

THE UNIVERSITY OF CALGARY

CONSTRUCTIVE EMPIRICISM, REALISM AND SCIENTIFIC METHODOLOGY

by

J.L.COOKE

A THESIS

SUBMITTED TO THE FACULTY OF GRADUATE STUDIES

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE

DEGREE OF MASTER OF ARTS

DEPARTMENT OF PHILOSOPHY

CALGARY, ALBERTA

JULY, 1987

© J.L.COOKE 1987

Permission has been granted to the National Library of Canada to microfilm this thesis and to lend or sell copies of the film.

The author (copyright owner) has reserved other publication rights, and neither the thesis nor extensive extracts from it may be printed or otherwise reproduced without his/her written permission.

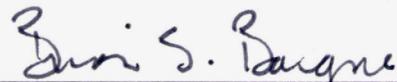
L'autorisation a été accordée à la Bibliothèque nationale du Canada de microfilmer cette thèse et de prêter ou de vendre des exemplaires du film.

L'auteur (titulaire du droit d'auteur) se réserve les autres droits de publication; ni la thèse ni de longs extraits de celle-ci ne doivent être imprimés ou autrement reproduits sans son autorisation écrite.

ISBN 0-315-37975-8

THE UNIVERSITY OF CALGARY
FACULTY OF GRADUATE STUDIES

The undersigned certify that they have read, and recommend to the Faculty of Graduate Studies for acceptance, a thesis entitled "Constructive Empiricism, Realism and Scientific Methodology," submitted by J.L. Cooke in partial fulfillment of the requirements for the degree of Master of Arts.



Brian S. Baigrie, Supervisor
Department of Philosophy



John W. Heintz
Department of Philosophy



Terence Penelhum
Department of Religious Studies

July 24, 1987

ABSTRACT

Bas van Fraassen has offered his constructive empiricism as an alternative to the more popular scientific realism. Constructive empiricism is often referred to as 'anti-realist' and standard realist arguments are brought against it. I argue that while constructive empiricism has anti-realist aspects, it also has realist aspects and is, in fact, best understood as presuming basic forms of 'metaphysical' and 'semantic' realism. I suggest that there is only one crucial point of disagreement between constructive empiricism and scientific realism -- that is, whether our best scientific theories are to count as knowledge.

In Chapter One I provide a critical exegesis of van Fraassen's presentation of constructive empiricism and scientific realism. I focus on where and how van Fraassen contrasts the two positions and I discuss how those contrasts relate to the structure of van Fraassen's argument against scientific realism. In Chapter Two I offer an analysis of van Fraassen's discussion of the notion of 'literal truth'. I argue that constructive empiricism is to some extent a realist position and that van Fraassen need not be seen as counselling that truth has

no significance for science. In Chapter Three I apply my articulation of constructive empiricism to some methodological concerns which have been brought as arguments against van Fraassen. I show how each argument fails to damage constructive empiricism and I offer a general argument to the effect that constructive empiricism is at least the methodological equal of scientific realism.

ACKNOWLEDGEMENTS

I am very grateful to Professor Brian Baigrie for his patience and careful supervision, and to Professors John Heintz and Terence Penelhum for their time and advice. I would also like to thank my friends Mark Gardiner, Damon Gitelman, and Geoff Gorham for listening to me and supporting me when scepticism ran deep and argument ran thin. In addition, I would like to thank Martin Cunningham, John Pugsley, and Alastair Tait who helped me keep the wheels on long before this thesis was an idea, let alone a concept. Finally, I am especially grateful to my parents, Muriel and Dale Cooke, who have given me incomparable support and encouragement.

.DEDICATION

For my sister and very good friend Karen Anne, whose supererogatory kindness and good will has sheltered me from many a storm.

TABLE OF CONTENTS

INTRODUCTION	1
CHAPTER ONE	
I. Scientific Realism and Constructive Empiricism ...	5
CHAPTER TWO	
II. A Hierarchy of Realisms	32
III. SR and CE in the Hierarchy	38
IV. The Divorce of Science from Truth	51
CHAPTER THREE	
V. The Issues of Scientific Methodology	66
1. The Miracles Argument	66
2. The Theory Dependence of Experimental Design ..	74
3. The Unity of Science Principle	78
VI. Concluding Remarks	83
BIBLIOGRAPHY	89

INTRODUCTION

In spite of the attacks coming from writers such as Thomas Kuhn (1970) and Paul Feyerabend (1975) scientific realism has remained perhaps the most widely held view in current philosophy of science. Bas van Fraassen (1980a) has offered his constructive empiricism as a serious rival to scientific realism. He has argued strongly for the comparative superiority of his constructive empiricist account of science. Van Fraassen's arguments for the viability of the constructive empiricist point of view have given rise to much recent response from the scientific realist camp. Any such critical interplay is likely to be valuable to the philosophy of science in general, if only because it naturally leads to progressively more careful and complete articulations of the positions involved. This thesis is intended as part of that process of articulation.

Van Fraassen's constructive empiricism is commonly labelled anti-realist. I think that this practise is unfortunate and that it has lead to some rather misguided criticism. The criticism to which I refer is the charge that constructive empiricism is unable to offer a plausible explanatory account of the progress of science and the use of scientific method. Some of the critics who bring this charge maintain that scientific realism offers the only

such plausible account. In this thesis I will address the question of just to what extent van Fraassen's constructive empiricism is an anti-realist position so that I may respond to this criticism. What I intend to offer, then, is a partial defence of constructive empiricism. The central claim in the defence I will offer is that the criticism under consideration depends upon a misconstrual of constructive empiricism for its success. My thesis is that when constructive empiricism is correctly understood this criticism can be seen to have no force. I will argue that the elements of realism which allow scientific realism to offer its account of science are present in constructive empiricism also. I will argue, therefore, that scientific realism and constructive empiricism are in equally good positions to offer an account of scientific progress and the scientific method.

Much of my discussion will consist of an exegesis of constructive empiricism, explications of crucial or obscure aspects of the position, and arguments for those elements of my exegesis which might be considered controversial interpretations of the constructive empiricist position. The criteria upon which the defence I offer should be judged include not only the application of my interpretation of constructive empiricism to the criticism which I consider, but also my ability to present an

exegesis which is coherent, cogent, and as relevantly complete as van Fraassen's writings will allow.

In Chapter One I will present constructive empiricism (CE) and the scientific realism (SR) to which it is opposed. I will discuss theses concerning theory acceptance and the aim of science, theses which van Fraassen uses to contrast SR and CE. I will attempt to illustrate the structure of van Fraassen's argument against SR and I will comment on the appropriateness of the contrasts he draws in view of that structure.

In Chapter Two I will introduce a hierarchy of realisms which I will use to explicate some aspects of realism common to SR and CE. I will consider the significance of van Fraassen's contention that theories should be 'literally construed', as well as his acceptance of the meaningfulness of the notion that scientific statements might be 'literally true'. I will use the implications of constructive empiricism's acceptance of 'literal truth' to establish the extent to which CE is, in fact, a realist position. I will explicate the apparent divorce of science from truth which van Fraassen advises. And I will comment on the significance of the divorce from truth for van Fraassen's position.

Finally, in Chapter Three, I will address some examples of the misguided criticism to which I have

alluded. I will consider three such distinct examples in turn and, with reference to my interpretive exegesis of CE, I will offer an explanation of just how they have gone awry. I will conclude with a general argument to the effect that all arguments of a similar type to those which I have considered must similarly fail to be telling for SR as against CE.

CHAPTER ONE

I. Scientific Realism and Constructive Empiricism

When van Fraassen introduces SR and CE he casts them in terms of theses about the aim of science and about the belief involved in our accepting a scientific theory. His statement of SR is:

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. (1980a p.8)

By contrast, van Fraassen characterizes CE as follows:

Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate. (1980a p.12)

Van Fraassen's notion of empirical adequacy is central to his characterization of CE. He begins to explicate the notion as follows:

... a theory is empirically adequate exactly if what it says about the observable things and events in the world, is true -- exactly if it 'saves the phenomena'. A little more precisely: such a theory has at least one model that all the actual phenomena fit inside. (1980a p.12)

A claim that a theory is true says more about the theory than a claim that it is empirically adequate does. But the two claims are not radically different in character. To be

empirically adequate is to be partly true; to be true so far as what is observable is concerned.

In discussing truth and empirical adequacy van Fraassen makes the comment: "A statement is true exactly if the actual world accords with this statement." (1980a p.90) This comment is correctly to be taken as a venture toward a statement of correspondence truth but as such it is evocative of Donald Davidson's (1983 p.425) parenthetical remark: "There is no straightforward and non-misleading way to state this [correspondence truth]" The question of what it is for a theory to be true is of course philosophically interesting in its own right. I will say more to explicate the notion of truth at work in both SR and CE in Chapter Two. For now it is worth noting that van Fraassen uses the notion of truth as a tool for contrasting SR with CE. His reliance on truth in this way is indicative that van Fraassen does not regard the notion of truth as problematic so far as the debate between SR and CE is concerned.

Van Fraassen continues:

Semantic properties and relations are those which concern the theory's relation to the world, or more specifically, the facts about which it is a theory. Here the two main properties are truth and empirical adequacy. Hence this is the area where both realism and constructive empiricism locate a central aim of science. (1980a p.90)

To compare truth and empirical adequacy, then, both are relations which might hold between a theory and the world (or facts about the world). To contrast them, truth is a relation which might hold between a theory and all the facts about the world of which it is a theory; empirical adequacy can hold between the theory and the facts about the phenomena only. A theory which is called 'empirically adequate' is supposed to be true so far as some of the facts are concerned -- specifically the facts about the observable world. A theory which is called 'true' is supposed to be true not only regarding the facts about the observable world, but also regarding the facts about the unobservable world. (Of course this is not meant to imply that a theory need be complete to be true. A theory can be true as far as it goes, without being an account of literally every fact.) A true theory is empirically adequate. An empirically adequate theory may or may not be true, depending on how it accords with the unobservable component of reality.

The notion of theory acceptance occurs in the statements of both SR and CE. There are, I think, two aspects of theory acceptance which one needs to be clear on in order to understand van Fraassen. The first aspect concerns the determining factors in the decision to accept a theory, the elements that contribute to theory

acceptance. According to SR as presented by van Fraassen, theory acceptance involves the belief that the theory is true. But also, so far as SR is concerned, believing that a theory is true entails accepting the theory; it is not an open question, not a separate decision, whether having come to believe a theory one will then go on to accept it. Accepting a believed theory is the only rational thing to do. For the scientific realist then, believing a theory is not an active ingredient in theory acceptance, that is, it is not something one does on the road toward theory acceptance. Rather, for the scientific realist, to believe a theory is to accept it and vice versa. The answer to the question of what the determining factors are for theory acceptance will thus be the same as the answer to the question of what the determining factors are for belief. Those factors, as it turns out, are judgements concerning the theory's manifestation of what I will call the 'theoretical virtues', including: empirical adequacy, empirical strength, internal and external consistency, explanatory power, and simplicity.(1)

The constructive empiricist, on the other hand, does not equate the acceptance of a theory with the belief that the theory is true. I want to argue, as well, that the constructive empiricist does not equate acceptance of a theory with the belief that it is empirically adequate. I

want to argue that, even for the constructive empiricist, theoretical virtues other than empirical adequacy can sometimes play a role in determining what theory is accepted. But such a thesis would require that theory acceptance is more than, and thus is not strictly equivalent to, the belief that the theory is empirically adequate.

Unfortunately, van Fraassen explicitly states: "I equate the acceptance of a scientific theory with the belief that it is empirically adequate." (1980a p.20) But van Fraassen not only contradicts me on this point, he also contradicts himself. In the same text van Fraassen writes that there is more to theory acceptance than belief. He also writes of consistency, both internal and with the facts, as being one of the "rock-bottom criteria of minimal acceptability".(2) But if consistency is a minimal criterion of acceptability, then acceptable theories must be not only empirically adequate but also consistent. Thus the simple equation van Fraassen suggests is misleading.

There are, I think, other good reasons to see van Fraassen's equating of theory acceptance with belief in empirical adequacy as a regrettable error on his part. First, if acceptance of a theory just is the belief that the theory is empirically adequate, then a problem arises for the constructive empiricist in the event of the

availability of two empirically adequate, empirically equivalent, but conflicting theories. Given the equation of acceptance and belief in empirical adequacy, the constructive empiricist would seem to be in the unfortunate position of having no choice but to accept the two conflicting theories. It might be argued that an anti-realist need not see such a situation as especially problematic. An instrumentalist, one who thinks that the value of theories is determined solely by their usefulness, might care only about what the theories say about what is observable and there, of course, two such empirically equivalent theories would not disagree. I will argue in Chapter Two section III below, however, that van Fraassen is not that sort of anti-realist. Moreover, van Fraassen's acceptance of the unity of science principle, the claim that ultimately we want a single theory which covers all scientific domains, which I discuss in sections IV and V-3 below, would suggest that he does view the acceptance of two conflicting theories as problematic.

