AN ARGUMENT IN DEFENCE OF SCIENTIFIC REALISM

by

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A thesis submitted in conformity with the requirements for the degree of Doctor of Philosophy in the University of London

1994
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ABSTRACT

Scientific realism deals primarily with the scope of scientific knowledge. Based on the realist assumption about the independence of physical reality of man's mental faculty and mental constructs (such as languages, conceptual schemes, and conventions), it asserts that science can and does provide us with knowledge about both the observable and unobservable aspects of the physical reality.

Anti-realists, from different denominations, reject the central tenet of scientific realism. While few amongst them may deny the very possibility of acquiring scientific knowledge, all would dispute the scope of such knowledge as considered by realists. In anti-realists' view, science, at best, can provide us with the knowledge of observable phenomena.

In this essay, after a brief exposition of the main issues involved in the (scientific) realist — anti-realist dispute (Ch.1), and a short historical excursion concerning the development of the central themes in this controversy since the late nineteenth century (Ch.2), I shall concentrate on the cases of a number of modern anti-realists (mostly van Fraassen and Larry Laudan — Chs. 3 & 4), as well as a number of realists who have broken rank with the realist tradition (Ch.5). The bearing of the developments in quantum mechanics on the realist — anti-realist debate has also been critically examined (Ch.6). In the last chapter (Ch.7) an attempt has been made to introduce a more comprehensive theory of science which, it is hoped, will overcome the difficulties which have weakened the stance of the currently better known realist theories.
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Preface

Does science provide us with a reliable knowledge about the world? Are there such things as electrons, genes, black holes, sub-conscious and the like which our celebrated scientific theories tell us to really exist in the world? Or should all such claims to be regarded as useful fictions which facilitate our practical life but add nothing to our theoretical knowledge? Does technological advancement have any bearing on epistemological advancement? Should one take as epistemologically reliable only those scientific claims which are empirically adequate, or is it also rationally legitimate to accept other scientific assertions which are inferences concerning unobservable things and objects? For science to be epistemically reliable, do we require to adopt some metaphysical assumptions concerning the nature of reality and the structure of scientific knowledge, or are no such assumptions are needed? Are scientific assertions concerning the nature of reality *anthropocentric*, or is it the case that scientific judgements are free from subjective aspects and values? Does science make progress, and if it does, in what sense? Can there be a rational (albeit fallible and non-mechanical) method for the discovery or development of new scientific theories which would help scientists to find out more and more about the world and thus enhance their understanding of it, or should one take side with Bacon ([1962], p.145) in calling all such methods as, ‘a method of imposture.’?

The above list furnishes us with a number of typical questions discussed in the debates between scientific realists and their opponents. Each of these two groupings are divided into many more sub-groups. Among various factions of scientific realism, the more ardent supporters of this thesis maintain that the *main* aim of science is to provide
us with the knowledge of both observable as well as unobservable aspects of the physical reality. These realists advocate the theses that most of the posits of the more mature sciences have some sort of reference in the real world, that they are not pure constructs of human’s mind, and that the more mature scientific theories provide us with a more or less *true* picture of the physical reality, which realists postulate to be independent of anything mental. In the view of this group, scientific realism is, by and large, a more fruitful methodological framework for the working scientists than its anti-realist alternatives.

Scientific realism, as stated above, faces objections and criticisms of different sorts and from different corners. In general, the opponents cast doubt on both the *desirability* and the *realizability* of the *realistic* interpretation of science. They would argue, among other things, that the aim defined by the more ardent scientific realists is neither desirable (since we have no warrant to go beyond the available evidence in our knowledge claims), nor realizable (since we have no access to those aspects of reality which fall beyond the phenomenal world).

The above objection have been elaborated by the critics of scientific realism in various ways. Among these critics, three distinct and prominent trends can be discerned. In the first place, there are neo-instrumentalists who would deny that science’s objective is to provide us with *truth* about the unobservable aspects of the physical world. In their view the basic task of science is to acquire *empirical adequacy*, i.e. to save the phenomena and help us in practical (technological) matters. According to this group of anti-realists, the epistemic attitude of scientists towards those claims of science to knowledge which go beyond the (in principle) accessible evidence, should be agnosticism.

The second group of opponents of scientific realism are those who would advocate
a quasi-Kantian thesis that there are two kinds of reality; reality-for-us, and reality-in-itself. While the latter is forever beyond the reach of human beings, the former is a coproduct of human's cognitive faculties and the reality-in-itself. In view of this group, science is a social construct, and as such lacks the objective validity which realists would claim.

The last, though by no means the least, group amongst the critics of the more ardent stance towards scientific realism are other fellow realists who would like to restrict the extent of epistemic reliability of science only to the claims concerning the real existence of scientific posits while rejecting its claims about the truthfulness of scientific theories.

The present essay is a search for a sound version of scientific realism which can withstand the above criticisms. To conduct our investigation we shall try to accomplish four tasks in this essay. In the first chapter, after providing a short account of various versions of the theses which are collected under the general rubric of scientific realism and scientific anti-realism, and introducing a set of basic criteria for an acceptable theory of science, we shall introduce a versions of scientific realism which will serve as a test case in the subsequent chapters; the strength of the opponents' criticisms will be gauged (mostly) with regards to this test case. In the next chapter (Ch.2) we shall make an excursion (albeit a rather brief one) into the recent history of the dispute between realists and their opponents. This excursion is aimed at providing us with the opportunity of looking at the types of arguments and counter-arguments produced by each party to the dispute in the past decades, and to see whether either of these two groups have been able to make better sense of the science of their time, and if so, how.

Thirdly, in the subsequent chapters (Chs.3 to 6) we shall look at three sets of
arguments against scientific realism, namely; the arguments raised by a number of modern anti-realist writers (mostly van Fraassen [Ch. 3], Laudan [Ch. 4]); the arguments of a number of dissident realist writers (Cartwright, Hacking, Harré and Ellis [Ch. 5]); and the arguments raised in the context of quantum mechanics (Ch.6), to see how much these arguments cut ice against scientific realism. The discussions in these interim chapters will pave the way for our main debate which constitutes the last task of this essay.

In the final chapter (Ch. 7) we will pull together different strands discussed in the previous chapters, and will introduce a version of scientific realism embedded in a more comprehensive realist theory of science known as *Aim-oriented Empiricism* due to N.Maxwell. This theory, it will be argued, is less vulnerable to the objections made by anti-realists to many of the current versions of scientific realism. It will also be argued that this theory will better satisfy the basic criteria for an acceptable theory of science and therefore, (it is hoped) it will provide a more fruitful framework for working scientists.

Writing a Ph.D thesis is an arduous task. It is also an intellectually rewarding experience. I find it difficult in a preface to acknowledge fully the help and support of all those who made the difficult task of completing my thesis less onerous and more pleasant. I would like to thank my family for their patience, understanding and constant encouragement during the long period of completion of this work.

I would also like to express my appreciation to those who took the trouble of commenting on various chapters of my thesis. It is my pleasure to mention in particular Professor Bill Hart, Professor Mark Sainsbury, Professor Heinz Post, Professor Michael Redhead, Professor Arthur Miller, Professor P.M. Rattansi and Professor David Papineau. My thanks also to Philip Bowman and David Hall for commenting on some finer points of English usage.
Most of all I would like to thank my supervisor, Mr. Nicholas Maxwell. It is no understatement to say that this work would have not been completed, much less in its present form, were it not for his constant criticisms, advice and encouragement. If I had only taken more of his advice, this would have been a better work.
Chapter One
This Thing That Is Called Scientific Realism

The Problem

The dispute between scientific realists and their opponents is about the scope of Scientific knowledge: realists hold that science provides us with knowledge of unobservable entities, while anti-realists deny this. The history of the dispute over scientific realism can conveniently be reconstructed by focusing on the development of the ideas concerning this fundamental question. A cursory glance at this history would reveal how much the parties to the dispute have moved in the direction of sophistication and maturity. One consequence of this development, as we shall see in the course of this essay, has been a welcome convergence in the views of the discussants.

Perhaps the classic episode in the history of science which vividly shows the nature of the dispute between the older generations of realists and anti-realists is the controversy over the Two Great World Systems: those of Ptolemy’s and Copernicus’s. Anti-realists represented by figures like Osiander and Cardinal Bellarmine were of the view that the Copernican system was only a mathematical model which served to save the phenomena and help scientists to predict the future course of events. Realists, represented by Copernicus, Kepler, and especially Galileo argued however, that the Copernican system presented a true picture of reality and was not a mere mathematical model.

This central theme has been given many variations over the years. However, notwithstanding its different guises, the main issue has always been the possibility of acquiring theoretical knowledge about the unobservable aspects of physical reality. The history of the dispute over this central issue is by no means a monotonous one. On the contrary, it is full of nuances, unexpected turns of fortune and spectacular manoeuvres on
the part of the proponents of each side of the debate. Thus, for example, in the late
nineteenth century, philosophically-minded physicists and chemists in Austria and
Germany tried to show how the fruits of classical mechanics and Maxwellian
electrodynamics could be retained without commitment to the distinctive entities of each
theory. In contrast, in the earlier parts of the same century, many eminent scientists,
including J.C.Maxwell, were advocating the physical reality of ether.

With the benefit of hindsight it can be observed that realists occasionally have
become over-confident and have unwarrantedly claimed truthfulness for their theories and
reality for their theoretical posits. This over-confident approach towards science has
occasionally paved the way for the emergence of a group of realists who could be called
naïve scientific realists. This group of realists have treated science as an almost infallible
source of knowledge. In their view science portrays a faithful picture of unobserved and
unobservable reality and scientific claims should be taken at their face-value.

Naïve scientific realism perhaps has been its own most effective enemy. Its
imprudent claims over understanding the nature of reality has often given enough
ammunition to its critics to undermine it. One of the anti-realists’ most effective counter-
attack has always been a call for epistemic prudence: if the validity of the scientific
claims is to be judged by appeal to the verdict of empirical tests, then one is always on
the safer side, and indeed more consistent, if one limits one’s claims to knowledge to the
realm of observable phenomena.

Anti-realists’ call for epistemic prudence raises a number of important questions.
For example, whether such prudence as preached by anti-realists is in fact conducive to
improving our knowledge of the external world and the progress of science, or whether
knowledge-garnering requires, inter alia, readiness to take measured risks in putting
forward unorthodox speculations (or bold conjectures, *a la* Popper\(^5\)) about the nature of reality. To what extent are the arguments of anti-realists against scientific realism effective? Given the fact that there are many versions of scientific realism\(^6\), can a viable version of scientific realism be found which is not susceptible to the onslaught of anti-realism?

Any sound theory of science should be able to suggest; i) a (number of) basic, essential aim(s) for scientific enterprise; ii) a set of methodological rules which are conducive, in a reasonably effective way, to the defined aim(s) and which are governing the choice of the best theory from among a number of rivals; and iii) a demonstration to show that the proposed aim(s) and methodological rules are better than the alternative candidates. We combine these three criteria under the slogan, "making better sense of science". The present essay is in effect a search for a sound version of scientific realism which can withstand the criticisms of anti-realists and satisfy the above criteria, that is to say, which can make better sense of science.

Admittedly, the existence of varied and diverse views among realists and their opponents and the fact that they can hardly agree on a putative definition for the doctrine which is called scientific realism, makes the task of finding a viable version of this doctrine even more difficult. This task requires, among other things, an appraisal of various models suggested by different realist writers, and, a critical assessment of the objections raised by their anti-realist opponents.

To make the ultimate task of the present essay more manageable, it seems appropriate to sort different realist theories of science by focusing on the three main components which can be discerned in various models of scientific realism, namely, ontological, epistemological, and semantic components. In each case, as we shall see, the
positions advocated by various writers form a spectrum starting from strong realistic assertions and ending with anti-realist views concerning the component or dimension in question.

The first component (dimension) of scientific realism, namely, the ontological component deals with the existence of the theoretical (unobservable) entities in the world. The following spectrum of views can be found in the literature of scientific realism:

1. Classical Essentialism: "The best, the truly scientific theories, describe the 'essences' or the 'essential natures' of things – the realities which lie behind the appearances. Such theories are neither in need nor susceptible of further explanation: 'they are ultimate explanations, ...'."\(^{17}\)

2. Conjectural Essentialism: "It is legitimate to interpret (appropriate) physical theories as attributing necessitating properties to postulated physical entities – properties in virtue of which the entities must, of necessity, obey the laws of the theory."\(^{18}\)

3. Hard-line Physicalism: Theoretical entities are in a sense more real than the ordinary, commonsensical objects. The so-called secondary qualities have no real existence.\(^{19}\)

4. Anti-essentialist conjecturalism: Scientific theories are tentative conjectures about the unobservable furniture of the world.\(^{20}\)

5. Entity-realism: Although the scientific theories may be false, nevertheless, "a good many theoretical entities really do exist."\(^{21}\)

6. Structural realism: There are certain structural or formal invariance in our perceptual field, and there exists an isomorphism between these structural invariants of perception and structural invariants of an independently-existing world. The most we can know of this reality is its structural properties.\(^{22}\)

7. Enlightened instrumentalism: Theoretical entities may well exist and we can certainly
talk *meaningfully* about them. However, we need not bother about them since we will not be able to obtain *knowledge* about them.

8. **Kantian Noumenalism:** Beyond the realm of phenomena, there lies a realm of noumena. However, we are not able even to talk *meaningfully* about them, let alone obtain knowledge of them.

8. **Classical instrumentalism and positivism:** Theoretical posits are instruments or fictitious constructs useful for easing our calculations or helping our memory.

The second, epistemological, dimension of scientific realism deals with the nature of the knowledge provided to us by our scientific theories. The following positions can be discerned among various writers:

1. **Strong rationalism:** Certain and indubitable knowledge about the reality is achievable. The basic metaphysical principle proved by reason together with empirical investigations, suffice to produce infallible scientific knowledge.

2. **Dogmatic Inductivism:** Absolute knowledge about the forms and essences of nature can be obtained by means of inductive investigation of empirical data.

3. **Fallible or probabilistic inductivism:** Inductive verification of laws and theories remains fallible or probabilistic, open to revision.

4. **Conjecturalism:** All our scientific knowledge forever remain conjectural. Science proceeds by proposing scientific conjectures and empirical attempts to refute them. Metaphysical speculations do not form part of this knowledge.

5. **Constructivism:** Objects of scientific knowledge are artificial constructs of the scientific community.

6. **Phenomenalism:** Scientific knowledge is confined to the knowledge of phenomena.

The last, semantic dimension, deals with issues such as meaning, truth and
The following positions can be found among various writers:

1. **Strong convergentism**: Scientific theories (at least in the ‘mature’ science) are typically approximately true, and more recent theories are closer to the truth than older theories in the same domain. The observational and theoretical terms within the theories of a mature science genuinely refer (roughly, there are substances in the world that correspond to the ontologies presumed by our best theories).

2. **Modest conjecturalism**: It is both legitimate and desirable for scientific theories to aim at achieving truth about the physical reality. Well corroborated scientific conjectures tell us roughly what the world is like and what sort of entities constitute it. In this sense these theoretical propositions have truth-value.

3. **Relativism**: Successive theories in any scientific field would be semantically incommensurable due to a radical change in the meaning and references of their terms.

4. **Enlightened Instrumentalism**: Theoretical statements (propositions) are meaningful. They also have truth value. However, it is not known to what sort of reality (entities) they correspond. For this reason, these theoretical statements do not constitute scientific knowledge. Scientific knowledge is only embodied in empirically adequate statements.

5. **Classic Instrumentalism and semantic anti-realism**: Evidence-transcendent statements have either no truth-value or are meaningless.

The above three lists, which contain various stance concerning the three components or dimensions of scientific realism, though by no means exhaustive, put us in a better position to consider some of the difficulties which accompany the discussions of scientific realism and thus prepare the ground for a more constructive analysis of the main issues involved between realists and their opponents. Such an analysis, it is hoped, would help us to appreciate the features of a realist theory of science, to be introduced.
in the last chapter, which is devised to produce a better framework for resolving the
difficulties which have afflicted the present day realist theories.

It is worth-mentioning that almost all of the modern discussions on scientific
realism take place within a tacit or explicit framework which may be called *standard
empiricism*. The following quotation due to Popper [1963] is a clear statement of this
framework:

*The principle of empiricism... asserts that in science, only observation and experiment may
decide upon the acceptance or rejection of scientific statements, including laws and theories...*

*The principle of empiricism can be fully preserved, since the fate of a theory, its acceptance
or rejection, is decided by observation and experiment — by result of tests. So long as a theory stands
up to the severest tests we can design, it is accepted; if it does not, it is rejected.*

We shall conduct our inquiry for a sound version of scientific realism within this
same framework and assess the strength and weakness which it imparts to the doctrines
which remain loyal to its main stricture. We shall investigate whether scientific realism
can be satisfactorily defended within the framework of standard empiricism. In the last
chapter however, we will consider the possibility of relaxing the rather stringent
requirement of empiricism and replacing this framework with a less restrictive one.

Even a cursory glance at the literature on scientific realism shows that there is no
consensus among realists (or anti-realists for that matter) on the significance of each of
the above three components and their relative priorities. Anti-realists, by and large, and
surprisingly some realists in their wake, *transpose* ontological issues to epistemological
and semantic ones; they claim that statements about being can be analyzed in terms of our
knowledge of being. This typical anti-realist move has been responsible for much
confusion in the literature of scientific realism. In order to avoid this confusion it is
advisable to keep the three constitutive components of realism apart and to bear in mind
that *scientific realism is based on realism*. Realism is the view that "reality exists
independently of our conceptions of it, though it may coincide with them."41 It is the claim to the possibility of acquiring knowledge about this reality by scientific means which, as stated at the outset, constitutes the main issue between the scientific realists and their opponents.

One of the other issues that has caused confusion in the literature on scientific realism is the status of a theory of truth within any doctrine of scientific realism. While anti-realists typically favour theories of truth other than the correspondence theory42, scientific realists are not of the same mind on this issue. Some scientific realists (e.g. Popper) would regard correspondence theory as a natural part of scientific realism43. Others, (e.g. Brian Ellis), would urge for a pragmatic theory of truth.44 Still others (e.g. Michael Devitt) are of the view that scientific realism and correspondence theory of truth are two separate doctrines and as such must be kept apart45.

To facilitate the ultimate objective of our investigation, namely, to find a sound version of scientific realism, during the course of this essay, while assessing the soundness and validity of the anti-realists’ and dissident realists’ arguments against their specific targets, we shall also assess the viability of a minimal version of realism. This version which embodies the common denominator of the more conventional versions of scientific realism, will be used as a test case against the criticisms made by the opposition. This exploration will help us to find out which aspects of the more conventional versions of scientific realism need to be modified or even abandoned in the light of the objections raised by the opponents.

The minimum version of scientific realism which will be used as a test case against objectors’ arguments embraces many of Popper’s prescriptions for a realist theory of science though, it is not quite identical to his views.46 The minimum version can be
defined in terms of the following theses:

**M1- A thesis concerning the aim of science**

The scientific enterprise will be benefited and not hindered if scientists aim at obtaining knowledge of not only the observable but also the unobservable aspects of physical reality. Such an aim is not only legitimate and desirable (because of the paramount value of knowledge and understanding); there are also good grounds for believing that it is (at least partially) realizable.

**M2- A thesis concerning the general method of scientific investigation**

In achieving the goal stated above in ‘M1’, scientists are supposed (and also best advised) to put forward conjectures concerning the natures (essences) of the most fundamental and invariant parts of the physical reality which give rise to diverse phenomena. These conjectures (assuming a reasonable degree of empirical success for them) will tell us roughly what the world is like and what sort of fundamental entities constitute it (with approximately what sort of essential properties).

**M3- A thesis concerning the appropriate way of interpreting theories realistically**

To be a realist does not require one to take the theoretical posits as defined by our conjectures to be a perfect mirror image of reality. All that scientists need to do is to regard their posits as approximately representing the real entities in nature. This rather imprecise way of defining fundamental posits allows scientists to talk meaningfully of the real entities with a useful degree of flexibility. The imprecise picture can be improved in successive steps by means of more informed conjectures about the nature of reality.

**M4- A thesis concerning approximating the truth about physical reality by means of scientific enterprise**

Looking at the history of physical sciences, one cannot help noticing that some
conjectures regarding the nature of the most fundamental invariant constituents of physical reality, roughly form a series in which each successive term or conjecture is; more comprehensive in that it covers a wider range of phenomena; more empirically successful; and more unified in that it explains more phenomena by assuming less fundamental entities with less essential and invariant properties than its predecessor. In this way the scientific enterprise can be regarded as a (possibly never ending) march towards a unified theory of the most basic constituents of matter.

Having stated the main tenets of the minimal version of scientific realism, we can now embark on our historical excursion to see how, during the past few decades, realists and their opponents have argued for their cases.
NOTES (Chapter One)

1. This way of formulating the main issue between scientific realists and their opponents shows that the nature of dispute between these two groups is different from what was at stake between classic realists and idealists. During the Middle Ages, as N. Rescher [1987] has pointed out, the dispute between realists and their opponents (namely, idealists and nominalists) concerned the independent existence of abstract objects or universals. Nominalists were of the view that abstracta only exist in and through the objects that exhibit them. Idealists maintained that universals exist through their being conceived in the minds of people who are naturally disposed to group various items that roughly answer to these conceptions. Realists, however, held that universals exist independent of things mental in a non-spatial, non-temporal, non-material realm. This classic dispute is nowadays being pursued in the realms of philosophy of maths and philosophy of logic. In this essay, where there is no risk of confusion, we shall drop the prefix scientific.

2. If as Popper once said, "All science is cosmology" ([1963/1972], p.136), then it would be rather easy to give an account as to how and why the (scientific) realist — anti-realist dispute has arisen in the arena of the intellectual development of mankind. Cosmologists (philosophers and scientists alike) in their pursuit to understand the world in which they live, have come up with ideas and theories which transcend the boundaries of sense perception and appeal to unobservable entities and processes. Among these conjectures, those which have been successful in predicting the observable course of events and providing reasonable explanations for the disparate and diverse phenomena by subsuming them under unificatory schemes, have naturally given rise to the question of whether these conjectures are truthful representations of unobserved reality or whether they are purely fictional accounts only useful for computational or pragmatic purposes.

3. A brief history of the more recent debates between the proponents of the two camps is provided in the second chapter of this essay.

There are many studies concerning the realist, non-realist episodes in the history of science. For an anti-realist reading of the history of science see, L. Laudan [1981a]. G. Holton, on the other hand, has written about various periods in the history of science from a realist point of view. See for example his [1973].

