were greatly abstracted from the natural environments within which wild mice of that sort are usually found. Sometimes there were no females, sometime it was impossible for the mice to find hiding places, and so on. For Sismondo, this programme is best understood in terms of constructivist metaphors. From the point of view of this constructivist philosophy of science, reality "co-responds" with science, science and nature shape each other in experimental practice.[46] In the case of

Scott's programme, the reality studied by Scott was a constructed reality wherein the objects of his research were also products of its practice.

For Sismondo, then, Scott's research is to be understood in constructivist terms. In so far as Sismondo emphasises the great abstraction of the mice's environment from any natural environment in which similar mice might be found and in his claim that the mice themselves were scientific constructs, he clearly rules out a realist reading of Scott's work. But what has Sismondo revealed with regard to methodology? In other words, what function does Sismondo's characterisation play other than to reconstruct Scott's research from a particular philosophical perspective? This case study differs from the previous two that I have considered in that Scott's programme does not explicitly proceed from a metaphysical dictum like "nothing can travel faster than the speed of light" nor is it involved in the adjudication between rival foundational interpretations that proceed from opposing views of the realism question. Scott's work simply involved the laboratory isolation of a quantity about which information was desired. Should not Scott's work then just be taken then to be relevant to aggression in his particular laboratory mice? In other words what useful thing has been accomplished by Sismondo's digression into metaphysics, would not this work be better approached from the perspective of NOA?

However, Sismondo in his constructivst characterisation of Scott's is not simply sketching a metaphysical approach to this research but is utilising this approach to delineate the scope and limits of Scott's research. The constructivist interpretation can be seen to highlight the methodological approaches and limitations of the work. Scott minimised the chance of the contamination of his results by the myriad variables that exist in wild populations by breeding more or less genetically identical mice and by placing them in rigidly controlled environments greatly abstracted from the natural environment of similar mice. While this approach permitted Scott to maximise the objectivity of his observations, it also determined the limitations of the domain of its conclusions.[47] Scott restricted his claims to the mice in question and generalised about the traits he was interested in (aggression etc.) only in so far as they provided very abstract models for which analogies could be loosely constructed in other populations.

Thus a constructivist view of the metaphysical stance of Scott's research demonstrates ways in which a realist perspective could have led to reductions in the work's ability to isolate certain social tendencies. That is, rather than try explain natural behaviour in terms of real properties or features of the social world in question, Scott essentially isolated the mice in environments so artificial that he could generate the tendencies of interest to him and predict and control their course in future experiments. A realist might object that the cause and effect relationships he constructed in the artificial mice societies were built on everyday behavioural tendencies that are found in wild mice, and thus Scott's results were true of "real" properties of mice to an extent and perhaps for social populations in general. However, Scott constructed even the basic tendency of "aggression" in his mice. This was done by specially training certain mice in his studies to fight readily. With such mice available they were placed in special environments in order to manipulate this constructed behaviour.[48] Thus Scott's methodology was intimate with a constructivist perspective. In this sense, according to my interpretation of Sismondo's characterisation, Scott's research does take a stand on the realism question. Scott's work is difficult if not impossible to reconcile fully with a realist view. Scott's research for example eschews the general realist demand for explanation of nature in terms of real entities or properties and instead proceeds by constructing properties that it then seeks to track and control. Thus by reducing the number of social and genetic variables to account for and by refining his experimental techniques to enhance their objectivity, Scott's methodology renounces realist metaphysics. In so far as Scott's work was a success, the validity of his research's stance on the realism question can stand for judgement.

A position that follows Sismondo's takes a pluralistic stance with regard to the interpretation of particular scientific research programmes and eschews a unified picture of science with regard to the realism question. Moreover as the Scott case demonstrates (with the methodological role of its constructive stance), the deflationary approach recognises the methodological role the metaphysical interpretations play. I will further develop the deflationary position that I favour by setting it in the context of the literature that deals with "naturalised philosophy of science".

3-6 The Normative Role for the Deflationary Approach

NOA, Sismondo's deflationary metaphysics and the deflationary position that I am developing can be compared in many respects to the ideas propounded in the literature dealing with "naturalised" philosophy of science. Naturalised philosophy of science draws essentially on the idea that the goals and methods of the philosophy of science ought to be informed by the goals and methods of science itself. It must be noted, at the outset of such a comparison, that this literature takes as its main focus the general question of how the philosophy of science should use science as its model to analyse science and does not focus specifically on the realism question. Nevertheless this work is relevant to the discussion of this thesis because, if philosophy's approach to science has normative implications for science, then this includes the proper stance to the realism question. As the discussion of Sellars-style realism and van Fraassen's constructive empiricism demonstrates, the stance on the realism question taken by a scientist (or by a philosopher making recommendations) does have methodological import, e.g., realism's demand for causal explanation and van Fraassen's recommendations about the priority of empirical adequacy.

However, as many commentators like David Stump have noted, naturalised philosophy of science has given rise to questions about the possibility for a normative role for the philosophy of science: noting what science *does do* in no way implies what science *ought to do*. In essence this is a point which NOA might be said to adopt fully with its "hands off" approach to science on the part of the philosophy of science. Thus a very general question faces a deflationist or a naturalist, how can normative judgements be applied to either the methods or goals or interpretations of science, if it is taken naturalistically, without committing the naturalistic fallacy?

Furthermore, adopting a philosophy of science that tries to take science as its operating model to identify goals and methods can lead one into problems as Stump and Sismondo have both noted. Which science best serves as the best source for the philosophy of science? Taking any one particular model of scientific operation can defeat the purpose of naturalism by failing to account for the goals and methods of many sciences other than the one that served as the general model. This is so because relying on the approach of one particular science to characterise scientific practice generally amounts to the same thing as trying to impose an *a priori* philosophical interpretation. Both see science as a unified endeavour and seek to extend their interpretation across the domain of science and both ignore the very real plurality that exists within science. This can lead to the production on the part of the naturalist of a picture of science that is just as distorted as the traditional *a priori* models of science that naturalism seeks to replace. This is indeed one way of looking at the failure of both NOA and van Fraassen's constructive empiricism, each while claiming to learn from science extend their resulting analysis across the whole domain of science. How then should a properly conceived deflationary or naturalistic philosophy of science address a given science?

Fine, Sismondo and the various proponents of naturalistic philosophy of science like Stump or Larry Laudan all offer, to some extent, answers to these questions that are either explicitly or implicitly laid out in their respective views. Fine, as we have seen, seeks to characterise both the methods and goals of science according to the dictates of NOA with its realist-like demand for causal explanation and its non-realist view of theoretical entities. But it is clear that Fine's view is inadequate because, as Sismondo has pointed out, that NOA is not characteristic of most science and this defeats its deflationary credentials. Simondo's deflationary metaphysics correctly recommends instead recognising the plurality that exists across the domain of science. But what of NOA's anti-normative stance? Might it not be the case that although NOA's views on explanation and its anti-metaphysical stance to interpretation fail in many cases, limits can still be placed on the role of the philosophy of science in virtue of warnings against committing the naturalistic fallacy? Indeed, Fine's recent work has downplayed the specifics of NOA arguing instead for "procedural objectivity". However, he still seems to see the goals of science as more or less fixed. His procedural objectivity is only pluralist about the means by which specific sciences achieve "trust in [their] product".[49] Normative approaches to interpretation of the realism question (even on a case by case basis) are still ruled out in this picture and he retains his hostility to interpretation of science in terms of the realism question.

Laudan and Stump have both argued against such a ban on philosophy's normative role. Indeed, as Stump has noted, a complete ban would imply the maintenance of a strict fact/value distinction.[50] This of course seems quite impossible. As we have seen from the discussion in the first chapter, the main reason for the failure of logical empiricist lexically based distinctions between the observable and unobservable parts of science was that fact that so-called strict observation terms received their interpretations in part through theoretical (non-observable) concepts. Scientific language is theory-laden, so it is impossible to isolate the strictly observational or factual. Facts then are never independent from background assumptions. Just as this is true within science, so it is true of the study of science. So-called facts about a given science will be permeated with background assumptions. Given this, is it not reasonable to suppose that conclusions about how science can and does operate will influence conclusions about science should operate and vice versa? In a somewhat related way, Laudan has argued for a normative naturalism on the basis that normative dictates of scientific epistemology can be seen as "hypothetical imperatives" that facilitate the realisation of certain goals.[51] Thus, even though the goals and methods of science have changed over history, certain methods are appropriate for achieving certain goals and the validity of such epistemological imperatives can be scrutinised empirically in virtue of their relative success at achieving the desired goals. That is to say, facts about how science does operate, in Laudan's view, are predicated on values about how best to achieve the goals of science and value judgements about what those goals should be. For Laudan, then, facts that are uncovered by the philosopher of science about how science operates imply value judgements about what the goals of science are and how these are best achieved.

These proposals from Stump and Laudan certainly make adequate response to critics of the prospects for a normative role for a deflationary philosophy of science. Moreover, Laudan has offered some interesting proposals on the nature and function of normative principles that the philosophy of science can identify and deploy. However, Laudan's position while certainly superior to Fine's in that it recognises the normative role possible for the philosophy of science, is inferior to Stump's and Sismondo's in that it is insufficiently pluralistic. While Laudan does recognise that the methods and goals of science have changed over history, he makes no note of the vast plurality that exists within science at any given time.[52] As Sismondo has noted in his critique of NOA, failing to take into account the real plurality of various sciences' different stands on for example metaphysical interpretation, and its concomitant effect on methods and goals, fails to do justice to science as philosophy finds it. But this is, presumably, supposed to be part of the point of Laudan's naturalistic position. As I have already been noted, a non-pluralist naturalism is just as inadequate as a non-naturalist position. Nevertheless, Laudan's views do contain valuable lessons for the naturalist, in particular his suggestions as to how normative principles are to be formulated deployed and judged.

Recall that Laudan argues that epistemological principles play a normative role in so far as they serve as hypothetical imperatives, valid in so far as they are successful, that serve as recommendations on how to proceed methodologically. Likewise, lessons can be drawn from his proposal that the methodological dictates of a given scientific programme can be judged on the basis of their relative success in achieving their specific goals. As we have seen, the interpretation of the realism question (as the Scott case described above and the discussion of van Fraassen in that last chapter show) plays a methodological role as well as playing a role in dictating the goals of science (e.g. explanation or prediction). The respective interpretative stance on the realism question on the part of a science then can be seen as part of the source of these same normative hypothetical imperatives, to be judged successful or not on the basis of their success in achieving their stated aims. While these proposals certainly do not seem inconsistent with Stump's quite general proposals or Sismondo's treatment of Scott's programme, neither of these accounts fully develops the normative potential latent in deflationary or naturalised philosophy of science. Sismondo's view tends to leave the normative role as an implicit possibility. Stump does directly argue that such a normative role is possible. Nevertheless, neither his nor Sismondo's account treats the question of how this role for the philosophy of science is to be deployed in cases where foundational debates on goals, interpretation and method are taking place *within* a specific scientific programme. How exactly is this adjudication to take place? In the next chapter I shall address this question directly. This will be done by further developing the account of the normative role that I have so far provided by deploying it in a detailed case study of they way the major positions treated in this chapter deal with quantum mechanics.

Thus, in spite of the fact that Sismondo's account of a deflationary philosophy of science is appealing in many respects and clearly identifies the problems with NOA, it does not by itself supply an account of how competing metaphysical stances are to be adjudicated within a given scientific programme. In the example of Scott's programme discussed above, Sismondo deployed a constructivist philosophy of science to account for Scott's practice but left the question of the success of that approach to the judgement of the reader and history. Sismondo's account, as far as it goes, deploys the deflationary approach to characterise and account for the practice and nature of the scientific endeavour after the fact. Sismondo does not address the question as to how the philosopher should proceed in situations where rival research programmes that extend over the same theoretical domain approach the realism question differently. If a philosophy of science is to have anything but a descriptive role, it ought to be able to help adjudicate the more plausible stance in a given situation at any given time and, as I have shown, philosophy can indeed play such a role.