Moreover, van Fraassen does not generally write as if the acceptance of more than one competing theory is a live option, for example:

Even if two theories are empirically equivalent, and acceptance of a theory involves as belief only that it is empirically adequate, it may still make a great deal of difference which one is accepted. (1980a p.4; my italics.)

Van Fraassen's intent that just one such theory is to be accepted seems clear.

Most importantly, so far as resolving the problem of this particular textual inconsistency is concerned, in a more recent essay in which he responds to criticisms of The Scientific Image, van Fraassen quite clearly leaves the simple equation of theory acceptance with the belief that the theory is empirically adequate behind, in favour of accepting an account consistent with my reading of him:

If some reasons for acceptance are not reasons for belief, then acceptance is not belief. And indeed some reasons for acceptance hinge crucially on the audacity and informativeness of the theory. So acceptance is not belief. (1985 p.281)

Van Fraassen adds that acceptance "involves a commitment to maintain the theory as part of the body of science" and that the unity of science principle together with his construal of acceptance "allows for the use of choice criteria that are logically independent of truth, empirical adequacy, or likelihood thereof". (1985 p.281) The effect of van Fraassen's construal of theory acceptance for CE is to allow that the determining factors in theory acceptance

are, just as they are for the scientific realist, judgements concerning the theory's manifestation of the theoretical virtues.

The second aspect of theory acceptance one needs to be clear on is what can be taken to follow from accepting a theory, or what theory acceptance amounts to. If SR and CE agree on what goes into theory acceptance, that is, on what the determining factors are, they disagree on what theory acceptance amounts to. As I have mentioned, van Fraassen describes the scientific realist as holding that the belief involved in theory acceptance is the belief that the theory is true. Also, I have argued that that belief is not an active determining factor for theory acceptance, rather, that the belief that the theory is true just is theory acceptance. It follows then that the belief that the theory is true is, as I have put it above, what theory acceptance amounts to for the scientific realist.

On the constructive empiricist account, the belief involved in theory acceptance is the belief that the theory is empirically adequate. At first blush, it might appear that a judgement of empirical adequacy is what determines theory acceptance rather than what, or part of what, theory acceptance amounts to. Empirical adequacy is one of the theoretical virtues and I have said that it is judgements concerning a theory's manifestation of those virtues which

determines theory acceptance. But van Fraassen is careful to point out that the belief that a theory is empirically adequate is something quite beyond a simple acknowledgement that a theory is, so far as we can tell, empirically adequate. Van Fraassen writes that when we believe a theory to be empirically adequate:

... we stick our necks out: empirical adequacy goes far beyond what we can know at any given time. (All the results of measurement are not in; they will never all be in; and in any case, we won't measure everything that can be measured.) (1980a p.69)

Thus we can see that although a judgement of how a theory fares with respect to empirical adequacy so far as we can tell may be a determining factor in theory acceptance, the belief that a theory is empirically adequate is something over and above that judgement. That belief is not itself prior to and separate from theory acceptance, but is rather, part of what theory acceptance amounts to.

One parallel between the scientific realist's belief that a theory is true and the constructive empiricist's belief that a theory is empirically adequate is that both beliefs are associated with having come to accept a theory. Each belief, that is, is for its respective theory at least part of what theory acceptance amounts to. At this point we can identify the locus of one major disagreement between

SR and CE. If we assume that there is no 'simple' empirical disagreement over how good the theory is, then we can take it that SR and CE start from the same place, that is, they begin with the same judgements as to how the theory fares with respect to the theoretical virtues. But they end up in different places when they come to accept a theory, the scientific realist believing that the theory is true, the constructive empiricist believing that it is empirically adequate. The disagreement then is over which belief the judgements concerning the theoretical virtues should give rise to.

I should admit at this point that I am accepting a simplifying hypothesis, not only through the assumption that scientific realists and constructive empiricists do not disagree on their judgements concerning the theoretical virtues, but also in the notion that they would agree on just when it is appropriate to accept a theory. If I were to try to avoid the simplifying hypothesis I would run into a problem of too many variables. If, for instance, there might be a dispute over just when it is appropriate to accept a theory, as well as a dispute over what theory acceptance amounts to, the real disagreement between SR and CE, if there were such a thing, would be much more difficult to locate with any degree of certainty. Under those circumstances the only issue might be one of

linguistic convention, or it might be that there is no disagreement at all -- that SR and CE simply talk at crossed purposes. It might be the case that the scientific realist is claiming that it is appropriate to accept* a theory under circumstances C* while the constructive empiricist is claiming that it is appropriate to accept** a theory under circumstances C**. But let me lay such worries to one side. It is van Fraassen who has chosen to contrast SR and CE over the belief involved in theory acceptance; CE is van Fraassen's invention; we are not being overly charitable in assuming that he has achieved a real contrast. My simplifying hypothesis, then, amounts to a charitable, but not overly charitable, interpretation.

My analysis leaves SR and CE having the same determining factors involved in theory acceptance, but theory acceptance amounting to something different for SR than for CE. This situation seems to be very much as van Fraassen would have it:

... we can distinguish between two epistemic attitudes we can take up toward a theory. We can assert it to be true (i.e. to have a model which is a faithful replica, in all detail, of our world), and call for belief; or we can simply assert its empirical adequacy, calling for acceptance as such. (1980a pp.68-69)

Here van Fraassen has SR and CE accepting the same theory but making different claims about it. Van Fraassen casts

the dispute over what theory acceptance amounts to in terms of what epistemic attitude one should take toward the theory. The question of what epistemic attitude is appropriate in theory acceptance is a slightly more general version of the question of what belief or how much belief is involved in theory acceptance. These questions of epistemic attitude and belief, then, are more specific variations of the question of what theory acceptance amounts to.

Leaving aside the question of what epistemic attitude is appropriate, we might wonder whether SR and CE have further disagreements over what theory acceptance amounts to. I have maintained so far that for SR theory acceptance just is the belief that the theory is true. But I have also mentioned that van Fraassen claims that there is more to theory acceptance than belief. Van Fraassen writes:

While the only belief involved in acceptance, as I see it, is the belief that the theory is empirically adequate, more than belief is involved. To accept a theory is to make a commitment, a commitment to the further confrontation of new phenomena within the framework of that theory, a commitment to a research programme, and a wager that all relevant phenomena can be accounted for without giving up that theory. That is why someone who has accepted a certain theory, will henceforth answer questions ex cathedra, or at least feel called upon to do so. (1980a p.88)

Is there not, then, a further disagreement over whether there is more to theory acceptance than some sort of belief?

The answer, I think, is that this further disagreement is merely apparent, not substantial. As I have mentioned above, one reason why the CE version of theory acceptance needs a component beyond merely the belief that the theory is empirically adequate is to be able to offer an account of the choice situation that might arise given the possibility of two or more apparently empirically adequate but conflicting theories. The unity of science principle in tandem with the minimal criterion of consistency ultimately call for a choice in the event of such conflict. An important aspect of theory acceptance then is its exclusivity, that is, while conflicting theories might be each empirically adequate only one such theory can be accepted. Hence van Fraassen comments that commitment to a particular theory goes beyond the belief that it is empirically adequate and yet is a component of theory acceptance. When one accepts a theory one makes a commitment as van Fraassen has described, and that commitment is to a particular theory as opposed to its competitors.(3)

SR, as van Fraassen presents it, does not have an explicitly stated exclusivity component in theory

acceptance. But the belief component in the SR version of theory acceptance, that is, the belief that the theory is true, already calls for the sort of exclusivity that must be explicitly stated in the CE version. To accept a theory as being true, in consideration of the consistency criterion, must at least amount to an implicit rejection of the competing theories with which it conflicts as being false.(4) The explicitly present exclusivity component in the CE version of theory acceptance is implicitly present in the SR version. Thus what might initially appear to be an extra component in the CE version does not contribute to the dispute over what theory acceptance amounts to. Rather, the seemingly extra component is in fact a point of agreement between the two versions.

Van Fraassen's writings are not suggestive of any further disagreement between SR and CE as far as theory acceptance is concerned. To summarize quickly, the dispute over theory acceptance is a dispute over what theory acceptance amounts to, not a dispute over what goes into, or determines, theory acceptance. More specifically, the dispute is over what epistemic attitude is appropriate to theory acceptance. According to SR the proper epistemic attitude is the belief that the theory is true; according to CE, the belief that the theory is empirically adequate.

Whether the scientific realist or the constructive

empiricist is better able to defend her position with respect to what theory acceptance can and should amount to is a question well worth pursuing. Indeed, an answer to that question might be decisive in the overall consideration of which of the two offered accounts of science is better. A general defense of CE with respect to what theory acceptance amounts to, however, is beyond the scope of my immediate concerns in this discussion. For now, I will be content to have raised the question as central to van Fraassen's contrasting of SR and CE, and to have given it a clear voice.

To understand CE it is as important to note where van Fraassen does not contrast CE with SR as it is to note where he does. Van Fraassen does not contrast the two with respect to their interpretations of what theories say, the conceptions of truth they might accept, or any underlying metaphysical doctrines they might embrace. But surely there is more to SR, for example, than a thesis about the aim of science and a thesis about the belief involved in theory acceptance. The aim of science might be said to be truth, but what theory of truth would be accepted?(7) Fortunately van Fraassen does go on to offer a more fully developed description of both SR and CE, but what can be made of the fact that his initial contrasting of them is so minimal? I suggest the following simple rule for

understanding van Fraassen's account of the two positions: Take it that SR and CE conflict where van Fraassen's statement of them indicates that they do, that they conflict wherever we can derive such conflict from their descriptions, but that they need not conflict anywhere else, unless of course van Fraassen indicates such additional conflict areas during his completion of their explication.(8)

Van Fraassen makes his descriptions of SR and CE more precise in his discussion of the significance of the phrase 'literally true'. Before considering that discussion, however, I want to comment on the appropriateness of van Fraassen's initial statements of SR and CE in light of the structure of his argument against SR. It has been argued against van Fraassen that he has no direct argument against SR. Specifically the complaint has been made that "an argument against an argument for realism is not an argument for anti-realism (or against realism for that matter)".(7) Part of van Fraassen's approach is to argue against arguments for SR. But within the structure of the overall case that van Fraassen is offering, an argument against an argument for SR does count as an argument against SR and as an argument for CE. Perhaps we can best fully understand the structure of van Fraassen's argument by considering an example. One of the arguments for SR which van Fraassen

considers is the 'inference to the best explanation'. He presents a simplified version:

Let us suppose that we have evidence E, and are considering several hypotheses, say H and H'. The rule then says that we should infer H rather than H' exactly if H is a better explanation of E than H' is. (1980a p.19)

Van Fraassen identifies the inference to the best explanation as an empirical hypothesis about how scientists reason. As an argument for SR it is maintained that the inference is descriptively true of how we do in fact reason and that if we follow the rule consistently we are lead to SR. Van Fraassen offers an alternative hypothesis of his own:

... we are always willing to believe that the theory which best explains the evidence, is empirically adequate (that all the observable phenomena are as the theory says they are). (1980a p.20)

Van Fraassen argues against the argument for SR by denying that the evidence which might be brought forward in support of the inference is telling against his alternative constructive empiricist hypothesis.(8) Van Fraassen presents us with a situation in which we have competing hypotheses. Thus he undermines the cogency of the inference to the best explanation as an argument for SR by suggesting an alternative to the inference, an alternative which does not lead to SR. But he does not stop there; he

goes on to argue that the inference to the best explanation is flawed and comparatively inferior to the alternative hypothesis which he has offered. Van Fraassen's direct argument against the inference to the best explanation is that, as a theoretical virtue, being the best explanation is characteristically pragmatic and not epistemically relevant.(9) Van Fraassen argues that our judgement that a given theory can serve as an explanation is independent of our judgement of whether or not the theory is true. We can recognize that a theory might explain a situation even when we are in no position to tell if the theory is true. Thus, van Fraassen argues, the inference from our willingness to accept a theory as an explanation, even as the best available explanation, to the belief that the theory is true, is unwarranted. It might be the case that a theory which we accept as an explanation is later discovered to be false.

Van Fraassen is advancing a sceptical argument against the scientific realist's inference.(10) In the context of the two competing hypotheses, and the two competing accounts of science which give rise to the hypotheses, any such argument against the inference to the best explanation will count against SR in so far as SR depends upon that inference for support, and will count for CE by default. When choosing between only two competing theories, an

argument against one can be as good as an argument for the other. Such is the structure of van Fraassen's argument against SR.

Van Fraassen's arguments against SR for the most part reflect epistemic concerns relating to the epistemic attitudes involved in theory acceptance. Van Fraassen's CE alternative, seeking and claiming only empirical adequacy, risks less in terms of rational belief and is thereby epistemologically safer than SR. Van Fraassen's contentious claim is that it is significantly epistemologically safer. Van Fraassen writes:

Recall that I defined scientific realism in terms of the aim of science, and epistemic attitudes. The question is what aim scientific activity has, and how much shall we believe when we accept a scientific theory. What is the proper form of acceptance: belief that the theory, as a whole, is true; or something else?(1980a p.18)

Given what van Fraassen identifies as the question to decide between SR and CE, and given the structure of his argument against SR, his initial statements of the two positions in terms of the belief involved in theory acceptance seems quite appropriate. But what of the aim of science thesis? SR offers that true theories are to be seen as the aim of science; van Fraassen's CE offers that empirically adequate theories are the proper aim of science. Which account is better?