Apart from historical case studies, there are a number of rather concise accounts of the history of the dispute between scientific realists and their opponents. See for instance, E. Mackinnon [1972b/1974], J. Heil [1989], G. Dilworth [1990].

4. As one significant example of such a convergence, one can refer to the gradual understanding of the profound epistemological point that all our knowledge, far from being certain and indubitable, is conjectural in nature and revisable.

The ideal of achieving secure foundations for knowledge was being pursued by philosophers like Descartes who maintained that the sceptic challenge can be met, provided one can produce clear and distinct ideas about reality. Anti-realists, on their part, while sceptic about achieving this goal at the level of theoretical knowledge, would hardly challenge it as far as the knowledge about observable phenomena was concerned. Thus for example, logical positivists were trying to put the whole edifice of knowledge on the secure foundation of observation-statements.

It was to the credit of Popper who cleared the way and showed that such an ideal is unattainable because all of our knowledge will remain forever conjectural. (See Popper [1959/68], [1963/72], [1972/79])

The development of the philosophical discussions towards more objective theory of knowledge which is epitomised in the shift from the notions such as ideas (Descartes, Locke, Hume) to meanings (Wittgenstein, Logical positivists) to sentence (or more accurately propositions) (Quine, Dummett, Popper) is another evidence of a general convergence in the views of advocates of the realist and anti-realist
doctrines. For an account of this development see I.Hacking [1975/1981].

5. Galileo Galilei [1953].

6. There are many accounts of the instrumentalist — realist dispute over the case of *The Two Great Systems*. P.Duhem’s [1969] and T.Kuhn’s [1957] are among the best.

7. The central issue between realists and their opponents can be expressed in various ways and can be enriched by adding some other related questions. For example, it can be asked whether (scientific) knowledge can be achieved solely on the basis of the available empirical evidence, or should one assume some (if any) basic conditions (e.g. metaphysical assumptions about the world) for obtaining such (conjunctural) knowledge? Can different (successive) theoretical accounts of the same phenomena be somehow reconciled or will they forever remain incommensurable? If one has to choose between the seemingly conflicting accounts of one phenomenon, then, is there any way to decide between them? What are the criteria (if any) for such a preference? Since the whole of physical reality is not out of the reach of individuals, then can one hope for a piecemeal and gradual improvement and growth in one’s understanding of it? What must science be like to give us knowledge about physical reality? Is there a rational (albeit fallible) method to enable the scientists to discover more and more about physical reality? Should one try to understand (in a conjectural fashion) the very nature of physical reality, or should one subscribe to the thesis which asserts that the world of noumena — whatever it is — exhibits, under specified conditions, some observable properties and one is (epistemically) better placed if one limits oneself to these observables (i.e. the realm of phenomena) and does not insist on rather useless questions concerning the very nature of physical reality? There is also a third rival thesis which asserts that the evidence is such that it is as if there are certain unobservable entities responsible for what is happening at the observable level, and while one is using the concepts of those unobservable entities as useful fictions, one needs not attach any undue ontological status to these claims? These questions, and other related ones, will be discussed during the course of the present essay.

8. See: P.Clark [1976], M.Gardner [[1979]. A brief account of this view is discussed in the next chapter.

9. Whittaker’s [1951, 1953 /1981] seems to be the definitive history of ether. A concise account of this history in the nineteenth-century can be found in P.M.Harman [1982].

10. Maxwell’s confident belief in the existence of the optical ether, despite the fact that this entity had no bearing on his field equations is a good case in point. It seems Maxwell’s adherence to the notion of ether was a result of his belief in the principle that undulation cannot occur unless there is something to undulate. Apparently, this argument was so convincing that even Hertz who had differentiated between the formalism of Maxwell’s equations and their mechanical models, could not free himself from its straitjacket and seek a better interpretation of it. In 1890, despite the fact that he had succeeded in presenting Maxwell’s equations in an axiomatic form, and had declared that "Maxwell’s theory is Maxwell’s system of equations", he did not renounce the mechanical view of nature, and remained committed to the belief that the electromagnetic waves were produced by an ether whose parts were connected by mechanical structure. (See, Harman [1982]).

11. The term naïve scientific realism is different from what is commonly known in the literature as naïve realism. While the former, as discussed in the text, takes the findings of science for granted, the latter endorses, rather uncritically, the findings of common-sense. In this respect it can be called naïve commonsensical realism. This view, according to some writers, is incompatible with scientific realism. The following quotations from Russell and Ryle are self-explanatory.

"We all start from 'naïve realism ', i.e. the doctrine that things are what they seem. We think that grass is green, that stones are hard, and that snow is cold. But physics assures us that the greenness, the hardness, and the coldness are not the greenness, the hardness, and coldness that we know in our own experience, but something very different. The observer, who seems to himself to be observing a stone, is really, if physics is to be believed, observing the effects of the stone upon himself. Thus science seems to be at war with itself: when it most means to be objective, it finds itself plunged into subjectivity against its will. Naïve realism leads to physics, and physics, if true, shows that naïve realism is false. Therefore naïve realism
realism, if true, is false; therefore it is false." (Russell [1940/1962], p.13, emphasis added).

"When we are in a certain intellectual mood, we seem to find clashes between the things that scientists tell us about our furniture, cloths and limbs and the things that we tell about them. We are apt to express these felt rivalries by saying that the world whose parts and members are described by scientists is different from the world whose parts and members we describe ourselves, and yet, since there can be only one world, one of these seeming worlds must be a dummy-world. Moreover, as no one nowadays is hardly enough to say 'Bo' to science, it must be the world that we ourselves describe which is the dummy-world." (Ryle [1966], p.68).

A. Eddington, also in his famous two tables story, has discussed the same predicament for naïve realists. See Eddington [1928], introduction.

Naïve realism may be defended by arguing that it is the starting point of all more mature and more comprehensive understanding of nature. In fact science can be viewed as the continuation of the process of understating reality by correcting and elaborating the findings of common-sense. A.O’Hear [1985, p.38] has referred to a useful distinction originally made by Michael Dummett in his [1979, pp.33-35] between absolute and relative forms of description. Such a distinction makes room for the view that creatures with quite different sensibilities might be able to agree on the characteristics attributed to things by the absolute form of description even if their relative form of description radically differ from each other. A direct consequence of this distinction is that the finding of common-sense and science, say in the case of Eddington’s two tables, can happily be reconciled as being two different descriptions about one and the same reality. While the common-sense report is the description provided by our unaided senses, the scientific report, provides us with the description we would observe had we were equipped with much more powerful sense organ capable of observing the microphysical universe.

Dummett’s distinction is in fact an expansion of the point made by S. Stebbing [1937] in her vigorous critique of Eddington. Stebbing had pointed out that Eddington’s main mistake had been his failure to observe that the language of theoretical physics and the language of common sense are not equivalent. According to Stebbing, Eddington was not at all confronted with two tables. For the word "table" refers to an experimental idea which does not occur in electron theory, and the word "electron" signifies a theoretical notion that is not defined in the language of common sense. It follows that since there is thus one table, there is no problem of finding out which one is the real one. (For further defence of common-sense realism see, N. Maxwell [1966]), Popper [1972].

As for naïve scientific realism M. Hesse [1974] has isolated two of its basic assumptions particularly in the context of seventeenth-century science. The first is the assumption that true (i.e. indubitable) theories could be attained in practice, and the second that “the hidden entities and processes of nature that are to be discovered by science are of the same kinds as the observable entities and processes, and hence describable in the same descriptive vocabulary and satisfying the same laws”. (pp.285-6) These assumptions, or variations of them, can be found, time and again, in the history of science. Contrary to naïve realism, naïve scientific realism, cannot and should not be defended. As is mentioned in the text, far from being a positive factor in the advancement of science, it could create dogmatic attitudes among scientists and either blind them to what R. Trigg ([1980], p.174) has aptly called "the strangeness of reality" or disseminate the spirit of scientism amongst them. (For a discussion of the hazards of scientism see T. Sorell [1991], R. Trigg [1993]).

12. Francis Bacon can be regarded as a model naïve scientific realist. He maintained that science can achieve certain and indubitable knowledge through constructing a pyramid of propositions which has the most general principles at its apex. He sought to discover these general laws of nature by means of empirical search among phenomena and collecting, in an inductive fashion, as many facts about them as possible. For Bacon the ultimate aim of science was prediction of the phenomena and control of nature. He also maintained that the methods of science (i.e., unbiased observation, impartial collection of facts, and drawing general conclusions from these data) confer objectivity on scientific results.

Bacon’s views were warmly received by the members of the Royal Society who turn it into the official doctrine of the Society and tried to conduct their researches according to its teachings. French Encyclopedists who paved the way for the Enlightenment were also deeply influenced by Bacon. Since then, many generations of scientists as well as philosophers and historian of science, have accepted Bacon’s inductivism as the best approach to science and scientific activities.

13. Thus for example, Kant attacked the naïve scientific realism embraced in the era of Newtonian science. In his *Prolegomena* [1966] (§36 — §38) Kant criticises the inductivist (Baconian) view that scientific knowledge can be gained by inductive collection of facts. Upholding his dictum that "the understanding does not draw its laws (a priori) from nature, but prescribes them to nature" (p.82) he gives examples of Kepler’s and Newton’s physical astronomy and states that the concepts of circles and conic sections, of force varying with the inverse square of distance, and of relation between such a force and conic sections, are conceived a priori and cannot possibly emerge from the phenomena. (See J.Agassi [1963])

14. Cardinal Bellarmine and the Roman Catholic Church, for instance, were arguing that Galileo would be on the safer grounds if he adhered to the phenomenological interpretation of his theory.

15. cf. Popper [1963].

16. Scientific realism is a *generic* term. It means different things to different writers. In his anthology, J.Leplin [1984] has noted that, 'Like the Equal Rights Movements, scientific realism is a majority position whose advocates are so divided as to appear a minority.' (p.1) This valid observation can easily be confirmed by a quick look at the vast literature on this subject. Scientific realists do not agree on the main theses which constitute their position (Leplin has listed ten different doctrines which constitute the main tenets of different scientific realist views). Moreover, they also oppose each other on the very status of scientific realism itself. Hence, some regard it as a metaphysical thesis (see N.Capaldi [1974], M.Bradie [1972, p.368]), while others hold that it is an empirical (testable) doctrine (see G.Maxwell [1970, p.11], J.Agassi [1975, p.116], Putnum [1978], Boyd [1973] among others). Some scientific realists maintain that this thesis is basically an epistemic view (see D.Papineau [1984], A. O’Hear [1989]), some others take it as a semantic one (e.g. C.A.Hooker [1974], Dummett [1978]), and others regard it as primarily an ontological doctrine (see M.Devitt [1984], [1991]). Still others have introduced other distinctions. For example, I.Hacking [1983, pp.27-8] has distinguished between two types of scientific realism which he dubs as *truth-realism* and *entity-realism*. These differences, no doubt, have contributed to the inherent difficulties of the issues involved in discussions concerning scientific realism.

17. Popper [1963/72], pp.103-4. Popper has ascribed this view to Aristotle and Newton among others.

18. N.Maxwell [1993a], p.90.

19. J.J.Smart is a modern representative of such a school. He writes: "I wish to urge that the physicist’s language gives us a *truer* picture of the world than does the language of ordinary common sense. ... Up to a point, Susan Stebbing’s strictures against Eddington were perfectly justified. But Stebbing went too far. There is also a perfectly good sense in which it is true and illuminating to say that the table is *not* solid. The atoms which compose the table are like the solar system in being mostly empty space. (This was Eddington’s point.) So though most common sense propositions in ordinary life are true, I still wish to say that science gives us a ‘truer picture’ of the world." (Smart [1963], p. 47) "But when all is said and done, all secondary quality concepts concern the classifications of sensory stimuli made by complex neurophysiological mechanism. There is no reason to expect a close correspondence between these classifications and the way things in fact are in nature." (*ibid.* p.86)

20. See Popper [1963], pp.114 -119. Popper’s views are briefly discussed in the next chapter. See also note 49 below.

21. See I.Hacking [1983], p.27. We shall discuss the views of Hacking and other entity-realists in Chapter Five below.

22. This position has been formulated by Russell [1948] and developed by G.Maxwell [1970a], [1970b].

23. Van Fraassen is an articulate representative of this position. We shall discuss his views in Chapter Three.

24. I.Kant [1933], p. 267 *passim.*
25. P. Duhem and E. Mach are two better known exponents of these positions. We shall briefly discuss their views in the next chapter.

26. The best representatives of this view have been the Rationalists like Descartes and Leibniz.

27. Bacon can be regarded as the champion of this view.

28. Logical positivists (see next chapter) can be regarded as major exponents of this view.

29. This is the view advocated by Popper [1959/68], [1963/72].

30. Modern sociologists of knowledge are among the better known representatives of this view. We have briefly discussed the position of this group in Chapters Two and Five.

31. Phenomenalism covers a wide range of doctrines including instrumentalism and positivism. We shall discuss some variants of this view in the subsequent chapters.

32. This view is advocated by Boyd [1973], and Putnum [1978] among others. We shall further discuss it in the Fourth Chapter.

33. Popper's view can be regarded as a ramification of this version.

34. Feyerabend [1975] has produced a strong version of this theory. Feyerabend's views will be briefly discussed in the next chapter.

35. See van Fraassen [1980].

36. Classic instrumentalism can be found in Duhem [1969]. Duhem's views will be introduced in the following chapters. Semantic anti-realists maintain that the dispute between realists and anti-realists is not primarily over the ontological matters but over the semantic matters, namely truth / falsehood of certain class of statements. Hence Dummett [1978] for example, has claimed that the realism / anti-realism dispute: "concerns the notion of truth appropriate for statements of the disputed class [i.e. evidence-transcendent statement]; and this means that it is a dispute concerning the kind of meaning which these statements have." ([1978, p.146]) This view will be briefly discussed in Chapter Five.

37. The name, though not the idea, is due to N. Maxwell (See for example his [1984], p.21). Maxwell has distinguished between two types of standard empiricism, namely, the naked and the dressed. The first type refers to those writers who, openly and unhesitatingly, embrace this doctrine and remain loyal to its basic tenet. The second type, refers to those who apparently endorse this doctrine and nonetheless go beyond the limits set by this doctrine. We shall discuss this latter case in the last chapter.

   Empiricism, or as Maxwell has called it standard empiricism, is an age-old philosophical doctrine. In addition to the quotation in the text, the following two quotations due to Hume and Mach respectively, also capture the essence of empiricists' doctrine:

   "When we run over our libraries persuaded of these principles, what havoc must we make? If we take in our hand any volume: of divinity or school metaphysics, for instance: let us ask, does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion". (D. Hume [1777/1979])

   "The ultimate un-intelligibilities on which science is founded must be facts, or, if they are hypotheses, must be capable of becoming facts. If the hypotheses are so chosen that their subject can never appeal to the senses and therefore also can never be tested ... [then] the investigator has more than science, whose aim is facts ..." (Mach [1911], p.57)

   There are many versions of this basic tenet of empiricism due to different thinkers. In its earlier stages, empiricism, in the shape of nominalism, was opposing medieval realism. Writers like William of Ockham were arguing that contrary to realists, universals do not refer to anything real; they are simply words or names assigned to a number of similar things. These words or names are composed of letters and expressed
as vocal emissions and are therefore only air. Later on, during the seventeenth and eighteenth centuries, this doctrine in the hands of philosophers like Locke, Berkeley and Hume was turned into a theory to oppose and undermine the rationalism of Descartes, Spinoza and Leibniz who were of the view that all accurate, true information about the world is logically deducible from first principles. Still later, in the eighteenth and nineteenth centuries, thinkers like August Comte and Ernst Mach upheld empiricism by advocating a positivist view of science against realist interpretations.

38. Popper [1963/72], p.54. italics in original, emphasises added.

39. The following quotations are self-explanatory:

"Scientific realism [asserts that] token of most current unobservable scientific physical types objectively exist independently of the mental" (M.Devitt [1984], p.22, italics in original)

"I shall apply the term scientific realism to non-scepticism about semantico-ontological objectivity for scientific theories, to the view that scientific theories are answerable to an independent reality ... there is a question for them to be more or less right (or wrong) about." (Papineau [1979], p.126, quoted in Devitt [1984])

"Formally, scientific realism is a semantical thesis, it is the view that the intended and proper sense of the theories of science is as literal descriptions of the physical as saying what there is and how it behaves." (Hooker, [1974], p.409, quoted in Devitt [1984])

40. Transposing ontological issues to epistemological ones is a common (one can even say an essential) thread in all anti-realists philosophies. This move, as R.Trigg [1980] has pointed out, invariably leads to an epistemic view which states that the statements about being can be reduced to or analyzed in terms of our knowledge of being. One of the direct results of this move is to put prohibition on any unobservable entities; the ultimate furniture of the world are either events, sense data, or are forever out of reach of human beings (noumena).

41. R. Trigg, op.cit., p.3.

42. Theories of truth will be discussed in Chapter Five.

43. Popper [1963/72], [1972/79].

44. Ellis [1991]. We shall discuss Ellis's view in Chapter Five.

45. Devitt ([1991], p.49) has given the following formulations of each of these doctrines:

"Strong Scientific Realism: Tokens of most unobservable scientific types objectively exist independently of the mental and (approximately) obey the laws of science;

Correspondence Truth: Sentences have correspondence truth conditions.

Strong Scientific Realism is a metaphysical doctrine about the underlying nature of world in general. To accept this doctrine we have to be confident that science is discovering things about the unobservable world. Does the success of science show that we can be confident about this? Is inference to the best explanation appropriate here? Should we take sceptical worries seriously? These are just the sort of epistemological questions that have been, and still largely are, at the centre of the realism debate...

Correspondence Truth is a semantic doctrine about the pretensions of one small part of the world to represent the rest... Do we need to ascribe truth conditions to sentences and thought to account for their rôles in the explanation of behaviour and reality? Do we need reference to explain truth conditions? Should we prefer a conceptual-rôle semantics? Or should we, perhaps, near enough eliminate meaning altogether? These are interesting questions but they have no immediate bearing on scientific realism."

46. As we shall see, in some important respects, for example, the case of causal explanation, minimal realists' thesis is different from Popper's. In these respects, the minimal view is closer to the views of R.Bhaskar [1979].
47. It is traditionally accepted, by both realists and anti-realists, that the realist goal of science is achieving theoretical truth. For example, Popper [1963/72, p.229] writes: “Thus we accept the idea that the task of science is the search for truth, that is, for true theories (even though as Xenophanes pointed out we may never get them, or know them as true if we get them).

An anti-realist like van Fraassen has defined scientific realism in the following terms: "Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true". ([1980, p.8])

"Scientific realism... says that a theory is a sort of thing which is either true or false; and that the criterion of success is truth. As corollaries we have that acceptance of a theory as successful is, or involves, the belief that it is true; and that the aim of science is to give us (literally) true theories about what the world is like." ([1989, p. 191], italics added).

M.Hesse, who has moved from realism to relativism has this to say on the realist aim of science: "... it is clear that science does accept some constraints on theories ... Traditionally, the constraints have been ... formulated ... by Francis Bacon in terms of the attainment of 'light' and of 'fruit'. In other words, the true and hidden structure of the natural world is to be revealed in scientific theory, and in virtue of this discovery science is to exploit nature for the benefit of man. In recent philosophy of science these two aims have been discussed in terms of the realist and instrumentalist aspects of science ..." (op.cit. p.283)

In recent years a number of realist philosophers under pressure from the anti-realist camp have abandoned the notion of truth as a legitimate aim for science and have introduced other aims. (See Chapter Five)

48. From the viewpoint of the minimal realism, yesterday’s frontier of science is today’s rather naïve realism and whilst science is an attempt to explain the existing picture of the world, this attempt is not based on the adoption of this picture; rather it leads to changes of this picture. (See Agassi, [1975], p.119)

49. Note that our conjectures concerning the nature of reality do not commit us to the sort of essentialism which Popper denounces as an impediment to scientific progress. The conjectural essentialism introduced in the text is acceptable even to Popper. For example, in his "Reply to my Critics" [1974, p.1115] Popper explicitly discusses the possibility of calling his own philosophy a ‘a modified essentialism’.

What Popper [1962 and 1963] and many other philosophers of science have called essentialism and have tried to criticize and reject, can be better and less misleadingly called linguistic essentialism, that is to say, a doctrine which states that there is something which a word really means. According to this doctrine our definitions of things really and truly capture their essences. This doctrine should be distinguished from the conjectural essentialism (Popper’s modified essentialism) which simply states that each kind of being has a constituent structure causally responsible for its properties.

However, the point mentioned in the text differs in a rather significant way from Popper’s view of science. M2 is a version of causal model of explanation (as discussed by a number of writers for example, R.Bhaskar [1975/78]) which is different from the traditional Hypothetico-Deductive model embraced by many philosophers including Popper.

50. This step would serve to refute the charge of naïveté on the part of the minimal realism discussed here. The minimal position defended here is a critical one. That is to say, it does not subscribe to Cartesian foundationalism, namely, the doctrine that holds that firm indubitable foundations can be found for our scientific knowledge. In fact, minimal realists maintain that scientific knowledge, at each stage of its development, is naïve to certain degree and needs to be further completed.

51. The issue of the progress of science is at the heart of all realistic accounts of science. In fact apart from the relativists and those who are influenced by Hegelian views, other anti-realists (for example, logical positivists) do believe in the idea of progress. The view stated above captures the main ingredient of the concept of progress in a non-technical way.

The above account of progress should not of course be taken as the naïve view that whatever development occurs in science amounts to a move forward. On the contrary, all sensible realists share the view that progress (in the sense defined above) is an anachronistic process; at any given time, many new conjectures may be introduced which may not amount to progress and thus cannot be regarded as part of the series defined in the text. Moreover, realists emphasise that in the case of the successive unificatory theories, progress via unification does not mean that the superseding theory has been able to completely
absorb or accommodate the superseded theory. As Popper [1972/79] has made this point clear while discussing one of the classic example of progress via unification in science, namely, the cases of Kepler's laws of planetary motion, Galileo's laws of inertia and free fall and Newton's theory of gravitation, "Newton's theory unifies Galileo's and Kepler's. But far from being a mere conjunction of these two theories — which play the part of explicanda for Newton's — it corrects them while explaining them. ... Far from repeating its explicandum, the new theory contradicts it and corrects it." (p.202)

The notion of progress via unification (in the above sense) can also be found in less fundamental theories of science. A good case in point is the unification of Boyle's law of 1661 (pV = R given constant temperature), by Bernoulli's Ideal Gas law of 1738 (pV = RT), and the unification of the latter by van der Waals's law of 1873 for the behaviour of gases under high pressure (p + a/V^2)(V - b) = RT, where for a=b=0 it reduces to the Ideal Gas law. (For details see C.Dilworth [1981], ch.10) The idea of progress is further discussed in Chs. 3 & 4 below.