In the next chapter I shall address directly the question of the adjudication of the correct stance to the realism question. I will further develop the thesis that the stance on the realism question taken by a research programme will come hand in hand with methodological maxims that will play a role in directing the course and goals of that programme. These maxims; along with their relation to philosophical goals, their practical import and relative success when deployed in practice; are identified by the philosopher of science. In keeping with my previous suggestions about normative principles, I will argue that the adjudication of the correct stance to the realism question in any given situation proceeds in so far as any given stance will imply methodological maxims that will bear practical fruit. And as has been previously noted, these maxims (and hence the stance to the realism question that they imply) can be judged against their respective success at achieving their stated goals. To illustrate this point the next chapter will develop both the descriptive and normative role for deflationary philosophy of science. This will take place in the context of a discussion of quantum mechanics as a case study.

3-7 Conclusions About the Deflationary Approach

The deflationary view advocated here does urge that science be considered within its own context. However, this must come with the recognition that scientific research programmes do come hand in hand with metaphysical perspectives that play an important role with regard to actual scientific practice. We have seen that the metaphysical commitments of a research programme imply normative methodological maxims that guide the course of research. In this sense the stance on the realism question does have a normative role to play in guiding research. The example of the problem of superluminal velocities demonstrates how general metaphysical principles can draw limits on the sort of approach that scientists can consider as valid solutions to the problem. The example drawn from quantum mechanics demonstrates that the relative validity of a research programme's stance on the realism question can indeed sometimes be adjudicated by the relative success of the research they imply. Finally, my discussion of Sismondo's treatment of Scott's work demonstrates how the philosophy of science can function in the role of making the metaphysical stance of a research programme explicit and assist in judging the validity of that stance in its particular context.

The sort of deflationary stance urged in this chapter is contrasted with both of the accounts against which its merits are compared. As with Miller's piecemeal realism, it urges that the realism issue be solved on a case by case basis but rejects Miller's general demand for explanation and Miller's general causal account of explanation as the general goal of science. Furthermore, while NOA may be a preferable account to the various versions of realism that have been considered, it, like them, cannot settle the realism issue. NOA simply cannot sustain its ban on "philosophical additives" without defining the demarcation between science and metaphysics. And as we have seen, the quest for such a demarcation is a vain one. NOA claims the ground that is supposed to form the core position shared by all scientists and philosophers, but as we have seen the core position of NOA is a chimera. I propose instead that the best deflationary view restrict itself to the central deflationary insight, that is the dictum that the realism issue is not to be solved by a global account of the scientific endeavour. Indeed when the vast sets of activities that fall under the rubric "science" are taken in their own multifarious contexts it becomes difficult to see how one account of the nature of the endeavour could satisfactorily account for every activity called science. This becomes especially clear when it is considered how much the individual sciences have changed through the centuries.

However, as the view outlined in the previous section of this chapter has made clear, none of this means that there is no solution to the realism question. The approach I have recommended shows how the stance on the realism question functions in actual scientific practice and how an adjudication of the correct stance can be made through analysis of the relative success of the deployment of that stance in practice. While I have already given some indication of how the view I recommend settles the realism question for the case of Quantum Mechanics, all of the major positions on the realism question have claimed to be the account most able to deal with the vast conceptual and philosophical difficulties that attend this science. Thus, it is necessary to demonstrate the difficulties that Quantum Mechanics presents for these accounts in the context of illustrating clearly how a genuine focus on practice resolves the issue.

[1] Fine uses the term "deflationary" to describe his position and the term is also used by Sergio Sismondo to describe Fine's view and other approaches that share some similarities with Fine's view (this work will be treated in detail in later sections of this chapter). I propose that the term can be extended to Miller's account because of its avoidance of a priori interpretations of science and its refusal to provide general philosophical theories for the key concepts like explanation and cause that serve as the basis for its realist standpoint. Fine would also include the late works of Putnam in this category but this is an error. As I demonstrated in chapter one, Putnam's recent work can still be reconciled with scientific realism (something he does not deny) and unlike Miller, Putnam would extend his realism (internal or not) to all cases of science. Furthermore, internal realism does have a theory of truth and cause and so on. While there are different truth in different schemes for the internal realist, what makes something true within a particular scheme is its relation to the world that is described by the scheme.

[2] R. Miller, *Fact and Method.* Princeton: Princeton University Press, 1987, p. 155.

[3] *Ibid.*, p. 74.

[4] *Ibid.*, p. 74.

[5] *Ibid.*, p. 79.

[6] J. Rouse, "Fact and Method Confirmation and Reality in the Natural and Social Sciences," in *History and Theory*, Vol. 28, 1989, p. 132

[7] Miller in fact draws his account of causation as a core concept from the works of Elizabeth Anscombe on the subject. Anscombe argues that the tradition of causal analysis in philosophy since Hume has been misguided because it has tended to rely on the analysis of observed regularities and not on causation itself. For Anscombe this has been because of Hume's dictum that causal efficacy is not directly experienced. However, for Anscombe this is an error because the action of ordinary causal verbs like shoving and breaking are experienced in ordinary life. What is not experienced is a general metaphysical property of "efficacy" that operates on all cases of causation. Anscombe argues that when this approach is abandoned in favour of an ordinary language analysis of the usual causal verbs, the metaphysical problems of causation disappear and it becomes possible to focus on causation itself. *Cf.* Miller, *Op Cit.*, p. 72 and G.E.M. Anscombe "Causality and Determination" in Sousa, A. & Tooley, M. (eds.), *Causation.* London: Oxford U.P.,1993.

[8] Rouse, *op cit.*, p. 128.

[9] Miller, *op cit.*, p. 382.

[10] Rouse, *op Cit.*, p.129.

[11] *Cf.* Rouse, *op cit.*, pp. 130-3.

[12] *Ibid.*, p. 129.

[13] *Ibid.*, p. 130.

[14] *Ibid.*, p. 130.

[15] Arthur Fine, "Piecemeal Realism" in *Philosophical Studies*, Vol. 61, 1991, p. 92.

[16] *Ibid.*, p. 517.

[17] *Ibid.*, p. 117.

[18] *Ibid.*, p. 119.

[19] *Ibid.*, p. 119.

[20] *Ibid.*, p. 112.

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[21] *Cf. Ibid.*, p. 141, *cf.* also the discussion of Kuhn's views that are provided in chapter one of this thesis.
[22] *Ibid.*, pp. 141-2.
[23] *Cf. Ibid.*, p. 143 f.
[24] *Ibid.*, p.134.
[25] A. Musgrave, "NOA's Ark- Fine for Realism", *The Philosophical Quarterly* Vol. 39, 1989, p. 398.
[26] Fine, *op cit.*, p. 128
[27] Although correct in his claim that the debate in the philosophy of science turns around the status of a class of entities not around a theory of truth, it must be noted that his account of realism and anti-realism's position on truth does not really do justice to van Fraassen's work. Recall that Van Fraassen supports a theory or truth quite close to one that is held by many realists, something like the correspondence theory. He does not deny the truth of a scientific theory's correspondence with the world, he just denies that knowledge of this can be extended to unobservable entities. It is curious that Musgrave here seems to recognise that the debate over scientific realism is over the status of a certain class of entities because his arguments that seek to conflate NOA with realism ignore this point and proceed on the basis of their compatible theory of truth. This point will be discussed at a later point in this section.
[28] Fine, *op cit.*, p. 384.
[29] Musgrave also expresses doubt that anti-realists would assent to the wording of the core position. In this he is probably correct, van Fraassen for example flatly denies that statements about unobservables ought to be accepted as true, although he would assent to it with regard to observables. Musgrave grants Fine the point here in order to show that even if all sides assent to the wording no basic position common to all is revealed.
[30] *Ibid.*, p. 387.
[31] *Ibid.*, p. 387. It must be noted that neither Musgrave nor Fine really gives a correct reading of Tarski's semantic view of truth. Musgrave is clearly trying to conflate it with a form of the correspondence theory and this is quite incorrect. Fine seems to be making a similar error by focussing only on Tarski's material criterion and not his far more important formal criterion.
[32] R. Schlagel, "Fine's Shaky Game", *Philosophy of Science*, 58, pp. 321 *Cf.* also Fine , 1986, p. 130.
[33] *Ibid.*, p. 321.
[34] J. Hawthorne, "Not a Metatheorem, in Fine" *Mind*, XCVII, p. 585.
[35] A. Gopnik, "Reply to Commentators", *Philosophy of Science*, 63, 1996, p. 457.
[36] E.P. Brandon, "California Unnatural", *Philosophical Quarterly*, 47, 1997, p. 234.
[37] *Ibid.*, p. 235.
[38] A. Kukla, "Scientific Realism, Scientific Practice and the Natural Ontological Attitude", *British Journal for the Philosophy of Science* 45, 1994, p. 973.
[39] Conclusions about NOA similar to Brandon's (that in fact cite him) are reached by Sharon Crasnow in her very recent article "How natural Can Ontology Be", *Philosophy of Science*, 67, 2000, pp. 114-132. Crasnow argues for a "philosophical" attitude rather than the Natural Ontological Attitude, although she provides only a very sketchy account of the "philosophical attitude", it shares the pluralism of David Stump's position (whom she also cites). I will treat Stump's views later in this chapter.
[40] Indeed, in the U.S. at least, the question is, from time to time of direct and crucial importance to working scientists and philosophers alike, especially for biologists working with the ideas of

evolutionary theory. There have, of course, been a number of court cases over the years (so far unsuccessful) that have attempted to undermine the teaching of Darwinian evolution in schools. Scientists and philosophers alike have often been called to testify in these cases. In one such case, McLean vrs. Arkansas in 1982, the question of mainstream biology's superiority to so-called creation science was directly addressed, *cf.* D. McArthur, "What Good is the Philosophy of Science?", *Contemporary Philosophy,* vol. XIX 1997, pp. 16-20. Even now, yet another such court case is taking place in Kansas, *cf.* Stephen Jay Gould , "Dorothy , It's Really Oz", TIME, 154, No 8, 1999, p. 39.

[41] Kukla, *op cit., p.* 973.

[42] S. Sismondo, "Deflationary metaphysics and the Construction of Laboratory Mice", *Metaphilosophy,* 28 1997, p. 222.

[43] J.M. Pasachoff, *From the Earth to the Universe.* Toronto: Saunders College Publishing, 1987, p. 541. The explanation for the phenomenon is actually quite straightforward. The apparent velocity of the gas jet is derived by dividing the distance moved by the interval between measurements of it. If it assumed that the jet must be moving slower than the speed of light then it becomes clear that the jet fails to keep up with the light it emits. Given this, a measurement taken at say one year in the past detects light emitted from a time *before* the jet was at the position is was one year ago. A difference exists then between the distance the jet moved between the time it was at the position from which its light was first observed and the distance it moved during the interval between measurements. When this difference is corrected, the velocity comes out to be several times slower than the apparent velocity and well below the speed of light.

[44] Indeed David Bohm's approach to the EPR case is an example of just such an approach (although he tries to show the faster than light signals do not violate special relativity in that one case), there are of course good reasons to consider this approach unsound, but it is certainly not an *a priori* irrational approach. The next chapter will discuss Bohm's approach and the EPR case in more detail.

[45] *Cf.*. Richard Healey, *The Philosophy of Quantum Mechanics.* Cambridge: Cambridge University Press, 1989. 9-11.

[46] Sismondo, *op cit.*, p. 222.

[47] *Ibid.*, p. 226.

[48] *Ibid.*, p. 224.

[49] Arthur Fine, "The Viewpoint of No-One In Particular", *Proceedings and Addresses of the American Philosophical Association.* 72, 2, 1998, p. 18.

[50] David Stump. "Naturalised Philosophy of Science with a Plurality of Methods" *Philosophy of Science* 59. 1992. p. 458.

[51] Larry Laudan. "Normative Naturalism", *Philosophy of Science* 57. 1990. p. 46.

[52] Stump. *op cit.*, *p.* 457.

Chapter 4: Realism and the Problems Posed by Quantum Mechanics

The preceding chapter developed the outline of a "deflationary" approach to the realism question. In this chapter I will further develop this position from the outline developed in the last chapter. That outline borrowed some of its features from the position advocated by Sergio Sismondo. In spite of the fact that Sismondo's account of a deflationary philosophy of science is appealing in many respects and clearly identifies the problems with NOA, it does not by itself supply an account of how competing metaphysical stances are to be adjudicated within a given scientific programme. Further, in Sismondo's discussion of Scott's, he deployed a constructivist philosophy of science to account for Scott's practice but left the question of the success of Scott's approach to the judgement of the reader and history. Sismondo's account, as far as it goes, deploys the deflationary approach to characterise and account for the practice and nature of the scientific endeavour after the fact. Sismondo does not address the question as to how the philosopher should proceed in situations where rival research programmes that extend over the same theoretical domain approach the realism question differently. If a philosophy of science is to have anything but a descriptive role, it ought to be fruitful in resolving debates in particular situations. That is to say, it ought to be able to help adjudicate the more plausible stance in a given situation at any given time and not just provide a plausible after the fact interpretation for a philosopher or historian. In other words, Sismondo's approach simply accounts for practice but it has no normative or adjudicative role with regard to practice. In the previous chapter I tried to fill this gap by providing a very brief discussion of how such adjudication can take place by evaluating the relationship between philosophy and science referring to the example of Quantum Mechanics.