To understand van Fraassen on the aim of science thesis it is worth keeping in mind that when he presents his alternative to SR he claims that there is no evidence which will count for SR and against CE (see note 8). But concerning the aim of science thesis, van Fraassen offers no separate and direct argument as to why the SR version is unacceptable. He never considers that an account of science might combine the notion that the proper epistemic attitude to take toward good theories is to believe that they are empirically adequate, with the notion that science, nonetheless, aims at true theories. Neither does any convincing argument to the effect that science aims at empirical adequacy fall out of the arguments directed at the SR version of theory acceptance. Even if the sceptical argument that claims of truth for theories are not called for (as against claims of empirical adequacy) is convincing, even if the point that we are unduly risking error by believing theories to be true is compelling, what possible risk can be involved in aiming? Even if we can not know that our theories are true, it might still be expedient and need not be incoherent to take it as our aim that they should be.

There are, at this point, two significant alternative responses to questions concerning the role and importance of the aim of science thesis in van Fraassen's constructive

empiricist case. The first response would be to try to maintain that the aim of science thesis is roughly as important as the theory acceptance thesis for CE as a whole. The second response is to try to downplay the importance of the aim of science thesis and to maintain that van Fraassen's version, while appropriate to CE, might not be necessary for CE. I reject the first response as being comparatively implausible. The first response would have it that van Fraassen offers the aim of science thesis as equally important as the theory acceptance thesis but that he somehow overlooks the need to provide the sort of argument for the former which he provides for the latter. But not only does van Fraassen never argue directly for his version of the aim of science thesis, he never explicates the notion of an aim of science thesis. Van Fraassen never outlines, for instance, just what aiming at empirical adequacy, as opposed to aiming at truth, amounts to -- except in terms of the accompanying theory acceptance thesis. To view the aim of science thesis as importantly distinct from the theory acceptance thesis and, at the same time, as crucial to CE in general, would be to imply that van Fraassen's CE alternative is only half explicated and only half supported by argument. The reader would be left to speculate whether van Fraassen simply forgot to include half of his case for CE or whether some other explanation

might account for the rather unlikely hole in his account.

Fortunately we do not have to accept the first response; I endorse the second. The second response takes it that the aim of science thesis is much less important to van Fraassen's case than is the theory acceptance thesis. The purpose of the aim of science thesis is simply to indicate what counts as success for science. Thus to say that the aim of science is empirically adequate theories is just to say that science is to be judged successful if, and in so far as, its theories are to be judged empirically adequate. The aim of science thesis is of less importance because the case for CE does not turn on it. Van Fraassen could maintain his sceptical worries about SR and allow that the aim of science might be truth without opening himself up to charges of incoherence. The notion of aiming at truth is not itself an epistemic notion and thus need not conflict with the sceptical worries directed toward epistemic concerns. There are, however, reasons of appropriateness which explain and warrant van Fraassen's including his aim of science thesis in his statement of CE. It would be odd, at least, to set the success criterion for science at truth while at the same time questioning our justification for accepting theories as true. Van Fraassen sets the success criterion for science at empirical adequacy because he feels that the most we need to claim

for our theories is that they are empirically adequate. His setting empirically adequate theories as the aim of science brings accepting a theory into line with the claim that the theory is successful. Though his case might not otherwise be completely incoherent, van Fraassen's aim of science thesis makes his case as a whole more coherent and is thus more appropriate to his case than is the alternative SR aim of science thesis.

We can extend our judgement then concerning the appropriateness of van Fraassen's initial statements of SR and CE. Given what van Fraassen identifies as the questions to decide between the two positions and given the type of argument he offers against SR the theory acceptance thesis is crucial. The aim of science thesis, though perhaps not crucial to van Fraassen's case, is certainly an appropriate counterpart to the theory acceptance thesis. It might seem odd that van Fraassen would include a non-crucial aspect of CE in his initial statement of that position but not nearly so odd, I would hold, as the gaping hole in his presentation suggested by the first response. And although van Fraassen's aim of science thesis may not be crucial to CE, it is not inert either; it does help to fill out the picture of what CE amounts to by suggesting that CE recognizes empirical adequacy, and not truth, as success in science.

Having argued for the comparatively lesser importance of the aim of science thesis, I will leave the question of whether SR or CE gives a better account of that thesis completely unaddressed. My main purpose in taking up the discussion of the aim of science thesis, aside from simply filling out my exegesis of van Fraassen's CE, has been to present that thesis in such a way as to have it fit coherently with my overall assessment of what van Fraassen's case amounts to. When I do begin to defend van Fraassen against particular charges further on in this discussion it will be important that my reading of him is complete and coherent. If I seem to shy away from what van Fraassen would consider to be the interesting questions to decide between SR and CE, it is because my eventual purpose is to offer a defense of van Fraassen aimed at certain arguments which themselves are not directed toward those 'interesting questions'.

NOTES

(1) I do not want to claim that this list is complete. Generally speaking, anything which might be said in favour of a theory will point to a theoretical virtue. Of the ones that I have mentioned explicitly: I have begun to explicate empirical adequacy above; empirical strength refers to a comparative notion of just how much a theory does by way of "saving the phenomena" and may be understood in some cases in terms of the number of phenomena or kinds of phenomena saved; internal consistency ensures us that the theory does not disagree with itself; external consistency involves the compatibility of the theory in question with other theories one might want to hold; the explanatory power of a theory generally refers to its ability to provide acceptable explanations of the phenomena within its domain; and simplicity refers to a comparative notion regarding the extent to which a theory is ad hoc -- a simpler theory is one which is more comprehensively testable and thus can be shown to have fewer ad hoc elements. I offer these comments only as brief introductions to the theoretical virtues. I will offer explication of individual virtues as it is called for later in my discussion.

(2) Van Fraassen's claiming that there is more to theory acceptance than belief can be found on 1980a p.88. Van Fraassen's reference to consistency as a "rock-bottom" criterion of minimal acceptability is on 1980a p.94.

(3) Richard Creath (1985) acknowledges in a footnote (p.344) that van Fraassen seems to have two positions on theory acceptance: first, the equation of acceptance with a belief in empirical adequacy, and second, the position that there is more to acceptance than belief. Creath chooses to interpret van Fraassen as primarily intending the former (p.333). He then infers that "we can simultaneously accept mutually inconsistent theories". As a result he finds that truth is a matter of "utter indifference" for van Fraassen, that acceptance "involves no commitment to a theory's truth" and "no ontological commitment either". He continues: "It is thus perfectly correct to describe van Fraassen's position as an anti-realist one"(p.333). I have so far argued that it is a mistake to interpret van Fraassen as Creath has with respect to what theory acceptance amounts to. My interpretation of van Fraassen disallows the possibility of accepting mutually inconsistent theories and thus cuts off this particular line of inference to the notion that truth

is a matter of "utter indifference" for van Fraassen. Creath largely ignores van Fraassen's comments on the unity of science principle and the consistency criterion, comments which I take to be extremely important.

(4) As my discussion of "literal truth" in section III will bear out, the notion of truth active for the scientific realist here is not radically relativistic in character.

(5) The semantic conception of truth as outlined by Tarski springs to mind as one possibility; another might be the "pragmatic theory of truth" which Brian Ellis (1985) argues the scientific realist ought to accept.

(6) This rule amounts to something like "let's agree not to put words in the writer's mouth" -- surely a good policy when evaluating any writer. An explicit statement of such a rule might be thought to be unnecessary but I feel it is warranted given the writings of those who have treated van Fraassen as the certain sort of anti-realist which he is definitely not. I will present arguments from these misguided critics as well as my reasons for thinking them misguided later in my discussion.

(7) Richard Creath (1985) pp.46-47.

(8) "It will be countered that it is less interesting to know whether people do follow a rule of inference than whether they ought to follow it. Granted; but the premiss that we all follow the rule of inference to the best explanation ... is shown wanting. It is not warranted by the evidence, because that evidence is not telling for the premiss as against the alternative hypothesis I proposed, which is a relevant one in this context."(van Fraassen 1980a p.21)

(9) Van Fraassen divides the theoretical virtues (empirical adequacy, simplicity, explanatory power, etc.) into those which are and are not of epistemic importance. His criterion of epistemic relevance is that such virtues are to some extent truth-related. Van Fraassen labels the non-truth-related virtues "pragmatic". He offers no account of just what it is for a virtue to be a pragmatic virtue but rather uses the notion as a 'catch-all' for aspects of theories which, though of some importance for theory acceptance and thus for science, he holds to be epistemically irrelevant. For more on the epistemic status of the pragmatic virtues see note (17).

(10) Some of the details of van Fraassen's sceptical argument are as follows. He endorses as epistemically relevant a distinction between the observable and the unobservable. His claim is that whether we are dealing with the observable component of reality or the unobservable component is relevant to our epistemic attitude. Van Fraassen claims that we are never warranted in claiming knowledge of the unobservable. It is worth mentioning that van Fraassen's notion of what is "unobservable" is a technical one. He would class quarks, electrons, and the force of gravity, for example, as unobservables. But dinosaurs, tomorrow's newspaper headlines, and undiscovered stars on the other side of the universe would all count as observables. Van Fraassen directs this scepticism toward the inference to the best explanation which the scientific realist would claim legitimately takes us beyond the observable/unobservable distinction. Gary Gutting (1983) acknowledges that the case for CE places a great weight on the epistemic relevance of the observable/unobservable distinction and that a substantial excursion into the theory of knowledge would be necessary to justify that relevance. Paul Churchland (1985), among others, has argued that the observable/unobservable distinction is not at all relevant to epistemic matters. If Gutting is correct, and I believe that he is, then if arguments such as Churchland's are effective they would be fatal to CE. A detailed defense of van Fraassen's endorsement of the observable/unobservable distinction and its epistemic relevance would be an important addition to his case as a whole. For my purpose in this discussion, however, I wish only to provide a sense of the structure of van Fraassen's case against SR. Therefore I need not attempt any such defense of that case.

CHAPTER TWO

II. A Hierarchy of Realisms

At this point I want to introduce a rough outline of some realist and anti-realist positions, or views, to be used as a backdrop for my analysis of CE. I call this outline a hierarchy of realisms because it seems to me that it does admit of different levels. Each successive level presumes a realist position on a more basic level. I am not offering this sketch as the correct approach, or even as the best approach, for a general understanding of realism and accordingly I will not argue to that effect. Rather, I contend only that it will be useful to accept my outline for the purpose of understanding SR and CE and evaluating their abilities to offer acceptable methodological accounts.

By a methodological account I mean an account of science which is suitably informed by the institution of science as it does or should exist, an account which pays attention to what scientists actually do or should do. Any acceptable methodological account must plausibly square scientific progress with the practises or proper practises of scientists. The term 'scientific method' is often used to refer to the proper practises of scientists -- whatever

those practises may be. I fear that I may be suggesting more questions than I can reasonably hope to answer in this thesis. What is the "institution of science"? What exactly is the "scientific method"? Is there such a thing as the "scientific method"? I will not attempt to answer these questions in any detail. For my purposes the institution of science is that group of people who are the practitioners of the natural sciences, their doings as practitioners, and their means of recording their results and communicating them to each other. The institution of science can perhaps be identified by the scientific method, as before, whatever that may be. The term 'scientific method' handily refers to a complex mass of procedures and practises which may never be completely sorted out and specifically identified. I certainly do not plan to give an account of the scientific method here. But neither do I mean to suggest that the notion of the scientific method is itself unproblematic. Although I will not question the putative methodological base of the complaints which I will consider in Chapter Three, someone else in some other thesis might quite reasonably do just that. Instead, I will accept those complaints as they are offered and present what I take to be the appropriate constructive empiricist replies. My replies depend on the putative methodological base to no greater extent than do the

complaints which spark them.

I construct my hierarchy of realisms very much in accordance with the four tactical maxims Michael Devitt has offered as being useful for understanding realism. Devitt's maxims, then, also offer a useful breakdown for understanding the structure of the hierarchy I am offering. These four maxims are:

1. "In considering realism distinguish the constitutive and evidential issues."
2. "Distinguish the metaphysical (ontological) issue of realism from any semantic issue."
3. "Settle the realism issue before any epistemic or semantic issue."
4. "In considering the semantic issue, don't take truth for granted." (1984 pp.3-4)

Devitt's first maxim suggests the separation of the questions: what is realism (or any particular sort of realism), and what might count as evidence for a given realist position. For my present purposes I can lay the evidential issue to one side. I am offering the hierarchy as a structure relating a range of possible positions; I am not arguing that any one or some of them ought to be embraced. My initial concern is the constitutive issue; I am presenting definitions of the positions in the hierarchy.