52. The quest for a grand unified theory of the most basic constituents of matter is indeed an old pursuit which goes back to the beginning of the intellectual history of mankind. Evidence of man's desire for achieving this goal can be found in many of the established civilizations and cultures. For a rather light account of this endeavour in the Western tradition see J.Agassi [1968]. M.Hesse [1961] is a more thorough study of this idea. W.Berckson [1974] has concentrated on the thesis of unified theory since Faraday.

The idea of a unified theory which had its place in the heart of Einstein, has recently attracted more and more physicists. For some recent contributions see Abdus Salam [1990], J.D.Barrow [1991], S.Weinberg [1993]. Anti-realists, as noticed in the text, are, for various reasons, against this idea. For a strong criticism of the idea of a unique description of the world see H.Putnum [1981].

The unified grand theory of the fundamental constituents of matter is customarily named as the "theory of everything", see for example, J.Barrow [1991]. However, as M.Gell-Mann [1994] has pointed out it is a misnomer. Discussing the possible candidacy of the 'heterotic superstring theory' for the honorific position of the unified grand theory he asks that: "Assuming that it is correct, is it really the theory of everything? Some people have used the expression, and even the abbreviation TOE, in describing it. However, that is a misleading characterization unless 'everything' is taken to mean only the description of the elementary particles and their interactions. The theory cannot, by itself, tell us all that is knowable about the universe and the matter it contains. Other kinds of information are needed as well." (p.129)
Mach’s Programme

The rapid developments in physics at the end of the nineteenth century which heralded the advent of modern physics in the twentieth century, were accompanied by a great deal of philosophical controversy over the aim and status of scientific theories and their ability to provide scientists with knowledge of the world\(^1\). One of the more influential figures in these discussions was Ernst Mach, who played a major rôle in the rise of the anti-realist philosophy of science in the late nineteenth century and subsequent decades.

Mach’s programme for science is a typical positivist\(^2\) approach with all its classic ingredients\(^3\). In his view the aim of science is not explaining phenomena but "to replace or save experience, by the reduction and anticipation of facts in thought"\(^4\). And facts are those complexes of elements which come to us in sense perception\(^5\). In this phenomenalist approach\(^6\) there is no room for the notion of causality\(^7\). Scientific knowledge consists solely in "knowledge of connections among appearances"\(^8\). Scientific theories are particular ways of organizing and summarizing our representations of facts\(^9\). Therefore, theoretical science is not a body of knowledge. It is a technique for memorizing information, memoria technica\(^10\), whose purpose is to provide us with the most economical representation of facts\(^11\). Finally, like all thorough positivists, Mach’s main concern was to base science on a firm empirical ground and free it from all metaphysical ingredients\(^12\).

Mach’s influence upon his contemporaries and the younger generations of philosophically inclined scientists was mixed and varied. Some philosopher-scientists like Planck and Einstein, who had started their careers as followers of Mach, gradually
departed from his path and developed robust realist views of science\textsuperscript{13}. Some other scientists like Boltzmann, Poincaré and Duchem, each in their own way, tried to forge accounts of science which, in their view, would be more loyal to the true nature of this enterprise. Still others praised Mach's approach and tried to develop it\textsuperscript{14}. These attempts soon gave rise to a new version of \textit{standard empiricism} known as logical positivism advocated by members of the so-called Vienna Circle\textsuperscript{15}. In the meantime, the advent of quantum mechanics provided new grist for the mill of the anti-realists\textsuperscript{16}.

**Boltzmann and Planck: Scientists' Defence of Realism**

Against Mach's positivism and Ostwald's \textit{energeticism}\textsuperscript{17}, Ludwig Boltzmann, almost single-handedly\textsuperscript{18}, though somewhat hesitantly\textsuperscript{19}, was defending a (rather weak) version of realism and was arguing for the possibility of acquiring \textit{knowledge} of the theoretical entities, in this case atoms. The main arguments of the opponents of the atomists were as follows\textsuperscript{20}:

"(i) No atomic hypothesis was required to account for chemical combination, electrochemistry or the gas laws;

(ii) Science should keep as closely as possible to direct sense data;

(iii) Science should not introduce unnecessary (atomic) models of mechanisms not directly accessible to experiment, but should confine itself to the most economical universal description;

(iv) Thermodynamic processes are irreversible, whilst mechanics is reversible in time; thus the former, contrary to Boltzmann's claim, cannot be explained in terms of the latter (i.e. molecular mechanics)."\textsuperscript{21}

Boltzmann tried, in a rather cautious way, to show that Mach and Ostwald's claimed prudence, is only illusory, since even they themselves, in their thorough anti-
realist way, do and must transcend the available data and go beyond the limit allowed by the principle of empiricism\textsuperscript{22}. Moreover, Boltzmann observed that contrary to what the opponents of atomism would claim, their own programmes are as metaphysically loaded as the one they were rejecting\textsuperscript{23}.

Having challenged his rivals' claims to epistemic superiority for their approaches, Boltzmann turned to one of the realists' favourite arguments, namely, \textit{inference to the best explanation}\textsuperscript{24} to make room for the plausibility of atomism\textsuperscript{25}. However, despite the fact that he had taken a sound line of reasoning\textsuperscript{26}, due to the uncertain position of the kinetic theory\textsuperscript{27}, his counter-attacks were less than decisive and did not change the views of his opponents in an effective manner. Perhaps this factor has played some rôle in Boltzmann's taking a seemingly reconciliatory approach towards the anti-realist stance of his rivals\textsuperscript{28}.

The great influence exerted by Mach on the mind of philosophically inclined scientists can be seen from the cautious and gradual conversion of Max Planck to realism. Although he was against the approach of energeticists like Ostwald\textsuperscript{29}, nevertheless, he himself was in favour of Clausius' phenomenological method and maintained that the problems of thermodynamics can be solved "without the help of special assumptions about the molecular constitution of bodies."\textsuperscript{30}

This attitude made Planck take a rather hostile stance towards the kinetic theory of gases and Boltzmann's statistical explanation of entropy\textsuperscript{31}. Although, during his works on the so-called black-body radiation, Planck came more and more to appreciate and in fact that imitate Boltzmann's statistical method\textsuperscript{32}. Nevertheless, he did not renounce his own nonstatistical conception of irreversibility until around 1914\textsuperscript{33}. And although after the death of Boltzmann in 1096, Planck effectively occupied his place as Mach's leading opponent\textsuperscript{34}, nevertheless, as late as 1910 he was still saying he was leaving the atomic
Planck attacked Mach's views on two grounds: on the one hand, he tried to show that Mach's subjectivism robs science of any lasting value in providing scientists with the knowledge of reality, and on the other, he declared that the principle of economy which once both he and Boltzmann subscribed to, was barren of any significant result:

But even in its widest sense the principle of economy is not capable of leading the way for physical research, for the simple reason that one can never tell in advance from which point of view economy will appear most valuable and durable. So a physicist who wishes to advance his science must be a realist, not an economist - that is, he must first and foremost seek among varying phenomena whatever is lasting and eternal, and try to bring it to light.

The positivists' methodology, notwithstanding the victory of the kinetic programme after 1905 and Planck's criticisms, was not overthrown. Not only Mach retained his firm positivist conviction, but also his views continued to exert a great deal of direct and indirect influence on the minds of many younger thinkers. It gave rise to three rival approaches, namely a language-based version of positivism, a realism which was struggling for survival, and a methodology which wanted to get the best of two worlds, without total commitment to either of them. This latter methodology was best discussed in the works of two great French philosopher-scientists, i.e., Pierre Duhem and Henry Poincaré.

Duhem, Poincaré and the Middle Position

A major difficulty for the positivists' programme, in the opinion of those scientists who would consider it favourably, was that it would leave no room for human creativity and the free exercise of creative imagination. Poincaré shared with the empiricists (positivists) the belief that experiment is the final arbiter in the question of scientific truth, but with the rationalists he shared the belief in the revealing power of mathematical intuition, which can often anticipate empirical results. Poincaré's main
epistemological concern was the question of how objective knowledge and continuous progress were possible in spite of apparently disruptive changes in mathematics and in science⁴⁴.

To answer these presumably most important questions in the philosophy of science, Poincaré introduced a hybrid philosophy, partly conventionalist and partly invariantist⁴⁵. For Poincaré scientific knowledge consisted only in the relations between observables or those relations between unobservables which have observational effects. Consequently, progress of science consists in the accumulation of the observational consequences or the invariance and isomorphism of formal structure of theories expressed in differential equations. Poincaré argued that, in the process of theory change, it is metaphors and models (e.g., various models of the atom, or the wave and the particle picture of light, etc.) which changes. These metaphors, though very important for the development of science do not express knowledge, for all that we can know are observable relations⁴⁶.

This emphasis on the relational nature of knowledge, whether in mathematics or in physics, and on the unknowability of objects as such, i.e., apart from their relations to others, when combined with the thesis that there always are both observationally equivalent and experimentally indistinguishable theories in science (a thesis he shared with Duhem), gave rise to a rather mild instrumentalism⁴⁷.

Instrumentalism however, found its more explicit expression in the views of P.Duhem. Like Ostwald, Duhem was an advocate of energetics⁴⁸, and like Mach, though not a positivist himself⁴⁹, he was of the view that a physical theory is independent of metaphysics⁵⁰; it is also not an explanation⁵¹ but a system of mathematical propositions that represents empirical laws and observable phenomena without pretending to disclose their underlying causes⁵².
For Duhem the question of whether such symbolic representations are true or false was a misguided question. He maintained that: "A law of physics, is, properly speaking, neither true nor false but approximate." This was because, in his view, all physical laws are always underdetermined by empirical data. Nevertheless, Duhem came so close to a realist position as to suggest that mature physical theories approximate a natural classification. The main characteristic of such a system in Duhem's view was its fruitfulness, i.e. in so far as physical theory approaches the ideal of a natural classification then it will anticipate experimental laws, not yet observed, and promote their discovery. The closer we come to this ideal, the more we are convinced that the logical and mathematical classification is real.

**Logical Positivists**

Whereas Duhem's metaphysical commitments made him not discredit metaphysics altogether but only sever it from physics, the members of the Vienna Circle were explicitly against metaphysics. This opposition which perhaps started as a reaction against some of the extravaganza of German Idealism, was soon turned into a fully-fledged ideology, with the aim of purifying empirical sciences from the influence of metaphysics. R. Carnap's influential article 'The Overcoming of Metaphysics through Logical Analysis of Language' which is an attack on Heidegger's philosophy is a typical example of the sort of attitude taken by logical positivists & logical empiricists towards metaphysics:

Let us now take a look at some of metaphysical pseudo-statements of a kind where the violation of logical syntax is especially obvious, though they accord with historical-grammatical syntax. We select a few sentences from that metaphysical school which at present exerts the strongest influence in Germany:

"What is to be investigated is being only and — nothing else; being alone and further — nothing; solely being, and beyond being — nothing. What about this Nothing?... Does the Nothing exist only because the Not, i.e. the Negation, exist? Or is it the other way around? Does Negation and the Not exist only because the Nothing exist?...We assert: the Nothing is prior to the Not and the Negation...Where do we seek the Nothing? How do we find the Nothing... we know the Nothing... Anxiety reveals the Nothing...That for which and because of which we were anxious, was 'really' — nothing. Indeed: the Nothing itself — as such — was present...What about this Nothing — The Nothing
Logical positivists not only intended to purge all metaphysical elements from the realm of exact science, but also wanted to create a purely scientific philosophy which was supposed to replace the traditional (or in the rather derogatory jargon of the members of the circle speculative) philosophy. Though in complete agreement with Mach’s programme, they decided to place their brand of empiricism on a firmer footing.

This aim was to be achieved by means of a logico-linguistic turn. From Wittgenstein’s Tractatus Logico-Philosophicus, they adopted the thesis that all meaningful propositions are either analytic (tautologies or contradictions) or synthetic (empirical). Analytical propositions are devoid of factual information. Empirical propositions, on the other hand, are either reports of sense experiences (protocol sentences) or finite generalizations from such reports whose truth can be determined only by experimental verification. Metaphysical and theological propositions which would fit into neither category were regarded as meaningless pseudo-propositions. Similarly, because a robust doctrine of scientific realism could not even be formulated in sentences that accord with this school’s theory of meaningfulness, realism was dismissed as a pseudo-problem.

One of the off-shoots of logical positivism was a school known as Operationalism or Operationism founded by P.W.Bridgman. Operationalism was rather influential among physicists between 1940s and 1960s. Amongst the basic ideas advocated by operationists was the operational definition of physical quantities which was epitomised in the claim that the meaning of any physical concept is synonymous with a corresponding set of operations.

**Popper and Other Veteran Realists**

One of the earliest realist reactions to logical positivism came from Karl Popper.

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Popper wanted to defend science against the excesses of the logico-linguistic approach and to uphold realism. To these ends, whilst respecting a distinction very dear to logical positivists, (namely, the distinction between the context of justification and the context of discovery), Popper, like Poincaré before him, took the progress of knowledge as the main issue of the philosophy of science. He made it clear that the progress of knowledge is only possible through a continuous process of conjectures and refutations. He then introduced falsifiability as a multipurpose criterion, which among other things would serve as a measure for progress: more verisimilar theories are more falsifiable.

Until the late 1950s realism had very few proponents. One of the better known advocates of realism in this period was J.J.C. Smart who argued for a hard-line physicalist, reductionist version of realism. His main line of reasoning against phenomenolists can be viewed as an earlier form of an argument which known in today jargon as the miracle argument.

If the phenomenalist about theoretical entities is correct we must believe in a cosmic coincidence. That is, if this is so, statements about electrons, etc., are of only instrumental value: they simply enable us to predict phenomenon on the level of galvanometers and cloud chambers. They do nothing to remove the surprising character of these phenomena.

Another prominent realist of this period was Grover Maxwell whose early defence of the ontological status of theoretical entities has now gained a quasi-classic status. Maxwell, arguing against the phenomenalists, instrumentalists, and logical positivists, rejected the ideas of observation-theoretical dichotomy, desirability and even possibility of eliminating all references to unobservable (theoretical) entities, and the view that realism – instrumentalism debate, is a non-genuine dispute, reducible to mere verbal difference.

During the 1960s and 1970s there were more realist voices to be heard. Amongst the better known realists of this newer generation one could name E. MacKinnon,
E. McMullin\textsuperscript{77}, R. Boyd\textsuperscript{78}, and R. Harré\textsuperscript{79}. MacKinnon was advocating a version of scientific realism he had dubbed functional realism:

The question of whether or not postulated theoretical entities should be accepted as real is a practical physical question, not a metaphysical question. ... I believe it reduces to the question of whether or not the proposition, 'There are \( Ns \)', can be detached from particular theories and considered a part of the background assumptions, proper to a domain which otherwise competing theories, actual or possible, must accept, or whether this proposition is an assumption proper to one particular theory. ... In this evaluative judgement on the part of the scientific community two general criteria seem to be operative, ... First, there should be some evidence for the existence of the entities in question independent of their rôle in a particular theory. ... Secondly, if the entity is not detected experimentally, there should be adequate reason to explain why this non-detection is expected ... \textsuperscript{80}

Recourse to explanation for arguing in favour of realism found a more explicit appearance in the works of E. McMullin and Harré who used the ideas of structural explanation and capacities and powers to argue for the existence of the unobservable entities. In the structural explanation, the properties and behaviour of a complex entity are explained by alluding to the inner structure (i.e. set of constituent elements or processes and the relationships between them) of that entity. Structural explanations are supposed to be causal explanations which do not reduce to mere regular sequence of events, but are manifestations of Nature's capacities and powers\textsuperscript{81}. Boyd, on the other hand, resorted to a causal theory of references and made use of a version of miracle argument. "... experimental evidence for a theory which describes causal relations between 'theoretical' (that is, unobservable) entities, is evidence not only for the correctness of the theory, but it is also evidence that the particular causal relations in question explain the predicted regularities in the behaviour of the observable phenomena."\textsuperscript{82}

The Relativists

Whilst realists were trying to win back the lost ground from logical positivists, a strong relativist approach under the influence of philosophers like Ludwig Wittgenstein asserted itself as an alternative to both logical positivism and realism\textsuperscript{83}. One such philosopher was Norwood Hanson\textsuperscript{84}, whose views were further developed by Stephen
Toulmin\textsuperscript{85}. The main tenets of Toulmin's philosophy of science, traces of which can be found in the works of some subsequent writers, can be summarized as follows:

1) An evolutionary model for the progress of science. In this model, notions like \textit{truth} or \textit{verisimilitude} play no rôle. Instead Darwinian theory of variation and natural selections is used to account for the process of evolution and maturation of science.

2) The prominence of the notions of problem and problem-solving. In Toulmin's view science is basically a problem-solving activity.\textsuperscript{86}

3) Contempt for the logical appraisal of theories. Toulmin is of the view that logic is an internal property of propositional systems. It produces a static and synchronic picture of scientific enterprise. However, theories are not propositional system. They heavily rely on models and analogies and can only be regarded as ongoing processes.

4) A great emphasis on the rôle of sociological and psychological factors in the evolution of science\textsuperscript{87}.

Perhaps the most influential figure of this new trend was Thomas Kuhn\textsuperscript{88}. In his [1962] Kuhn advocated the notion of \textit{revolution} against \textit{progress}\textsuperscript{89}, and introduced the intuitively appealing though not well defined term \textit{paradigm}\textsuperscript{90} to account for the issue of \textit{changes} in the history of science. Despite advocating the relativist thesis of meaning change (see below), Kuhn was of the view that one can still talk of growth in human's knowledge. However, this is not in terms of advancing towards (theoretical) truth but a more instrumentalistic conception of progress, namely, progress only in terms of the increase in the number and precision of problems solved by science. The growth of knowledge, for Kuhn, is like biological evolution; from primitive beginnings towards ever more sophisticated products, although without any goal:

Imagine an evolutionary tree representing the development of the modern scientific specialities from their common origins in say, primitive natural philosophy and the crafts. A line drawn up that tree,
never doubling back, from the trunk to the tip of some branch would trace a succession of theories related by descent. Considering any two such theories, chosen from points not too near the origin, it should be easy to design a list of criteria that would enable an uncommitted observer to distinguish the earlier from the more recent theory time after time. Among the most successful would be: accuracy of prediction, particularly of quantitative prediction; the balance between esoteric and everyday subject matter; and the number of different problems solved. Less useful for this purpose, though also important determinants of scientific life, would be such values as simplicity, scope, and compatibility with other specialities ... scientific development is, like biological, a unidirectional and irreversible process. Later theories are better than earlier ones for solving puzzles in the often quite different environment ...  

During the late 1960s and 1970s, realism received further setbacks at the hands of philosophers like Paul Feyerabend and I.Lakatos. Feyerabend was initially defending Popper’s philosophy against its critics. He attacked the issue of meaning change in transition from one theory to its successor. However, his arguments backfired and he himself, under the influence of Kuhn, came to endorse the radical view of incommensurability. Subsequently, he abandoned his earlier realist tendencies and advocated an anarchic approach towards science.

Feyerabend’s recent views, though amusing and perhaps not without some heuristic value, are nevertheless, as noted by many, inconsistent and self-defeating. For example, in his [1978], drawing on his previous ideas, he has offered a set of contradictory arguments. On the one hand, he urges total freedom of all methodological rules: "There is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other. Such violation are not accidental events ... on the contrary ... they are necessary for progress ... there is only one principle that can be defended under all circumstances, and in all stages of human development. It is the principle: anything goes." On the other hand, in the same book, contrary to his own admonition, he has offered a number of methodological advice (or as he calls them counter rules) to the reader. Among these rules is the idea of proliferation of theories, an idea already advocated by Popper.

I.Lakatos, another colleague of Popper and a historian of mathematics, who had
been educated in the Hegelian tradition, wanted to reconcile the realist aspirations with the historian-relativist views⁹⁷. In the best tradition of Hegelian approach, Lakatos tried to make a synthesis of the views of Hegel, Popper, Whewell and Kuhn among others in order to create a rational theory of science and its growth. However, his dislike of realists’ correspondence theory of truth, and his concessions on the notion of objective reality, rendered his synthesis vulnerable to relativism ⁹⁸.

One of the most explicit manifestations of relativism was appeared in the form of Sociology of Knowledge. A number of schools of sociology of knowledge were flourished during the 1970s⁹⁹. However, despite the difference in style and approaches, a common core can be distinguished in the writings of various sociologists of knowledge. The main ingredients of this common core are as follows: scientific knowledge is not a clue to understanding an independent reality but a social construct shaped by factors such as culture, environment and human interests. Objective reality and correspondence truth in the realist sense, are unintelligible. Truth is the best idea people currently and locally have to explain what is going on. Knowledge claims of various disciplines within any society or among different societies are symmetrical and on a par.

By the mid-1970s the task of upholding anti-realism had passed to a new, more sophisticated generation, whose views we are going to discuss in this essay. Before turning to these modern anti-realist philosophies however, it will be useful to list the general pattern of the arguments which can be discerned in the views of the older anti-realist writers.

The types of arguments used by the advocates of anti-realism fall under the following general categories, namely:

1. The argument from rejection of metaphysics and upholding the principle of
empiricism: If all knowledge, at the final analysis, should be based on empirically testable evidence, then any attempt to appeal to entities which are not directly testable should be regarded as illegitimate.

2. The argument from epistemic modesty: The phenomenological / instrumentalistic interpretation of successful theories is epistemologically less risky than the realistic interpretation.

3. The argument from the history of science: History of science shows that there are theory changes but not progress in scientific knowledge concerning unobservable entities.

4. The argument against the aim of science and the notion of verisimilitude: Constant falsification of even the best-cherished theories renders untenable realists’ claim concerning knowledge of reality.

5. The argument from underdetermination of theory by data: The existence of rival, equally successful interpretations of empirical data, leaves no room for claim to possession of truth.

6. The argument from historical / sociological and cultural relativism: All claims to knowledge are on equal footing.

As we shall see in the subsequent chapters all these arguments are being re-used, albeit in a more sophisticated guise, by the modern anti-realists.
NOTES (Chapter Two)

1. Like all pre-birth traumas and pains in real life, the birth of modern physics was preceded by tensions and uneasiness in the scientific community. One such sign of uneasiness was Mach’s and Hertz’s quest for a sound foundation for mechanics. The most heated debate, however, was about the claim to knowledge of scientific theories. Heinrich Hertz in his influential book *Principle of Mechanics*, advocated the view that in science we make ourselves pictures of the facts, or of reality, and we choose our pictures of reality in such a way that the logically necessary consequences of the pictures agree with the necessary natural consequences of real objects or facts. This picture theory of reality was later on in the hands of Wittgenstein turned into a full blown philosophy with anti-realist lineage. Amongst the scientists however, the issue of the truthfulness of theories best manifested itself in the discussions between the proponents of Atomism and their opponents. Boltzmann was a typical representative of atomism, while Ostwald and Mach were leading the opposition.