I argued in the previous chapter, from the Quantum Mechanics example and others, that the stance on the realism question taken by a research programme will come hand in hand with methodological maxims that will play a role in directing the course and goals of that programme. In this chapter I address the question of the adjudication of the correct stance to the realism question directly, again using the example of Quantum Mechanics. I return to this example because its discussion in the last chapter served simply to demonstrate how the deflationary approach that I defend functions in the general task of adjudicating between rival programmes that take a different stance to the realism question. However, the interpretative problems arising out of Quantum Mechanics are very contentious and warrant a more detailed discussion which focuses on the problem of realism in Quantum Mechanics itself. The previous discussion simply alluded to Quantum Mechanics in order to highlight some of the features of the general deflationary approach that I favour. It is not enough to simply state generally how my approach is supposed to work when applied to a case like Quantum Mechanics, a direct treatment of the realism problem in Quantum Mechanics and my position's resolution of it is required. This is especially true given the fact that the deflationary approach prescribes a case by case approach and cannot be finally argued for in purely general terms.

The debate over the correct philosophical stance to the Quantum Theory has been extremely complex, long and fierce and goes back to the very beginnings of the theory. Furthermore, both major deflationary positions rejected in the last chapter: "Piecemeal

Realism" and NOA purport to solve the debate over the philosophical interpretation of Quantum Mechanics. Thus, the discussion in this chapter will take the following form. First, I will outline the difficulties with Quantum Mechanics that relate to the question of realism. Because this thesis is about the realism question specifically, I will restrict this discussion to those aspects of Quantum Theory that relate directly to realism and will therefore emphasise its philosophical difficulties and not its technical ones. Second, I will demonstrate how competing stances on the realism question have actually provided the conceptual impetus for two rival interpretative programmes, the "Copenhagen" interpretation and the "local hidden-variables" programme. In other words I will address the debate from the point of view recommended by the deflationary approach that I defend in this thesis. This section will discuss in much more detail than the last chapter why the Copenhagen interpretation has prevailed in the debate. I will also show how the debate over "local hidden-variables" demonstrates the role of the methodological maxims arising out of an approach to the realism question. Finally I will consider the approach to the realism question offered by the two types of deflationary views that I rejected in the last chapter, Miller's "piecemeal realism" and Fine's NOA.

These positions will be considered again for two reasons. If these positions were to genuinely provide a solution to the realism question in Quantum Mechanics in a more satisfactory way than the position I defend, then this would tell against the deflationary view I defend and its account of the relation between philosophy and science. Furthermore, I return to these positions for the same reason that I return to Quantum Mechanics. Deflationary accounts have in common the fact that they recommend that the realism question be approached on a case by case basis. In this context, general arguments, like the ones offered in the last chapter, lose their force if they cannot be applied in specific cases. Not only is Quantum Theory an excellent locus for this excursion into case study because of the specific foundational problems, but also because it is the resolution of the difficulties associated with Quantum Mechanics that is supposed to serve as an important case study for both Piecemeal Realism and NOA.

This account of the foundational difficulties that attend Quantum Theory with regard to the realism question will restrict itself to the examination of "non-relativistic" Quantum Mechanics. That is to say I will avoid extending this discussion to cover those theories that make up the body of work known under the rubric of Quantum Field Theory as much as this is possible. The discussion will be so restricted for several reasons. The main reason is because realist approaches in this field have not been as robustly formulated in this area as they have in non-relativistic Quantum Theory. In addition, it is not clear that a realist (of even the most extreme sort) would, at this stage, absolutely require a realist reading of this field given its highly speculative nature at the present time. James Cushing, for example, a strong advocate of a realist interpretation of Quantum Theory, notes that Quantum Field Theory might simply turn out to be a convenient way to "implement the axioms" of yet another more foundational theory.[1] Furthermore, as commentators like Cushing have also noted, the foundational problems in Quantum Field Theory match those of more ordinary Quantum Theory exactly.[2] That is to say, the difficulties that attend Quantum Field Theory are features it shares with ordinary non-relativistic Quantum Theory The difficulties relating to realism that arise from the puzzling features of Quantum Theory: the implications of measurement, superposition, and the status of the mathematical formalism, are just the same problematic features that attend Quantum Field Theory. Thus any reference to Quantum Field theory will be restricted to those areas where it has a direct bearing on the main interpretation of non-relativistic QT.

4-1 Quantum Theory and Its Interpretative Problems

Quantum Theory (QT) is at this point a century old and has been, by almost all accounts, one of the most successful predictive theories of all time. However, the science is attended by a wide variety of conceptual and foundational conundrums. Almost all these problems can be summed up by the observation that it is very difficult to imagine a description of the world that would be both consistent with the predictions of QT and at the same time consistent with many features of the world described by classical physics. This problem is particularly acute for the scientific realist because of the realist contention that an adequate scientific theory must do more than predict observations. Thus, for a realist approach to QT to be successful it must both be consistent with classical theories like relativity theory (that realists contend admit realist construal) as well as provide a descriptive and explanatory account of underlying structures that account for the peculiarities of QT. However, as even some that favour a realist approach to QT have noted, QT is "a marvellous predictor but an incompetent explainer".[3] The problem for the realist is further compounded by the fact that the dominant methodological approach in the field has proceeded from a decidedly anti-realist standpoint, the so-called "Copenhagen" interpretation associated with the pioneering works in QT by Bohr, Heisenberg, Pauli and others. Nevertheless throughout the history of

QT, many philosophers of science, and a fairly small minority of scientists, have sought a realist interpretation. Before describing the details of this debate, I will provide an account of the general characteristics of QT with the aim of highlighting its features that have proved contentious to the realism debate for the bulk of the century.

There are a wide variety of difficulties associated with the foundations of QT that confront the realist that can be encapsulated under the rubric of the "measurement problem". Essentially the measurement problem arises from ambiguities associated with the nature of quantum mechanical predictions. QT, as I noted in the last chapter, provides predictions (of a probabilistic nature) relating to the value various quantities (spin, position, momentum etc.) will have when measured. The interpretative difficulty relates to the relationship between the value of a particular quantity and the measurement of it. Does the measurement describe the value of a particular quantity or does it describe a value acquired on measurement? And what of the value of quantities not measured? Do they have distinct values? Does QT have anything to say about these quantities at all? Should it, if it currently does not? The exact nature of the way these questions apply in practice can be clarified through a general account of the way QT derives its probabilistic predictions and then through the consideration of some striking experimental examples which illustrate the measurement problem.

QT is essentially a mathematical theory. Thus a general description of the conceptual features of QT must provide at least a rough sketch of the nature of the mathematical tools which are used to derive QT's predictions. Essentially the quantities measured in QT are represented as vectors in specially defined co-ordinate ("Hilbert") spaces of various dimensions (in the parlance of QT, the measured quantities are awkwardly described as "observables", but this should not be taken quite literally, an "observable" is simply a measured quantity in QT). QT specifies the probability of an observable having a particular value when measured, given a certain initial preparation of the experiment. The initial state of the experimental system is represented as a vector and the possible resulting states of a given observable are represented as multiples of the initial state vector (known as "eigenvectors" whose values, are "eigenvalues"). Performing various operations on the state vector derives the resulting eigenvectors. The practicalities of QT revolve around the operations that manipulate the state vector. In other words, they revolve around how the state-vector evolves through the experiment. The operator that manipulates the state vector corresponds by very loose analogy with the classical notion of the total energy stored in the system known as the "Hamiltonian". The Hamiltonian operator is determined on a case by case basis taking into account the particularities of the system being measured. The state vector (the initial state of the system), subject to the Hamiltonian operator, then represents the evolution of a quantum system over time. The absolute squares of the various resulting vectors correspond to the probabilities of the given observables having the corresponding eigenvalues when that observable is measured.[4]

This system of using vectors in a complex Hilbert space to represent the values of the various magnitudes in quantum systems is admittedly quite abstract. However, it is not abstraction that leads to the difficult nature of providing a causal description of the world using QT. One very difficult feature is the fact that QT provides only probabilistic values of the chances of finding an observable to have a given value when measured. Furthermore, on measurement, only the value of the observable actually being measured is specified. For example, if an experiment measures the position of a given particle, then its value of position is specified by the experiment, but the values of other observables (spin and momentum for example) are not specified. Does QT have anything to say, then, about the values of observables in a given system that are not being measured? Do those observables have definite values at or before measurement at all? Indeed, does measurement specify the value that a given observable had in the instant before measurement or does it specify the value that the observable obtains on measurement? A philosopher with realist sympathies might be tempted to respond that all the observables in the system do have definite values before measurement and that at any given point in time a particle does have a well defined position and momentum. This implies that a philosopher with realist instincts might suggest an interpretation of QT where the vector space formalism must correspond to a physical description of the actual system being measured. However, when certain crucial experiments from the history of QT are considered it becomes obvious that it is by no means clear that the realist instincts expressed in the last paragraph can be sustained. The next section of this chapter will review two well-known experimental situations in QT that highlight the foundational difficulties well. These will be presented with the aim in mind of bringing into sharp relief the difficulties with a realist construal of QT.

4-2 Problems for Realism: The Stern-Gerlach and the Two-slit Experiment

The first experiment that I will consider calls into question the typically realist notion that the constituents of a Quantum System have well defined values for their various observables. That is to say, regardless of what observable is being measured, the other observables (spin, momentum, etc.) do have definite values. This assumption can more or less be rejected when the results of the classic "Stern-Gerlach Experiment" is taken into account.

In this experiment silver atoms in a vapour are directed into a beam by a diaphragm and directed through a magnetic field formed by a specially shaped magnet. The magnetic field is structured such that the field is much more intense near one pole of the magnet relative to the other. The beam of silver atoms then proceeds to strike a glass plate that records the resulting pattern. The resulting effect is that the beam is split into two, each striking a respective spot on the glass plate. Obviously the magnetic field is responsible for splitting the beam of silver atoms. However, the effect is a little unusual and the behaviour of the silver atoms cannot be compared to the ordinary magnetic behaviour of the large objets of our every day experience. If the atoms did behave in the manner of ordinary large objects with their magnetic axes randomly distributed in the beam then those with the axes aligned parallel to the field will be deflected the greatest amount in one direction. Those aligned in the opposite way will be deflected the most in the other. However, given a random arrangement of the atoms in the beam, a large number will be aligned in a fashion neither parallel nor anti-parallel to the magnetic field gradient, so the pattern on the glass ought to be smeared out across a continuum and not a pattern of discrete values. Of course, the resulting pattern is one of discrete spots not a continuum. Clearly the analogy between the silver atoms and familiar objects cannot be readily sustained.[5]

Speaking generally, QT deals with the results in the following way. All electrons posses a quality known as "spin". Although the term "spin" draws an analogy with the familiar classical concept of angular momentum, the relation is purely one of analogy and, it shall emerge, a very loose one at that. The spin of a charged electron gives it a particular "magnetic moment", and the spin taken in any direction (vertically, horizontally or anything in between) can have one of two values, -½ or ½ (thus the electron is known as one of the spin - ½ particles). The positive and negative values are assigned conventionally, like the designations of the poles of the magnet. In the case of a silver atom, there are 47 electrons, 46 of which have paired opposite spins, and therefore cancel each other out. The magnetic moment for the atom then is derived from the spin of the unpaired electron left over (there are other influences on the magnetic moment from the nucleus, but these are too negligible for our purposes here). In the presence of an asymmetrical magnetic field like the one present in the Stern-Gerlach experiment, the atoms will be deflected in either one direction or the other, depending on their magnetic moment, in other words they will be split into two beams, just as the results show. In QT's account of the experiment no continuum is to be expected on the glass plate because the electrons have only one magnetic moment or the other relative to the measured axis. Not surprisingly, if a second magnet is aligned at right angles to the first and if one of the beams is directed through it, this beam is again spilt in two. This is so for the straightforward reason that the electrons possess only one of two values for spin (and hence magnetic moment) relative to this axis.[6]

To conceptualise the experiment in terms of the vector space formalism briefly sketched earlier, the beam of silver atoms is represented by the state vector. The system evolves over time subject to the Hameltonian. In this case the Hameltonian operator is derived from the effect of the magnetic field on the beam. With regard to the dynamical magnitude spin, two eigenstates are possible, corresponding to the two possible spin values. In either case the probability of a given atom being found to have a positive or negative value for spin is ½.