Devitt's second and third maxims counsel one to distinguish and settle the "metaphysical issue" of realism

before settling epistemic or semantic issues. Accordingly let me introduce what I will call "metaphysical realism". Metaphysical realism is simply a speculation about the nature of reality. It is captured in the thesis that there is an independent reality, a reality, that is, independent of the perceptions of any perceiver. This thesis, for my purposes, captures the weakest and therefore the most basic form of realism. Metaphysical realism forms the ground level of the hierarchy. All realist positions on successive levels presume metaphysical realism.(1)

The basic metaphysical realist position being settled I can now move on, in accordance with maxims two and three, to introduce the second level of the hierarchy -- semantic realism. Semantic realism, as the second level realism, presumes metaphysical realism, the first level realism. But what I am calling 'semantic realism' is to be distinguished from metaphysical realism. Semantic realism contains two theses: first, the thesis that we can successfully refer to, and theorize about, the entities which are constitutive of the independent reality, and second, that truth is a correspondence relation which can hold between our theories and the independent reality. A theory (or statement) is true just in case it accurately corresponds to the reality about which it is a theory (or statement).

Devitt's fourth maxim counsels not to "take truth for granted". It would be taking truth for granted not to specify what notion of truth is to be accepted. It might be argued that it is also taking truth for granted not to argue for the recommended notion of truth or not to explicate the notion in great detail. I will not argue here that a correspondence notion of truth ought to be accepted by anyone, just as I will not argue that anyone ought to be a semantic realist. As I have mentioned, I am simply defining the positions in the hierarchy. I am defining it to be the case that what I call 'semantic realism' incorporates a correspondence notion of truth. And neither will I explicate truth in great detail.(2) My later employment of the hierarchy as a backdrop for SR and CE will not be enhanced by going into greater detail concerning the notion of truth accepted by the positions in the hierarchy than van Fraassen provides concerning the notion of truth accepted by SR and CE. Van Fraassen's explication of truth is abrupt. He views truth as a relation that may hold between a theory and the world. He does not regard truth as relative to any perception or perceiver. Truth, for van Fraassen, amounts to accurate correspondence. Further elucidation of what truth amounts to is not to be found in van Fraassen's text, and so to avoid misrepresenting him it will not be found in mine.

For my purposes, not taking truth for granted amounts to explicitly including a statement of the notion of truth to be accepted as one of the theses of semantic realism but does not amount to providing much explication of that notion.

It should be clear that there is a presumption of metaphysical realism by semantic realism. If there was no independent reality to begin with, then we could hardly refer to its entities, and a correspondence notion of truth would be empty. There is no presumption in the other direction however; one could certainly be a metaphysical realist without being a semantic realist. We may call such a position 'anti-semantic-realism' but we must be clear that it is a form of metaphysical realism.

On the third level of the hierarchy fit both SR and CE as van Fraassen conceives of them. Just as semantic realism and anti-semantic-realism both presume metaphysical realism, SR and CE both presume semantic realism. I have not yet provided sufficient exegesis of either SR or CE to make clear their presumption of semantic realism. I shall attend to that immediately, so as not to get too far ahead of myself.

III. SR and CE in the Hierarchy

As I have mentioned, van Fraassen's initial statements of SR and CE are not complete descriptions of the two positions. Let us consider, then, just how van Fraassen fills out those descriptions. One way he fills them out is through his discussion of the phrase 'literally true'. So when van Fraassen says that SR is committed to the view that science aims to give us a "literally true story of what the world is like", what does he mean by the phrase 'literally true'? Van Fraassen presents the notion of a 'literally true' account as having two aspects: "... the language is to be literally construed; and so construed, the account is true." (1980a p.10) If the aim of science is literally true theories, the theories must, first of all, be literally construed.

So to begin, what is a literal construal? Van Fraassen does not pretend to take on the task of fully explicating the notion. Instead he delegates the task of such an explication to the philosophy of language. But neither does he leave the term completely unexplicated. He gives us some hints -- enough, I think, to satisfactorily place SR and CE in my hierarchy. Van Fraassen writes that the idea of a literal construal, "comes perhaps from theology, where fundamentalists construe the bible

literally, and liberals have a variety of allegorical, metaphorical, and analogical interpretations, which 'demythologize'."(1980a p.10) We can consider, as an example, the proverb: "Don't cry over spilt milk." Obviously it is not meant to be construed literally. Such a literal construal would limit the appropriateness of the imperative to situations involving the actual spilling of milk. According to van Fraassen, theories, on the matter of construal, are not to be treated like proverbs. Van Fraassen writes:

To insist on a literal construal of the language of science is to rule out the construal of a theory as a metaphor or simile, or as intelligible only after it is 'demythologized' or subjected to some other sort of 'translation' that does not preserve logical form. If the theory's statements include 'There are electrons', then the theory says that there are electrons. If in addition they include 'Electrons are not planets', then the theory says, in part, that there are entities other than planets.(1980a p.11)

Perhaps the best way to understand the notion of a literal construal is in terms of reference. To construe a theory literally is to take the theory as making a genuine attempt to refer to real entities. The theory is supposed to be about the world in such a way that the entities which it names are the constituents of the independent reality. The relations which the theory describes are supposed to be real relations between those entities. It may be too

strong to suggest that the notion of a literal construal demands or entails an explication in terms of an attempt at genuine reference, but such a construal seems consonant with, even suggestive of, such an understanding. Any non-literal construal may exclude the possibility of genuine reference across the board and must exclude such reference at some point -- if not the reference to a particular entity, then to a relation between entities. If a theory were taken to be genuinely referring to all the entities and relations that its statements mentioned, then it would be being construed literally. To take a theory as not genuinely referring is to 'demythologize' the theory in the way that van Fraassen rules out. It seems reasonable then, given the hints van Fraassen supplies concerning the character of a literal construal, to understand such a construal as involving genuine reference. Even if the idea of a literal construal is not completely reducible to the idea of an attempt at genuine reference, even if the notion could be fully explicated without reference to 'reference', it is not incorrect to say that in so far as reference is discussed, a literal construal of a theory will involve viewing the theory as making an attempt at genuine reference.

Further evidence that it is plausible to see a literal construal as involving genuine reference is that such an

understanding rules out just the possibilities that van Fraassen wants to see ruled out. Van Fraassen writes:

I have added 'literally' to rule out as realist such positions as imply that science is true if 'properly understood' but literally false or meaningless. For that would be consistent with conventionalism, logical positivism, and instrumentalism. (1980a p.9)

If interpreting a theory literally involves viewing it as making an attempt at genuine reference, the theory will be true if what it says about its referents is true. Thus, to see literal truth as involving successful reference provides a way of offering an exclusive alternative to accounts of science which depend on accepting that theories can be claimed true only if 'properly', not literally, understood.

The second aspect of the notion of a literally true theory is that, once literally construed, the theory must be true. I have so far depicted van Fraassen as accepting a correspondence theory of truth and noted his use of the notion of truth both in his introduction of SR and in his introduction of 'empirical adequacy'. Van Fraassen never gives us reason to think that he is equivocating on the notion of truth. He never indicates, explicitly or implicitly, that he might be using one notion of truth in one part of his account and a different one elsewhere. In

particular, he never contrasts SR and CE over the notion of truth accepted by each account.

When van Fraassen comments on Michael Dummett's understanding of "realist disputes of various sorts" he supplies further support for my contention that it is a correspondence notion of truth which is operative for SR and CE, as well as for my suggestion that an acceptable way of understanding 'literally true' is in terms of reference. Van Fraassen writes:

Certainly I wish to define scientific realism so that it need not imply that all statements in the theoretical language are true or false (only that they are capable of being true or false, that is, there are conditions for each under which it has a truth-value); to imply nevertheless that the aim is that the theories should be true. And the contrary position of constructive empiricism is not anti-realist in Dummett's sense, since it also assumes scientific statements to have truth-conditions entirely independent of human activity or knowledge. But then, I do not conceive the dispute as being about language at all. (1980a p.38)

This short passage from van Fraassen is especially rich and revealing. To begin with, he does not want to imply on behalf of SR that all statements in the theoretical language are true or false. This accords well with both a correspondence notion of truth and a reference understanding of a literal construal. Consider, for example, a statement in the theoretical language

attributing some property to some sub-atomic particle. If in fact the particle did not exist, then it could be maintained that there is no genuine reference to it by the theory. Reference was supposed by the theory but the attempt at reference failed. At the same time, statements concerning the particle could not be true by correspondence because the conditions which are necessary for such statements to be correspondently true, the conditions of the particle's actual existence in the independent reality, are not met. It might be taken that any statement asserting the existence of such a non-existent particle is false by correspondence (or lack thereof). But can a statement be made about such a particle without asserting its existence? If so, would such a statement be true, false, or something else? Supposing the non-existence of quarks, is the statement "quarks are green" false? Van Fraassen wants to leave SR uncommitted so far as these questions are concerned. Accordingly, the reference-correspondence understanding of 'literally true' also leaves these questions open. The statement "quarks are green" may not be true by correspondence to reality but in just the same way it may not be false. If the term 'quark' fails to refer the literal construal is also misguided and the answer to whether the statement is correspondently true or false might be "neither".

Van Fraassen does want to commit SR to the view that all statements of the theory are capable of being true or false. He explicates 'capable of being true or false' in terms of truth-conditions. And he claims that the truth-conditions are "independent of human activity or knowledge". With truth-conditions independent as described, van Fraassen seems to rule out any notion of truth other than correspondence. A statement is capable of being true or false in that it is supposed to be about the independent reality. In terms of my semantic realism a statement is capable of being true or false if it counts as an attempt at genuine reference. As such an attempt it will be true just in case the conditions for its truth are the conditions existing in the independent reality. Any true statement is true, then, in virtue of its correspondence with the existent conditions in the independent reality. What makes any scientific statement 'capable of being true or false', given that it is supposed to be about reality, is that the existent conditions of the independent reality just might be the truth or falsity conditions for that statement.

Van Fraassen's notion of SR is beginning to take shape. Van Fraassen tells us that according to SR theories should be literally construed and that the aim of science is that such theories be true. I have argued that van

Fraassen is using a correspondence notion of truth and that one way of understanding 'literally construed' is in terms of an attempt at genuine reference. Given the understanding of 'literally construed' which I am suggesting, successful reference is a requirement of a literally construed, correspondently true theory. Recalling that my notion of semantic realism is marked by the two theses that we can successfully refer to the entities which are constitutive of the independent reality and that we should accept a correspondence notion of truth, it should now be abundantly clear how SR is to fit on the third level of my hierarchy. For SR accepts a correspondence notion of truth and, as I have argued, it is plausible to understand 'literally construed' in terms of an attempt at genuine reference. It is plausible to say that, according to SR, a literally true theory is a theory which successfully refers to the entities of the independent reality and whose statements correspond to that reality. What I am suggesting is that it is plausible to view SR as presuming the semantic realism which I have defined. I do not think that this suggestion should be surprising. Scientific realism is most often connected with a correspondence notion of truth. And the notion of scientific statements corresponding to reality without referring to the constituents of reality strikes me as more

mysterious than its alternative. Of course SR as van Fraassen conceives of it is more than just semantic realism; SR also includes the theses about the aim of science and the belief involved in theory acceptance, the theses with which van Fraassen characterizes the position. With its presumption of semantic realism, however, it should now be clear just how SR properly takes its place on the third level of my hierarchy of realisms.

Compare the constructive empiricist. We know that CE differs from SR on the matters of epistemic attitude and the aim of science. But, continuing the focus on the same passage, we see that van Fraassen gives a concrete indication that SR and CE agree on the notion of truth. For it is CE which is mentioned as also assuming that "scientific statements have truth-conditions entirely independent of human activity or knowledge".(3) And, as I have mentioned before, van Fraassen never tries to contrast SR and CE over the notion of truth. So it would seem that CE too accepts the correspondence truth thesis of semantic realism.

As for the other thesis of semantic realism, the reference thesis, it too can be seen to be in van Fraassen's presentation of CE. Van Fraassen discusses the notion of a literal construal in relation to CE:

The idea of a literally true account has two aspects: the language is to be literally construed; and so construed, the account is true. This divides the anti-realists into two sorts. The first sort holds that science is or aims to be true, properly (but not literally) construed. The second holds that the language of science should be literally construed, but its theories need not be true to be good. The anti-realism I shall advocate belongs to the second sort. (van Fraassen 1980 p.10)

So van Fraassen's constructive empiricist is committed to a literal construal of theories. For the constructive empiricist, just as for the scientific realist, the commitment to a literal construal of theories is suggestive of the acceptance of the reference thesis of semantic realism. So CE, just like SR, can be seen to be in full accord with semantic realism as I have presented it. (4) But CE is more than just semantic realism; it is semantic realism plus the appropriate theses concerning the aim of science and the belief involved in theory acceptance. Thus the constructive empiricist denies that truth need be the aim of science, but has no problem with the idea that some theory, literally construed, might be true.

At the same time, there is no disagreement between the scientific realist and the constructive empiricist concerning just what it is for a theory to be literally construed or what it is for a theory so construed to be true. Although CE disagrees with the scientific realist claim that the aim of science is literally true theories,

the constructive empiricist agrees that theories are to be literally construed and agrees that, so construed, theories will be true if they accurately correspond to reality. Given the reference-correspondence understanding of 'literally true', CE can thus be seen to presume semantic realism and thus to take its position opposing SR on the third level of my hierarchy of realisms.