2. For an account of Mach’s life and thought see J.T.Blackmore [1972]. For a history of Positivism from the Middle Ages to the mid-twentieth century see L.Kolakowski [1972].

3. I.Hacking [1984, pp.41-2] has given a useful summary of the main tenets of positivist philosophy. These are: I) an emphasis upon verification (or some variant such as falsification): significant propositions are those whose truth or falsehood can be settled in some empirical way. II) pro-observation: what we can see, feel, touch, and the like, provides the best content or foundation for all the rest of our non-mathematical knowledge. III) anti-cause: there is no causality in nature, over and above the constancy with which events of one kind are followed by events of another kind. IV) down playing explanation: explanations may help to organize phenomena, but do not provide any deeper answer to why-questions except to say that phenomena occur in such and such a way. V) anti-theoretical entities; and VI) anti-metaphysics.


*Prediction (anticipation) of facts / events and the control of nature (as against understanding it) is the main aim of science for all anti-realists. Claud Bernard, an eminent positivist has stressed this conviction in a succinct way: “The whole of natural philosophy is summed up in a simple phrase: to discover the laws that govern the phenomena. Even the most elaborate experiment comes down to predicting and controlling phenomena.”* (Quoted in Kolakowski [1972], p.93)

5. "Let us look at the facts in an unbiased way, without preconception. The world consists of colour, sounds, heats, pressures, spaces, times, etc, which we shall now call neither sensations nor phenomena, because either name already implies a one-sided arbitrary theory. We shall call them elements. The fixing of the flux of these elements, whether mediately or immediately, is the real object of physical research." (Mach, [1898], quoted in H.Post [1968], p.7)

6. Among the modern philosophers of science the anti-realists / non-realists by and large regard the real objects of scientific inquiry as the knowers’ actual or possible experiences. Mach for example, clearly states that: "Nature is composed of sensations as its elements. Primitive man, however, first picks out certain compounds of these elements — those namely that are relatively permanent and of greater importance to him. The first and oldest words are names of 'things'. ... No inalterable thing exists. The thing is an abstraction, the name of a symbol, for a compound of elements from whose changes we abstract. ... Sensations are not signs of things; but on the contrary, a thing is a thought-symbol for a compound sensation of relative fixedness. Properly speaking the world is not composed of 'things' as its elements, but of colours, tones, pressures, spaces, times, in short what we ordinarily call individual sensations". (Mach [1960], p.579, italics added.)
7. In the best tradition of positivism, the rejection of the notion of causality is linked to the introduction of the notion of brute fact: "There is no cause nor effect in nature; nature has but an individual existence; nature simply is. Recurrence of like cases in which A is always connected with B, that is, like results under like circumstances, that is again, the essence of the connection of cause and effect, exist but in the abstraction which we perform for the purpose of mentally reproducing the facts." (Mach, [1960], p.580)

"Once a hypothesis has facilitated, as best it can, our view of new facts, by the substitution of more familiar ideas, its powers are exhausted. We err when we expect more enlightenment from an hypothesis than from the facts themselves". (ibid, p.599)

8. As we have noted in the text, the scope of scientific knowledge has been the main issue of dispute between realists and anti-realists. Hume as a typical standard empiricist made two moves: first he reduced knowledge of the world to that of atomistic events perceived in sense experience, and second, he identified these events as the constituents of the world. Hume's moves have been endorsed and upheld by scores of philosophers of science who subscribe to the thesis of 'standard empiricism'. The main question these philosophers have attempted to answer has been whether our knowledge is exhausted by our knowledge of these events and their conjunctions. It was never questioned whether experience can adequately constitute the world.

In his [1914, p.42] Mach writes: "I maintain that every physical concept means nothing but a certain definite kind of connexion of the sensory element ... A, B, C, ... The elements ... are the simplest materials out of which the physical, and also the psychological, world is built up". See also note 12 below.

9. Mach treats theories as something unessential. In his view, only the limitation of men's minds makes the formulation of theories desirable, since it is practically impossible to keep in mind all the necessary representations of individual facts: "Thence is imposed the task of everywhere seeking out in the natural phenomena those elements that are the same, and that amid all multiplicity are ever present. By this means, on the one hand, most economical and briefest description and communication are rendered possible; and on the other, when once a person has acquired the skill of recognizing these permanent elements throughout the greatest range and variety of phenomena, of seeing them in the same, this ability leads to a comprehensive, compact, consistent, and facile connection of the facts. When once we have reached the point where we are everywhere able to detect the same few simple elements, combining in the ordinary manner, then they appear to us as things that are familiar; we are no longer surprised, there is nothing new or strange to us in the phenomena, we feel at home with them, they are explained." (Mach [1960], pp.6-7, all italics, except the first one, in the original).

Mach's view concerning explanation is taken up by many like-minded writers. Realists, on the contrary, advocate the opposite view. Thus for example, Popper [1963/72, p.63] writes, 'It has often been said that scientific explanation is reduction of the unknown to the known. If pure science is meant, nothing could be far from the truth. It can be said without paradox that scientific explanation is, on the contrary, the reduction of the known to the unknown. In pure science, as opposed to an applied science which takes pure science as 'given' or 'known', explanation is always the logical reduction of hypotheses to others which are of a higher level of universality; of 'known' facts and 'known' theories to assumptions of which we know very little as yet, and which have still to be tested.'

10. "What we represent to ourselves behind the appearances exists only in our understanding ... [having only the value of a memoria technica or formula whose form, because it is arbitrary and irrelevant, varies very easily with the standpoint of our culture". [E.Mach 1911, p.49. Quoted in D.Oldroyd [1986], p.178].

11. "Science always has its origin in the adaptation of thought to some definite field of experience. The results of the adaptation are thought-elements, which are able to present the whole field. This fundamental view ... is consequently the one that accommodates itself with the least expenditure of energy, that is, more economically than any other, to the present temporary collective state of knowledge". (Mach [1914], pp.31-2).

"Economy of communication and apprehension is of the very essence of science. Herein lies its pacificatory, its enlightening, its refining element." (Mach [1960], p.7)

J.Blackmore [1972, pp.173-4] has produced a rather long list of the different ways in which Mach has employed his principle of economy. These are as follows: 1) economy of thought, 2) economy of energy, 3) economy of work and time, 4) methodological economy, 5) economy as mathematical simplicity, 6)
economy as abbreviation, 7) economy as abstraction, 8) logic as incomplete economy, 9) ontological economy, 10) no economy in nature, 11) linguistic economy.

12. "I should like the scientists to realize that my view eliminates all metaphysical questions indifferently, whether they be only regarded as insoluble at the present moment or whether they be regarded as meaningless for all time. I should like then, to reflect that everything that we can know about this world is necessarily expressed in the sensation, which can be set free from the individual influence of the observer in a precisely definable manner ... Everything that we can want to know is given by the solution of a problem in mathematical form, by the ascertainment of the functional dependency of the sensational elements on one another. This knowledge exhausts the knowledge of 'reality'." (Mach [1914], quoted in Blackmore, op.cit. pp.167-8).

13. Planck's views are briefly discussed in this chapter. Einstein's ideas concerning the issue of realism are discussed in Chapter 6, in the context of his debates with Bohr.

G.Holton [1973, p.226] has distinguished four stages in Einstein's philosophical development, namely:

A) Einstein's early acceptance of the main features of Mach's doctrine (pre-1909);
B) The Einstein's — Mach's correspondence and meeting (1909-1916);
C) The revelation of Mach's unexpected and vigorous attack on relativity theory (1921), and,
D) Einstein's own further development of a philosophy of knowledge in which he rejected many, if not all, of his Machist beliefs. (1921-1955).

14. Blackmore, op.cit. chapter 13, "World Influence", has produced a list of those scientists, philosophers and intellectuals who were influenced by Mach's ideas and approach. Among the better known figures in this list, one can refer to Viktor Kraft, Philipp Frank, and Hugo Dingler.

15. In 1922 Moritz Schlick was given Mach's chair of 'philosophy of the inductive sciences' at the university of Vienna. Around Schlick the nucleus of what came to be known as 'the Vienna Circle' rapidly took shape. The best known members of the group were M.Schlick, R.Carnap, F.Waismann, O.Neurath, H.Feigl, H.Hahn, K.Menger, and K.Gödel. The circle worked in close association with the 'Society of Empirical Philosophy' at Berlin, which included as members H.Reichenbach, and a number of others. See J.Passmore [1957/68].


17. The discovery of energy conservation during the 1840s and 1850s, plus the technological developments in the field of thermodynamics (most notably the advent of steam engine) paved the way for the downfall of the caloric theory. In place of this theory, two new theories emerged. The one, based on the successful kinetic gas theories of the 1860s, was a kinetic theory of heat. The other, stemmed from the considerations concerning the conservation of energy. Conservation of energy had been discovered in many different fields simultaneously. Traditional Newtonian mechanics was only one of these fields. This suggested that a physical theory employing the concepts of thermodynamics, and especially the concept of energy, was both more general and, more fundamental than Newtonian mechanics with its associated concepts of mass and force.

Wilhelm Ostwald, the nineteenth century well known chemist, was the leading exponent of the energetics view as a rival account to the traditional methods of Newtonian mechanics and the use of a priori hypotheses. He was of the view that molecules, atoms, and ions were only mathematical and a priori fictions and that the real underlying component of the universe was energy in its various arrays. (See Mary Jo Nye [1972])

Ostwald's objection to atomism was different from that of Mach's, who was not in favour of the doctrine of energetics. Mach, as we have already noticed, was opposed to all theoretical entities on the grounds that, in his view, sensations were ontologically fundamental.

18. There were a number of other prominent scientists, namely Meyer, Rayleigh, and Kelvin, who were working within the framework of the kinetic research programme. However, it was Boltzmann who put forward the most persistent defence of atomism. (See P.Clark [1976])
19. Nye (op.cit, p.20) confirms that Boltzmann never asserted unequivocally the physical reality of atoms and molecules, though, he was convinced of the necessity of these hypotheses.

20. Quoted from H.Post [1968], p.7. See also E.MacKinnon [1982], especially pp.107-116.

21. The laws of new thermodynamic theories obviously contradicted those of Newtonian mechanics. The clearest example of this was the second law. The equations of Newtonian mechanics are symmetrical with respect to time, or time reversible. Another way of putting this is to say that given a Newtonian description of some system evolving from state A to state B, it is impossible to decide on the basis of the descriptions alone, which is the initial and which is the final state. By contrast, the second law of thermodynamics may be viewed as defining an absolute direction in time for the evolution of physical systems: that of increasing entropy. According to the second law any system must evolve from a state of maximal ‘order ’ and minimal entropy to a state of minimal ‘order ’ and maximal entropy. Thus the initial and final states of the system could be unambiguously differentiated by finding out their entropy. (See D.Halliday & R.Resnick [1966], ch.25)

22. In reply to Ostwald’s objection (iv above), Boltzmann writes:

"Quite generally it seems to me that no direct description of a comprehensive field of facts is ever possible, but only of a mental pictures. Therefore, one must not say, as Ostwald does: ‘you should not make a picture to yourself ’, but only: 'you should put as little arbitrary matter as possible into it ... ‘ I would almost go as far as to say that it is in the nature of a picture that certain arbitrary features have to be added in forming it, and that strictly speaking, one transcends experience every time one infers from the picture, adapted to certain facts, just one new fact.’

To Mach he replied: "Only our sensations are given, therefore, it is said, we must not advance a step beyond them. But if one were consistent, one would have to ask further: ‘Are yesterday’s sensations given also?’ after all, only the sensations or thought we are thinking at this moment is immediately given to us". (Boltzmann [1905], Quoted in H.Post [1968], p.8, italics and emphasis added.)

In another attack on Mach, Boltzmann suggests that if Mach can use the classical empiricists’ argument from analogy to establish that people have thoughts and feelings, atomists can also appeal to analogy to establish the existence of unobservable entities. (See Blackmore, op.cit, p.206)

23. Boltzmann’s view was that the proponents of the continuity of matter (energetists) and of a phenomenalist-positivist approach to nature had allowed themselves to forget that "the conceptions of an integral and differential calculus released from all atomistic representations ... are purely metaphysical, if we understand by that — according to a famous definition of Mach — things which we have forgotten how we arrived at. ’ The differential equations and calculus were attained by taking the limit of discontinuous points: thus ’Atomistic appears inseparable from the very concept of continuity ‘. (Quoted in Nye [1976], p.260)

24. "Inference to the best explanation" which is also known as, "abduction", "the method of hypothesis", "hypothetic inference", "the method of elimination", "eliminative induction" and "theoretical inference" (See G.Harman [1965] is a method of inference in which the following pattern is applied:

1. Some surprising phenomenon P is observed.
2. P would be explicable as a matter of course if Hypothesis H were true.
3. Hence there is a reason to think that H is true. (See N.Hanson [1958/65], p. 85ff)

Hanson has traced back this argument to Aristotle and has noted that many scientists have used it in their argumentation. Peirce is also among the modern philosopher who has taken this scheme seriously. He sometimes calls it "retroduction" and has discussed it in some length. He writes: "Deduction proves that something must be; Induction shows that something actually is operative; Abduction merely suggest that something may be." (Quoted in Hanson, ibid) For a defence of inference to the best explanation see P.Lipton [1991].

25. Boltzmann’s statistical treatment of the heat theory was a major development of the kinetic theory. This approach makes it possible to recover the results of macroscopic (or phenomenological) thermodynamics from postulates about the behaviour of micro-entities. The fundamental relations obtaining between micro-entities were described only statistically and were of a radically different nature of those envisaged by
Newtonian. Boltzmann interpreted the success of his theory as good evidence for the existence of micro-entities (atoms and molecules). (See Blackmore, op.cit). See also next note.

26. Boltzmann was alone in his defence of realism against extreme and powerful anti-realist views preached by Mach and others. Although at the end the psychological pressures proved to be too strong for him to bear, it must be stressed that in many ways his arguments were interesting and on the right track. For example, apart from the main arguments mentioned in the text, against the irreversibility objection (no.v), propagated by Ostwald and strengthened by Planck's assistance, Zermelo, Boltzmann put forward a rather clever defence.

Ostwald had pointed out that in all equations of mechanics the time $t$ only occurs in the square, $t^2$, ... thus every process described by these equations are reversible. Whereas, in thermodynamics all processes are irreversible. During 1895 Ernst Zermelo developed what has been known as the recurrence paradox. Applying a mathematical theorem published by Poincaré five years before, Zermelo argued that no mechanical proof of the second law of thermodynamics is possible, because any mechanical system left to itself must ultimately return to a configuration arbitrarily close to the one from which it began. He concluded that in such a system irreversible process are impossible since (aside from singular initial states) no single-valued continuous function of the state variable, such as entropy, can continuously increase; if there is a finite increase, then there must be a corresponding decrease when the initial state recurs. (Kuhn, [1978], p.26).

To these rather strong objections, Boltzmann responded by producing a sufficiently plausible reply;

To Ostwald: "From the fact that one may reverse the sign of time in the differential equations of mechanics, without altering them otherwise, Ostwald concludes that the mechanical interpretation of the world could not explain why processes in nature always prefer to run in a certain direction. It seems to me that he has overlooked here that mechanical processes are determined not only by differential equations but also by the initial conditions. Precisely contrary to Ostwald, I have called it one of the most marvellous confirmation of the mechanical interpretation of nature that the latter supplies an extraordinarily good picture of this dissipation of energy, provided one assumes that the world started from an initial states which fulfils certain conditions, and which I called ... an improbable state" (quoted in H.Post [1968] op.cit, pp.8-9)

To Zermelo: "States of considerable demixing, or large temperature differences, are not absolutely impossible, but only extremely improbable ... If therefore, we just postulate the world to be sufficiently large, then according to the laws of the calculus of probability there will occur, sometimes in one place and sometimes in another, regions of the dimension of the sky of fixed stars with quite improbable distribution over states. During their creation, as well as during dissolution, the development in time will be one-way; thus, if there are thinking beings in such a region, they will gain just the impression of time that we have, though the development in time for the universe as a whole is not one-way."

27. According to P.Clark [1976, sec.4], kinetic programme was a degenerative research programme during the period between 1880 and early nineteenth century. Thus Planck commenting on the state of the kinetic programme at a conference in 1891 concluded that: "Despite a short meteoric rise in the early sixties, every attempt at elaborating the theory has not only not led to new physical results but has run into overwhelming difficulties". Similarly, Ostwald writing in retrospect of the state of the programme at the end of the last century stated that he saw in the atomic-kinetic hypothesis, "a superficial habit to cover up rather than promote actual scientific tasks by arbitrarily assumptions about atomic positions, motions and vibrations."

M.Gardner [1979], has also discussed the shaky status of the atomic hypothesis in the lasts decades of nineteenth century.

28. In response to Ostwald who deplored the fact that today everybody believes in atoms and forces as constituting the ultimate reality, Boltzmann tried to forge a (partial) compromise. On the one hand, he tried to minimize his differences with his opponents, and on the other, he tried to make a case for atomism by invoking 'inference to the best explanation' argument:

"I once took up the cudgels myself for the mechanical conception of nature, but only in the sense that it represents a huge progress compared to the earlier purely mystical conception. On the other hand, the conception according to which there can be no other explanation of nature than that which proceeds from the motion of material points, the laws of which are determined by central forces, had already been, and for
a long time, almost universally abandoned before Mr Ostwald developed his considerations. Today we are much more cautious; this representation is for us only an image, which we do not worship, and which we may possibly succeed in bringing to perfection but which also must one day be completely abandoned. Today it has for us, in any case, the utmost value, in the sense in which it is the only image developed right up to the end in a consequential manner and which concords with experience by many important characteristics" (Boltzmann [1896], Quoted in J.Bouvresse [1986], p.7)

"... The word hypothesis already implies that this assumption goes beyond the observed fact of the most jump-like change of properties, and that the possibility of a quite different, perhaps simpler and more easily grasped, description of this change is not excluded. On the other hand, the possibility remains that many further consequences of the old hypothesis are confirmed, and that in this way we reach a somewhat clear conception of how we are to think of those atoms, so that the retaining of this hypothesis remains extremely useful for a long time ..." (Boltzmann [1905], Quoted in H.Post, op.cit, p.8)


31. Planck had a great desire for the universal laws and absolute constants which he regarded as the loftiest goal of science. He was of the view that the order of the universe should not be explained as a merely phenomenal chaos of random events but ultimately be explained through laws whose validity is objective. For him the second law of thermodynamics and especially the concept of irreversibility had absolute validity. However, Boltzmann statistical mechanics made the increase of entropy into a highly probable rather than an absolutely certain feature of nature. This was against Planck's firm conviction concerning the validity of the second law. (Klein, ibid. MacKinnon, op.cit. p.130)

32. See Ch.6 of the present essay.


34. For Planck's criticism of Mach see his lecture of 1908 entitled 'The Unity of the Physical World-Picture'. A more direct attack on Mach's view was produced by Planck in his [1910] entitled 'On Mach's Theory of Physical Knowledge'. Both articles are reprinted in S.Toulmin [1970].

G.Holton [1973, p.227] writes: "... by 1909 Planck was one of the few opponents of Mach, and scientifically the most important one. He had just written a famous attack, Die Einheit des physikalischen Weltbildes. Far from accepting Mach's view that, as he put it, "Nothing is real except the perceptions, and all natural science is ultimately an economic adaptation of our ideas to our perceptions", Planck held to the entirely antithetical position that a basic aim of science is, "the finding of a fixed world picture independent of the variation of time and people", or, more generally, "the complete liberation of the physical picture from the individuality of the separate intellects".

35. Kangro, ibid.

H.Krips [1986] following a lead from Laudan [1981] argues that "There was a wide-spread late-19th century methodological tradition which motivated the change in status of certain ontological claims — e.g., that atoms exist — from 'inaccessible to science' to 'scientifically acceptable' even though those claims were not strictly 'observable'. This methodological tradition is a hybrid of positivist and realist views". (p.43)

According to Krips, before his final conversion to realism later in his life, Planck was, for a rather long time, a weak dualist: On the one hand, unlike Helmholtz, he was explicitly 'anti-atomist' in the sense of preferring a continuum model for matter. (This of course was a rather mild anti-realism, in that he seemed to have preference for continuity theory at least in his own investigations and perhaps only for its local usefulness not its universal applicability, and without committing himself to its literal truth.) On the other hand, he appears simply to have taken atoms and molecules for granted, while at same time denying that atoms are accessible to scientific investigation at least pro tem. (See ibid. pp. 47-9).

Krips maintains that Plank, in embracing Atomism, did not (contra Holton [1973]) change his methodology from a non-realist one to a realist one. Holton [1973] nevertheless, holds that Planck
conversion to atomism was a clear evidence of his abandoning his earlier positivist methodology. However, it seems one can reconcile the views stressed by Krips and Holton, by simply pointing out that realism in the time of Planck was in a stage of immaturity and still needed to mature further.

36. "Although I am firmly convinced that Machian system, if it is pursued with complete consistency, cannot be proved to contain any inner contradiction, it seems to me just as certain that its significance is, at bottom, only formalistic, which does not affect the essence of natural science. This is because the outstanding characteristic of all scientific research — the demand for a constant world picture, independent of changing time and peoples — is alien to it. ... A constant unified world picture is, as I have tried to show, the fixed goal which true science, in all its forms, is perpetually approaching, ..." (Planck [1910], reprinted in Toulmin [1970], p. 25.

With the passage of time, Planck enhanced his realistic conceptions and hence was able to produce better structured arguments. In his Where Is Science Going? he writes:

"If the scope of physical science extends no further than the mere description of sensory experiences, then strictly only one’s own experience can be taken as the object of such description: because only one’s own experience are primary data. Now it is clear that on the basis of a mere individual complex of experience not even the most gifted of men could construct anything like a comprehensive scientific system. So we are faced with the alternatives of either renouncing the idea of a comprehensive science, which will hardly be agreed even by the most extreme positivist, or to admit compromise and allow the experience of others to enter into the groundwork of scientific knowledge. But we should thereby, strictly speaking, give up our original standpoint, namely, that only primary data constituted a reliable basis of scientific truth". (Planck [1932/1977], pp.77-8)

37. Laudan [1981a, p.221] notes: "Thus we can see that, even among the most partisan of the atomic theory, there was a general acceptance that methodological criteria by which Mach sought to evaluate such theories were sound and reasonable. Thus Ludwig Boltzmann and Max Planck both agreed that an economical representation of facts is the central aim of science."