A philosopher who wished to interpret these results in a realist fashion might well suppose that the magnets simply sort out the particles according to their intrinsic spins relative to the axis being measured, either vertical or horizontal. However, this view ultimately fails to deal with further results. Given the view that the magnets simply sort out the particles according to their spin, one would expect a beam coming out of the horizontal field of the second magnet and entering a third magnetic field, that is vertical like the first, to be not effected by it. This is so because the silver atoms ought to have already been sorted out according to their spin on the vertical axis by the first vertically aligned magnet. However, this is not what is observed if the experiment is carried out. What in fact occurs is that the beam is split in two, just as it was when it passed through the first magnetic field. These quite surprising results call into question several of the tacit assumptions that lie behind a realist and straightforwardly descriptive interpretation of the system.

As I noted above, a realist account of QT assumes that the particles in question do possess a value for each quantity at any given instant. In the case of the Stern-Gerlach experiment this would be a definite value for spin relative to both the vertical and horizontal axes. In this case, a realist postulates that the asymmetrical magnets sort out the particles according to their pre-possessed spin values. It is further assumed that this process has no effect on the other values of the system, for example on the spin of the particles relative to another axis. This view comes from a general realist conviction to the effect that measurement discovers the intrinsic value possessed by the particle before measurement takes place and that measurement does not change the quantities of the system that are not being measured.[7]

Clearly the assumption that measurement leaves the values that are not being measured unchanged must be abandoned. This is so because the horizontal magnet leaves a beam that is not sorted out according to the vertical axis, but it received a beam that was sorted out with regard to this axis. A realist might argue that this is not a disaster because a one need only account for the effect of the measuring process and one can still maintain the cardinal realist thesis that the particles do have definite values for all observables. However, it is unclear how this can be done. The beam entering the second magnet has already been sorted out (according to the realist) with regard to its vertical axis, that is every particle has the same value (say positive). The second magnetic field seems to effect only half of the particles in the beam. After all, one half of the particles entering the third magnetic field retain their positive spin, how is the selectivity of the effect of the second magnet to be explained? One way to avoid this difficulty might be to abandon the assumption that the particles possess definite values for all observables at the same time. In this case, for example, the values of spin relative to the vertical and horizontal axes might be incompatible. That is to say, a definite value for one precludes a definite value for the other. However, if this move were to be made the assumption to the effect that the magnets simply sort the particles out according to their respective spins must be abandoned as well. This is so because if vertical and horizontal spin are incompatible and, say, the horizontal spin has a definite value (as is the case when one of the beams from a horizontal magnet is passed through a vertical one) then a measurement of the spin along the vertical axis will not simply sort out the particles between those that have a positive and those that have a negative spin. After all, given that the horizontal axis has a value, the particles can have no definite value for the vertical axis (given the assumption of incompatibility). In this situation then, the spin value on the vertical axis must somehow be acquired when the measurement is made.[8] Given all this it must be concluded that if a realist interpretation of QT is to be forthcoming it will not resemble the straightforward interpretation discussed in the preceding paragraphs.

4-2 a) The Two Slit Experiment

Some even greater difficulties with the realist project of providing an explanatory causal description of QT come to light when the much-discussed two-slit experiment is considered. The general features of this experiment can be summarised as follows. A stream of electrons is emitted from a source at a sensitive screen that records the contact of the electrons. Between the emitter and the screen is a plate with two-slits, *a* and *b*. There are three possible configurations to this experiment: slit *a* is open and slit *b* is closed; *b* is open and *a* is closed; *a* and *b* are open. The electrons, of course, must pass through one of the open slits in order to contact the detecting screen that records the pattern that results from the contacting electrons. It turns out that the distribution patterns on the screen that result from the experiment are somewhat counterintuitive.

When only one slit is open, either *a* or *b*, the result is straightforward. In these cases the pattern on the detecting screen resembles what would result in a similar experiment using large classical particles. However, this pattern does not extend itself to the situation where both slits are open. In this situation the resulting pattern is exactly that of an interference pattern that would arise if waves were passing through the slits. However, the experiment cannot be fully interpreted with the assumption that what passes through the slits really are waves. As we have seen, in the case of one open slit, a particle interpretation is fine. Furthermore, if a detector is placed right behind one of the open slits, the electrons are detected as passing through one slit as particles. Nevertheless, if what passed through the two open slits were really particles, then the probability of a given point on the detector screen recording the contact of an electron would equal the probability of that section being struck by a particle passing though slit *a* added to the probability of it being struck by a particle passing through slit *b*. But this is not the case. Indeed, if the particle description is retained some reason would need to be found explaining why the particles passing through slit *a*, for example, would be effected by the opening of slit *b*. Such an approach would seem to be indicated because the opening of both slits (in the particle model) seems to modify the behaviour of the particles with regard to the locations at which they strike the screen.[9]

In the example of the two-slit experiment an important component of Quantum Mechanics emerges. A particle account of the two-slit experiment is adequate to explain the results when one slit is open but breaks down when both slits are open. However, the wave account breaks down when only one slit is open. Using the classical concepts of either waves or particles then cannot provide a consistent or complete interpretation if only one of the interpretations is used. However, when both (However, incompatible) analogies are used a complete and consistent interpretation emerges. Moreover in this 'combined' sort of interpretation the applicability of a classical concept like 'wave' is determined by the conditions of the experiment. That is to say, in the micro world of the quantum domain these concepts take on their meaning from the particular experimental situation where they apply, and are not meaningfully applicable outside this domain. This doctrine is now widely known as the doctrine of "complementarity" and is widely associated with the work of Bohr.[10]

4-3 The Realism Question and the "Copenhagen Interpretation"

The doctrine of complementarity with its limited and contextually defined appropriation of classical concepts lies at the heart of what is now known as the "Copenhagen Interpretation" named after the city where many of its originators (like Bohr) worked for much of their careers. The interpretative apparatus of the Copenhagen school is quite minimal and is concerned primarily with measurement. There are several variations of the interpretation but speaking generally, the interpretation conceptualises experiments in QT in terms of an initial preparation of the system (i.e. the experimental apparatus) and in terms of the measurements that result from the preparation. With reference to the vector space formalism of QT, the Copenhagen interpretation takes the experimental apparatus as the basis for specifying the initial state function and the Hameltonian operator, and uses the resulting probabilistic predictions concerning the resulting eigenstates to account empirically for measurement results. In this sense the confusing and non-classical aspects of the experiments pose no interpretative difficulties because the vector space formalism is not regarded as corresponding to anything that might really be there in nature. It is simply seen as a convenient calculating device that produces results that account for and indeed predict observed measurement results. In this sense the confusing and seemingly paradoxical attempts to reconcile results with some consistent causal account of the processes that lie behind are avoided, because these processes are not regarded as part of the domain of the QT.

Returning to the discussion of the last chapter's brief account of QT, it is easy to see why this interpretation derives methodological guidance from a robust anti-realism. This thesis has characterised the main methodological maxim of a realist approach as the view that goals of science are, in their most general sense, characterised by a demand that a causal explanation must be found for observed phenomena. If this is a fair account of realist methodology, then the Copenhagen interpretation can be seen to reject this stance by regarding its general goal as the empirically adequate treatment and prediction of measurement results. As was demonstrated in the last chapter, realism's defence rests on an undefended assumption that explanation is the unambiguous goal of science. The Copenhagen interpretation does not proceed from this premise.

The Copenhagen interpretation, while avoiding many of the confusing and even paradoxical consequences of realist inspired attempts at explanation that attend the experiments that have been outlined above, has nevertheless been the subject of much controversy. Albert Einstein, for example, found it particularly unsatisfying and devoted much energy in the latter part of his career to the quest for a more satisfactory replacement. The essential feature with the Copenhagen interpretation that its critics find unappealing is that from a realist point of view it seems wilfully incomplete given its rejection of the main realist doctrine. From this viewpoint, the Copenhagen interpretation fails to explain the strange findings of QT and simply claims that the business of scientists working in the field is to provide an account to QT that takes the experimental findings as basic brute facts to be coped with. It restricts itself to mathematical models that describe and predict measurement results at the expense of the explanation of the underlying structures that must ultimately really be there and admit an explanatory account. The debate over the correct interpretation of QT, then, is clearly as much a philosophical one as it is a scientific one.

For many that espouse a realist view of the nature and goals of the scientific endeavour, the Copenhagen interpretation is, thus, an unsatisfactory solution to the puzzling implications of QT. The main reason for the dissatisfaction with the standard Copenhagen view is its inability to causally describe what is happening in a quantum system, given the fact that it restricts itself to the probabilities of measurement results given the initial state of the system. Some realists have argued against it not only from the view that a theory must explain its results but also from the fact that the Copenhagen interpretation itself leads to paradoxical results. One very common attempt to do so proceeds from the supposed consequences of the much-discussed "Shrödinger's Cat" thought experiment.

In this thought experiment, a live cat is placed in a box connected to a quantum system of some sort (say a quantity of radio active material) such that the system has a certain probability of being in either one state *a* or another state *b* (say *b* if a particular quantity of radiation is emitted). In the first case, the cat is unaffected but in the second case the animal is killed by a poison gas that is released when the system is in state *b*.[11] When the box is opened, the cat is either alive or dead, the probability of each having been correctly predicted by QT. The question posed by the realist is this, what state is the cat in before the measurement (i.e. the opening of the box) is taken? The paradox that is supposed to confront the standard view is that if QT really means that observables only take on definite values at measurement then one has to believe that the standard view forces one to conclude that the cat is both alive *and* dead at the same time until the box is opened. This striking conclusion clearly is inconsistent with our ordinary opinions of the way ordinary sized objects (like cats) behave, they always have definite values for their properties at all times.

The experiment is supposed to tell against the Copenhagen interpretation's doctrine of complementarity, arguing that it forces one into supporting an absurd ontology when the features of the quantum domain are shown (as in the "Cat" example) to have consequences in the macro domain of everyday experience. In this view, the Copenhagen interpretation postulates that a system is in all its possible resulting eigenstates until measurement when a sudden "collapse" mysteriously takes place at measurement into one of the resulting possible states. The difficulty lies not just in the failure to explain the so-called "collapse" but the absurd ontology the view implies. Cushing, for example accuses the Copenhagen interpretation of cleaving to "a truly bizarre ontology"[12]

However, it must be noted that this reading of the Copenhagen interpretation is highly inaccurate. Recall that the Copenhagen view is anti-realist in character and methodological approach, it does not impute any real-world correspondence to the devices it uses to calculate the probabilities of measurement outcomes. The Schrödinger's Cat based objection to it seems to take a realist reading of QT for granted and takes the standard view to be simply arguing that the puzzling features that are associated with classic examples like the two slit experience are to be taken at face value. If this were indeed the case, then the Schrödinger's Cat example would indeed be telling of an implausible ontology, and there really would be an unresolved problem relating to the mysterious "collapse" of the state at measurement. However, this is a gross distortion of the standard view and is a straw-man or psudo-version of the standard view, and even some realists, like David Finkelstein have noted as much,

> The concept of "collapse" has no counterpart in the Copenhagen formulation and is the telltale sign of the pseudo-Copenhagen formulation…[it is] as if I told you that a gun emits the command "Fire!" and that this collapses into the cry "Ouch!" if it hits the target.[13]

The Copenhagen interpretation, as we have seen, describes only the initial preparation of the system and the measurement results. It has nothing to say whatever about what happens in between those two points. As Finkelstein notes, "ψ describes what we do at the beginning of the experiment and Φ what we do at the end, and one does not evolve into the other."[14] Thus, if the correct view of the Copenhagen interpretation is taken with regard to the Shrödinger's Cat example, one does not have to suppose that the cat is both alive and dead before measurement. This is because QT has nothing to say whatever about the status of the animal until the box is opened. There is, of course, no paradox in this case because nothing paradoxical is being imputed to the cat. Nothing at all is being imputed to the cat. The argument for realism on the basis of the paradoxical nature of the Copenhagen ontology is therefore a straw-man argument. Any argument for a realist reading of QT then must come not from the *prima facie* implausibility of the standard view, but from the clear superiority of a realist alternative to it. I will subsequently examine the merits of some important examples of exactly this sort of argument.