Gary Gutting has written a dialogue in which his constructive empiricist character aptly claims:

I'm not saying theoretical entities don't exist or that talk about them is meaningless. I don't even say there's anything wrong with believing in them if you want to. My point is simply that there's no evidence that makes it irrational to withhold judgement about their existence. I'm defending my right to be an agnostic on the issue.(1985 p.119)

For the constructive empiricist and scientific realist alike, a theory's being true means more than its being empirically adequate. A true theory must save more than just the phenomena; it must save reality. All of what the theory says about reality, about both the observable and unobservable components of reality, must be true in order for the theory to be true. And of course it is the judgement that scientific theories are true which the constructive empiricist always finds questionable.

In the hierarchy of realisms I have offered metaphysical realism is basic. Semantic realism presumes

metaphysical realism. I have argued that SR, as van Fraassen presents it, can be seen to presume semantic realism. And I have argued that CE, again as van Fraassen presents it, can be seen to presume semantic realism. Thus SR and CE stand as alternatives on the third level of my hierarchy of realisms.

Pursuant to my discussion of van Fraassen's treatment of the notion of 'literally true' theories, I am now in a position to offer a more complete description of both SR and CE. Having argued that the two positions can be seen to presume semantic realism, I can construct a more complete characterization of SR and CE by adding the theses of semantic realism to the theses concerning the aim of science and the belief involved in theory acceptance.(5) Of course, in attempting to understand the SR and CE hypotheses more fully I have not uncovered any unexpected points of contention between the two. Instead I have explored areas in which SR and CE agree. In those areas of agreement SR and CE do have definite positions, and it may be important to recognize just what those positions are in order not to misjudge the potential abilities of SR and CE to offer acceptable methodological accounts of science.

Above I quoted van Fraassen as distinguishing two sorts of anti-realism. It will be useful at this point to give the two anti-realisms distinct names. Actually, I

have already named the first sort of anti-realism above. The first sort of anti-realist holds that good theories are true if 'properly construed'. The locus of disagreement between this first sort of anti-realist and the scientific realist is semantic realism. To reflect the fact that it is the actual semantic realism which is in dispute, I have called the first sort of anti-realism anti-semantic-realism (ASeR). The second sort of anti-realist disagrees specifically with the theses concerning the aim of science and the belief involved in theory acceptance which van Fraassen used to characterize SR. To reflect the fact that the second sort of anti-realism is generally in conflict with what is characteristic of SR over and above its semantic realism, I shall call the second sort of anti-realism anti-scientific-realism (AScR).(6) The constructive empiricist is an anti-scientific-realist, but not an anti-semantic-realist.

Given that metaphysical and semantic realism are not areas of dispute between van Fraassen and the scientific realist, one might wonder why I am being so careful to include those aspects in my description of the two positions. The answer is that a failure to take adequate account of van Fraassen's realism can lead to a blurring of the distinction between AScR and ASeR. Many arguments the scientific realist might advance against the latter are out

of place if advanced against the former. In those arguments it is specifically the semantic realism underlying SR which gives the scientific realist the ability to come up with his account. In this way the scientific realist may gain an advantage over the anti-semantic-realist. But because of the semantic realism which underlies ASr, the scientific realist does not gain a similar advantage over the anti-scientific-realist, and in particular, does not gain an advantage over the constructive empiricist.

IV. The Divorce of Science from Truth

At least part of what makes van Fraassen's CE seem such a radical departure from the SR to which it is opposed is the appearance that van Fraassen would have science divorced from truth. Van Fraassen sets out very firmly against the idea that truth should be seen as the aim of science. He argues that we are not irrational to suspend judgement on the truth of even our best theories. He wants to portray science in such a way that success in scientific endeavor is something other than truth. Van Fraassen has been interpreted as viewing the truth of theories as a matter of "utter indifference".(7) On closer inspection, however, van Fraassen's divorcing of science from truth is neither so radical nor so thoroughgoing as it might first

appear.

Van Fraassen has offered the dual theses that the proper aim of science is empirically adequate theories and that the belief involved in theory acceptance is that the theory is empirically adequate. In so far as van Fraassen wants to replace the notion of truth operative in the scientific realist account of science with the notion of empirical adequacy, it is true that he wants to steer us away from an understanding of science which is explicitly cast in terms of truth. But there is a great deal of difference between counselling that science does not aim at truth, or that we need not believe in the truth of the theories which we accept, and counselling, rather, that science has no proper connection with truth whatsoever. While van Fraassen's entire project is concerned with presenting and defending the former advice, it is a mistake to see him as a proponent of the latter.

There are three important lines of argument to support the idea that van Fraassen does not completely sever science from a concern for the truth. The first line involves the relation between truth and empirical adequacy. The second line concerns the constructive empiricist's means for discarding knowably false theories. The third line incorporates a lack of evidence, in van Fraassen's account, for the thesis that truth is no concern of

science, and a lack of need, on the part of van Fraassen's account, for that thesis.

In considering whether van Fraassen is really directing the aim of science away from truth in any way which is detrimental to an understanding of science, it is worth taking a close look at empirical adequacy. There is a very definite connection between empirical adequacy and truth. When van Fraassen introduces the notion of empirical adequacy he explicates it in terms of truth. A theory is empirically adequate if what it says about what is observable is true.

Arguably, it must be the case that empirical adequacy is explicable in terms of truth. The constructive empiricist agrees that theories should be literally construed. Given a reference understanding of literal construal, the constructive empiricist must see science as attempting to give an account of reality. Both truth and empirical adequacy are relationships between accounts of reality and reality itself. Both truth and empirical adequacy are accuracy relationships. To be true a theory must be perfectly accurate in all that it says about reality, that is, in all of the ways it corresponds to reality. To be empirically adequate a theory must be perfectly accurate too, but only in its corresponding to observable reality. To be true is more than merely to be

empirically adequate. But it is more of the same kind of thing -- the accuracy relation itself is not different in each case; what is different is that all of a theory's purported correspondences to reality must be accurate in order for the theory to be true. But if the relation named by 'empirical adequacy' is not of a different sort than the relation named by 'truth', then it must be possible to understand empirical adequacy in terms of partial truth. So empirical adequacy must be explicable in terms of truth.

If the aim of science were true theories the fact that a theory seemed false could be grounds for rejecting it. If we construe the aim of science to be empirically adequate theories, will we be relinquishing the grounds for rejecting theories which seem false? It might be troubling if it were the case that a theory's seeming false did not either provide or stem from grounds for our ultimately rejecting it. But quite apart from van Fraassen's own two-thesis description of CE, he does secure the means for discarding knowably false theories. Those means are connected to the semantic realism presumed by CE.

When might we judge a theory to be false? Two situations come to mind: first, we judge a theory to be false if what it says about what is observable is false; second, we judge a theory to be false if it seems that its internal structure is unacceptable, if, for example, it is

inconsistent. The first situation is not a problem for the constructive empiricist. Such a theory would not be judged to be empirically adequate. The second situation is not really a problem either. While the sort of theory described in the second situation might be empirically adequate, the constructive empiricist can reject it on other grounds. He can reject such a theory because it is metaphysically unacceptable but in order to do so he must draw upon his underlying realism. This last point calls for some explanation.

In his discussion of the pragmatic virtues van Fraassen writes:

In so far as they go beyond consistency, empirical adequacy, and empirical strength, they do not concern the relation between the theory and the world, but rather the use and usefulness of the theory; they provide reasons to prefer the theory independently of questions of truth. (1980a p.88)

Here van Fraassen groups consistency together with empirical adequacy and empirical strength as truth-related theoretical virtues. In Chapter One (page 8) I quoted van Fraassen as referring to consistency, both internal and with the facts, as a "rock-bottom" criterion of minimal acceptability. Van Fraassen views consistency, like empirical adequacy, as relevant to the relation between the theory and the world. He also sees questions of truth and

consistency as not completely independent. Given his correspondence notion of truth he probably considers consistency a necessary condition for truth. The connection between truth and consistency is relevant for the present discussion because it is indicative of one way that science, in so far as it is concerned with consistency, maintains at least an indirect concern with truth on van Fraassen's CE account.

Perhaps van Fraassen cannot at once maintain that true theories are not the aim of science and reject inconsistent theories on the grounds that they are not true. Consistency, however, can be seen to be a necessary condition not only for truth, but also for a sensible literal construal. On van Fraassen's account a theory's statements are supposed to be 'about the world'. But an internally inconsistent theory will give rise to a contradiction and a contradiction cannot properly be said to be 'about the world'. Consider again van Fraassen's acceptance of a correspondence notion of truth. Van Fraassen claims that CE assumes scientific statements to have truth-conditions entirely independent of human activity or knowledge. The truth-conditions for scientific statements are supposed to be in the world. But a contradiction is not made false by the existent conditions in the world; it is false regardless of those conditions.

A contradiction cannot refer to any existent state of affairs.(8)

Another way of putting the objection to inconsistency which I am offering on van Fraassen's behalf is to say that an inconsistent theory is metaphysically unacceptable. To accept an inconsistent theory as possibly corresponding to reality is to accept that reality itself might be, in the same way, inconsistent. And for a realist the suggestion that reality is inconsistent is incoherent. The argument in this form also betrays a realist point of view. But van Fraassen has claimed that there is no evidence that might count for SR as against CE. At the same time, he evidently accepts that consistency is an important theoretical virtue. I suggest that the best way to provide an account of van Fraassen's position at this point is to view him as a realist, rejecting inconsistent theories on metaphysical grounds, and to offer as explanatory background information the presumption that reality is coherent and the requirement that science should accord with his realist metaphysical commitments.(9)

Theories which are inconsistent can be discarded on van Fraassen's account, not because their statements are untrue, but because their statements are not, as a whole, susceptible to a sensible literal construal. Van Fraassen's acceptance that theories are to be literally

construed points to the presumption of realism for which I have argued. It is because van Fraassen is to some extent a realist that he is able to secure the ability to discard inconsistent theories while at the same time holding that truth is not the aim of science.

The third line of argument, to the effect that van Fraassen's separation of science and truth is not so complete as a preliminary consideration might suggest, involves a reconsideration of the structure of van Fraassen's case. Specifically, we need to inquire as to what concrete evidence is offered by van Fraassen to support the view that all connections between an understanding of science and the notion of truth must be cut. The quick answer to this question is "none". Van Fraassen supplies an alternative to the hypothesis that true theories are the aim of science and that the belief involved in theory acceptance is the belief that the theory is true. But he does not try to argue for, and hence provides no evidence in support of, the view that the notion of truth can have no role to play in an understanding of science.

In fact, even concerning the theses which he uses to characterize CE, van Fraassen seems to allow that at least some of the scientific realist's beliefs, beliefs concerning the truth of the theory, may not be entirely

wrongheaded. He allows as much through the rather weak way in which he formulates his agnosticism concerning the realm of the unobservable. Van Fraassen notes distinctions between 'compelling arguments' and 'inclining reasons' and between 'rational to believe' and 'irrational not to believe'.(10) He is generally concerned with denying that there are compelling arguments which make it irrational not to believe in the truth of the statements of a theory which concern the unobservable component of reality. But he leaves open the possibility that there could be inclining reasons which might make such belief rational. He certainly never presses an argument to the effect that beliefs about the observable must be irrational. He never follows through, then, with an argument to the effect that it is irrational to believe as the scientific realist does.

In conclusion, van Fraassen's apparent divorcing of science from truth can be seen to be less substantial than it might first have seemed. The ways in which a concern with truth may still be seen to be active on the CE account are through the fact that van Fraassen explicates his notion of empirical adequacy such that it can be understood as partial truth, and through van Fraassen's maintenance of the means for discarding theories which we might recognize as false. In addition, there is a complete lack of argument on van Fraassen's part to the effect that truth

can have no role to play in an understanding of science. The thesis which he in fact argues for is much weaker than that; it is that truth need not be seen as the aim of science and, of course, that theory acceptance need not involve the belief that the theory is true. Van Fraassen rejects the inference from a theory's goodness to its truth. But the sceptical argument which van Fraassen advances against SR, and which I outlined in Chapter One, does not go so far as to completely rule out every understanding which would have science, to some extent, concerned with truth.

There can be no objection, then, based on his apparent divorce of science from truth, to the claim that van Fraassen is, in my sense, a semantic realist. And, as I have argued, it is plausible to see van Fraassen as such a semantic realist given his agreement that theories should be construed literally and his acceptance of a correspondence notion of truth. In addition, viewing van Fraassen as a semantic realist may provide the only way of reconciling his acceptance of consistency as a criterion of acceptability, with his overall endorsement of CE. And finally, through having argued that SR and CE can be seen to fit on the third level of my hierarchy of realisms, that is, that they share a presumption of semantic realism, I have completed enough of a justified exegesis of van

Fraassen's account, to facilitate my defence of CE in the next chapter.