38. (Planck [1910], reprinted in Toulmin [1970], p.52)

Mach intended to achieve objectivity by factoring out the peculiarities of individual observers. Planck accuses Mach of having double standards in his defence of the principle of economy:

"We cannot prevent anyone from defining a concept as he pleases. But it really will not do, first, to play the principle of economy as a trump card against metaphysics, by express reference to its human-practical meaning, and later, when it no longer quite meets the case, to deny the human-practical aspect of economy with equal emphasis. Using this flexible concept of economy, one can of course do anything, or rather one can do absolutely nothing precise". (ibid., p.47)

39. For details see P.Clark, op.cit. [1976].

40. According to Blackmore (op.cit), Mach, until the end of his life (1916) remained a staunch positivist to the extent that he never accepted the reality of atoms and had no faith in the veracity of the theory of relativity. The following quotations written by Mach between 1910 and 1915 are a clear testimony to his steadfastness in his positivistic beliefs.

"Atoms cannot be perceived by senses; like all substances they are things of thought. Furthermore, atoms are invested with properties that absolutely contradicted the attributes hitherto observed in bodies. Still less, will the monstrous idea of employing atoms to explain physical processes ever get possession of us, seeing that atoms are but symbols of those peculiar complexes of sensational elements which we meet with in narrow domains of physics and chemistry. The result of atomic theory can be just as manifold and useful if one is not in such a hurry to treat atoms as realities. Therefore all honour to the beliefs of physicists! But I myself cannot make this particular belief my own. ... I do not consider the Newtonian principles as completed and perfect; yet in my old age, I can accept the theory of relativity just as little as I can accept the existence of atoms and other such dogmas". (p.321)

41. J.Passmore [1957/68, p.326] has traced this motive in Poincaré: "If a law, as the positivists had argued, is a bare summary of experiences, then the rôle of scientist is restricted to the recording and summarizing of his observations; the scientist, indeed, is no more than a sensitive machine. But if, on the contrary, laws
are conventions, definitions in disguise, a language we deliberately construct in order to talk about the movements of particles, then the scientist is a creator.”

Mature Einstein, too, was very much in favour of this imaginative or creative aspect of science. On this issue see his ideas and Opinions [1954], see also G.Holton op.cit. For a comparative study of the issue of scientific creativity in Poincaré and Einstein see A.Miller [1992].

42. In his [1902/1952] Poincaré writes: ”Experiment is the sole source of truth. It alone can teach us something new; it alone can give us certainty. These are points that cannot be questioned”. (p.140)

43. ”... If experiment is everything what place is left for mathematical physics? ... it is not sufficient merely to observe; we must use our observations, and for that purpose we must generalise ... Science is built up of facts, as a house is built of stones; but an accumulation of facts is no more a science than a heap of stones is a house. Most important of all, the man of science must exhibit foresight ... That is the rôle of mathematical physics. It must direct generalisations, so as to increase what I called ... the output of science.” (ibid. pp. 140-5)

44. Poincaré has discussed this major point in chs IX & X of his [1952].

45. See J.Giedymin [1982] who, in his account of Poincaré’s philosophy, argues (contra Popper [1963] and Holton [1970]) that his conventionalism should not be overstated. In Giedymin’s view Poincaré’s philosophy is a combination of conventionalism and invariantism. These two ingredients can be defined as follows:

**conventionalism** — the view that the axioms of geometry are conventions, that the choice of one of metric geometries is (empirically) arbitrary, that scientists often elevate empirical generalisations to the status of conventional principles, that some hypotheses (indifferent ones) are conventions freely invented by the mind.

**Invariantism** — the view that the object of geometry is to study a particular group, that in our minds the idea of a number of group pre-exists, that from all possible groups we choose one to which to refer natural phenomena, that there is no absolute space and no absolute time, that we have no direct intuition of the simultaneity of distant events, that the principle of relative motion is impressed upon us because the commonest experiments confirm it and because the consideration of the contrary hypothesis is singularly repugnant to the mind.” (ibid. pp.viii-ix)


47. Instrumentalist view of science has commonly been represented as one in which scientific theories serve as useful aids to the classification of phenomena or data, but lack any real explanatory function, and do not purport to explain state of affairs in the real world.

D.Oldroyd [1986] notes that Poincaré’s discussions of psychology of invention and aesthetic sense in the process of mathematical discovery, and the rôle he had assigned to considerations like elegance and simplicity in the choice of scientific theories, are only remotely connected with positivism. Indeed, as Oldroyd emphasises, in talking about such matters as the “sUBLIMAL ego” Poincaré was using a language that would have been an anathema to strict positivists such as Mach. However, Oldroyd concludes that Poincaré’s conventionalism can fairly be represented as a close relative of instrumentalism which lay within the domain of the positivist thought. (p.194).

48. Duhem, a master of thermodynamics and an eager pursuer of the energetics research programme, tried to construe energetics as a rational phenomenological continuum theory without any assumptions about the ultimate inner reality of matter. (See S.Jaki [1984])

49. Duhem was a devout catholic and as such could not endorse all the implications of a positivist philosophy. He writes: ”To be a positivist is to state that there is no other logical [rational] method than the method of the positive sciences, that whatever cannot be approached by that method and whatever cannot be known by the positive sciences is in itself absolutely unknowable”; and he rather disapprovingly adds, ”Is it what we support?”. (Quoted in S.Jaki [1984], p.325)
50. Duhem’s overriding interest in acquiring independence for physical science had its nerve centre in his view that precisely because the particularly positive statements of metaphysical systems were highly hypothetical, they could never furnish an unambiguous law of physics, however elementary and fundamental. See Duhem [1954] especially the first chapter and the appendix).

51. In his classic [1954, p.7], Duhem writes: "A physical theory is an abstract system whose aim is to summarize and classify logically a group of experimental laws without claiming to explain these laws". (italics in original, emphasis added)

52. *ibid.* p.19.


54. This is the so-called Duhem-Quine thesis. Duhem stated that: "The physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one group of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed" *(ibid. p.187)*

R.Ariew [1984, pp.313-25] has argued that Duhem’s thesis as summarized above is not the same as what has come to be known as the Duhem-Quine thesis. The latter, also known as underdetermination, is the view that any theory can be maintained in the face of any evidence, provided we make sufficiently radical adjustments elsewhere in our beliefs, and that from underdetermination it follows that social factors must be invoked to explain why a scientist adopts a particular theory. In Ariew’s view the latter thesis, popular among the sociologists of knowledge, cannot be attributed to Duhem on the basis of his own text. Quine has discussed this thesis in his [1953, p.43].

55. Duhem distinguished between classification according to the physicists and classification according to the naturalists (e.g., zoologists, physiologists, paleontologists). In Duhem’s view the latter establish homologies which are comparative synoptic tables and amount to artificial classifications. The former, in contrast, seek to establish natural classification which is characterized by neatness, clarity, order, perfection of an ordered group of experimental laws:

"... what does a naturalist mean in proposing a natural classification of vertebrates? The classification he has imagined is a group of intellectual operations do not referring to concrete individuals but to abstractions, ... These homologies are purely ideal connections, not referring to real organs but to generalized and simplified conceptions formed in the mind of the naturalist; the classification is only a synoptic table which summarizes all these comparisons." (Duhem [1954], ch. II, p.25.)

"The neat way in which each experimental law finds its place in the classification created by the physicist and the brilliant clarity imparted to this group of laws so perfectly ordered persuade us in an overwhelming manner that such a classification is not purely artificial, that such an order does not result from a purely arbitrary grouping... we see in the exact ordering of this system the mark by which a natural classification is recognized." *(ibid. pp.25-26)*

56. "This characteristic of natural classification is marked, above all, by the fruitfulness of the theory which anticipates experimental laws not yet observed, and promote their discovery.) *(ibid. p.30)*

"... the more complete it [classification] becomes, the more we apprehend that the logical order in which theory orders experimental laws is the reflection of an ontological order, the more we suspect that the relations it establishes among the data of observation correspond to real relations among things." *(ibid. pp.26-7)*

Further aspects of Duhemian system of thought especially in comparison to that of van Fraassen’s are discussed in Ch.3.

57. Duhem’s classic [1954] consists of two rather distinct parts. Whereas the second part portrays Duhem as a positivist physicist and a realist metaphysician, the first part plus the extended appendix pictures him as somewhat more sympathetic to the potentially positive rôle of metaphysics in relation to physics. His views in these latter parts are closer to scientific realists than the positivists. For an account which mostly concentrates on the second part of Duhem’s book and thus stress his positivist conviction see M.Hesse
S.Jaki [1984], on the other hand, stresses on the realist elements in Duhem.


59. Perhaps one of the best expository works on the so-called scientific philosophy, which contrast this approach with the speculative philosophy is H.Reichenbach’s The Rise of Scientific Philosophy [1957].

60. A.Naess ([1968, p.34]) has summarised the main presuppositions of this new scientific philosophy in the following way:

I. All necessary truth are analytic. There are no synthetic a priori truths.

II. Scientific and thus also cognitive meaning in general is a property only of statements that can be tested, directly or indirectly, by means of observation. Metaphysics does not satisfy this condition.

III. Science, and thereby all knowledge, can be expressed in concepts whose meaning is due to their occurrence in directly testable statements or can be reduced to concepts which acquire their meaning in this way.

IV. The testability of statements presupposes a set of rules of language. In our choice of these we are free (the Principle of Tolerance). The rules themselves, however, are not knowledge, and attempt to talk as if they were result in metaphysics.

V. Philosophy is an activity through which the consequences of the above four theses are drawn within the various existing areas of scientific research and which helps to incorporate new areas under such research. But philosophy itself cannot be an area of knowledge, alongside or beyond science.

Further aspects of logical positivists’ philosophy are discussed in the next chapter.

61. See his [1927], [1936].

62. W.Bridgman ([1927], p.5) for example, has noted that: "If a concept is physical, as of length, the operations are actual physical operations, namely those by which length is measured; or if the concept is material, as of mathematical continuity, the operations are mental operations, namely, those by which we determine whether a given aggregate of magnitude is continuous." However, as many critics have pointed out, in cases which we measure a quantity by means of two or more different methods, Operationalists’ procedure will be unable to establish the concepts involved in these different operations are indeed identical.

Apart from this undesired proliferation of theoretical concepts, Operationalists’ programme introduced a severe limitation on the part of scientists to extend theoretical concepts into new areas. For a critical assessment of Operationalism see D.A.Gillies [1972].

63. As Popper himself relates in his intellectual autobiography [1976], his first publication [1959/68, German edition 1934] was, "... largely a critical discussion and ... a correction of the doctrines of the Vienna Circle". (pp. 80-90). Popper also emphasises that it has been his views which were responsible for the demise of logical positivism (ibid).

64. Right from the beginning of his philosophical deliberations Popper, was rightly suspicious of any intrusion of linguistic manoeuvres for replacing the world-Knower relation, with that of words-knower relations:

"Language analysts believe that there are no genuine philosophical problems, or that the problems of philosophy, if any, are problems of linguistic usage, or of the meaning of words. I, however, believe that there is at least one philosophical problem in which all thinking men are interested. It is the problem of cosmology: the problem of understanding the world — including ourselves, and our knowledge, as part of the world. All science is cosmology, I believe, and for me the interest of philosophy, no less than of science, lies solely in the contributions which it has made to it. For me, at any rate, both philosophy and science would lose all their attraction if they were to give up that pursuit. Admittedly, understanding the functions of our language is an important part of it; but explaining away our problems as merely linguistic 'puzzles' is not.

Language analysts regard themselves as practitioners of a method peculiar to philosophy. I think they are wrong, for I believe in the following thesis.

Philosophers are as free as others to use any method in searching for truth. There is no method peculiar to philosophy.
A second thesis which I should like to propound is this.

The central problem of epistemology has always been and still is the problem of growth of knowledge. And the growth of knowledge can be studied by studying the growth of scientific knowledge.

I do not think that the study of the growth of knowledge can be replaced by the study of linguistic usages, or of language systems." (Popper [1959/1968], pp.15-16)

65. Popper [1976] emphasises that, contrary to the members of the Vienna Circle who have had embraced idealism implicitly or explicitly, he has had a realist turn of mind from early on. However, due to the fact that there were still some loose ends in his earlier views, he was unable to make a more explicit move in defence of realism: "Apart from a restatement of my theory of knowledge, one of my aims in the Postscript was to show that realism of my Logik der Forschung was a criticizable or arguable position. I stressed that Logik der Forschung was the book of a realist but at the time I did not dare to say much about realism. The reason was that I had not then realized that a metaphysical position, though not testable, might be rationally criticizable or arguable. I had confessed to be a realist, but I had thought that this was no more than a confession of faith. Thus I had written about a realist argument of mine that it 'expresses the metaphysical faith in the existence of regularities in our world (a faith which I share, and without which practical action is hardly conceivable)' . " (p.150).

66. It is widely believed that the distinction between the two contexts, and laying emphasis on the first context while relegating the second to the realm of psychology, was first made by H.Reichenbach in his Experience and Prediction [1938]. However the distinction between the two context goes much further back. As P.Hoyningen-Huene [1986] has observed, Popper made use of this distinction in his Logik der Forschung [1934]. It can also be found in Carnap's Aufbau [1928], and in Schlick's Allgemeine Erkenntnislehre [1918]. Husserl in his Logische Untersuchungen [1913] and Frege in Grundlagen Arithmetik [1884] and Begriffsschrift [1879] have used the distinction. One can also trace back the distinction to the methodological tradition of 19th century in the works of Whewell Philosophy of Inductive Science [1847] and Herschel Preliminary Discourse on the Study of Natural Philosophy [1830/31]. Popper in his [1934, ch.i] has attributed the distinction to Kant's quid juris-quid facti in his the Critique of Pure Reason. Feigl in his ([1970], The 'Orthodox ' View of theories) has even gone as far as Aristotle and Euclid.

67. This theme has ben repeatedly discussed by Popper in many of his publications. See Bibliography.

68. Popper [1963/72].

69. The criterion of falsifiability is a measure for many things including, simplicity, testability, and demarcation between science and pseudo science. See Popper [1959/68].

70. ibid.

Popper's views have been subject of numerous studies. For a critical assessment of his theory of science, in line with the approach taken in this essay, see N.Maxwell [1974, I&II].


72. See note in Chapter Three.

73. Smart [1963]. p.39, italics in original.

74. G.Maxwell [1962].

75. ibid. passim.

Maxwell was arguing against Thomas Nagel who, is his classic The Structure of Science [1961, pp.129-152], had advocated the view that there is no substantial difference between realism and anti-realism. At the end of his chapter on the "Cognitive Status of Theories" Nagel had observed that, "It is therefore difficult to escape the conclusion that when the two apparently opposing views on the cognitive status of theories are each stated with some circumspection, each can assimilate into its formulations not only the facts concerning the primary subject matter explored by experimental inquiry but also all the relevant facts
concerning the logic and procedure of science. In brief, the opposition between views is a conflict over preferred mode of speech." (p.152)

76. E.MacKinnon [1972a], [1972b], [1979].
77. E.McMullin [1971], [1978].
78. R.Boyd [1973], [1979], [1980].
79. R.Harré [1970], [1975] with H.Madden. Harré, despite his realist conviction, was advocating the untenable thesis that there is an ontological difference between the realms of observable and unobservable entities (see his [1961]). In recent years, as we shall see in Ch.5, Harré has embraced a rather weak version of realism which borders on anti-realism.
81. See Harré and Madden [1975], McMullin [1978].
83. See R.Trigg [1991], M.Hodge and G.Cantor [1990]. It should be noted that although logical positivism, with respect to its of theory of meaning (i.e. meaningful statements are verifiable; meaning is use) and its empiricist language for translating theoretical terms into observational terms, was defeated, nevertheless, it survived in other forms.
84. See N.R.Hanson [1958], [1963]. Hanson views are best discussed in Toulmin & Wolff (eds.) [1971] and Humphreys (ed) [1973].
85. Toulmin has expanded his views in his [1961] and [1972]. In his [1953] he also gives a short account of his theory of science. Toulmin [1972] drawing on Hegel, Wittgenstein, Polanyi, Hanson and Kuhn among others, has expanded and expounded a series of theses which were supposed to present an alternative theory of science contrasting with the more mainstream philosophies of science including logical positivism and Popper's. Toulmin views were received sympathetically by writers like Harré and Laudan who either shared or adopted many of his basic theses.
86. This notion has been emphasised by many writers before Toulmin including Peirce, Dewey, and Popper. Popper, following a lead from Collingwood, brought 'problems' to the attention of subsequent philosophers and methodologist. In his Conjectures and Refutations Popper pointed out that a philosopher of science should spotlight the fact that science starts only with problems; that every worthwhile new theory gives rise to new problems, problems of reconciliation and problems of devising novel tests; that one way to measure the fruitfulness of a theory is by the number of new and significant problems it gives rise to, and that science is best seem as moving from problems to problems of ever increasing depth. In his objective knowledge, Popper emphasized that "the history of science should be treated not as a history of theories, but as a history of problem-situations and their modifications (sometimes imperceptible, sometimes revolutionary) through the intervention of the attempts to solve problems". Popper has summarized his own methodology in the shape of a tetradic scheme namely, Problem, --- Tentative Theory --- Error Elimination --- problem, which begins and ends with 'problems '. (See H.Sarkar[1981])
88. T.S.Kuhn [1962], [1977]. Kuhn's views have been extensively discussed in the literature. The following are among the better known appraisals; I.Lakatos and A.Musgrave (eds.) [1970/2], R.Trigg [1973/77], G.Doppelt [1978], G.Gutting [1980], H.Seigel [1987].
89. The two major themes of revolution and progress in philosophy of science have their modern background in the contrast between the Enlightenment philosophy and the Hegelian approach. Popper [1945] and [1963] has produced one of the best critical accounts of these two rival schools.

The notion of progress is discussed in Chs. 3 & 4. of this essay.

90. M. Masterman [1970/2, pp.59-89] has identified twenty-one usages of the word paradigm in Kuhn [1962]. According to Masterman the various connotations of the term can be divided into three groups namely, 1) the metaphysical formulations of the concept, or metaparadigms, that is, the principles which organize our perceptive activity and by which we thus understand reality; 2) paradigm in the sociological sense; and 3) paradigm in the concrete sense which means among other things an actual text-book (which has acquired the status of a classic), or an analogy or a diagram. However, it seems the more relevant sense to be the second one, namely, universally recognized scientific achievements that for a time provided model problems and solutions to a community of practitioners [1962, p.x].


92. Kuhn [1962], [1970], [1977], and Feyerabend [1965], [1975], [1978] have invoked a cluster theory of meaning to argue for meaning variance in theory change. Kuhn for example, says: "In transition from one theory to the next words change their meanings or conditions of applicability in subtle ways. Though most of the same signs are used before and after a revolution — e.g., force, mass, element, compound, cell — the ways in which some of them attach to nature has somehow changed. Successive theories are thus ... Incommensurable" ([1970], pp.266-7, italics added).

Feyerabend, in a similar vein, has emphasised that: "That the relativistic concept and the classical concept of mass are very different becomes clear if we ... consider that the former is a relation, involving relative velocities between an object and a coordinate system, whereas the latter is a property of the object itself and independent of its behaviour in coordinate systems". ([1963], italics in original) Incommensurability, is then, used by Kuhn and Feyerabend to reject the realist notion of progress.

93. See P. Feyerabend [1975], [1978], [1988]. Feyerabend has taken a more radical approach in comparison to that of Kuhn’s and has denied the concept of progress of scientific knowledge altogether. However, this move has led him to a rather untenable relativism: "If theories are commensurable, ... then we simply have addition of knowledge. It is different with incommensurable theories. For we certainly cannot assume that two incommensurable theories deal with one and the same objective state of affairs (to make that assumption we would have to assume that both at least refer to the same objective situation. But how can we assert that they both refer to the same situation when they both never make sense together? Besides, statements about what does and what does not refer can be checked only if the things referred to are described properly, but then our problem arises again with renewed force.) Hence, unless we want to assume that they deal with nothing at all we must admit that they deal with different worlds and that the change (from one world to another) has been brought about by a switch from one theory to another. Of course, we cannot say that the switch was caused by the change (though matters are not quite as simple as that; waking up brings new principles of order into play and thereby causes us to perceive a waking world instead of a dream world). But since Bohr’s analysis of the case of Einstein, Podolsky and Rosen we know that there are changes which are not results of a causal interaction between object and observer but of a change of the very conditions that permit us to speak of objects, situations, events. We appeal to changes of the latter kind when saying that a change of universal principle brings about a change of the entire world. Speaking in this manner we no longer assume an objective world that remains unaffected by our epistemic activities, except when moving within the confines of a particular point of view. We concede that our epistemic activities may have a decisive influence even upon the most solid piece of cosmological furniture — they may make gods disappear and replace them by heaps of atoms in empty space." ([1978], p.70, italics in original, emphasis added)


95. ibid, pp.51-52
96. Popper [1959/68].

97. See I.Lakatos [1970], [1978]. The critical literature on Lakatos is relatively extensive. See for example, P.Feyerabend and M.Wartofsky (eds.) [1976], K.Gavroglu (et.al) [1989].

98. Difficulties which accompany the notion of truth (especially the problems concerning correspondence theory of truth), plus an influence from the Hegelian philosophy (notably in the form of notions like praxis, alienation, and the historicity of knowledge) have led many philosophers to seek alternative ways to account for the progress in scientific knowledge. We will discuss the issue of correspondence truth in chapter 5 below.

Popper, in a number of his publications (e.g., [1962a], Vol. II, [1963/72]) has argued that Hegel has largely been responsible for rise of relativism (in its various forms) in this century. In his view Hegel’s philosophy fostered an anti-scientific tradition: “the claim that every period, every ‘spirit of age’, has its own characteristic science, rejection of correspondence (objective) truth, regarding truth as relative to some framework (historical or otherwise), identifying the real and the ideal, denying the validity of the law of non-contradiction, down grading empirical sciences as well as formal logic and mathematics all paved the way for various forms of relativism including variances of instrumentalistic philosophies of science.” (Popper op.cit, also [1974], p. 1157, [1983], pp. 155-6) (See also Agassi [1975], pp. 267-8).