4-4 Deflationary Philosophy of Science and Realist Alternatives to the Copenhagen Interpretation

I argued in the last chapter for a deflationary account of scientific practice. There, I held the foundational debates in QT to be an example of how philosophical interpretations of a science have methodological implications that can bear empirical fruit. I also argued that the best interpretative stance is adjudicated by looking at the empirical implications of the relative methodological approaches in question. In this section, I will illustrate the QT situation in much more detail, focusing on the empirical implications of the debate in QT spawned by the prevalence of the Copenhagen interoperation.

In this section, I will examine one set of widely discussed alternatives that have been proposed to the Copenhagen interpretation, the "local-hidden variable theories". I will focus on this alternative to the Copenhagen interpretation because the debate over "local-hidden variables" has been settled, more or less, to everyone's satisfaction and serves as a clear and uncontroversial illustration of how the deflationary approach can account for and adjudicate over outcome of the debate.

Hidden variables approaches find their support from theorists who find the standard (Copenhagen) approach to QT incomplete. The essential idea stems from the realist methodological demand that observed regularities in nature must be explained. In the case where a cause is not observed, inference to a postulated one (a hidden variable) is implied. In the case of QT, the strange empirical consequences of the various classic experiments (like the two-slit experiment and the Stern-Gerlach experiment) stand in need of such an explanation. The difficulty in QT for this approach has been the seemly paradoxical results of the classic experiments that might imply that any attempt at explanation is bound to lead into confusion and contradiction. Fenyman, for example has given this frame of mind eloquent statement,

> Do not keep saying to yourself, if you can possibly avoid it, "but how can it [QT] be like that?", because you will get "down the drain," into a blind alley from which nobody has yet escaped. Nobody knows how it can be like that.[15]

The opposing realist conviction has, however, led to some quite interesting attempts to reconcile the experimental results of QT with some sort of classical causal explanation. The family of hidden variable approaches to QT that is of interest to this discussion found their inspiration in Einstein's arguments to the effect that QT must be incomplete.

The previous chapters briefly discussed the Einstein-Podolsky-Rosen (EPR) thought experiment and its consequences for realism. They dealt with the experiment only in so far as it serves (for van Fraassen) as a counter example to Reichenbach's idea that correlations must have a common cause explanation. Here, I will discuss EPR and its history in more detail to show how realist hidden variable approaches to QT take their inspiration in large part from the problem posed by Einstein in this much discussed thought experiment.

Recall that in the EPR case a correlation between two quantities Fn and Gn exists such that if the F attribute is F_1 then the G attribute will be G_1, F_2 will correspond with G_2 and so on. The physical situation that this is usually related to is the case of the production of a particle pair such that the total spin of the system is 0, that is, if the spin of particle F is ½ then the spin of G will be – ½ and so on.[16] However, in EPR, F and G are physically separated (or "spacelike separated"). When one of the quantities (say F) is measured, for example, by passing through a Stern-Gerlach magnet, the spin of G obtains a definite value too. This is so because if the total spin of the system is 0, and if the spin of one of the particles is known (for a particular axis) and there are only two possible values (which is the case for spin- ½ particles) then the value of the spin of the other particle must be the opposite of the measured particle. Thus if the spin of F is measured to be + ½ for the vertical axis then the spin of G for the same axis must be – ½. For Einstein this thought experiment has significant results. First, the thought experiment is supposed to demand a realist interpretation. This is so because, for Einstein and his collaborators, a theory that provides the value of a quantity with certainty in the absence of measurement must be describing an underlying physical reality that deserves realist description. Einstein, Podolsky and Rosen note,

> If, without disturbing the system, we can predict with certainty (i.e. with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this certainty.[17]

Furthermore, if the two particles have a spacelike separation then the measuring of one cannot have an instantaneous effect on the other unless there is instantaneous action at a distance (or "non-local" effects), but this is ruled out by relativity theory. Therefore, the Copenhagen interpretation, to be complete must contain non-local effects, but this is impossible so the Copenhagen interpretation is incomplete. Some sort of "hidden-variable" approach must be found that can explain EPR type correlations without invoking non-local effects. In these approaches, the resulting measurements must be explained in terms of the initial preparation of the system and must not appeal to the affects of one measurement of the other spacelike separated particle in order to rule out non-local effects. Thus, for a local-hidden variable approach to be successful, the initial state interpretation must be able to predict the measurement outcomes to have the same probability of occurring as the experiment reveals them to have. Several attempts at such explanations have been formulated.[18]

Recently several scientists like John Bell and Alain Aspect have carried out various versions of the EPR experiment in order to test the validity of the non-local hidden variable theories. Their results tend to provide strong evidence for the empirical inferiority of the hidden variable approaches relative to the Copenhagen interpretation. In these experiments the system is set up in a fashion similar to the one described above. That is, a pair of spin – ½ particles is prepared such that the total spin of the system is 0, and the particles move off in opposite directions from their source and pass through Stern-Gerlach detectors where spin is measured. What is interesting about these experiments is that the angle of each of the detectors can be swivelled from 0° to 90°. QT gives the probability that both particles are spin + with respect to their axis as a function of the square of the sin of the angle α between the detectors ($\frac{1}{2} \sin^2 (\alpha/2)$).[19] In the case where the angle α is 0°, of course, the resulting probability is 0 and in every case where the detectors are parallel there will be a perfect anti-correlation between the spins of the particles. In the case where, for example, α is 120° the probability of both spins being + turns out to be ¾. Thus, for a local hidden variables interpretation to work, the probability for anti-correlation between the two particles must be 1 where α is 0°, and the probability of any given measurement of a particular particle resulting in a spin + must be ½ for any arbitrary axis (when the measurement of each particle is considered alone). Furthermore, these requirements must be reconciled with the probabilities observed in the experiments without holding that the measurement of the spin of one particle in the pair effects the spin of the other. This is so because the spins of both particles must be set in the initial preparation in order to rule out non-local effects.

It turns out that, if non-local effects are ruled out (the effect of the measurement of one particle effecting the spin of the other), the usual predictions of QT differ from those derived from the hidden variables interpretation. Consider the case where α is 120°.

According to the usual formulation of QT, if one particle is measured to be spin + relative its axis then the probability of the other particle having spin + relative to its axis is ¾. However, on the hidden variable interpretation the spin for both particles is set in preparation and the measurement of one should have no effect on the spin of the other. Thus, the measurement of spin + for one particle should still yield a probability of ½ that the spin of the other will be + relative to its axis if α is 120° (or in any case where $\alpha \neq 0°$). When the experiments are carried out the predictions of the local-hidden variable theories are violated in favour of results that are exactly predicted by QT.

The philosophical implications of these results with regard to the realism question are easy to interpret in terms of the deflationary philosophy of science defended in the last chapter. According to that interpretation, the preferable stance on the realism question ought to be decided on a case by case basis, according to the relative merits of the deployment of the interpretations in practice. In the variation of the deflationary view outlined in the last chapter, the stance on the realism question taken in a given situation will imply methodological maxims that will guide the direction of research. However, I also argued that philosophy should not only be able to identify these maxims after the fact but ought to be able to judge between the merits of competing maxims in cases where competing maxims preside over the same experimental domain. The EPR case demonstrates that this is so in a fairly transparent way.

In the case of QT, the causal intractability of some well known experiments led many scientists to reject the demand that causal explanation is always the goal of science and that the goals, at least in the case of QT, ought to be reformulated in terms of accounting for and predicting those results without reference to underlying causal agents. This corresponds to the Copenhagen interpretation. However, scientists and philosophers favouring a realist interpretation propose rival interpretations that purport to do the same empirical work as the Copenhagen interpretation within a causal (and therefore realist) framework. In the EPR case, these so-called hidden-variable theories actually were able to generate different predictions that have not been born out in practice. Thus, not only does the realism debate in the case of QT have methodological import but it also has empirical import. The case of the EPR experiments carried out by Bell and others clearly imply the methodological inferiority of a realist approach to QT at least with regard to its local-hidden variables variation. That is to say, with regard to QT the relative methodological success of the Copenhagen approach tends to lend stronger support for an anti-realist stance to QT than the relative success (or lack of the same) of the hidden variables stance lends to the realist demand for causal explanation. At present then, the balance of methodological success supports the proposition that the mathematical formalism of QT does not represent any actual state of affairs. In other words, QT is a calculating device not a description of the world.

A realist, of course, might object that the resolution of the realism question recommended above does not really tell against realism *per se* but only in so far as it inspires local-hidden variable approaches. In other words, a realist might still deploy the demand for explanation and provide an empirically equivalent interpretation to Copenhagen QT by basing explanation on non-local hidden variables. For example, James Cushing and others have argued that "non local" approaches, like his own approach derived from the work of David Bohm, can be shown to be empirically equivalent to the anti-realist inspired interpretations. However, to comment on this debate in detail would be somewhat digressive since the purpose of this chapter is not to provide a full account of every position possible in the philosophy of QT, but to use QT to show how competing positions are to adjudicated in context. This realist move admits that the Copenhagen anti-realism is a superior option to local hidden variables and this is all that my argument requires. Nevertheless this possible turn in the realism debate does warrant some comment.

Even if an interpretation like Cushing's view were to eventually become superior this would neither tell against my arguments nor imply a general adoption of realism as the stance for all science. It would only imply the success of a realist inspired methodology in the case of Quantum Theory, and this is fine from the deflationary point of view that I defend in this paper. It would, however, rob the anti-realists of one key example of a science that functions perfectly well without the realist demand for explanation.

Nevertheless, there are some good reasons to entertain grave doubts about the "non-local", that is Bohm style, realist programmes. For one thing they are very difficult to reconcile with relativity theory, given their non-local effects. While this need not rule them out *a priori*, these approaches do imply that the demand for causal explanation in QT does do violence to at least the spirit of relativity theory. This is difficult for the realist of course because realism demands not only causal explanation, but it also demands that different theories be consistent (a realist could hardly call for belief in the truth of two mutually exclusive theories). Moreover, non-local approaches involve the belief in a number of theoretical entities for which there is at present no evidence (pilot waves, "the subquantum ether" and so on, depending on the non-local approach in question) and indeed for which there is no way of testing. It is simply not clear that giving up the realist demand for explanation in this one case is a higher price to pay. This becomes more telling when one considers the fact that unlike the local hidden variable theories, no feasible experiments currently exists that could potentially show the non-local approaches to be empirically superior.[20] However, if such experiments could be conducted then it would serve as an excellent example of how a philosophical standpoint imposes methodological demands that can bear empirical fruit whichever way they came out. For the time being, then, it is not unreasonable to take the Copenhagen Interpretation to be the most successful to date, certainly this opinion is the majority stance among working physicists who concern themselves with foundational debates.

In a well-formulated deflationary philosophy of science, explanation can of course be considered to be a virtue, but it must be balanced against other concerns in context. As I have argued above, the price for it in the Bohm case may well be too high. In addition to the other concerns with it, the Bohm approach has not proved as fruitful as standard QT in deriving new theories. Thus far, theories that extend QT into the relativistic domain have developed alongside of and are generally interpreted in terms of standard QT.[21] I have already noted that it is true that Quantum Field Theory holds no really new conceptual issues for the question of realism in quantum mechanics (the main issue here is again the difficulty associated with the status of the mathematical formalism just as it is with QT). And of course, this case study is directed at the resolution of the interpretative debate associated specifically with ordinary QT, and its methodological implications.

Nevertheless, it must be noted (at some risk of digression) that while Quantum Field Theory arose directly from standard QT it does pose some compatibility problems for a Bohm style view which is not yet able to fully account for Quantum Field Theory.