NOTES

(1) Lest it be thought that the thesis that captures metaphysical realism says nothing of interest at all, let me point out that Michael Devitt considers a similar minimal notion of realism as well as the lines of thought that might oppose it. Devitt writes:

For the realist the material or metaphysical world he believes in has to exist not only objectively but non-mentally. We can roughly distinguish two aspects of this: (1) the world does not consist in mental objects of experience, neither in 'ideas' as idealists like Berkeley thought, nor in 'sense data' as many phenomenologists thought; (2) the world is not made up of minds, as Leibniz thought, nor of something ultimately spiritual as the absolute idealists thought. (1984 pp.13-14)

(2) A quick explication is another matter, however. Attempts to state correspondence truth 'in a nutshell' are not difficult to unearth. In discussing the meaning of the term 'true', Alfred Tarski links correspondence truth with the formula: "The truth of a sentence consists in its agreement with (or correspondence to) reality." (Tarski p. 15.) Michael Devitt gives the correspondence notion of truth the following form:

Sentences of type x are true or false in virtue of: (1) their objective structure; (2) the objective referential relations between their parts and reality; (3) the objective nature of that reality. (1984 p. 36.)

(3) Again I am quoting from van Fraassen 1980a p.38. As I have mentioned, with truth conditions so described, any notion of truth other than correspondence would seem to be ruled out. It is here then that the internalist positions advocated by Hilary Putnam (1979) and Brian Ellis (1985) can be seen to be peripheral to the disagreement between SR and CE. The internal realist case as Ellis presents it would involve a disagreement with van Fraassen over what theory of truth is to be accepted:

For I want to show how scientific realism can be defended by ... accepting a pragmatic theory of truth. Also, in retrospect, I think this has

always been the basic issue on which van Fraassen and I have disagreed. (Ellis 1985 p.67)

But van Fraassen casts SR and CE in disagreement not over what truth is, but whether truth should be seen as the aim of science and whether belief in the truth of theories is called for. It is in the context of the correspondence notion of truth as it is accepted by SR and CE that I am offering my defense of CE.

It is appropriate here also to set aside the Davidsonian view of correspondence truth as not pertinent to the question at hand. Davidson (1983 p.423) takes as his slogan "correspondence without confrontation". SR and CE take correspondence truth to be a relation which might hold between theory and reality, thus the notion of theory confronted with reality is central to the truth relation as they see it. Davidson calls the notion of such confrontation "absurd" (also p.423). Clearly there is a question to be settled between van Fraassen and Davidson, but equally it is a question between SR and Davidson, and therefore it is not a question to be settled here.

(4) I am not the first to advocate a reference-correspondence understanding of van Fraassen's notion of 'literal truth'. From Brian Ellis' assessment of van Fraassen it is clear that Ellis would agree that the constructive empiricist would accept the semantic realism theses I have proposed:

Van Fraassen's concept of literal truth is a correspondence concept: a statement is literally true if, literally interpreted, it accurately describes or corresponds to reality. The rules for literal interpretation are not clearly specified, but he has in mind at least this: any apparent reference to a theoretical entity is to be construed as a genuine attempt to refer, unless there are good specific reasons for not so construing it. (1985 p.49)

These amount to explicit statements that Ellis interprets van Fraassen as accepting both components which make up what I am calling 'semantic realism': the genuine reference component and the correspondence truth component. It is perhaps significant that in his response to Ellis' paper, van Fraassen does not disagree with this aspect of Ellis' interpretation of CE. (1985 pp.286-89)

(5) A more contentious argument than what I have offered might draw the conclusion that SR and CE, as presented by

van Fraassen, simply entail the theses of semantic realism. I have been careful, thus far, to argue the comparatively weaker thesis that, given van Fraassen's actual writings, it is plausible to understand SR and CE as presuming the theses of semantic realism. Still, I do not intend to be offering a modified version of van Fraassen's account, distinctly different from the original. Instead, I intend my expanded versions of SR and CE to be taken as an articulation of van Fraassen's account. Whether it is an articulation which van Fraassen would find agreeable is an interesting question. Whether a plausible conflicting articulation can be constructed is certainly a matter worthy of consideration.

(6) Interestingly, Michael Devitt (1984) also uses the term 'anti-scientific-realism' to describe the position held by van Fraassen:

"Bas van Fraassen is a common sense realist but an anti-scientific-realist." (1984 p.6)

(7) The phrase 'utter indifference' comes from Richard Creath 1985 p.333. For more on Creath's appraisal see my note 3 of Chapter One.

(8) The inspiration for this line of argument against inconsistent theories comes from Wittgenstein's Tractatus Logico-Philosophicus. In passage 4.461 Wittgenstein writes:

Propositions show what they say: tautologies and contradictions show that they say nothing.

A tautology has no truth conditions, since it is unconditionally true: and a contradiction is true on no condition.(1974 p.34)

And in 4.462 he continues:

Tautologies and contradictions are not pictures of reality. They do not represent any possible situations. For the former admit all possible situations and the latter none.(1974 p.35)

(9) Alternatively, it might be argued that van Fraassen could dismiss inconsistent theories using the logical point that anything follows from a contradiction. From a theory containing a contradiction it would be possible to derive a false statement about the observable component of reality, whether or not the contradiction purported to be 'about' that component. Hence any such theory may be dismissed as

failing to be empirically adequate. This strategy might work for van Fraassen. It is worthy of note, however, that the assertion that "anything follows from a contradiction" might be rejected. In so far as adopting a system of logic may be a matter of convention, it might be maintained that inference from a contradiction should be ruled out of court. In support of barring inference from a contradiction in the context of a scientific theory, it is difficult to see how any statement which is supposed to be 'about the world' should follow from a statement which fails to be 'about the world'.

(10) In discussing an argument by Hilary Putnam, van Fraassen writes:

Putnam concludes this line of reasoning by asking what more could be wanted as evidence for the truth of a theory than what the realist considers sufficient: 'But then ... what further reasons could one want before one regarded it as rational to believe a theory?' The answer is none -- at least if he equates reasons here either with empirical evidence or with compelling arguments. (Inclining reasons are perhaps another matter, especially because Putnam uses the phrase 'rational to believe' rather than 'irrational not to believe'.) Since Putnam has just done us the service of refuting Verificationism, this answer 'none' cannot convict us of irrationality. He has just argued forcefully that theories could agree in empirical content and differ in truth-value. Hence, a realist will have to make a leap of faith. The decision to leap is subject to rational scrutiny, but not dictated by reason and evidence. (1980a pp.36-37)

Van Fraassen answers "none" because he does not have a quarrel with the scientific realist over what evidence is relevant to theory acceptance, just over how the evidence is relevant and what sort of belief is involved. Thus, it can be conceded that SR calls for a consideration of all the relevant evidence. Van Fraassen's point is that that evidence in combination with the process of reason does not dictate the scientific realist's 'leap of faith'. But the possibility that the same evidence might 'incline' someone to 'leap' and might make such a 'leap' rational, that possibility is quite clearly left open in this passage. And indeed, it is a possibility which van Fraassen never attempts to rule out.

CHAPTER THREE

V. The Issues of Scientific Methodology

The distinction between ASeR and AScR and its significance for van Fraassen's general theory of science has been overlooked in at least three important cases. In each of these cases van Fraassen has been charged with omitting in CE exactly what enables SR to give an adequate account of scientific methodology. But in each case, what is really enabling such an account is the semantic realism underlying SR. In this section I will confront each of these three arguments against van Fraassen and argue for the equality of SR and CE with respect to their abilities to account for scientific methodology.

1. The Miracles Argument

One argument which has been advanced in support of SR is what I will call the miracles argument. Alternatively this same argument might be called the cosmic coincidence argument or the Ultimate Argument. The reference to "cosmic coincidences" is from J. J. C. Smart's formulation (1968 p.151). The "Ultimate Argument" is a name used by both van Fraassen (1980a p.39) and Alan Musgrave (1985 p.209). Unfortunately, van Fraassen and Musgrave do not

use the name to refer to the same argument. When Musgrave introduces his own formulation as a refined form of the Ultimate Argument he allows that its roots are in Putnam's miracles argument and refers to Putnam's remark that "realism is the only philosophy that doesn't make the success of science a miracle". (Putnam 1975 p.73) Musgrave focuses attention on the situation in which "a theory devised to accommodate some phenomenal regularities" turns out to predict new regularities. Musgrave continues:

The realist has a ready explanation: the entities postulated by the theory really exist, and what the theory says about them is true (or nearly so). The antirealist seems forced to say that figments dreamed up for one purpose have turned out, miraculously, to be well adapted for quite a different purpose. (1985 p.210)

Quite clearly, Musgrave has in mind a problem about how particular theories can be successful in particular ways. When van Fraassen discusses the Ultimate Argument he has something quite different in mind. Van Fraassen writes:

Well, let us accept for now this demand for a scientific explanation of the success of science. Let us also resist construing it as merely a restatement of Smart's 'cosmic coincidence' argument, and view it instead as the question why we have successful scientific theories at all. (1980a p.39)

Van Fraassen then goes on to suggest that science is a 'biological phenomenon' (1) about which we can take a 'Darwinist' attitude:

For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive -- the ones which in fact latched on to actual regularities in nature. (1980a p.40)

Not surprisingly, Musgrave finds that the suggestion that "only the successful theories survive" does not address the point about which he is worried. Musgrave's concern is over how it is that we can manage to construct a theory which not only fulfils the explanatory purpose of accounting for previously noted phenomenal regularities, but which goes beyond those to predict, and thus to explain, other regularities which may not have been recognized before. Van Fraassen, on the other hand, is providing an answer to the question: how is it that the theories which we do have are so good at dealing with the phenomena. The rather banal point that an ability to account for the phenomena is a survival criterion for scientific theories is relevant to this latter question. But Musgrave's worry remains untouched by it.

Van Fraassen's reference to theories in fact latching on to actual regularities in nature, however, is suggestive of the response appropriate to Musgrave's worry. I suspect that, in the context of his Darwinist suggestion, van Fraassen intended to refer to observable regularities, since he would hold that those are the ones to which we could have access. But in the context of a response to

what Musgrave offers as the "Ultimate Argument", it is important to note that van Fraassen need not limit his reference to what is observable. When van Fraassen accepts that theories should be literally construed, he does not distinguish between the part of the theory which concerns reality's observable component and the part which concerns the unobservable. To repeat van Fraassen: if a theory's statements include "There are electrons" then the theory asserts the existence of electrons. All of a theory's statements are supposed to be capable of being true. Because of the presumption of semantic realism by CE, the notion that there are 'actual regularities in nature', even beyond the observable/unobservable line, and that our theories might 'latch on to' those regularities, is completely consonant with van Fraassen's position. What van Fraassen denies is that we can have knowledge of the truth of our theories beyond the observable/unobservable line. But he would not be guilty of being inconsistent in, at the same time, postulating that there are unobservable regularities and that those regularities explain the unexpected predictions which sometimes characterize successful theories.

There is a very important distinction to be considered here. The first question to confront is whether we can make sense of the idea that there exist entities which are

unobservables. Both SR and CE answer affirmatively. Scientific statements are capable of being true. The second question is whether we should believe that what our scientific theories say about unobservables is true. It is in response to this second question that SR and CE disagree. But it is the answer to the first question which decides whether there is an available answer, of the sort found acceptable by the scientific realist, to the miracles argument. If one can make sense of the idea that unobservable entities exist then it is open to postulate that the behaviour of those entities might account for otherwise unexplained regularities in the phenomena. It is, thus, the semantic realism which SR and CE have in common which allows for a response to the miracles argument.

I want to mention in passing that the best statement of the miracles argument should only hold that the realist's explanation is better than what is available for the antirealist. Whether the scientific realist's explanation in terms of the theory being true is an acceptable explanation, or even the best available explanation, is a separate point, debatable in its own right.

In spite of the fact that the ability to respond to the miracles argument comes from the semantic realism

common to SR and CE, the form of the scientific realist's answer as Musgrave has proposed it is quite different from the answer which would be offered by the constructive empiricist. The constructive empiricist can allow that unobservable entities are playing a role in causing the newly discovered regularities. The scientific realist will likely go beyond that to say that the specific entities referred to by the theory do in fact exist and are correctly described by the theory. In view of the fact that there are two different responses offered, it makes sense to ask which one is better.

It seems to me that an adequate response to any version of the 'miracles' argument, as applied in any philosophical domain, ought to refrain, as much as possible, from replacing one miracle with another. There is a sense in which replacing one such miracle with another is unavoidable. If regularities in the phenomena are to be explained through the postulation of regularities in the unobservable, it will always be open to ask what explains the regularities in the unobservable. Surely if our recognition of the existence of the observable regularities justifies a demand for explanation, then the supposed existence of the postulated unobservable regularities justifies a similar demand. Van Fraassen recognizes that the attempt to eliminate all such 'miraculous' regularities

is doomed to fail. His response to the miracles argument, then, is to claim that if the call for the theoretical elimination of all unexplained coincidences could be precisely formulated "it would lead to absurdity".(2) Hence, van Fraassen denies that it can be such a call to explain away all apparent miracles which motivates the scientific demand for explanation.