Chapter Three
The Challenge of Neo-Instrumentalism

I. The Legacy of Empiricism and the Mantle of Duhem

Van Fraassen is an ardent and sophisticated advocate of instrumentalism. His main objective is to produce a new empiricist interpretation of science, which is free from the deficiencies of the views of his empiricist predecessors. He is not deterred by the demise of Duhemian instrumentalism or the downfall of logical positivism. On the contrary, being well aware of the pitfalls of classic instrumentalist and positivist programmes, he intends to portray a view of science which, while retaining the better parts of these programmes, avoids their undesirable shortcomings. The outcome of his endeavour is a new version of anti-realism which he has dubbed constructive empiricism and claimed that: "it makes better sense of science and scientific activity, than realism does." Constructive empiricism has, as we shall see shortly, much in common with the Duhemian approach towards science and as such can conveniently be named neo-instrumentalism.

To see the ways in which van Fraassen has improved on the earlier versions of standard empiricism and especially that of P.Duhem, it is better to compare the main aspects of his philosophy with the views of his distinguished predecessor.

1) Like Duhem, van Fraassen is mainly concerned with giving an empiricist (or rather, an enlightened empiricist) account of the aim and structure of scientific theories. However, contrary to his predecessors he has not stopped here and has also explored the pragmatics of theory use, i.e., the relations of the theory to the theory user:

1-a) The aim of science for van Fraassen is to provide us with empirically adequate knowledge of the observables rather than true knowledge of unobservables. In his view such knowledge suffices for the main purpose of scientists, which according to
van Fraassen is prediction and control of the phenomena. Duhem had already introduced the same aim for science. There is however a significant difference between approaches of the two instrumentalists. While for Duhem (and the positivists for that matter) the divide between observables and unobservables had ontological significant, van Fraassen has played down the ontological importance of this distinction and has shifted his emphasis on its epistemological prominence. This move, as we shall see later, has enabled van Fraassen to avoid many of the criticisms levelled at the earlier versions of instrumentalism.

1-b) As for the structure of scientific theories, van Fraassen has claimed that he intends to introduce a view which frees philosophy of science from the excess of philosophy of language. He intends to achieve this aim by supplanting the old-fashioned syntactic (axiomatic) approach to science with a semantic (model-theoretic) approach. Moreover, in further contrast to the older generations of anti-realists, van Fraassen has insisted on literal construal of the language of theories. This move has enabled him, in contrast to Duhem and logical positivists, to admit truth-value for theoretical propositions.

1-c) As for the relations of theory to the theory-user, van Fraassen, has emphasised the general trend in standard empiricism by upholding the view that explanation does not have epistemic value. He has also tried to develop a programme (emphasised by Duhem, and pursued by Carnap) of introducing pragmatic elements in the choice and the evaluation of theories.

2) Like all standard empiricists, van Fraassen is not interested in the issue of providing scientists with a logic for scientific discovery (in fact he does not appear to think that such a logic is possible). Nevertheless, he has attempted to provide an account
of theory construction. This account is supposed to replace standard empiricists' rather naïve view on this issue which Popper had aptly called "the bucket theory of mind".

3) Following a lead from all faithful standard empiricists, van Fraassen maintains that metaphysics should be ousted from the realm of science. For van Fraassen, like other thorough standard empiricists, all necessities are only verbal and the best attitude towards metaphysical claims is nominalism. He has rejected the traditional accounts of laws of nature, and has replaced it with symmetry considerations.

4) In line with all anti-realist philosophies, the main drive in van Fraassen’s approach is to transpose ontological issues to epistemological ones. His move is, however, more subtle than his standard empiricist predecessors. In the first place, while in line with all traditional standard empiricist philosophers, he has appealed to the notion of brute fact, he has also acknowledged that "all our language is thoroughly theory-infected." Moreover, contrary to the traditional empiricists, he does not reduce the entities in the world to either sense-data or events; instead he has invoked an approach closer to the transcendental idealists. Consistent with this approach and somewhat different from the older generation of standard empiricists, van Fraassen maintains that scientific activity is one of construction rather than discovery.

To uphold this constructive empiricism, van Fraassen has resorted to two main anti-realist arguments, namely, the argument from epistemic prudence and the argument from undesirability of metaphysics. The first argument simply states that scientific realism is epistemically imprudent (or at least less prudent than anti-realism). In van Fraassen’s view, within the framework of standard empiricism, evidence at most compels us to regard our theories as empirically adequate, that is to say, only what it says about the observable state of affairs is true:
When a scientist holds up a theory, accepts it, and advocates its acceptance by scientific community – what exactly is he doing? ... The empiricist answer to [this] question is [that] the arbiter is empirical adequacy; unqualified acceptance involves belief only that the theory is empirically adequate; and the central aim of the science is to provide us with empirically adequate theories. This certainly implies truth for part of the theory: what it says solely about what is observable must be true. But it does not require truth in toto; a theory need not be true to be good. 32

As for the extra question of whether these theories are true, the best epistemic attitude, in his view, is to remain agnostic33. The second argument is based on a nominalist / empiricist’s long standing hostility towards metaphysics. It asserts that constructive empiricism saves scientists from the danger of excessive metaphysical baggage in their theories. Van Fraassen has summarized his two main arguments in this way;

... we can distinguish between two epistemic attitudes we can take up towards a theory. We can assert it to be true (i.e. to have a model which is a faithful replica, in all details of our world), and call for belief. Or we can simply assert its empirical adequacy, calling for acceptance as such. ... Nevertheless there is a difference. The assertion of empirical adequacy is a great deal weaker than the assertion of truth, and the restraint to acceptance delivers us from metaphysics.34

To nail the coffin of scientific realism as tightly as possible, van Fraassen has produced an alternative account for the central issue of progress of science and its remarkable success35. Contrary to realists who maintain that science progresses by producing ever more truthful pictures of reality, and that the very element of truth in the theories account for their partial success36, van Fraassen argues that a Darwinian explanation, (i.e, survival of the fittest), is a better account for both the success and progress of science than that of realists37.

Although there are other issues discussed by van Fraassen, it suffices for the purpose of this essay, to confine ourselves to three main areas of difference, namely, observables — non-observables dichotomy; empirical adequacy and the problem of theory choice; and the issue of metaphysics. We shall expound and critically assess van Fraassen’s views and arguments in these fundamental areas. However, before turning to
these issues, it is necessary to clarify one rather important point, namely, van Fraassen's claim concerning his use of a semantic rather than syntactic approach.

II. A New Picture of Theories?

In many of his publications, van Fraassen has made the claim that syntactical approach to scientific theories developed by logical positivists is outmoded and ought to be replaced by a more fruitful semantic approach:

This new tradition is called 'semantic approach' or 'semantic view'. It has been developed, since the mid-1960s, ... In this approach the rôle of language (and especially syntax or question of axiomatization) is resolutely de-emphasized. The discussion of models treats them mainly as structures in their own right, and views theory development as primarily model construction. Almost all questions in philosophy of science take on a new form, or are seen in a new light, when asked again within the semantic view.

Van Fraassen's claim concerning the superiority of the semantic (or model-theoretic) approach, invites a number of observations.

1) Many writers have noted that, contrary to van Fraassen's claim, the semantic and syntactic approaches are compatible and are best used as complementary approaches rather than as rivals. It is worth mentioning that van Fraassen himself, in his previous publications had, rather emphatically, endorsed the same position.

2) A number of writers who have taken a sympathetic stand towards van Fraassen's new approach have emphasised nonetheless that the semantic approach has no built-in anti-realistic characteristics and is (at least) equally, (if not far more readily) available to realists.

3) Some other writers have taken a rather sceptic attitude towards the merits of the new approach. This view needs to be explicated in some details. Van Fraassen's treatment of his semantic approach is rather sketchy. He has not applied his approach, in a complete and rigorous way, to any proper scientific theory. Instead he has relied on very brief (few
lines) sketches of one or two examples and has referred the reader to the works of Patrick Suppes (whom he frequently praises for his pioneering and insightful works) and his colleagues on the foundation of mechanics. At the end of his sketchy exposition of the new approach, van Fraassen has observed that, "Having insisted that the new picture of theories constitutes a radical break with the old, I wish to conclude by outlining some of its peculiar features. Of course, it too provides an idealization: only in foundational studies in physics do we see the family of models carefully described."

Van Fraassen's reference to Suppes et al works as a firm, and in fact the main support for the semantic approach, seems to be, in the light of an article by C. Truesdell, rather unfortunate. Professor Truesdell, the once editor of The Journal of Rational Mechanics and Analysis, in which Suppes et al article on the foundation of mechanics has appeared, has, in a lengthy article entitled Suppesian stews discussed the various works by Suppes and colleagues on semantic approach and has argued that these works, are in fact, much ado about nothing.

In the light of Truesdell's observations, it is interesting to note that van Fraassen, in his recent publications, has rather modestly stated that his suggested semantic approach, at most, can play a heuristic rôle and one should not expect too much out of it:

The approach that I take is that of semantic view of theories. That does not mean, for me, a systematic attempt to put everything into some standard form. The semantic approach gives a view of what theories are, and orients us towards models rather than language. It need, as such, do no more, but it does give us the task to explore and elaborate concepts which may be useful when one turns to a theory in this way. Once we have some such concept, we take them along to whatever science catches our interest, and see if they help us to gain some insight there. Such tools of the trade had better be used lightly; it is not much good to hammer in a screw, even if that hammer is your favourite tool. Nor is it appropriate to refer to any of these concepts as property of the semantic approach — that approach only leads us to appreciate them in a certain way.

4) There is however, another more mundane objection to the approaches taken by Suppes — van Fraassen (et al) towards the logico-linguistic reconstruction of theories. According to the Semantic approach, as advocated by Suppes and endorsed by his
colleagues, "the meaning of the concept of model is the same in mathematics and the empirical sciences." This view however, does not seem to be entirely correct. Logical models are devised to assign meaning to the strings of signs. Suppes has quoted Tarski's on this matter: "A possible realization in which all valid sentences of a theory are satisfied is called a model of $T$\[50]$, where, "a possible realization of a theory is a set-theoretical entity of the appropriate logical type."\[51].

In contrast, in science, a model is a set of assumptions attributing an inner structure, composition or mechanism, to an object or a system, which manifest itself in other properties exhibited by the object or system.\[52] In other words, a model (in the empirical sciences) is a set of propositions, which is purported to refer to reality, and not merely providing meaning for a string of signs. Moreover, scientists know that this set of proposition is fallible and falsifiable, but hope that it nevertheless will furnish them with accurate and even novel predictions. Furthermore, scientists expect a good model to have "surplus content", that is to say, it should be able to "suggest how the theory should be modified to meet new results – results which cannot be derived from the first simple theoretical formulation."\[53]

Good models, beside the rôle they play in solving difficult existing problems, may also play a further heuristic rôle, in that they become part of the structure of a new theory which deals with yet further unknown aspects of reality.\[54] In short, scientific models deal with physical reality, whereas the models used in logic or mathematics need not necessarily do so. In the case of the mathematical models, if the criterion of consistency is fulfilled, then the model can be accepted regardless of actual reference (or otherwise) to physical reality.\[55] In contrast, in the case of scientific models, applicability to reality (in the above sense) is a more important consideration.\[56]
5) As for van Fraassen's motive(s) for upholding the semantic approach, it seems that his main concern has been to get round two groups of fundamental issues which have always produced difficulties for anti-realists. The first group consists of the problems of truth and extra-empirical virtues (values) such as unity, simplicity and explanatoriness of theories, and the second one concerns the problem of necessity in nature (laws of nature). We shall postpone the discussion of significance of resorting to semantic (model-theoretic) approach for van Fraassen until sections IV and V. Suffice it to mention here that this approach is invoked by van Fraassen to enable him to both, as it were, have his cake and eat it. That is to say, he wants to both adhere to his instrumentalistic approach and to enjoy the credentials of a realistic interpretation of theories.

III. The Argument from Epistemic Prudence and the Observables — non-Observables Dichotomy

Van Fraassen's first argument against the scientific realists is admittedly a cogent one. It seems that if one subscribes to the principle of standard empiricism, namely, that "experience is a source of information about the world, and our only source." then there can be no escape from the conclusion that van Fraassen wants to uphold, namely, that it is more prudent to rest content with the empirical adequacy of the scientific theories than to demand more.

The above, rather self-evident, conclusion, clearly shows that something is badly wrong with the position defended by the realists who subscribe to the thesis of standard empiricism. But should one also conclude from the above that van Fraassen has been able to score a decisive and undisputed victory over the scientific realists, in that he has been able to establish his constructive empiricism over and above scientific realism? I think not. In what follows, I shall try to show that despite the apparent effectiveness of the above
argument, van Fraassen is not in a better position than his realist rivals, in that he has not been able to justify the superiority of his position over the views of the realists.

As noted above (Sec I) whilst the earlier generations of instrumentalists, who have rejected the existence of theoretical entities, would insist on a sharp ontological distinction between observable and unobservable (theoretical) entities, van Fraassen has escaped the undesirable consequences of the position of his predecessors by shifting the discussion from the ontological discussions to the epistemological ones. Unlike his precursors, he is not denying the existence of theoretical entities, nor he is attaching any ontological significance to the observable-unobservable distinction. What he is denying is rather the possibility of acquiring scientific knowledge of such entities.

Although van Fraassen no longer needs the old-fashion ontological distinction, nevertheless he requires distinctions of other sorts. In order to show that constructive empiricism is epistemically better placed than scientific realism in making sense of science, van Fraassen has based his first argument on two crucial premises. Firstly, the validity of an (alleged) difference in epistemic access to observable and unobservable entities, and secondly, the epistemic difference between accepting a theory as true, as opposed to accepting it as empirically adequate. We shall discuss the first distinction in this section and deal with the second in the next.

To substantiate his first premise, van Fraassen has made two moves. First, he has contrasted the notions of "observation" and "detection" and has rejected the latter as being too tainted by inferences to be regarded as equal to the former.

A look through a telescope at the moons of Jupiter seems to me a clear case of observation, since astronauts will no doubts be able to see them as well from close up. But the purported observation of micro-particles in a cloud chamber seems to me a clearly different case... while the particle is detected by means of the cloud chamber, and the detection is based on observation, it is clearly not a case of the particle's being observed.
Secondly, he has rejected the notion of ‘observable in principle’. Referring to Grover Maxwell’s classic article, "The Ontological Status of Theoretical Entities"61, where Maxwell has given negative answers to the following key questions, namely, 1) Can we divide our language into theoretical and non-theoretical parts? and 2) Can we classify objects and events into observable and non-observable ones? van Fraassen has noted that:

we should concentrate on ‘observable’ tout court, or on (as he prefers to say) ‘unobservable in principle’. This, Maxwell explains as meaning that the relevant scientific theory entails that the entities cannot be observed in any circumstances. But this never happens, he says, because the different circumstances could be one in which we have different sense organs—electron microscope eyes, for instance. This strikes me as a trick, a change in the subject of discussion. I have a mortar and pestle made of copper weighing about a kilo. Should I call it breakable because a giant could break it? Should I call the Empire State Building portable? Is there no distinction between a portable and a console record player? The human organism is, from the point of view of physics, a certain kind of measuring apparatus. As such it has certain limitations... it is these limitations to which the ‘able’ in ‘observable’ refers — our limitations qua human beings.62

Notwithstanding the apparent plausibility of van Fraassen’s two moves, I shall argue that van Fraassen himself can neither avoid the notion of ‘observable in principle’ nor sustain a meaningful distinction between observation and detection.

As noticed, van Fraassen has cited a look through a telescope at the moons of Jupiter as a clear case of observation, because astronauts can actually land on it. At the same time he has rejected the notion of ‘observable in principle’ on the ground that it strikes him as a trick, a change of subject. But take the case of observing an astronomical body which is 50 light years away from us and too dim to be seen without a telescope. Now, does van Fraassen call a look through a telescope at this distant body an "observation"? Presumably yes. But this admission clashes with van Fraassen’s own definition of observation. This is because in the case of the distant heavenly body nobody could live long enough to travel that far and observe it with naked eye. So in what sense should we call it observable? Perhaps van Fraassen would like to argue that the heavenly body is observable in the sense that had we been able to go near it, we could have
observed it with naked eye. But is it not another way of saying that the heavenly body is *in principle* observable?  

Van Fraassen’s distinction between detection and observation also seems to be quite arbitrary. In the first place, it can be argued that even in the case of simplest observations huge chains of *inference* are involved. This point can be best seen in the studies on visual perceptions in children and developing artificial visual mechanisms in Robots. Even recognising a simple item in the field of view depends upon such complex tasks as: discerning points of differing intensity; deciding how to group particular features; deciding which feature to ignore; deciding which contiguous elements are not related; making inferences about hidden parts of objects; using awareness of apparent inconsistencies to redirect attention to acquire new data; recognising clues as to the type of scene, in order to discriminate between different interpretations that are all supported by the available evidence; recognising that simple and seemingly self-evident interpretation may in fact be erroneous. Similarly, in *scientific* observations, as opposed to mere observations, a good deal of *inference* is involved. Centuries ago Aristotle had observed that: "There is a sense in which the taxonomist ‘sees more than’ the untrained observer of the same specimen". This sort of *vision*, as opposed to mere observation, seems to be on a par with detection.

The parity between (scientific) observation and detection can be better understood if one, contrary to van Fraassen, does not restrict *observability* to *visibility* and takes into consideration the contributions of the other senses. It is a well accepted view among working scientists that even if the human race had been blind, it would have still been possible for the mankind to achieve exactly the same level of scientific sophistication, which they would have achieved had they not been blind, albeit over a longer period.
However, if this is the case then it is hard to see what possible difference could a superficial distinction between detection / observation, make for a blind scientist.

Van Fraassen’s view of observation, apparently leads to amusing positions. Scientist must in many cases, contrary to common sense, deny the possibility of ‘observing’ properties of even medium sized objects. For instance, had Neil Armstrong been a constructive empiricist he would have to assert that while the brightness of moon’s surface is an observable fact, the coldness or roughness of its surface is not, since it would have been impossible for him (or anybody else for that matter) to "observe" this without an instrument: he (she) would have died instantly if he (she) had tried.

The observation — detection parity is also evident in the way working scientists talk about the act of probing the matter. Contrary to van Fraassen, the particle physicists, even the most hard-line positivists among them, would not hesitate equating detection and observation.

The futility of drawing a line between the realms of observables and detectable can also be shown by invoking a heap-type paradox. Van Fraassen himself has advised us to refer to science for distinguishing between the two realms. However, if we start from the bottom line of the purely observable consequences of a theory and ascend towards the highly theoretical ones, there can be no precise line in which suddenly the language of the theory ceases using observable predicates and invokes purely theoretical ones. More importantly, even while we are still in the area of purely observable terms, it is the theory that provides us with knowledge of observables and unobservables alike. Even simple terms like length or mass are embedded in a complex theory of measurement, involving all sorts of theoretical assumptions about rigid bodies, balances, movement, frames of reference and so forth.
This, of course, makes a mockery of the idea of a demarcation line, especially for a constructive empiricist who concedes that all our language is thoroughly theory-infected and does not want to go along with the logical positivists’ rejection of theoretical terms and who does not deny the existence of unobservable entities. Moreover, the fact that all scientific instruments, including a telescope (a look through which is an example of a legitimate observation for van Fraassen), heavily rely on scientific theories, means that one cannot separate the use of some scientific instrument as compatible with the ordinary observation and regard the rest as incompatible with observation, without running the risk of inconsistency or arbitrariness.

Van Fraassen has tried to anticipate and counter this argument by granting that "observable" is a ‘vague predicates’:

We may still be able to find a continuum in what is supposed detectable: perhaps more things can only be detected with the aid of an optical microscope, at least; perhaps some require an electron microscope, and so on. Maxwell’s problem is: where shall we draw the line between what is observable and what is only detectable in some more roundabout way? Granted that we cannot answer this question without arbitrariness, what follows? That ‘observable’ is a vague predicate. ... A vague predicate is usable provided it has clear cases and counter-cases.

Vagueness of observability poses no particular difficulty for realism. For realists and most (if not all) of the working scientists, the observable — unobservable (detectable) distinction in the realm of physical science is only a matter of convenience, i.e. a pragmatic matter, and not something which has either ontologic or epistemologic significance. However, it remains to be seen whether a constructive empiricist can claim the same. The question to be asked is how a constructive empiricist decides about the ‘vague cases’ of observables / unobservables as opposed to the ‘clear cases’? Van Fraassen’s answer, as we know, is to appeal to science. However, it seems the above claim raises the danger of inconsistency: according to van Fraassen we ourselves are measuring devices and as such can fairly decide the observable as against unobservable...
cases. However, when it comes to vague instances we should consult science about the limits of our ability to observe. But here comes the crunch; observability is decided by a science which, according to van Fraassen, we should only believe in its observable claims. Science, instrumentalistically interpreted, by definition, cannot decide over vague cases of observable predicates. And if we accept its judgement over vague cases, it is by constructive empiricist own standard, purely on pragmatic grounds.

Van Fraassen, of course, does not want to accept this conclusion. For him, as we have already noticed, to uphold the epistemologic prominence of the divide between observables and unobservables is essential. As a last resort therefore, he has formulated the standard argument used by realists against the epistemological significance of this distinction, in the shape of a modal argument, and has tried to show that the conclusion can be accommodated within the framework of constructive empiricism without accepting the realist premises. The argument is this: "We would be, or could become, X. If we were X, we could observe Y. In fact, we are, under certain realizable conditions, like X in all relevant respects. But what we could under realizable conditions observe is observable. Therefore, Y is observable". He has countered this conclusion by arguing that:

The crucial third premise, however, is justified by appeal to science (at best). If we assume only that science is empirically adequate, we can justify only the premise that we are, under realizable conditions, empirically indistinguishable from beings like X in all relevant respects. The conclusion then derived is only, 'Under certain realizable conditions, all the observable phenomena are as if we are observing Y', which is often true although Y is unobservable.

However, the point which seems van Fraassen has not taken into account is that invoking the language of 'as if' without an ontological commitment, brings the spectre of conventionalism, a position which, for obvious reasons, van Fraassen needs to steer clear of. However, he does not seem to have avoided this pitfall. Now, it seems van Fraassen is facing the two horns of a dilemma. Either he endorses the realist ontological
commitment, in which case, he is acknowledging that unobservable entities, postulated by our mature theories, exist. Or, he would reject the ontological commitment, and therefore opens up his position to the charge of conventionalism and thus deprives constructive empiricism of all its advantages over the previous versions of anti-realism.