Some, like Cushing for example, have maintained that in spite of some current difficulties Bohm's approach could, in principle, someday, be shown to be compatible with Quantum Field Theory. Moreover Cushing, as I noted above, holds out the hope that Quantum Field Theory will someday be replaced by a more fundamental theory more in line with Bohm's view. However, Cushing's views are less than convincing in that they call for the rejection of a perfectly adequate view in favour of one that might someday be made consistent with Quantum Field Theory or whatever theory that might someday replace it. Nevertheless, even if this turns out to be true, it cannot be denied that the ability to spawn new theories is a clear virtue relative to the mere ability *in principle* to be made consistent in an *ad hoc* way to theories that arise out of a rival interpretation. But there are good reasons to believe that Bohm style interpretations of QT cannot be extended to the Quantum Field Theory, indeed they are much more difficult to reconcile with Quantum Field Theory than they are with ordinary QT.

As Paul Teller has pointed out, mathematical techniques central to quantum field theory like the process of renormalisation of infinite divergent integrals cannot be equated with a causal description of the actual system without introducing considerable conceptual difficulties. For example, in some cases the "bare mass" of a "particle" before interaction with its own field would have to be taken as infinite and so also must be the value of the interaction with its field.[22] One is hardly required to believe in a particle having infinite mass, before interaction (even though this mass is never actually observed) if one takes the formalism of renormalisation to be simply a calculating device used to generate empirically adequate predictions like an anti-realist interpretation recommends. It is very difficult to see how a Bohm style demand for realist causal explanation can accept the use of a technique like renormalisation to be legitimate, yet some of the most accurate predictions ever derived by any science have come from Quantum Field Theory. Given all of this it becomes obvious that a Copenhagen style interpretation poses no problems for the relativistic extension of QT that takes place in Quantum Field Theory, it is also clear that the Bohm interpretation is not so fruitful. In addition, even if Cushing's hope that Quantum Field Theory is someday replaced by a yet more fundamental theory, there are already indications that this will not help his case for Bohm. The technique of renormalisation is very likely to play a central role and, of course, renormalisation is one of the main problems that Quantum Field Theory poses for a realist. As Teller has noted, this technique arose in the first formulations of Quantum Field Theory in Quantum Electrodynamics, but has persisted in all the newer Quantum Field theories that have arisen since.[23] Thus, a very strong argument exists to take non-local realist approaches to QT to be inferior in practice to the standard interpretation. While it must be admitted that a realist approach to Quantum Mechanics has not been finally ruled out, for the time being it would not be unreasonable to conclude that scientific practice has in this case so far tended to support and indeed profit from the anti-realist stance.

In any event, even if subsequent work in the field forces a reversal of the conclusions stated at the end of the last paragraph the failure of the local-hidden variable approaches still serves as an instructive example of the deflationary approach in action. In these instances the methodological goals of the rival positions derive in large part from the respective stances the rivals take on the realism question. Furthermore, the superior stance in this case emerged from the empirical fruit that resulted from the relative success of the rival programmes. That is to say, the Copenhagen verses local-hidden variables debate shows how the metaphysical stance on the realism question is settled in context empirically and not by general philosophical fiat. In the sort of deflationary philosophy of science that I am defending in this paper, nothing implies that the correct metaphysical stance to science for a research programme need remain static permanently. Just as science is not a monolithic entity with one correct methodological approach and stance on the realism question, variability also exists within specific research programmes over time, even though the best stance can sometimes be adjudicated at any given point in time.

4-5 Piecemeal Realism, NOA and Quantum Mechanics

Both sorts of deflationary approaches that I rejected in the last chapter, Fine's NOA and Miller's "Piecemeal Realism" purport to resolve the conceptual difficulties that attend the interpretation of Quantum Mechanics. We have seen a set of general objections to these positions, However, these arguments would of course lose some of their force if either of these positions proved a superior treatment to QT than the position that I favour. The plausibility of my position finds its support from its success in practice more than it does from general arguments describing how this is to be done; so too is its superiority demonstrated through its greater ability to be deployed in practice relative to its rivals.

In this section, I provide brief resumes of Miller and Fine's respective treatments of QT with the aim of analysing their relative success. Both of their positions possess some quite interesting features that make them more plausible than the straightforward explanatory-realist attempts at interpretation that I dealt with in the last section. In addition both positions bear some initial similarity to each other in terms of the features of their general interpretative stance. Nevertheless, their views are not identical. Miller tries to provide an interpretation of QT that is in line with his topic-specific account of realism. Fine of course tries to show that the interpretative stance that best accounts for QT is in line with NOA and, therefore, has "no additives" of a philosophical nature, either realist or anti-realist. Nevertheless, the particular approach that he favours is causally descriptive, and anti-Copenhagen in nature and bears some similarities to Miller's. I will begin this treatment of their positions with an analysis of Miller's views.

Miller, as I have noted, tries to show that the best interpretation of QT is in line with his account of realism and insists that it must be causally descriptive and explanatory. Miller, as the last chapter pointed out, does see explanation as the central virtue of any acceptable theory. Thus QT's status as a good predictor but poor explainer stands as an immediate counter example to this claim. Miller, however, simply denies that QT should be seen as primarily predictive and indeed claims that it is the value of QT's explanations that stand behind its success,

> This strict instrumentalist constraint would require the rejection of unquestioned explanatory achievements of quantum physics itself. For example, it is a great achievement of quantum physics to have explained the

radiation properties of the sun and other stars as due to the quantum states in stellar interiors.[24]

For Miller, given that quantum processes make the stars shine whether or not they are being measured, then must be describing real states of affairs. Miller, thus, contends that the acceptance of QT comes not from the predictive acumen of its mathematical formalism but from its superiority in fair causal comparison with other explanations of phenomena like the radiation properties of stars and hydrogen atoms.

Before treating Miller's specific proposals for interpreting QT, some comment is required on his contention that QT's appeal lies in its explanatory not its predictive fruitfulness. It must be noted that QT has been deployed in theories that explain the behaviour of stars, but it is nevertheless extremely difficult to provide an explanatory account of those quantum-phenomena that are deployed for explanatory purposes in other theories. For example, if Miller's point is granted that, for example, stellar theories are best seen as explanatory, then these theories explain the macro behaviour of stars not the micro-behaviour of the sub-atomic particles in the stellar cores. In other words QT is taken as a "black box" in these classical theories that are used to explain other macro-events and they do not of course offer any explanation of quantum phenomena. Miller's point seems to trade on an equivocation over which theories are to be taken as having explanatory features, QT or the theories that refer to it. Miller cannot claim that every adequate theory must only refer to theories that are explanatory without begging the question that every adequate theory must be explanatory. As I demonstrated in the last chapter, this is an assumption that cannot be taken for granted. Nevertheless, this point only tells against Miller's odd claim that QT really is more explanatory than it is predictive, his own explanatory approach to QT warrants analysis in its own right.

Miller purports to avoid the difficulties in providing a causally descriptive account of QT with a novel proposal. He argues to the effect that traditional difficulties arise out of the common but unwarranted assumption that properties of a quantum system are built up from the properties of the various parts of the system. For Miller the difficulties with realist QT are avoided if one replaces this idea of properties of with the idea of a sort of conceptual holism applying to quantum systems. That is to say, Miller proposes the idea that the properties of quantum systems are best understood as overall "system properties" that are irreducible to the properties of the parts of the system.[25] For Miller, the quantities of any given magnitude is determined by the holistic system properties and not by the individual properties of the parts of the system. However, quantum magnitudes in the various parts of a system do have definite individual values that are determined by the system properties, they do not just acquire these values on measurement. However, as some commentators like Paul Teller have pointed out, this last claim is a problematic response given the nature measurement problem.[26] For example, recall from this chapter's discussion of the Stern-Gerlach experiment that a definite value for one quantity in some cases precludes a definite value for another (in that case, spin taken along different axes). This seems hard to reconcile with Miller's conviction that quantities have definite values acquired from system properties not from measurement.

Miller engages the EPR case as an example of the way his approach is supposed to work, however, problems arise from his treatment of this situation as well. In Miller's view the difficulties that plague the local-hidden variable explanations of EPR derive from the unwarranted assumption that the state of the whole system results from the properties of the parts. When this view is abandoned in favour of his proposal for irreducible system properties the apparent non-local effects can be seen to be illusory. For Miller the correlations are not caused by the measurement but by the system properties. For example, the measurement of one particle's spin as + does not determine the value of the other as - (in the case of parallel detectors). The system properties determine an overall state of +:- for the particle pair. The correlation does extend across space but there is no non-local effect because the values for both particles exist before measurement and relate to the overall state of the system. A change in this overall state will change the state of all the parts but this is no more non local than the case where "the failure of a bank on Long Island instantaneously produces a loss of the value of a check in Montreal".[27]

The difficulty with this proposal is related to the inequalities that arise from the experiments performed by Bell. For Miller, the measurements on one part of the system are not the causes for the values of other parts of the system, the systems properties are. Thus, whatever the relative angle of the detectors is, the measurement at one detector has no effect on the result found at the other. If the plausibility of Miller's argument against non-local effects from the systems properties is granted and the other difficulties with this approach are set aside for the moment then Miller's view of EPR might be plausible if it proves to be empirically equivalent to the standard view of QT. However, this turns out not to be the case.

In Miller's view each particle has a well-defined spin for every axis prior to being measured and that measurement does not change the system state, it records the pre-existing state. Given this, the probability of any given part of the system having any particular value must be ½ (unless of course α is $0°$ and one particle has been measured). As Paul Teller has noted, claiming the existence of definite values that are possessed before and are unaffected by measurement turns out to satisfy the Bell-inequalities, in other words it provides the same predictions that the local-hidden variables approaches do.[28] Miller could respond that the holistic system properties might perhaps be invoked to explain EPR's unusual correlations if the demand that the particles have definite well-defined spins for each axis *before* measurement is dropped. Miller might for example argue that the particular sort of measurement situation (say a situation where the detectors are at 120° with respect to each other) changes the system properties that determine the joint spins, but this would negate his claim that measurement simply reads the real pre-existing system state. But this would rob his account of the main realist achievement to the effect that quantum magnitudes do have definite values before measurement and that QT causally describes those states. Indeed, if Miller were to make this move, his realist account would not only lose most of its force but he would be bound to provide an account of how the measurement configuration changes the real system properties, and this is the measurement problem all over again. Nevertheless this is not the approach that Miller takes, he does claim that the spin of the respective particles have definite values at all times for every axis that are unaffected by measurement.[29] Thus Miller's treatment of EPR and indeed his general interpretation of QT is no more successful than the hidden variables approaches are.

Arthur Fine's treatment of QT is somewhat more nuanced than Miller's is. Recall that Fine does not consider NOA to be a realist position because it is supposed to "add nothing philosophical" to the general scientific truth claims. Nevertheless, Fine's general approach to QT is more in line with the realist approach in so far as the "truth" that Fine feels QT should take at face value is the underlying causal structure implied by QT, not merely the "truth" of its acumen as a calculating device for making correct predictions. This approach to QT, of course, is vulnerable to many of the general difficulties with NOA outlined in the last chapter. Fine recognises that the locus of the debate in the philosophical foundations of QT is the status (or lack thereof) of its explanation of the underlying processes behind the mathematical formalism, not over debates about what metaphysical account of "truth" is best for science. But as the last chapter showed, it is precisely the former sort of question not the latter that characterises the realism debate in the philosophy of science. Fine's ideas about truth and NOA, then, can be reversed when discussing his own position. It is NOA and not the realism debate that adds nothing to our understanding of the workings of a science like QT. In spite of the general and insurmountable difficulties that attend NOA, Fine does offer some interesting arguments to the effect that the Copenhagen Interpretation is inferior to an explanatory interpretation, and it is these proposals that I will treat in this discussion.

In his book, *The Shaky Game*, Fine offers an insightful re-reading of Einstein's position on QT Fine correctly notes that Einstein's position has been unfairly characterised as entailing the local-hidden variable approaches that have been discredited by the work of John Bell and others. In an amusingly titled chapter "Is it Einstein for whom Bell's Theorem Tolls?" Fine argues that this is not necessarily the case. In this, Fine is probably correct, Einstein was always open to a radical reformulation of QT and did not simply seek hidden variables in order to preserve classical physics. Fine's parallel claim that Einstein was not scientifically conservative and closed minded about QT is probably true as well and Fine's evidence for this is ample.[30] Fine's clarification of Einstein's views is valuable for the historian interested in the thought processes of the great scientist. However, this does not alter the fact that Einstein's view that EPR demonstrated the incompleteness of QT and that a realist explanatory approach is needed to complete the picture has not fared as well in terms of practice as the standard approaches have.