Leaving aside the seemingly ineliminable aspect that any newly postulated regularity might itself seem miraculous, the scientific realist might be guilty of endorsing an avoidable miracle in claiming the truth of scientific theories. Taking seriously the thesis that the evidence for scientific theories always underdetermines those theories, and accordingly that scientific method cannot guarantee the truth of our best theories (assuming a correspondence theory of truth), how is it that, out of all the possibly true theories underdetermined by the evidence, we have come to postulate the only true one?(3) Is that not itself a miracle? Van Fraassen, on the other hand, would claim not that theories which predict new regularities are true, but could make the weaker claim that such theories have latched onto actual regularities in nature, while not limiting those 'actual regularities' to the observable realm. This weaker claim could of course be true of a number of the theories allowed by the underdetermining

evidence, not just the true one. As a result, van Fraassen would allow the scientist a larger 'target area' in which to hit upon a theory capable of predicting new regularities and thus van Fraassen's response to the 'miracles' argument would have the virtue of suggesting that a smaller miracle had taken place when such a theory was in fact constructed.(4)

In spite of the fact that the scientific realist's response to the miracles argument would involve the claim that the theory in question ought to be considered true, it is important to see that it is not the scientific realist's epistemic attitude which in fact dispels the would-be miracle. This point is easily missed, however, as the scientific realist's epistemic attitude certainly can be seen to presume the theses which do allow for non-miraculous successful novel predictions. But one can see that it is the semantic realism theses which are in fact active in dispelling such miracles because the question of whether a particular theory, or understanding of a theory, would make successful novel predictions seem miraculous can be considered prior to and separate from any epistemic question. It can be seen that a literal construal of a theory would not make a miracle of successful novel predictions because to accept a theory as perhaps true if literally construed is to take the theory's

postulations seriously in a way that would allow those postulations to explain the predictions. But a theory does not have to be true in order to make successful novel predictions. And while our belief in the theory may help us to feel comfortable with such successful predictions, it cannot help the theory make the predictions.

2. The Theory Dependence of Experimental Design

Musgrave is not alone in failing to appreciate the significance of the distinction between AS_{CR} and AS_{ER} for van Fraassen's constructive empiricism. Richard Boyd offers two arguments against van Fraassen which fail in a similar fashion. Boyd's failure to take account of the fact that CE is a form of AS_{CR} but not a form of AS_{ER} may be characterized by his regular reference to the position he is arguing for (SR) simply as "realism" and his casting of the debate between he and van Fraassen as between "realists and non-realists" (Boyd 1985 p.23). Perhaps the corresponding confusion on Boyd's part is not entirely his fault. Both before and after his distinguishing two kinds of anti-realism with respect to science, van Fraassen often refers to the position he argues for simply as "anti-realism" and to the position he argues against simply as "realism". In much the same way as Musgrave (above), what is characteristic of Boyd's misunderstanding of CE is

his failure to see that van Fraassen has at his disposal effective rebuttals to both of Boyd's arguments, and that it is semantic realism, not SR, which allows those answers.

Before presenting Boyd's arguments though, I want to mention that there seems to be at least one other area of confusion between Boyd and van Fraassen. Van Fraassen casts the scientific realist position he argues against in terms of truth. The belief involved in theory acceptance is, straightforwardly, the belief that the theories are true. Van Fraassen does not consider any possibilities for weakening SR by, for example, substituting the notion of verisimilitude for the notion of truth in the characterizing theses.(5) Boyd, on the other hand, seems to hold a relatively weaker position already. Although he does not refer to verisimilitude directly, Boyd is careful, when referring to what we are entitled to believe about our theories, to talk in terms of "approximate truth". He refers to the knowledge afforded us by science as "approximate" or "probable" (Boyd 1985 pp.15,21). Boyd evidently does not adhere to the 'hard-core' SR set as a target by van Fraassen.

Leaving aside this area of confusion, Boyd and van Fraassen apparently do see themselves as engaged in a significant disagreement. Boyd's first argument against van Fraassen charges him with an inability to give an

adequate account of the "theory dependence of experimental design". Boyd argues that for a particular theory L:

... the respects of similarity between the relevant background theories and the suggested alternatives to L lie in the theoretically relevant similarities between the accounts they offer of unobservable phenomena (that's how the examples are constructed). The problem for the anti-realist is then why these theory-determined respects of similarity are (out of the infinitely many possible respects of similarity) the relevant ones. (1985 p.21)

Boyd then claims that van Fraassen fails to provide an answer to "the basic question" from an earlier Boyd paper, "Realism, Underdetermination and a Causal Theory of Evidence":

Suppose you always "guess" where theories are most likely to go wrong experimentally by asking where they are most likely to be false as accounts of causal relations, given the assumption that currently accepted laws represent probable causal knowledge. And suppose your guessing procedure works -- that theories really are most likely to go wrong -- to yield false experimental predictions -- just where a realist would expect them to. And suppose that these guesses are so good that they are central to the success of experimental method. What explanation beside scientific realism is possible?(1973 p.12)

Well, the quick answer is CE but some explanation is in order. Let us take it as true that scientists do "always 'guess' where theories are most likely to go wrong experimentally" in much the way that Boyd suggests. What is it about SR which sets it apart as the exclusive

explanation? Any semantic realist can sensibly wonder about the relations between unobservables. To accept that certain "laws represent probable causal knowledge" may be done hypothetically. There is nothing about the character of those laws which would exclude them from the realm of interest of the constructive empiricist. Relative to alternative laws, alternative theories, alternative contexts, there is no reason to think the constructive empiricist would find herself ordering the significance of places where theories are most likely to go wrong any differently than the scientific realist. The context for theory testing will include some assumption of reference and truth or falsity. But that assumption may mean essentially the same thing for all who accept a correspondence theory of truth and a literal interpretation of theories. Just what a theory says, and what it would mean for a theory to be true, are certainly relevant to how we are able to design experiments. But the scientific realist and the constructive empiricist agree on those things. Whether the truth of a theory is believed before the test, after the test, or neither, is irrelevant to experimental design.

Strictly speaking, the answer to Boyd's question, of what explanation is possible of the theory dependence of experimental design, depends upon semantic realism. I have

argued that SR and CE are on equal footing regarding semantic realism. Thus they are in equally good positions to answer Boyd's challenge and the problem of the theory dependence of experimental design is yet another question to which our epistemic attitudes may be irrelevant.

3. The Unity of Science Principle

Boyd's failure to recognize the significance of the semantic realism present in CE leads him astray in a second argument against van Fraassen. Again a methodological complaint is raised; the argument is that the "anti-realist" can make no sense of "theoretical inductions" involved with the "unity of science" principle.

From Boyd:

Not only does sound scientific methodology dictate the deductive integration of theories described by the positivists' unity of science principle, it also dictates the inductive integration of theories -- the use of individually well confirmed theories (sharing common theoretical terms) as premises in inductive as well as deductive inferences. It is just such inferences which are methodologically crucial in the assessment of experimental evidence, and -- as we have seen -- these inferences make epistemic sense only if the evidence for particular theories is taken to be evidence for their approximate truth (and if our judgements of univocality for theoretical terms are reliable) -- that is, only if a realist conception of scientific inquiry is adopted. (1985 p.17)

The first thing to notice is that Boyd again makes the mistake of assuming that it is what is definitive of SR over and above its semantic realism that enables the scientific realist to make sense of a scientific practise. The assumption that it is epistemic sense that we need to be able to make of the inferences in question may be mistaken and is certainly question-begging. Of course the constructive empiricist will not try to make epistemic sense of the scientists' behaviour -- she thinks that there may be none to be made. But if the contention is that we need to be able to make epistemic sense of this, some supporting argument please!

Meanwhile, let us address the general question of whether the scientific realist and the constructive empiricist can make enough sense of the unity of science principle and the inferences involved to justify the practise. And if that justification is available, just what is it that provides it? Suppose we first accept as real and unproblematic the scientific practise suggested by Boyd. We might ask, to begin with, what makes the goal of a unified science a sensible one. The answer is, quite simply, the positing of a unified reality. If we posit a unified reality it makes sense to have our account of that reality similarly unified. The unity of science principle stems from the semantic realist conception of reality and

thus is made sensible by realist metaphysical views, prior to any epistemic concerns. As I have argued above, both SR and CE have access to the appropriate realist underpinnings. True to form, van Fraassen accepts the unity of science principle.(6)

Even with a sensible unity of science principle, it remains to be asked whether inductive inferences concerning theoretical postulates, in accordance with that principle, are themselves sensible on the constructive empiricist account. Semantic realism comes to the rescue; the inferences in question are justified by the unity of science principle -- they are made sensible by our literal construal of theories, by our genuine attempt to refer. The scientific realist may also hold that the inductions in question produce knowledge; the constructive empiricist will disagree -- but both will agree to the meaning of the conjectures, and specifically to their implications concerning reality if their truth is assumed. They will agree, that is, so far as they are united by their semantic realism.

Boyd's final plea on this matter runs as follows:

What is important for the debate between realists and nonrealists is that the modifications in question are themselves theory-determined. From the infinitely many possible modifications of current theories which might be occasioned by the adoption of a new theory (and by whatever new data support it),

scientists choose to consider those which are theoretically plausible -- which are suggested by inductive inferences at the theoretical (as well as the observational) level. (1985 p.23)

But because of the realist base to be found in both SR and CE, it is irrelevant to a choice between them to point out the fact that the modifications in question are theory-determined. It is a mistake to think that where ASeR fails AScR will also fail -- and thus it is a mistake to advance such arguments against van Fraassen. For SR and CE alike a scientific theory is a proposed account of reality. This is true for the component of the theory which deals with what is unobservable, just as for the component which deals with what is observable. Scientific statements are equally meaningful on SR and CE regardless of which component might claim them. A unity of science principle is called for by the realism manifest in SR and CE alike. Inferences called for by the unity of science principle will certainly transcend the observable/unobservable boundary because the principle itself does. Be that as it may, it is clear that those inferences, neither for their justification nor for their sensibility, are in any way dependent on their epistemic status. Their justifiability comes from metaphysical realism; their sensibility from semantic realism; and their

epistemic status is left, perhaps as it should be, in question.

VI. Concluding Remarks

Having considered only three complaints that CE fails to allow for a robust scientific methodology, perhaps I would be unwarranted in drawing an inductive conclusion that all such complaints must fail. I will refrain from making that inference. I will not, however, shy away from the conclusion -- at least so far as such complaints being grounds for a choice between SR and CE is concerned. On van Fraassen's behalf I have argued that his apparent divorcing of science from truth is to some extent an illusion. I have redescribed SR and CE to stress their common underlying realism, both metaphysical and semantic. And I have demonstrated that for three examples of methodological complaints aimed at CE it is that realism which enables CE to respond adequately to the charges. In each case questions concerning the appropriate epistemic attitude to take toward scientific theories have turned out to be irrelevant.

In concluding, I would like to offer the following short argument. We need to be able to apply the scientific method to all theories we might consider, whether they are true or false, whether they are believed or not. For the purposes of testing and manipulating the theories themselves, we can pretend they are true and examine their

consequences. We can, as it were, place them in a 'hypothetical bubble'; we need not believe them. It is true that we need some reason to prefer the good theories over the not so good ones, but this need not be (perhaps can not be) the fact that we believe them to be true. For that we believe them to be true itself calls for a reason. Whatever we might advance as a reason for believing a theory might just as well serve as our reason for preferring it. But there is no purpose which our belief in a theory might fulfill, so far as enabling an adequate account of scientific methodology is concerned, other than to explain or justify our treatment of that theory. The question of the proper epistemic attitude then is correctly to be seen as superfluous to the concerns of scientific methodology. Perhaps scientists, rightly or wrongly, do believe in the truth of our best theories but that belief has no crucial role to play in understanding or justifying the scientific method.

NOTES

(1) Van Fraassen writes:

I would like to point out that science is a biological phenomenon, an activity by one kind of organism which facilitates its interaction with its environment. (1980a p.39)

I do not think that van Fraassen is, or means to be, making a contentious statement in 'pointing out' that science is a 'biological phenomenon'. Specifically, he is not excluding the possibility that science is also a rationality governed, intellectual activity. He is merely pointing out that, whatever else science is, it is also part of our means of coping with our environment.

(2) Van Fraassen's discussion of this point is on 1980a pp.24-25. Apparently he thinks it is absurd to view science to as motivated by a demand which can never be met, the demand to explain all regularities by postulating new, and thus unexplained, regularities. I am not interested, at this point, in taking up the issue of whether van Fraassen can successfully dispatch the miracles argument in the manner he intends. The point I wish to make is that, even if science can be seen as motivated to eliminate what might be called 'miraculous' coincidences, the miracles argument can be seen to have no bite against van Fraassen's CE.