IV. Constructive Empiricism as a Straitjacket

Notwithstanding the incredibility of the distinction between epistemic access to observables and unobservables, van Fraassen still wants to argue that constructive empiricism is epistemically less risky than scientific realism, and is so for at least for two reasons. Firstly, because the former only involves belief in the empirical adequacy of accepted theories, while the latter asks for truth, a much stronger notion in comparison to empirical adequacy. Secondly, because constructive empiricism avoids the danger of metaphysical excess baggage (which, as mentioned earlier, constitute van Fraassen's second major argument and will be discussed in the next section). The first point is spelled out in this way:

According to constructive empiricism, the only belief involved in accepting a scientific theory is the belief that it is empirically adequate: all that is both actual and observable finds a place in some model of the theory. So far as empirical adequacy is concerned, the theory would be just as good if there existed nothing at all that was either unobservable or not actual. Acceptance of the theory does not commit us to the belief in the reality of either one.

There is no argument there for belief in the truth of the accepted theories, since it is not an epistemological principle that one might hang for a sheep as for a lamb.

It seems van Fraassen's first point in support of his claim to superiority of constructive empiricism flies in the face of his own admission that, as far as going beyond available evidence is concerned, constructive empiricism and realism are in the same epistemic boat:

... [W]e can distinguish between two epistemic attitudes we can taken up towards 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. In either case we stick our necks out. Empirical adequacy goes far beyond what we can know at any
However, even without this admission, due to his reliance on the *as-if* language, van Fraassen would have not been able to sustain the distinction he intends to make between accepting a theory as true and accepting it as only empirically adequate.\(^{88}\)

In view of the above, it seems rather odd that he has denied the merit of venturing further afield, on the grounds of avoiding risks. Surely, risk avoidance in *epistemic* (as against the technological / engineering) matters, is not always a benefit to strive for. Popper, for one, has long ago argued that excessive caution would not only prevent scientists reaching new frontiers of knowledge, but also would give rise to the danger of dogmatism.\(^{89}\) Incidentally, the old maxim, concerning the lamb and the sheep, exactly emphasizes the merit of boldness in epistemological matters.\(^{90}\) It is due to this epistemic attitude that realists claim an extra heuristic value for their position, whereas the same does not apply to constructive empiricists.\(^{91}\)

Van Fraassen’s has replied rather rhetorically to this line of reasoning:

If I believe the theory to be true and not just empirically adequate, my risk of being shown wrong is exactly the risk that the weaker, entailed belief will conflict with actual experience. Meanwhile, by avowing the stronger belief, I place myself in the position of being able to answer more questions, of having a richer, fuller picture of the world, a wealth of opinion so to say, that I can dole out to those who wonder. But, since the extra opinion is not additionally vulnerable, the risk is — in human terms — illusory, and therefore so is the wealth. It is but empty strutting and posturing, this display of courage not under fire and avowal of additional resources that cannot feel the pinch of misfortune any earlier. What can I do except express disdain for this appearance of greater courage in embracing additional beliefs which will *ex hypothesi* never brave a more severe test.\(^{92}\)

A possible realist reply to the above charge could be that van Fraassen’s own methodology is not only unnecessarily too conservative, but also has hardly anything to do with the reality of scientific research. We have already seen one aspect of this methodology, namely, its superficial distinction between observation and detection which is alien to working scientists. However, there are at least three further reasons which show the schism between van Fraassen’s world and that of the actual world of science. We shall
discuss these cases under three separate headings.

IV.A. Constructive Empiricism and the Dynamic Nature of Science

The first serious shortcoming of constructive empiricism, as far as the actual scientific enterprise is concerned, can be put rather briefly. It seems van Fraassen has a rather static image of science in mind, not giving due credit to the dynamic nature of scientific investigation which results in the rapid availability of constantly emerging new evidence. Einstein has wrapped up this point in this way:

... One might suppose that there were any numbers of possible systems of theoretical physics, all equally well justified; and this opinion is no doubt correct, theoretically. But the development of physics has shown that at any given moment, out of all conceivable constructions, a single one has always proved itself decidedly superior to all the rest.

The charge of pompousness against realists might have been valid had science always remained in a static state. But, due to rapid and continuous appearance of new data as the result of developing of new experimental techniques and technologies which are themselves the fruit of cooperation between various fields, in many cases the fate of temporary non-testable claims of the theories can be decided fairly quickly. It is customary nowadays to talk of the exponential growth of science. At one time, the scientific community might have to wait almost three centuries for empirical corroboration of one of the consequences of Copernican theory. Nowadays however, scientific claims can be verified or falsified in a relatively much shorter span of time. It is this real feature of the real scientific enterprise, which, contrary to what van Fraassen holds, gives credence to the (realist) methodology of proliferating conjectures and unremittingly attempting refutations.

IV.B. Neo-Instrumentalism and the Progress of Science

A comparison between the views of two instrumentalists Duhem and van Fraassen serves to substantiate the point that when it comes to the dealing with real science,
constructive empiricism is even less promising than classic instrumentalism, let alone realism. The case in point is the all-important issue of scientific progress.98

Duhem maintained that development of science consists principally in the gradual progress of physical theories towards a true description of relations among natural entities, a process which he portrayed as a progressive evolution99, although he did not view science through a Darwinian eye.100 This approach to the problem of progress of science brings Duhem very close to the camp of realists101. In Duhem’s picture, the closer a theory to the ideal of natural classification, the more comprehensive and successful the theory:

To the extent that physical theory progresses, it becomes more and more similar to a natural classification which is its ideal end. Physical method is powerless to prove this assertion is warranted, but if it were not, the tendency which directs the development of physics would remain incomprehensible. Thus, in order to find the title to establish its legitimacy, physical theory has to demand it of metaphysics102.

Duhem’s account of the progress of science, notwithstanding the severe shortcomings of his overall instrumentalistic doctrine, may be viewed as to have certain explanatory force103. The same however, is not the case with van Fraassen’s account.

Van Fraassen regards science as "a biological phenomenon, an activity by one kind of organism which facilitates its interaction with the environment."104. He has suggested this evolutionary picture as an alternative to realists’ explanation of success of science105. To see whether this account is satisfactory, we should look at the issue of progress of science more carefully. Here, there are four issues at stake, namely,

a) saying what scientific progress means;

b) specifying a methodology for assessing scientific progress;

c) providing justification for the methodology introduced; and

d) accounting for or explaining scientific progress106.

Now, it is clear that van Fraassen’s evolutionary picture only describes the survival
of successful theories (i.e. the *how* question). It does not however, say why successful theories are successful. Why is that some theories, contrary to the others, are better at prediction of novel facts and explaining the data in simpler and more economical ways? What is the secret behind the success of these theories? Why is that scientists occasionally *prefer* less empirically adequate, though perhaps more *elegant* theories, to those which are better at saving the phenomena?

Van Fraassen’s claim that only successful theories survive is in fact a veiled tautology to the effect that only those theories which survive do survive. The question however, is that what it is that makes these theories successful (i.e. more empirically adequate) and therefore increases their survival values? Van Fraassen has also reduced scientists’ preference for more elegant theories to pragmatic considerations. However, pragmatic considerations do not shed any light on the fact that most of the time the preferred theories, despite their initial weaknesses, have eventually superseded their rivals which are phenomenologically more successful.

Difficulties with explaining the success of theories aside, it seems the constructive empiricists have also difficulty in answering even the first question, namely, to spell out, in a general and not a technical way, what does progress of science mean or what does it amount to? Surely, a constructive empiricist does not want to endorse the views of an anarchist like Feyerabend who would deny that science is progressive. But the basic tenets of constructive empiricism, has apparently made it too difficult for its subscribers to produce even a general definition of progress in science. In the first place, neo-instrumentalists with their scepticism towards the theoretical entities and their doubts about a natural classification, can neither resort to Duhem’s march towards a natural classification nor to realists’ *verisimilitude*. Moreover, because they are not in favour
of the logical positivists' covering law model and progress via reduction and incorporation of confirmed theories into more comprehensive theories, they cannot use this scheme either.

Secondly, van Fraassen is well aware that the idea that science aims at unity plays an essential rôle in any viable definition of progress in science. Without producing more unificatory and more comprehensive accounts of the phenomena, in the shape of theories which unify as many phenomena as possible under one single theoretical structure, we cannot claim that we have made progress in our understanding of the world. This of course, has been the aim which has been relentlessly pursued by theoretical scientists throughout the ages.

Max Planck [1909] for example, has emphasised that: "A constant unified world picture is the fixed goal which true natural science, in all its forms, is perpetually approaching ...". Similarly, Einstein in his *Physics and Reality* has noted that: "The aim of science is, on the one hand, a comprehension, as complete as possible, of the connection between the sense experiences in their totality, and, on the other hand, the accomplishment of the aim by the use of a minimum of primary concepts and relations. (Seeking as far as possible, logical unity in the world picture, i.e., paucity in logical elements.)" On the other hand, as M.Friedman has pointed out: "'Everything is what it is and not another thing' is not a good slogan for the scientific methodologists."

However, it seems that van Fraassen's nominalist leaning which manifests itself, among other things, in his readiness to resort to the notion of "brute fact", plus his commitment to uphold the notion of empirical adequacy over and above the notion of truth, would prevent him of producing a sound account of scientific progress. The reason for this predicament can be spelled out in a straightforward way.
The notion of unity, contrary to the notion of brute fact, implies the cumulative nature of theoretical knowledge. A More unificatory theory, which explains more disparate phenomena by relating them to fewer underlying entities, would retain the success of its successful predecessors. Cumulation of scientific knowledge, which is what is actually meant by scientific progress, in turn, implies the conjunction of non-trivial truths (or as Popper has put it interesting truths about the reality). Conjunction however, is not a property of empirical adequacy, but is an exclusive property of truth. Therefore, unification, in the above sense, is not available to van Fraassen.

To remedy this deficiency, van Fraassen has resorted to a new conception of unification: unification by assembling empirically adequate theories, rather than conjunction of true theories:

We should perhaps not be too anxious to explain that supposed regulative ideal of the unity of science. The explanation might at some point come to look rather like a representationalist theory of art, to which almost all twentieth-century art is an exception. But we need not contemplate such outré possibilities, for it seems to me that the idea of science consisting of a family of such disparate theories is really not feasible, except in the philosophically innocuous sense in which it actually does. ... But there seems to me no doubt that the aim of empirical adequacy already requires successive unification of 'mini-theories' into larger ones, and that the process of unification is mainly one of correction and not of conjunction.

Unification by correction, does not, of necessity, lead to the unification in the above sense. It is the typical form of unification which is in use in technology. To claim, as van Fraassen seems to do, that it is the sort of unification which is being (or should be) used in science, is to reduce science to technology; a move popular among the instrumentalists. In technology and engineering, a faulty piece of machinery can be replaced by a better designed piece, an incorrect operation procedure may be changed by a correct method, an inefficient system may be scrapped in favour of a more efficient one. An inadequate phenomenological law which describes the behaviour of certain phenomena, may be superseded by a more empirically adequate theory.
However, in none of these, and many more similar cases, the final result need not be more unificatory in the sense sought by the scientists. In fact, there is nothing in van Fraassen’s conception of ‘unification by correction' to prevent his ideal science turning into a huge monstrous construct with many smaller parts, each good at describing certain phenomena better than their rivals, without any overall (necessary) harmony and unity between the different parts. Such a construct is of course, empirically adequate and can be progressively corrected and perfected. However, it is far from the scientists’ ideal of a unified theory.

Van Fraassen also has not tackled the second and therefore, the third issues above. He has also not prescribed a method for constructing successful theories. His conception of progress of science, at the end comes close to that of Kuhn’s notion of growth in human knowledge, namely, a biological evolution; from primitive beginnings to ever more sophisticated ideas which have greater ability to solve practical problems, though, lack any over all goal.

But what about the realists? Can they handle the all-important issue of progress in science in a better way? Popper’s famous notion of verisimilitude can be regarded as an attempt to tackle the first, the second issues. He could not however satisfactorily justify his prescribed methodology and remained content with the intuitive appeal of his proposal.

As for the last issue, Popper answers the how question (in the first sense) by means of his evolutionary model, which van Fraassen has described as "the sole evolutionary parallel that can be drawn for the development of scientific theory." He has answered the why question, once again by means of his notion of verisimilitude: superseding theories are more successful because they contain more interesting truth about
reality.\textsuperscript{129}

With regards to the \textit{how} question (in the sense of prescribing a method for bringing about scientific progress), Popper (due to his commitment to the distinction between context of justification and context of discovery) does not recognize it as a valid issue for philosophy of science and thinks that it falls into the realm of psychology of research\textsuperscript{130}.

Comparing the stand of a minimal realist like Popper and a constructive empiricist on the issue of scientific progress, it appears that, at least at an intuitive level, the minimal realist's account is superior to the constructive empiricist's. The realist has not reduced science to technology. Moreover, he has provided an intuitively appealing, though not fully justifiable, answer to most of the questions concerning the issue of progress.

\textbf{IV.C. Empirical Adequacy and the Problem of Theory Choice}

The third and perhaps the most important shortcoming of neo-instrumentalism in dealing with the actual enterprise of science shows itself in the incapability of this metatheory in accounting for the all-important issue of theory appraisal in science. While the problem of theory construction, i.e., how to initiate a promising line of research is, by and large, absent from almost all of theories of science, (van Fraassen's hand waving at the issue, as we shall see below does not make his position any better than the other common place approaches\textsuperscript{131}), the question of theory appraisal, i.e., how to choose the best theory from among a number of rivals is in the centre of all discussions concerning scientific method. This issue is linked with a catalogue of major topics in philosophy of science including the so-called Duhem-Quine thesis\textsuperscript{132} (which leads to the notorious problem of underdetermination of theory by data\textsuperscript{133}), explanatory power, content comparison, confirmation (evidential support), and extra-empirical values. All these problems are of
course, in one way or another, related to the main problem of rationality, namely, the problem of induction\textsuperscript{134}.

The following example, based on Goodman’s grue \& bleen, serves to show the difficulty which the problem of induction poses for constructive empiricism\textsuperscript{135}. Assume $T$ to be Maxwell’s electromagnetic law\textsuperscript{136}. Compare it with an aberrant theory $T'$ which states that, for all of the actually observed realms the electromagnetic law is as stated by $T$. However, for remote, i.e. as yet unobserved space or time regions, $T'$ (region III in the above chart) departs from $T$, in that $E$ and $B$ (electric and magnetic fields) will be shrunk by a factor of $\sqrt{2}$. Since both $T$ and $T'$ account equally well for the existing data, constructive empiricists cannot choose between them. From their point of view these two theories, and in fact infinitely many others which account, (in ad-hoc ways) for the available data, are equally good, i.e., they are equally empirically adequate.

The inadequacy of constructive empiricism to assist scientists to choose the best theory from among a number of rivals can be discussed in a more general way. The following diagram depicts, schematically, realists’ and van Fraassen’s conception of scientific theories.

$$
\begin{align*}
\text{Realists' Model} & \quad \text{van Fraassen's Model} \\
(T_{r}) & \quad (T_{i}) \\
\end{align*}
$$

$T_{r}$ (theory $T$ interpreted realistically) satisfies two distinctive criteria, namely, empirical adequacy and simplicity (or unity or explanatoriness), but $T_{i}$ (theory $T$...
interpreted instrumentalistically) only satisfies the first one. Now, the point is that empirical adequacy alone cannot help scientists to choose $T_1$ as a viable contender. If someone wants to know what is the difference between $T_R$ and a number of rivals, we would say it is uniquely simple. Whereas in the case of $T_1$, a notion like simplicity plays no rôle. Here, it is only the empirical adequacy which is regarded as the sole criterion for theory choice. However, the snag is that there will be infinitely many 'empirically adequate' theories that fit the observable evidence just as well as $T_1$ does.

Van Fraassen has denied any epistemic rôle for virtues like simplicity or unity or explanatoriness. However, in doing so he has failed to do justice to the realities of scientific investigation: working scientists, as a matter of course resorts to these values (while regarding them as epistemic virtues) in order to tackle the ever-present issue of theory choice. It is here in this context that part of the significance of van Fraassen's appeal to semantic approach and his insistence on the use of models becomes apparent. By using the model approach, van Fraassen is able to represent his instrumentalistically interpreted theory $T_1$ as a plausible theory on a par with $T_R$. Semantic approach enables van Fraassen to borrow the language of realists (models are supposed to depict reality and are not mere linguistic constructs) and conceal the fact that his instrumentalistically interpreted theory only deals with the region below the dotted line in the above diagram. The language of "models" provides van Fraassen with a convenient way of playing down the significance of the rôle of unobservables in theoretical explanations and thus blurring the issues involved in the realist — instrumentalist dispute.

To make his position even more plausible, van Fraassen has tried to replace the notion of "truth" with the notion of "informativeness" and then has related models and
their informativeness to *pragmatic* considerations,

Families of structures, mathematically described, are something quite abstract. This is true even if we take an example like ‘A Newtonian planetary system is a structure consisting of a star and one or more planets and is such that ...’. The nouns are not abstract ... the abstractness consists rather in the fact that the same family of structures can be described in many ways; it is something non-linguistic, ... Objectively, the conceptual anchor of informativeness is logical implication: if $T$ implies $T'$ then $T$ is at least as informative as $T'$. But informativeness for us depends on the formulation process, and the extent to which we can see its implications. This brings us from semantics into the area of pragmatics; our pragmatic reasons for accepting one theory rather than another may include that the former is more easily processable *by us*.\(^{139}\)

This reliance on pragmatics would greatly help van Fraassen, the anti-realist, to account for the fact that scientists, in their preference for certain theories, more often than not invoke extra-empirical virtues\(^ {140}\). Of course, he is not the only faithful advocate of standard empiricism who has resorted to these measures. Hume’s appeal to custom and habits\(^ {141}\), Carnap’s introduction of semantic and later pragmatic considerations\(^ {142}\), and Duhem’s emphasis on the rôle of *bon sens* and on essentially subjective extra-empirical factors in theory appraisal\(^ {143}\), are all significant cases in point.

However, by relying on extra-empirical values of pragmatic nature which are highly subjective and personal, van Fraassen is doing a disservice to his theory of science in two ways. Firstly, he is violating his own highly praised principle of empiricism and therefore undermining his position, and secondly, by allowing non-epistemic considerations in the all-important issue of theory choice, he opens up the flood-gate of relativism and hence jeopardises the possibility of rational theory choice. If all virtues, even the empirical adequacy, as he now says\(^ {144}\), are pragmatic and subject dependent, then there remains no objective way for preferring one’s personal taste to that of others’ and this means a complete capitulation to relativism.

Realists, for their part, uphold the epistemic rôle of simplicity and other similar virtues. However, at the same time they acknowledge the problematic nature of these problems. Popper for example has noted that: "It might also be worth bearing in mind
that, while in science, our quest is for simplicity, it is a real problem whether the world is itself quite so simple as some philosophers think. In fact, it should be admitted that all the better known realist theories of science which subscribe to a strictly empiricist outlook, are unable to justify the rationality of the scientists' appeal to these values. We shall postpone the discussion of this point from realists' point of view and the search for possible remedies for this shortcoming until the final chapter.

V. The Issue of Metaphysics

To uphold his alternative, and perhaps anticipating the unsustainability and eventual collapse of his argument from epistemic modesty, van Fraassen has, at the end, solely relied on a totally different argument, namely, the argument from the undesirability of metaphysics:

...[T]here is also a positive argument for constructive empiricism — it makes better sense of science, and of scientific activity, than realism does and does so without inflationary metaphysics.

The first part of this argument is just an unwarranted claim which begs the whole question. The second part however, is an important one and raises two crucial questions; firstly, whether or not metaphysics is harmful for the advancement of science, and secondly, whether van Fraassen's own theory is free from metaphysical presuppositions and assumptions. We shall take these two questions in turn.

Van Fraassen’s hostility towards metaphysics is in line with his instrumentalist aspirations. He has criticized Duhem for mixing his views with the very metaphysics he insistently wanted to banish from science. However, as we shall argue van Fraassen’s own brand of empiricism is squarely based on metaphysical assumptions.

Van Fraassen, as a loyal empiricist, albeit a more sophisticated one, wants us to
believe that scientific knowledge is actually exhausted by the testable outcomes of our theories. Of course, van Fraassen has not committed Hume's mistake (and the mistake of many of his empiricist successors) in reducing the world to these testable outcomes. He, instead, has appealed to a quasi-Kantian notion of things-in-themselves and has claimed that we can know only what we can observe (i.e. seeing is believing). For the rest, the unobservable entities, he would not deny their existence, but somewhat like Kant, would claim that we should take an agnostic stand towards them. Furthermore, as noted earlier, following a long-standing empiricist tradition, van Fraassen maintains that there is no necessary connection in nature, and that all necessities are merely verbal. His rejection of the objective necessary connections in nature has also led him to replace the notion of laws of nature with that of symmetry which he claims to be more in line with the actual activities of scientists.

To justify his position, van Fraassen has once again made use of his model-theoretic approach. He has transposed all modalities from reality to the models and has furthermore claimed that the generalities exhibited by symmetry-relations pertain to the description of models: "We have indeed found a significant notion of true generality, but not one of necessity. And that significant generality pertains to our description of models, not the structure of nature. The conceptual triad of symmetry, transformations, and invariance does not explicate or vindicate the old notion of law - it plays the counterpoint melody on the side of representation."

Van Fraassen's claims invites a number of observations. In the first place, it should be noted that his rejection of metaphysics leaves his position inconsistent. The very claim that all necessities are verbal is based on a metaphysical assumption about the structure of reality - an assumption which empirically can neither be established nor refuted.
Secondly, it should be asked, how can one who acknowledges the existence of things-in-themselves and yet urges withholding belief as to their natures, emphatically deny the existence of necessary connections in the inner structure(s) of reality? Thirdly, if as van Fraassen is claiming, the symmetry-relations only pertain to the inner structures of the models and not the real structures in the nature, then how does it come about that these relations are successfully applicable to reality? Should we believe in coincidences on a cosmic scale? 

Fourthly, Interestingly enough, the working scientists, contrary to what van Fraassen claims, maintain that symmetries are part of the structure of nature itself. One physicist has put this point in the following way: "Since universe is unique, its symmetries are probably *apriori* given (and appear to us as arbitrary symmetries). There is no compelling reason, I believe, that symmetries other than those determined by the universe as such exist." Moreover, these scientists do not believe that notions such as symmetry would make the notion of "laws of nature" redundant. Here is the verdict of one Nobel Laureate physicist: "The other feature of the substratum, which is the one I wanted more specifically to talk about, has to do with symmetry. The substratum underlying elementary particle physics exhibit a tremendously high degree of symmetry ... And when I say that laws of nature possess a certain symmetry I mean that the laws of nature allow a person to change his frame of reference without changing the form of the laws of nature." 