For Fine, in spite of his acknowledgement of the difficulties that attend the hidden-variable approaches, takes Einstein's concerns with the Copenhagen interpretation at face value. That is to say, Fine more or less assumes that, whatever his views on realism might be, science ought to explain its results causally. NOA with regard to QT, then, certainly cleaves to a realist methodology. As we have seen, however, this is a question that cannot be begged and that the quantum paradoxes that Fine seeks to eliminate derive from the begging of just this question. Nevertheless, Fine does propose a positive explanatory programme that he claims is able to avoid the difficulties that plague hidden-variables. The success of such a programme could, in the deflationary philosophy defended in this thesis, offer some support to a realist reading of QT if such an explanatory approach could do all the work of the Copenhagen interpretation without creating even greater interpretative problems than it solves. To examine the possibility of success of such a programme, I will examine Fine's explanatory proposals subsequently.

Fine's programme trades on the fact of QT's probabilistic nature. QT's predictions are, of course, of a statistical nature. A prediction that only gives the probability of a given result can only be confirmed with regard to a set of identically prepared runs of the experiment in question. The goal of an explanation then, given QT's probabilistic nature, is not so much to account for the results of a particular experiment but to explain the statistics that result from many such trials. The difficulty with the hidden variable approaches arises from the fact that they predict somewhat different statistical outcomes from repeated trials than the Copenhagen interpretation, statistics that are experimentally disconfirmed. Fine proposes that this difficulty is to be avoided by noting that the choice to measure a given magnitude (spin along a given axis) reduces the ensemble from which the statistics are derived to a subset of that ensemble, and the statistics are to be derived from that subset. Fine dubs this approach the "prism model" approach because there is not one ensemble of measurement results but a spectrum of possible ensembles. He proposes that for a given quantity, say A_1 (spin along a given axis for one particle in an EPR type experiment), quantum systems have some property that makes only certain systems within a given ensemble of systems such that they are "not suitable for having an A_1 measurement performed on them".[31] This applies to each magnitude. If a measurement is taken of subsystem A_1 and on B_2 (the spin of the other particle along another axis) as in the EPR case, the ensemble from which statistics are derived is limited to only those systems where both A_1 *and* B_2 measurements are possible. That is to say, calculation is restricted to the intersection of the sets in E (the whole ensemble) where both E_1 and E_2 measurements are possible ($E_1 \cap E_2$). Thus, Fine's proposal implies that quantum systems have built in properties that limit the possible measurement outcomes because only the subset of the ensemble is suitable for that sort of measurement. By thus limiting the possible outcomes by supposing the existence of these limiting properties, Fine notes that the statistical discrepancies between hidden variable approaches and Bell's experiments can be overcome "at least in principle".[32] The prism model approach is, as Fine notes, a refinement of the local hidden variable approach and therefore does not violate locality any more than they do. In this sense the prism approach to QT bears some similarity to Miller's approach to QT in so far as the observed correlations are explained by qualities of the quantum systems in question and not by influences travelling from one sub-system to the other.

Fine is quite cautious about proposing his prism models as *the* explanatory solution to the interpretative questions that plague QT. For him the prism models simply show that "there is no incompatibility... between quantum physics and realism as such".[33] Fine notes that any number of explanatory approaches might still work other that the prism models. For Fine, this situation simply means that the general truth of QT as describing the world is to be taken as true, no support for a particular realist stance is so far implied. It is enough in Fine's view for everyone to agree to "hold the theory true, or at least approximately true".[34] Naturally Fine takes the "truth" of the quantum world at what he deems to be face value, that is under NOA, but as we have seen this position is unsound. Rejecting NOA one must take Fine's prism models as a general plea for the validity of a causally explanatory approach, in other words of a realist approach.

How is Fine's plea to be judged? The role of the prism models in his argument is not necessarily supposed to serve as the final realist reading of EPR or QT in general. They are just supposed to show that an explanatory approach is possible and therefore to be recommended over the Copenhagen interpretation given Einstein's critique of anti realist QT that he accepts. However, valuable Fine's re-reading of Einstein's position may be, it is as I have shown precipitate to take his critique of the Copenhagen interpretation as valid. This is so because the so-called interpretative anomalies associated with it derive from begging the question of realism and reading the Copenhagen view as providing a strange and implausible ontology that ought to be replaced. But this, as Finkelstein points out, is to criticise a pseudo-version of the Copenhagen interpretation and both Fine and Miller are guilty of this straw man argument. In the Copenhagen view the only paradoxes to be eliminated are those that come hand in hand with the realist methodology it rejects. Still this is to find fault with Fine's general philosophical position on the need for a realist alternative not on his suggestions to the effect that the construction of one is possible.

In spite of the fact that Fine has argued that the prism models can produce better statistics than hidden variable approaches, there are very good reasons to reject this approach. The prism models assert that the statistics are to be derived from the limited set of cases where the systems are amenable to particular measurements. The questions that Fine does not treat are questions as to how and why particular systems are such that only certain sorts of measurements on them can be performed. In other words. Fine must be able to offer a explanation as to what causes the subsystems to be so differentiated given the fact that they are identically prepared. In Fine's proposal the choice of the type of measurements determines how the statistics are to be calculated. However, consider an individual experiment, how are the probabilities to be assigned? According to Fine particular sorts of measurements sort the ensembles to include only those that can be measured for that quantity. This implies that the measurement performed determines the properties of the systems in question in each given individual situation, but how does this happen? Fine has no answer. This difficulty of course is the measurement problem all over again and it is telling against Fine because the whole point of an explanatory approach is to provide a causal account of why the measurements have the results that they do. Fine's proposals amount only to the idea that measurement somehow determines the sort of hidden variables that are present such that the probabilities for given measurement outcomes match those given by QT. This proposal can be clearly seen to be unacceptably *ad hoc*, furthermore it eliminates the possibility that the prism models can account for measurement such that it describes the properties that a given quantum system has regardless of whether or not it is measured. It is this feature that was supposed to give hidden variables approaches their appeal (and this includes the prism models).

Indeed, it is clear that the difficulties that plague the prism models mirror those that plague Miller's "system properties" approach quite closely. Recall that a supporter of Miller could respond that the real system properties are changed by certain sorts of measurement configurations and thus avoid the statistical conflicts that are identified by Bell's work. This move is more or less exactly what the prism models amount to. However, as I noted in my discussion of Miller's interpretation, this robs realism of its appeal because it must still explain how this effect of the measurement process occurs. In fact, this sort of an approach simply shifts the ground of interpretative difficulty from the question as to what causes the observed correlations in a system like EPR, to what is it about he measurement process that causes the system properties to determine the results that they do. In Fine's case, the question simply shifts to the very similar problem of why particular measurement configurations cause the limitation to the sort of system such that only those that can be averaged to be consistent with QT are permitted.

All things considered, then, Fine cannot appeal to prism models to show that QT is amenable to some sort of explanatory account. Furthermore, given the other difficulties that attend realist approaches to QT that I have outlined in this chapter, it is reasonable to conclude that the realist demand for explanation in QT should be abandoned. In other words it is reasonable to adopt the interpretative stance recommended by my deflationary approach to the realism question. In the case of Quantum Theory, this approach is the non-explanatory Copenhagen interpretation.

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[1] James Cushing, "Foundational Problems in Quantum Field Theory", in H.R. Brown and R. Harré (eds.), *Philosophical Foundations of Quantum Field Theory*, Oxford: Clarendon Press, 1990, p. 33.
[2] *Cf.*, *ibid.*, pp. 32-33.
[3] *Ibid.*, p. 27.
[4] In QT, the vectors in a Hilbert space are defined in complex numbers (a complex number is the sum of a real with an imaginary number, i.e. $(a + ib)$, where i is the square root of -1. A real number, then, is an element of a subset of the complex numbers where $b = 0$, an imaginary number is an element of the subset of the complex numbers where $a = 0$). The absolute square of a complex number is not that number times itself, it is that number multiplied by its complex conjugate. The complex conjugate of $(a + ib)$ is $(a - ib)$ i.e. the plus or minus sign before the i is reversed. Thus the absolute square of $(a + ib) = (a + ib)(a - ib) = a^2 + b^2$. Thus, the absolute square is always both real and positive. If the value of the state vector v is "normalised" that is 1, then the absolute square of the vector v' resulting from the application of a projection operator (derived from the Hameltonian) will result in a number a where $0 \leq a \leq 1$. Thus, the absolute square of an eigenvalue is appropriate to represent probability *cf.*, R.I.G. Huges, *The Structure and Interpretation of Quantum Mechanics*. Cambridge Mass.: Harvard University Press, 1989. pp. 28 – 31.
[5] *Cf. ibid.*, pp. 2-5.
[6] *Cf. ibid.*, pp. 2-5.
[7] *Cf. ibid.*, pp. 6-8.
[8] *Cf. ibid.*, pp. 6-8.
[9] *Cf.*, *ibid.*, pp. 226-237 *cf.* also Miller (1987) pp. 522-527.
[10] *Cf.*. *ibid.*, pp. 226-237, also Henry Krips, *The Metaphysics of Quantum Theory*, Oxford: Clarendon Press, 1987, also James T. Cushing, *Quantum Mechanics*, Chicago: University of Chicago Press, 1994, p. 25.
[11] In Einstein's more flamboyant version of the "Shrödenger's Cat" experiment, the unfortunate beast is killed by a barrel of gunpowder that explodes when the system is in state *b*. *(cf.* Fine "Shakey Game" p. 85.).
[12] Cushing, *op cit.*, p. 185.
[13] David Finkelstein, "The Universal Quantum" in R. Kitchener (ed.), *The World View of Contemporary Physics*, New York: SUNY, 1988, p. 80.
[14] *Ibid.*, p. 80.
[15] R. Feynman, *The Character of Physical Law*, Cambridge Mass.: MIT Press, 1965, p. 165, also quoted in *ibid.*, pp. 1-2.
[16] This version of EPR is actually David Bohm's. However, it deals with exactly the same set of problems as Einstein's original example. *cf.* Huges, pp. 158-63. The Bohm version of the EPR is a little easier to grasp and has served as the basis for the EPR experiments that have actually been carried out. Therefore this version is almost always used in the literature when discussing EPR. I have followed this custom and also use Bohm's formulation.
[17] Quoted in Huges, *op cit.*, p. 158.

[18] *Cf.* R. Kitchener, "The World View of Contemporary Physics: Does it Need a New Metaphysics?", in R. Kitchener (ed.), *The World View of Contemporary Physics*, New York: SUNY, 1988, pp. 11-13, *cf.* also Huges, pp.158-164 .
[19] Miller *op cit.,* 1987, p. 588.
[20] *Cf.,* Richard Healey, *Philosophy of Quantum Mechanics,* Cambridge: Cambridge University Press, pp. 24-6.
[21] *Cf.,* R. Weingard, "Virtual Particles and the Interpretation of Quantum Field Theory" in H. Brown and R. Harré (eds.), *Foundations of QFT,* pp. 43-59.
[22] Paul Teller, "Three Problems of Renormalisation", in *Philosophical Foundations of Quantum Field theory*, ed. H. Brown and R. Harré , Oxford: Clarendon Press, 1990a, p. 76.
[23] Teller (1990a) *op cit.,* p. 74.
[24] Miller *op cit.,* p. 531.
[25] *Cf., ibid.,* p. 592.
[26] P. Teller, "Review of Fact and Method: Explanation Confirmation, and Reality in the Natural and the Social Sciences", *The Philosophical Review*, 99, 1990b, p. 646.
[27] Miller *op cit.,* p. 594.
[28] Teller (1990b) *op cit.,* p. 646.
[29] Miller *op cit.,* p. 593.
[30] *Cf. ibid.,* p. 57-8. Here, Fine cites Einstein as noting that simply adding something to existing QT (i.e. hidden variables) is inadequate. Einstein, in the passages cited, indicates that he favoured a comprehensive reworking of the theory, in other words he saw QT as merely an interim approach on the way to a more fundamental theory.
[31] *Ibid.,* p 52.
[32] *Ibid.,* p. 52 *cf.* also A. Fine, "Antinomies of Entanglement: The puzzling case of the tangled statistics." *Journal of Philosophy* 79, 1982, pp. 733-47.
[33] *Ibid.,* p. 169.
[34] *Ibid.,* p. 170.