(3) A good discussion of underdetermination is offered by Brian Ellis (1985). Ellis gives the thesis the following form:

All theories are empirically underdetermined in the sense that logically nonequivalent but empirically equivalent alternative theories always exist. (p.67)

Ellis argues that the metaphysical realism and correspondence notion of truth presumed by both SR and CE guarantee the underdetermination thesis. In van Fraassen's account empirical underdetermination can be seen to ensure the existence of the epistemic gap which the scientific realist might want to close via inference to the best explanation. The gap is opened because theories typically go beyond the range of what is empirically available as evidence in such a way that the theory cannot be deduced from statements of the initial conditions and evidence

alone. Some kind of non-deductive inference must be invoked in order for us to see a particular theory as exclusively following from the evidence. In the context of my present query concerning the 'miracle' of stumbling onto the one true theory, it is important to note that the number of possibly true theories underdetermined by a given body of evidence may be exceedingly large, perhaps infinite.

(4) To suggest that it would be miraculous were we to stumble across 'the truth' takes us back to the scientific realist's 'inference to the best explanation'. It is that inference which the scientific realist asserts, and which van Fraassen denies, warrants the belief in the truth of a theory. Van Fraassen's question is: even given that some theory best explains the data, why should we believe that theory to be true. My question here is: would it not seem miraculous, or at least very lucky, if that theory were to be true.

Larry Laudan has made a further attack in this same general area of the scientific realist's case. Laudan has argued that:

... many (now discredited) scientific theories of earlier eras exhibited an impressive sort of empirical support, arguably no different in kind from that enjoyed by many contemporary physical theories. Yet we now believe that many of those earlier theories profoundly mischaracterized the way the world really is. More specifically, we now believe that there is nothing in the world which even approximately answers to the central explanatory entities postulated by a great many successful theories of the past. (1984 p.157)

In the face of Laudan's argument the scientific realist must at least retreat from the theses van Fraassen attributes to her or view herself as a kind of epistemological Sisyphus, repeatedly pushing theories up the mountain of truth only to see them roll unendingly down into the valley of falsehood. The response van Fraassen can offer to the miracles argument, however, should not invite the same criticism from Laudan. For it is only in equating success in science with coming to know the truth about the world that the scientific realist sticks her epistemological neck out. The constructive empiricist withholds claims to the truth of theories and thus avoids the many historical beheadings to which the scientific realist seems prone.

(5) 'Verisimilitude' is a notion considered seriously by Karl Popper (1963). Popper variously refers to 'verisimilitude' as 'approaching truth', 'approximate truth', and 'truthlikeness'. (pp.228-235) He explicates verisimilitude in terms of 'truth content'. The idea is that whether a theory is true or false it can have more or less 'truth content', where 'truth content' is to be understood roughly in terms of the class of the true logical consequences of the theory in question. The suggestion, then, that SR could be weakened by using the notion of verisimilitude in place of truth amounts to the idea that science might aim at increasing verisimilitude and that the belief involved in theory acceptance might be that the theory increases, or perhaps sets the stage for increasing, verisimilitude. As I have presented it, this suggestion is very 'sketchy'. And the notion of verisimilitude is itself rather 'fuzzy'. (I think that Popper never succeeded in scraping all the fuzz off.) It is not clear, however, that the notions of 'approximate truth' and 'approximate knowledge' which Boyd refers to are in any better condition, particularly in consideration of the possibilities for failed reference to putative microparticles.

(6) On the idea of our ultimately accepting a non-unified science van Fraassen writes:

... it seems to me that the idea of a science consisting of a family of such disparate theories is really not feasible, except in the philosophically innocuous sense in which it actually does. (1980a p.86)

"Philosophically innocuous", I take it, in that the current state of science, for van Fraassen's CE, need not be seen as acceptable. Van Fraassen continues on to indicate that he views CE as calling for theory unification:

There remains then only the problem of living with lots of 'mini-theories' in practise, as we actually do. ... Philosophy of science could do with a more accurate picture of this situation -- it is the actual situation of the working scientist and may well harbour problems obscured by our preoccupation with global theories. But there seems to me no doubt that the aim of empirical adequacy already requires the successive unification of 'mini-theories' into larger ones, and that the process of unification

is mainly one of correction and not of
conjunction. (1980a p.87)

BIBLIOGRAPHY

- Boyd, Richard N., (1973)
"Realism, Underdetermination, and a Causal Theory of Evidence", Nous 7, 1-12.
- , (1976)
"Approximate Truth and Natural Necessity", Journal of Philosophy 73, 633-35.
- , (1981)
"Scientific Realism and Naturalistic Epistemology" in PSA 1980, vol. 2, P. D. Asquith and R. N. Giere eds., 613-62. East Lansing, MI: Philosophy of Science Association.
- , (1983)
"On the Current Status of the Issue of Scientific Realism", Erkenntnis 17, 135-69.
- , (1985)
"Lex Orandi est Lex Credendi" in Churchland and Hooker (1985), 3-34.
- Brown, J. R., (1982)
"The Miracle of Science", Philosophical Quarterly 32, 232-44.
- Cartwright, Nancy, (1982)
"When Inference Leads to Explanation", Philosophical Topics 13, 111-21.
- , (1983)
How the Laws of Physics Lie. Oxford: Clarendon Press.
- Churchland, Paul M., (1979)
Scientific Realism and the Plasticity of Mind. Cambridge: Cambridge University Press.
- , (1985)
"The Ontological Status of Observables: In Praise of the Superempirical Virtues" in Churchland and Hooker (1985), 35-47.
- Churchland, Paul M. and Clifford A. Hooker eds., (1985)
Images of Science. Chicago: University of Chicago Press.

- Creath, Richard, (1984)
 "Smart, Salmon and Scientific Realism", Australasian Journal of Philosophy 62, 404-09.
- _____, (1985)
 "Taking Theories Seriously", Synthese 62, 317-45.
- Davidson, Donald, (1967)
 "Truth and Meaning", Synthese 17, 304-23.
- _____, (1983)
 "A Coherence Theory of Truth and Knowledge" in Kant Oder Hegel. Stuttgart: Klett-Cotta.
- Demopoulos, W., (1982)
 Review of van Fraassen (1980), Philosophical Review 91, 603-7.
- Devitt, M., (1982)
 Review of van Fraassen (1980), Australasian Journal of Philosophy 60, 367-69.
- _____, (1984)
Realism and Truth. Princeton: Princeton University Press.
- Dummett, Michael, (1975)
 "What is a Theory of Meaning?" in Mind and Language, Samuel Guttenplan ed. Oxford: Clarendon Press, 1975.
- _____, (1976)
 "What is a Theory of Meaning? (II)" in Truth and Meaning: Essays in Semantics, Gareth Evans and John McDowell eds. Oxford: Clarendon Press, 1976.
- Earman, John ed., (1983)
Testing Scientific Theories: Minnesota Studies in the Philosophy of Science, vol. 10. Minneapolis: University of Minnesota Press.
- Ellis, Brian, (1985)
 "What Science Aims to Do" in Churchland and Hooker (1985).
- Feyerabend, Paul, (1975)
Against Method. London: NLB.
- Fine, A., (1984a)
 "The Natural Ontological Attitude" in Leplin (1984b).

- _____, (1984b)
 "And Not Anti-Realism Either", Nous 18, 51-65.
- Foss, Jeff, (1984)
 "On Accepting van Fraassen's Image of Science",
Philosophy of Science 51,
- Friedman, M., (1982)
 Review of van Fraassen (1980), Journal of Philosophy
 79, 274-83.
- Giere, Ronald N., (1983)
 "Testing Theoretical Hypotheses" in Earman (1983).
- _____, (1985)
 "Constructive Realism" in Churchland and Hooker
 (1985).
- Glymour, Clark, (1984)
 "Explanation and Realism" in Leplin (1984).
- Grimes, T. R., (1984)
 "An Appraisal of van Fraassen's Constructive
 Empiricism", Philosophical Studies 45, 261-68.
- Gutting, Gary, (1983)
 "Scientific Realism versus Constructive Empiricism: A
 Dialogue", The Monist 65, 336-49.
- Hacking, Ian, (1981) "Do We See Through a Microscope?",
Pacific Philosophical Quarterly 62, 305-22.
- _____, (1983)
Representing and Intervening: Introductory Topics in
 the Philosophy of Natural Science. Cambridge:
 Cambridge University Press.
- Hanna, J. F., (1983)
 "Empirical Adequacy", Philosophy of Science 50, 1-34.
- Harman, G., (1965)
 "The Inference to the Best Explanation", Philosophical
 Review 74, 88-95.
- Hausman, D. M., (1982)
 "Constructive Empiricism Contested", Pacific
 Philosophical Quarterly 63, 21-28.

- Healey, Richard ed., (1981)
Reduction, Time and Reality. Cambridge: Cambridge University Press.
- Hesse, Mary, (1981)
 "Anti-Realist Philosophy of Science", Nature 289, 207-08.
- Hooker, Clifford A., (1985)
 "Surface Dazzle, Ghostly Depths: An Exposition and Critical Evaluation of van Fraassen's Vindication of Empiricism against Realism" in Churchland and Hooker (1985).
- Kuhn, Thomas S., (1970)
The Structure of Scientific Revolutions, Second Edition, Enlarged. Chicago, IL: The University of Chicago Press.
- Laudan, Larry, (1984)
 "Realism Without the Real" in Philosophy of Science. Vol. 51, 156-162.
- Leplin, Jarrett, (1984a)
 "Truth and Scientific Progress" in Leplin (1984b).
- _____, (1984b)
Scientific Realism. Berkeley: University of California Press.
- Margolis, Joseph, (1986)
Pragmatism Without Foundations. Oxford: Basil Blackwell.
- Maxwell, Grover, (1962)
 "The Ontological Status of Theoretical Entities" in Minnesota Studies in the Philosophy of Science, vol. 3, 3-27. Minneapolis: University of Minnesota Press.
- McLaughlin, Robert ed., (1982)
What? When? Where? Why?. Boston: D. Reidel.
- Melchert, N., (1985)
 "Why Constructive Empiricism Collapses into Scientific Realism", Australasian Journal of Philosophy 63, 213-15.
- Musgrave, Alan, (1985)
 "Realism Versus Constructive Empiricism" in Churchland and Hooker (1985).

- Popper, Karl R., (1963)
Conjectures and Refutations: The Growth of Scientific Knowledge. New York, NY: Harper and Row.
- Putnam, Hilary, (1975)
 "What is Mathematical Truth" in Mathematics, Matter and Method: Philosophical Papers, Volume I. London: Cambridge University Press.
- _____, (1978)
Meaning and the Moral Sciences. London: Routledge and Kegan Paul.
- _____, (1982)
 "Three Kinds of Scientific Realism", Philosophical Quarterly 32, 195-200.
- Rosenberg, A. and C. L. Hardin, (1982)
 "In Defence of Convergent Realism", Philosophy of Science 49, 604.
- Salmon, Wesley C., (1982a)
 "Comets, Pollen, and Dreams: Some Reflections on Scientific Explanation" in McLaughlin (1982).
- _____, (1982b)
 "Further Reflections" in McLaughlin (1982).
- Smith, Peter, (1981)
Realism and the Progress of Science. Cambridge: Cambridge University Press.
- Sober, Elliot, (1985)
 "Constructive Empiricism and the Problem of Aboutness", British Journal for the Philosophy of Science 36, 11-18.
- Tarski, Alfred, (1944)
 "The Semantic Conception of Truth", Philosophy and Phenomenological Research 4, 13-47.
- Thornton, M., (1981)
 "Sellars' Scientific Realism: A Reply to van Fraassen", Dialogue 20, 79-83.
- Trigg, Roger, (1983)
 Review of van Fraassen (1980), Mind 92: 291-93.
- van Fraassen, Bas C., (1976)
 "On the Radical Incompleteness of the Manifest Image",

PSA 1976 vol. 2, F. Suppe and P. Asquith eds. East Lansing, Michigan: Philosophy of Science Association, 1977.

- _____, (1980a)
The Scientific Image. Oxford: Clarendon Press.
- _____, (1980b)
 "Theory Comparison and Relevant Evidence" in Earman (1983).
- _____, (1980c)
 "Glymour on Evidence and Explanation" in Earman (1983).
- _____, (1981a)
 "Essences and Laws of Nature" in Healey (1981).
- _____, (1981b)
 Critical notice of Churchland (1979), Canadian Journal of Philosophy 11, 555-67.
- _____, (1982a)
 "Rational Belief and the Common Cause Principle" in McLaughlin (1982).
- _____, (1982b)
 "Epistemic Semantics Defended", Journal of Philosophical Logic 11, 463-64.
- _____, (1985)
 "Empiricism in the Philosophy of Science" in Churchland and Hooker (1985).
- Wilson, Mark, (1980)
 "The Observational Uniqueness of Some Theories", Journal of Philosophy 77, no. 4.
- _____, (1985)
 "What Can Theory Tell Us about Observation?" in Churchland and Hooker (1985).
- Wittgenstein, Ludwig, (1974)
Tractatus Logico-Philosophicus, translated by D.F.Pears and B.F.McGuinness. London: Routledge and Kegan Paul.
- Worrall, J, (1984)
 "An Unreal Image", British Journal for the Philosophy of Science 35, 65-80.