Conclusion

The conclusions advanced in the previous four chapters have several striking implications for the question of scientific realism and the resulting philosophical view of scientific theorising. The most striking conclusion is perhaps that no general philosophical characterisation can be drawn of the nature of scientific theories or practice that has implications for the whole corpus of science. All parties in the realism debate have generally agreed that any philosophical position on the realism question must account for science as it is in fact practised. Moreover each major "global" position (general theses supposed to account for the whole of science) treated by this thesis has claimed to be superior to its rivals in doing just that. However, it is clear that the standard empiricist model, the versions of realism which sought to replace it and van Fraassen's "constructive empiricism" have all failed to make good this claim by failing to recognise the genuine plurality that exists in science.

In the case of the standard empiricist model, scientific theories were said to be characterised by a lexical distinction dividing the theoretical vocabulary into two parts: one whose terms' interpretation are unproblematic and another composed of "theoretical" terms whose interpretation can not fully be given empirically. Acceptance of a theory, as construed, is only supposed to imply commitment to the terms whose full interpretation can be given empirically. However, it eventually became clear that it is impossible to sustain such a distinction. The reason for this is the now widely accepted fact that a theory's terms take on their meaning in relation to the whole theoretical vocabulary. In other words, so-called observational terms take on their interpretation in part through reference to the theoretical vocabulary. Thus, the standard empiricist model fails to do justice to the structure of theories as we actually find them.

Realism, which purports to replace the standard empiricist model, trades on the problem in delineating a distinction between theoretical and observational terms. In the absence of such a clear distinction, to accept a theory implies acceptance of the whole theoretical apparatus, including the whole theoretical vocabulary including those terms that refer to unobservables. However, on close analysis realism fares no better than the standard empiricist model. As van Fraassen has pointed out, the failure to delineate a clear distinction in theoretical terms in no way implies necessary belief in the whole of a theory's constituents. Scientists can, in certain situations, doubt the general accuracy of a given theoretical model, and may wish to only hold onto it as a tentative posit used to achieve consistency with existing data. This is especially true when one considers cases where quite different rival theories are equally good at accounting for the same data.

Furthermore, realism implies descriptive explanation of the phenomena it deals with, and therefore must consider any theory that does not provide causal explanation incomplete. However, as van Fraassen makes clear it is simply not the case that explanation is the universal goal of science. Van Fraassen, thus, proposes an alternative to realism that he dubs "constructive empiricism" which while recognising the importance of the explanatory role of science sees this as a result of the broader goal of science, "empirical adequacy". However, van Fraassen's views rely on a resurrected distinction between the observable and the unobservable with his "semantic account" of theories, and this reworked distinction is supposed to apply to the whole of science. Nevertheless, the distinction proposed by the semantic account of theories fares no better, in the final analysis, than the earlier distinction proposed by the standard empiricist view.

One key difficulty that plagues all of these views is, as I have noted, their inability to be universally deployed. In response to this, a body of literature has taken what has been described as a "deflationary" turn. That is to say, accounts have been proposed as solutions to the realism question that purport to avoid general philosophical theses that are supposed to apply to the whole of science. Miller's "piecemeal realism" and Fine's Natural Ontological Attitude (NOA) represent two such views. However, while Miller accepts a plurality of interpretations with regard to the status of unobservables, he maintains a generally realist methodology. That is, he argues for causal explanation as the hallmark goal of science. And this claim is problematic, as we have seen. Fine seeks to avoid the problems of global approaches by imposing a general ban on metaphysical interpretation of the status of unobservables. However, this ban on interpretation requires that a clear distinction be drawn between the scientific and the metaphysical, and it is by no means clear that such a distinction can be drawn. Indeed, this is one way of looking at the difficulties that plagued the earliest versions of logical empiricism. In addition, Sergio Sismondo's work demonstrates that NOA also overlooks the fact that philosophical interpretation is indeed part and parcel of scientific theories and it thus fails to do justice to science.

Proceeding from the difficulties with piecemeal realism and NOA, I propose that a properly construed deflationary approach to the realism question take not only the plurality of science's interpretation of unobservables into account but also the plurality of methods that exist across the sciences. I also contend that a deflationary approach does not merely read the interpretations of unobservables from the theories as it finds them but also plays a normative role in adjudicating the best such interpretation. This adjudication takes place through the analysis of the methodological role that a metaphysical interpretation implies. The adjudication of an approach to the realism question taken by a particular research programme takes place through an evaluation of the relative success of the interpretation's methodological dictates as they are deployed in practice. This becomes particularly important when one takes into account theoretical domains where two distinct interpretations (along with their resulting methodological dictates) are in competition. In such a situation the deflationary approach that I propose plays a normative role by adjudicating the more plausible stance through a comparative evaluation of the relative success of the rival interpretations by tracing the relative success of the empirical ramifications of their methodological dictates. This is not simply to say that the better predictor is to be taken as the preferable choice. Rival programs come with rival methodological goals (e.g. explanation vs. empirical adequacy), what is adjudicated is the better set of methodological goals appropriate for that situation. The adjudication is based on the relative success of the programmes at achieving their goals.

One particularly striking such example lies in the domain of Quantum Mechanics where for most of the past century there has been fierce debate between the anti-realist "Copenhagen" interpretation and realist demands for causal explanations of quantum systems. Indeed, Quantum Mechanics has been plagued by constant conceptual problems and foundational debates regarding its methodological and metaphysical implications since its inception. Moreover van Fraassen, Miller and Fine all claim that their approach to the realism question resolves the conceptual problems that have plagued Quantum Mechanics. Thus an examination of the debate over realism in Quantum Mechanics serves as an excellent case study to demonstrate the fruitfulness of the deflationary approach to the realism question that I recommend.

It becomes clear upon examination that the conceptual problems that are said to plague the Copenhagen interpretation really are the result of begging the question of realism. The Copenhagen interpretation's failure to provide a causal explanation of the systems it deals with only present difficulties if one is committed to the idea that explanation must always be the goal of science. Explanatory power might well be a good reason to accept a realist reading of Quantum Mechanics (QT) if any such interpretation were as successful empirically, but as the results of the EPR experiments show that this is not the case. Moreover, close examination of various classic experiments in QT clearly show that serious conceptual difficulties attend any attempt at a realist explanation. Furthermore, Fine and Miller's various attempts to recast realist approaches to QT fare no better than traditional realist approaches.

A conclusion clearly emerges from this case study. The deflationary approach that I defend is clearly superior to Miller's and Fine's in accounting for standard practice in QT. It also becomes clear that it can play a normative role. In the case of QT it clearly recommends that the realist demand for causal explanation be dropped in virtue of the clear methodological superiority of the Copenhagen approach. In addition other conclusions emerge from this case study.

The treatment of quantum mechanics demonstrates how the realism question is settled, in context, empirically and that it is not settled by general philosophical fiat. In the sort of deflationary philosophy of science that I propose, nothing implies that the correct metaphysical stance on science for a research programme need remain static permanently. Just as science is not a monolithic entity with only a single methodological approach and stance on the realism question, so also does variation exist within specific scientific programmes over time, even though the best stance can sometimes be empirically adjudicated at any given point in time. Indeed the adoption of anti-realism in fundamental physical theories that the advent of quantum mechanics brought about can be seen as one such instance. Although in previous centuries much of physics had been characterised by a robustly realist stance, the strange experimental results of early quantum research made a shift to an anti-realist interpretative and methodological stance plausible and in fact prudent. It would be foolhardy to assume that the situation that currently prevails in fundamental physics will never again undergo change.

The point that I am making in the discussion of QT is not a general philosophical argument to the effect that a realist approach is always a failure, or that the realist methodological dictum that observed correlations stand in need of causal explanation is always to be rejected. As I have repeatedly stressed, I am not arguing for any particular *a priori* methodological or philosophical conception of science in general. What I am trying to make clear is that the correct lesson to be drawn from failure of certain realist inspired approaches in the case of EPR is that the methodological dictates of scientific theories derive in part from their philosophical and metaphysical stance. In addition I have demonstrated that the methodological dictates that have derived from a realist stance, in this case at least, are empirically inferior to their anti-realist rivals. Nevertheless, it must be noted that there have been a wide variety of scientific theories that have seen empirical triumphs arise from a methodological commitment to the demand for causal explanation. A deflationary philosophy of science demonstrates how a wide variety of interpretative stances have methodological import. For example, Sismondo, as I described in chapter three, shows how a constructivist philosophy of science can be directly related to the methodology of Scott's research.

The case of the relative empirical success of the Copenhagen Interpretation in the EPR case shows that the philosophical and methodological stance can lead to the formation of different empirical interpretations of the same laboratory situation. In the case of EPR, scientists of a realist bent formulated and gave their support to the "local hidden variable" theories which made different predictions than their anti-realist rivals, and the debate was settled empirically. So not only does the philosophical stance associated with a theory makes methodological demands on its adherents but the success or lack of success of the practical deployment of those dictates allows judgement to be passed, in that case, on the validity of those demands. These judgements, as I have already noted, cannot be generalised to the whole corpus of science but must come on a case by case basis, as was exemplified by the debate over the interpretation of EPR. Of course this comes as no surprise if philosophical theses are taken to be part and parcel of scientific theories. The domain of a theory's metaphysical content is the domain of the theory and no more, and the same is true for the methodological content: its domain is the domain of the theory and no more. The validity of those metaphysical and methodological dictates can be judged on the basis of the success of their deployment in practice relative to their rivals.

The example of quantum mechanics illustrates the role that philosophical approaches to the realism debate have for scientific methodology. There is no general *a priori* stance to be obtained from philosophy with regard to the realism question or the concomitant methodology. The individual theories within the field each have philosophical content and their own commitments with regard to realism, and this content makes metaphysical demands that take the form of maxims. In the case of the "hidden variable" theories we can see the realist methodological maxim "seek causal explanations for observed regularities" at work. In the Copenhagen Interpretation it is easy to see a methodological maxim, "don't worry about hidden causes, focus on what can be observed because there is nothing else to discuss" at work. This, of course, is in line with the anti-realist idea that the quest for empirical adequacy is primary and not the quest for causal explanation. The adjudication of the correct stance, in this case, came in the form of the success of the Copenhagen interpretation relative to its local-hidden variable rivals. While this example does not settle either the realism question or the proper methodological stance for every science it shows how the debate has methodological import in terms of formulating philosophically derived maxims that guide research.

The realism debate is not to be settled for the whole of science. It is to be settled on a case by case basis in terms of the relative success or failure of the deployment of the methodological maxims that are recommended by the various sides. This thesis demonstrates how it falls to the philosopher to interpret the metaphysical stance implicit in research programme and derive the implicit methodological maxims. In the Scott case treated in chapter three for example, the constructivist metaphysics recommends the deliberate abstraction of the experimental environments from the natural world in order to facilitate the isolation of the variables of interest. Here again, we see the success of an approach inconstant with some forms of realism, but again these conclusions are not to be taken as a general ban on realist interpretation, it merely serves to demonstrate one more case where a realist approach is not to be recommended.

I have demonstrated how NOA's anti-metaphysical stance is an inferior alternative to the proposal that the correct deflationary stance is metaphysical pluralism. Taking the sciences within their own context implies that the metaphysical content of the sciences must be taken seriously. Nevertheless, if a philosophy of science is to have anything other than a descriptive role with regard to scientific practice then it must be fruitful in adjudicating the best stance on the realism question on a case by case basis. This is especially true for those cases (like the EPR situation) where rival stances coexist. The various deflationary or naturalist positions that have hitherto been proposed have been conspicuous for not addressing this question fully. Thus, the discussion of the case of quantum mechanics outlined how a correct analysis of the relation between philosophy and science shows how a deflationary philosophy of science can play this rôle.

When science is taken within its own context it becomes clear that no sharp distinction exists between science and philosophy and that science, when taken at its most general, is clearly philosophical. Philosophy, then, has a central role in the conceptual foundations of scientific theories and a central role in the formation of the methodological dictates associated with scientific theories. Even more importantly, this foundational and methodological role, as the EPR case demonstrates, makes it possible for the adjudication of the best philosophical and methodological stance to take place on a case by case basis. Thus, a pluralist deflationary philosophy of science does not make *a priori* methodological demands on existing science but permits an understanding of the methodological role that philosophy does play in scientific practice.

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