Lastly, van Fraassen’s theory, is in yet another way indebted to metaphysics, though this dependency has not been acknowledged. In a bid to amend the shortcoming of the earlier empiricists’ system concerning the important issue of providing working scientists with a logic of discovery, van Fraassen has noted that: "... [A]lthough there may not be a logic of discovery, to think of scientific advance as the offspring of romantic
inspiration, dreams, or images seen in the flame is to ignore the systematic aspect of theoretical and experimental exploration. ... New theories are constructed under the pressure of new phenomena, whether actually encountered or imagined. By 'new' I mean here that there is no room for these phenomena in the models provided by the accepted theory. Van Fraassen however, has not explained why a particular problem or set of problems, out of literally infinitely many, props up, captures the imagination or attention of the scientists, and hence ascends to the status of a 'new' problem. Surely, this phenomenon requires rational explanation and cannot be pushed aside as a mere brute fact. The very question of how researchers coordinate their choice of scientific problems and decide to regard certain problems as important or fundamental, is in itself a basic philosophical question which any theory of science worthy of its name, should be able to answer.

While van Fraassen has not produced any reasonable explanation for this intriguing phenomenon, realists, long ago, have argued that one of the most (perhaps the most) important sources and origins of 'new' problems are scientists' preferred metaphysics. In any given period the investigators have chosen those problems as fundamental or important which were related to the dominant metaphysical views of the day or to their own preferred metaphysics.

Comparing van Fraassen's theory of science with that of his mentor, Duhem, one cannot help noticing that the latter, despite his opposition towards metaphysics, was open-minded enough to admit the usefulness and relevance of metaphysics to physical science. He writes:

In a word, the physicist is compelled to recognize that it would be unreasonable to work for the progress of physical theory if this theory were not the increasingly better defined and more precise reflection of a metaphysic; the belief in an order transcending physics is the sole justification of physical theory.
The constructive rôle of metaphysics in the advancement of science, and the inevitability and unavoidability of relying on metaphysical assumptions even for constructive empiricists, not only renders van Fraassen’s major argument from the undesirability of metaphysics untenable, but also make his claim to the superiority of his approach rather hollow. Van Fraassen’s theory of science far from being of any help to scientists is at best an intellectual game with little or no relevance to the actual practice of science, and at worse an utterly distorted image of science which can act as an impediment to the advancement of knowledge.
NOTES (Chapter THREE)

1. Two main aspects of positivists programme which were discredited in the subsequent developments in philosophy of science were (a) to translate the language of science into purely observational terms; and (b) to define syntactically the relation of confirmation and explanation, and show that only science bears these relations to observational evidence. As for classic instrumentalists, their view of theories was one of the main weaknesses of their model. Van Fraassen, as we shall see in the text, has tried to improve upon these undesired aspects. For critical accounts of logical positivists programmes see Schilpp’s volume on Carnap [1963] and F.Suppe [1979].

2. "... I shall argue for an empiricist position and against scientific realism. ... I shall give a momentary name, ‘constructive empiricism’, to the specific philosophical position I shall advocate. The main part of that advocacy will be the development of a constructive alternative to scientific realism ..." (van Fraassen [1980], p.5)

3. ibid, p.73.

4. Van Fraassen is a rather prolific writer and has discussed his ideas in many publications. In this chapter, while I have tried to consult many of these publications, I have mostly concentrated on his three published books, [1980], [1989], and [1991], which are sorts of collections of almost all of his previously published papers, and his [1985] which is an essay in reply to his critics. For a list of van Fraassen’s works consulted for this chapter see the Bibliography.

5. Both Duhem and van Fraassen are aware of the shortcomings of the old-style empiricist approach. Duhem did not endorse positivism in all its aspects. He was against the view that the world can be reduced to the sense-data or logical constructs. We have already noticed that in his endeavour to account for the match between our theories and the world, he introduced the idea of natural classification which was a great step towards realism, (cf. P.Duhem [1954] and S.Jaki [1984])

Van Fraassen, in a similar way, does not endorse all aspects of the older versions of standard empiricism including logical positivism. For example, while he agrees with Carnap that the primary philosophical tools are formal, logical, or mathematical methods, he is not in favour of pursuing a programme of translating theoretical terms into observational ones. Similarly, he does not endorse classic instrumentalists’s view who were regarding theories as useful tools for classification and organizing empirical data.

6. "In this part and the next we shall address issues in general philosophy of science. I mean by this issues which arise when we are not inquiring into the foundations of one of the special sciences, but into the content and structure of scientific theories in general". ([1989], p.216)

For van Fraassen’s views on the aim and structure of scientific theories see his [1980], chs. 3&4, [1986], pp.307-318, [1989] parts 3&4. Duhem has discussed his views in his [1954].

7. "Studies in philosophy of science divide roughly into two sorts. The first, which may be called foundational, concerns the content and structure of theories. The other sort of study deals with the relations of a theory on the one hand, to the world and to the theory-user on the other."

"I shall present three theories, which need each other for mutual support. The first concerns the relation of a theory to the world, and especially what may be called the empirical import of a theory. The second is a theory of scientific explanation, in which the explanatory power of a theory is ... radically context-dependent. And a third is an explication of probability ... ." (van Fraassen [1980], pp. 2 & vii)

8. "science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate." (van Fraassen [1980], p.12, italics in original)

"To develop an empiricist account of science is to depict it as involving a search for truth only about the empirical world, about what is actual and observable. ... scientific activity ... must involve throughout
a resolute rejection of the demand for explanation of the regularities in the observable course of nature, by means of truths concerning a reality beyond what is observable, as a demand which plays no rôle in the scientific enterprise." (Ibid. p.203)

9. "We want theories with great powers of empirical prediction." (van Fraassen [1991], p.3)

"In the most general terms, the end pursued is success. ... Science aims to give us acceptable theories. ... To believe a theory really means to believe that the theory bears a certain relation to empirical reality. That relation must be carefully described of course, but can be given the slogan formulation: it saves the phenomena." (van Fraassen [1989], pp.190-1, emphasis added, See also his [1986], p.308)

As noticed in the second chapter (note 4) the idea that the main task of science is to produce success (by means of predicting and controlling the phenomena), rather than knowledge and understanding (of the underlying causes of these phenomena), lies at the heart of anti-realist philosophies.

10. For Duhem, like van Fraassen, the aim of science is to save the phenomena. He has argued against the view that the aim of science is the discovery of theoretical truth. In his view, if this were so, science would lose its autonomy and be subjugated to metaphysics: "... if the aim of physical theories is to explain experimental laws, theoretical physics is not an autonomous science; it is subordinate to metaphysics." (Duhem, [1954], p.10). However, in his view, science is not subjugated to metaphysics, "... Physics proceeds by a method absolutely independent of any metaphysical opinion." ([1954, p.274]). See also his [1969].

For Duhem, science though independent of metaphysics, is not however, autonomous but subjugated to technology. In other words science is an instrument for making, controlling and predicting. (cf. Agassi [1975], pp.319-20)

11. For van Fraassen, contrary to the classic empiricists, observable and unobservable distinction is not equivalent to exists and does not exist. However, he, in a quasi-Kantian vein, is claiming that although unobservables, may well exist, and be even causally influential, they are out of our epistemic reach; from an epistemological point of view, observables are all that matter. (See [1980], p.18) We will discuss this point at some length below. See section III.

12. Van Fraassen, in a way which reminds one of Popper, has emphasised that philosophy of science should not be conflated with philosophy of language. Thus for example, he says, "The language of science, being a proper part of natural language, is clearly part of the subject of general philosophy of logic and language. But this only means that certain problems can be set aside when we are doing philosophy of science, and emphatically does not mean that philosophical concepts must be one and all linguistically explicated. The logical positivists and their heirs, went too far in this attempt to turn problems into problems about language. In some cases their linguistic orientations had disastrous effects in philosophy of science." ([1980], p.4)

However, as we shall see later, contrary to this sound observation, van Fraassen, unlike Popper, has failed to adhere to his own admonition, and has used one alternative linguistic form in place of the linguistic form invoked by the positivists.

13. "I shall present a view of theories which makes language largely irrelevant to the subject... In what is now called the received view, a theory was conceived of as an axiomatic theory... [whereas in semantic approach] to present a theory, we define the class of its models directly, without paying any attention to questions of axiomatizability, in any special language, however relevant or simple or logically interesting that might be. And if the theory as such, is to be identified with anything at all — if theories are to be reified — then a theory should be identified with its class of models... here models are mathematical structures, called models of a given theory only by virtue of belonging to the class defined to be the models of that theory." (van Fraassen [1989], pp. 310-11)

14. "The idea of literally true has two aspects: the language is to be literally construed; and so construed, the account is true. This divides the anti-realists into two camps. 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." ([1980], p.10)

Van Fraassen's view concerning literal interpretation, is rather vague, if not partly contradictory: in the first line of the above quotation he divides the followers of the literal construal of language of science into
two groups of which one, he claims in the third line, only believes in a proper, and not a literal, construal!

E. MacKinnon [1979, p.519] has commented that the first group holds that statements about theoretical entities are, for the most part, intended to be true, but that the language in which the statements are made is not to be construed literally. In the second alternative, the language of science, it is construed literally but it is claimed that the theories need not be true to be good.

15. "While truth as such is..., according to me, irrelevant to success for theories, it is still a category that applies to scientific theories. Indeed, the content of a theory is what it says the world is like; and this is either true or false." ([1989], p.193)

Duhem, as we remember from the first chapter, maintained that physical theories are neither true nor false but approximate. For logical positivists, theoretical terms were purely syntactical signs without any direct reference to reality.

Van Fraassen’s insistence on the literal interpretation of theories is a significant improvement over the older version of empiricism and has enabled him to distance himself from the view that theories are pure instruments for calculation or economical ways for representing information or mere fictions which would only serve pedagogical purposes but have no real bearing on reality.

16. "...We may ask whether explanation is a pre-eminent virtue in the sense of being required when it can be had. This would mean that if several theories were empirically equivalent, the one which explains most would have to be accepted. Against this idea count all the examples of scientists refusing to enlarge their theories in ways that do not yield different (or further) empirical consequences." (op.cit., [1980], p.95).

"... I wish to dispute three ideas about explanation ... The first is that explanation is a relation simply between a theory or hypothesis and the phenomena or facts, just like truth for example. The second is that explanatory power cannot be logically separated from certain other virtues of a theory, notably truth or acceptability. And the third is that explanation is overriding virtue, the end of scientific inquiry." ([1977b]), p.143.

Van Fraassen has devoted a whole chapter (ch.5, [1980]) to the discussion of pragmatics of explanation.

17. Duhem, discussing the case of underdetermination writes: "No doubt the physicist will choose between these logically equivalent theories, but the motives which will dictate his choice will be considerations of elegance, simplicity, and convenience, and grounds of suitability which are essentially subjective, contingent, and variable with time, with schools, and with persons ..." ([1954], p.288)

18. Logical positivists’ adherence to the principle of empiricism (i.e. all cognitively meaningful knowledge is empirical knowledge) and their dislike of metaphysics, had motivated them to look for ways of emancipating the language of science from the intrusion of non-empirical and non-analytic statements. The success of great mathematicians like Hilbert in providing a syntactical foundation for geometry, encouraged them to carry out the same programme for the language of science; hence their concentration on syntax. However, this programme soon ran into difficulty.

In order to rescue their programme, logical positivists adopted a two tier strategy. (a) They embarked on a long course of introducing more and more powerful logical apparatus (e.g. Carnap’s inductive calculi, modalities, probabilities) hoping that richer logical structures would offer richer and hopefully more faithful descriptions of science while leaving intact the essential positivist philosophical programme for interpreting science; and (b) they introduced pragmatic considerations, as empirically non-cognitive values to settle questions left unsettled by the resources of quantificational logic. Carnap has recollected this episode in his intellectual autobiography:

"I had said that the problems of philosophy or of the philosophy of science are merely syntactical problems; I should have said in a more theoretical way that these are mathematical problems... later we saw that the realm of philosophy must also include semantic and pragmatics ...” (Carnap [1963], p.1, quoted in Skyrms [1984])

Van Fraassen has closely followed Carnap’s example in resorting to pragmatic considerations, although he has not used his syntactic approach.

19. "Theory acceptance has a pragmatic dimension. 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.

... Briefly then, ... the other virtues [e.g. mathematical elegance, simplicity, greatness of scope, completeness in certain respect, unifying power, and explanatory power] are pragmatic virtues. 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." (ibid. [1980], p.88.) See also op.cit. [1985], [1989].

20. Van Fraassen, like the majority of philosophers of science, realist and anti-realist alike, believes in the distinction between the context of justification and the context of discovery and has concentrated solely on the former.

21. "New theories are constructed under the pressure of new phenomena, whether they are actually encountered or imagined. By 'new' I mean here that there is no room for these phenomena in the models provided by the accepted theory. (van Fraassen [1989], p.228-230) See also his [1980], ch.4., and [1985], pp. 269-270.

22. Popper [1972], ch.2 and appendix 1.

23. "Philosophy fathered science, and philosophy aims preeminently and perhaps only to remove wonder, as Aristotle said; but the children have long left the parental home." (van Fraassen [1980], p.96.)

"[R]ealist yearnings were born among mistaken ideas of traditional metaphysics" (ibid. p.23).
"[Constructive empiricism] ... delivers us from metaphysics" (ibid. p.69).
"Kant's Critique ended most decisively the relative placing of metaphysics and science which Descartes had described so strikingly; 'Thus philosophy as a whole is like a tree whose roots are metaphysics, whose trunk is physics, and whose branches, which issue from this trunk, are all the other sciences'. In practice as Kant must have perceived, the progressive separation of modern science from metaphysics was already clear: the trunk and branches grew without much attention to the shape of the roots, if any. Metaphysics was not the discipline on which science drew for its first principles. Instead, quite to the contrary, metaphysical theorizing had implicitly begun to take as its touchstone that it should 'save' the sciences in much the way that the science must 'save' the phenomena. If Kant's Critique succeeded, the relation of philosophy to science was henceforth quite different also from this submission, and of metaphysics there survives only the critical archaeology of ideas to uncover the actual presuppositions in actual history of science, plus the analysis of possible presuppositions that could constitute a foundation for science." (van Fraassen [1989], pp.8-9)

24. "Empiricists and nominalists ... believe that necessity lies in connections of words or ideas only, so ultimately the only necessity there can be lies in the necessity of the consequence." (van Fraassen [1989], p.30)
"... Empiricists have always eschewed the reification of possibility (or its dual, necessity). Possibility and necessity they relegate to relations among ideas, or among words, as devices to facilitate the description of what is actual." (1980, p.3. See also van Fraassen [1977a])

25. "The philosophical study of science as an inquiry into the laws of nature had a presupposition. It assumed that the structure of science is to be understood as a reflection of the structure of nature. I propose that we embark on a study of the structure of science — its theories and models — in itself. The clue, I shall suggest, is this: at the most basic level of theorizing, sive model construction, lies the pursuit of symmetry." ([1989], 233) In part three of the same publication, van Fraassen has offered a rather detailed study of the rôle of symmetries in science.

26. "From the medieval debate we recall the nominalist response that the basic regularities are merely brute regularities, and have no explanation. So here the anti-realist must similarly say: that the observable phenomena exhibit these regularities ... is merely a brute fact, and may or may not have an explanation in terms of observable facts." ([1980], p.24, emphasis added) The notion of brute facts, as we have seen in the case of Mach and other empiricists, is of prime importance for traditional empiricist anti-realists. This is because for them, the goal of science is not
obtaining knowledge of the unobservable aspects of the world, but control and prediction of the phenomena. The previous generations of empiricist anti-realists had at their disposal a set of mathematical statements by means of which they could predict, on the basis of certain sets of statements about the world, another set of statements about the world. As to why these predictions work they could not offer a clue. Instead they would resort to the claim that the deduction of those conclusions from those premises is simply a fact about the nature or a brute fact.

27. *op.cit.* [1980], p.14. In recent years, a number of writers, e.g. I.Hacking [1984] have argued that while our observations are, by and large theory-laden, there are however, a number of low-level observations which can be regarded as theory-neutral. For an argument in defence of theory-ladenness of our language see M.Hesse [1970].

28. Broadly speaking, anti-realists fall into two categories. Traditional empiricists maintain that the ultimate objects of knowledge which are indeed the ultimate furniture of the world, are atomic events or even sense data, (see for example, Hume [1979], Russell [1949/1962, pp.146-7). Such events constitute given facts, and their conjunctions exhaust the objective content of our idea of natural necessity.

The reduction of the ultimate furniture of the world to observable things puts traditional empiricists in sharp contrast to realists who would insist that the unobservable entities are as real as sense data and not reducible to them. Transcendental idealists, on the other hand, have taken a somewhat milder position. They hold that while the real constituents of the world (i.e. the noumena) are forever beyond human comprehension, the objects of scientific knowledge are models, ideals of natural order etc. Such objects are artificial constructs and though they may be independent of particular men, they are not independent of men or of human activity in general. (See R.Bhaskar [1975/78])

29. "If a theory says something exists then a literal construal may elaborate on what that something is, but will not remove the implication of existence." ([1980], p.11)

"According to constructive empiricism, the only belief involved in accepting a scientific theory is belief that it is empirically adequate: that is both actual and observable finds a place in some model of the theory. So far as empirical adequacy is concerned, the theory would be just as good if there existed nothing at all that was either unobservable or not actual." (ibid. p.197)

As Bhaskar (*op.cit.* p.25) has observed according to transcendental idealism, "the object of scientific knowledge are models, ideals of natural order etc. Such object are artificial constructs and though they may be independent of particular men, they are not independent of men or human activity in general."

30. "I use the adjective ‘constructive’ to indicate my view that scientific activity is one of construction rather than discovery ..." ([1980], p.5)

In moving from the process of *discovery* of the objective reality to that of *construction* of it, one’s concern will shift from knowing what there is to how we can know what there is. One will be engaged, not with the process of understanding the independent reality, but with analysing the reality as made or shaped by man in terms of various conceptual schemes. The great difficulty with this attitude, as we shall discuss in various places in this essay, is that it opens the flood-gate of relativism; when the reality itself ceases to be the final arbiter, then all mental constructs and conceptual schemes can claim equal credibility.

Realists, however, as R.Trigg (*op.cit.* [1980]) has argued, would always insist on the objective nature of reality. They would regard the reality as self-subsistent and independent of everything mental (e.g., languages, conceptual schemes, conventions). In realists’ view, science is an attempt to *discover* (and in this sense understand) this independent reality (or rather its physical manifestations). In their view, although it is a truism that we cannot conceive of reality or talk about it without conceiving of it or talking about it, that does not mean that at least some of our conceptions may be true: "The truism that we cannot know objects except under particular descriptions does not conflict with the view that some of these descriptions may be accurate." (ibid. p.xiii)

For realists the ontological issues must be kept separate from either the semantic or the epistemologic ones. M.Devitt [1984, pp.3,4] has suggested three useful maxims, which captures the realists’ insight:

*Maxim 1.* In considering realism distinguish the constitutive and evidential issues.

*Maxim 2.* Distinguish the metaphysical (ontological) issue of realism from any semantic issue.

*Maxim 3.* Settle the [metaphysical] realism issue before any epistemic or semantic issue.
There are two routes for vindicating the argument from epistemic prudence. One, taken by van Fraassen, is based on the difference between epistemic access to the two realms of observables and non-observables. We shall discuss this approach in the present chapter.

The other route is to cite the historical cases of theories which have been empirically highly successful and yet either non-realistic in their scope and structure or wrong in their claims concerning their theoretical posits. Amongst modern anti-realists, it is L.Laudan who has extensively used this argument against scientific realism.

Van Fraassen, of course, is in full agreement with this approach. However, he does not fully exploit it since he is not primarily a historian of science. In reply to his critics (1985) he writes, "Before I begin properly, I would like to mention some developments that I have found sympathetic. First of all, to my shame, I became only belatedly better acquainted with the writings of Larry Laudan. His collection of essays Science and Hypothesis ... gave me real new insight into certain philosophical issues, and I shall draw on it below." (p.245)

A point of clarification should be mentioned here. It seems van Fraassen's understanding of the central notion of empirical adequacy is somewhat at odds with the common usage of this concept among the working scientists and philosophers of science. In his view,

"[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 theory has at least one model that all the actual phenomena fit inside. I must emphasize that this refers to all the phenomena; these are not exhausted by those actually observed, nor even by those observed at some time, whether past, present, or future." ([1980], p.12, underline added.)

Working scientists and realist philosophers on the other hand, maintain that firstly, an empirically adequate theory at most, can account (successfully) for the actually observed evidence in a certain space-time region, plus as yet unobserved evidence in the near neighbourhood of the region of actually observed evidence. Theory's claims concerning this further evidence constitute its successful predictions. Secondly, within the above range of validity of the notion of empirical adequacy, the theory can also account for all counterfactual situations in which phenomena could have happened but have not happened. Beyond this second region, theory will fail in accounting for the phenomena. It is usually this failure which prompts scientists to develop more adequate theories.

The difference between the two positions can be shown schematically.

"I wish merely to remain agnostic about the existence of the unobservable aspects of the world described by science ..." ibid. (1980), p.72

op.cit (1980), 68-9 italics added.
35. The idea of growth of knowledge is one of the central problems of philosophy of science. This notion involves at least four different theses. See section IV-B. below. The issue of growth of knowledge will be further discussed in the next chapter.

36. The surprising success of natural science, which is exemplified in achieving (to a considerable and astonishing extent) the goal of mastering the nature, as defined by Bacon, is in itself an important phenomenon which requires rational explanation.

Realists, as noticed in the text, explain this success in terms of the nearness of the scientific theories to a true description of reality.

Among the realist writers, those who have regarded scientific realism as an empirical thesis have invoked this remarkable success as an inductive argument (the so-called miracle argument), in defence of their own position.

An early account of this argument, as noticed earlier (Ch.2) was produced by J.J.Smart in his [1963]. H.Putnum (while still a realist), following R.Boyd [1973] (See Ch.2), had used this argument as his main means to defend his version of scientific realism. He is the one who has coined the phrase 'miracle argument': "The typical realist argument against idealism is that it makes the success of science a miracle. ... and the modern positivists have to leave it without explanation ... that "electron calculi" and "space-time calculi" correctly predict observable phenomena if, in reality, there are no electrons ... If there are no such things, then a natural explanation of the success of these theories is that they are partially two accounts of how they behave. And a natural account of the way in which scientific theories succeed each other ... is that a partially correct/partially incorrect of a theoretical object ... is replaced by a better account of the same object or objects". (Putnum [1978], pp. 177-8)

"The positive argument for realism is that it is the only philosophy that doesn’t make the success of science a miracle". (Putnum [1978, pp.18-19]

Although the miracle argument is usually attributed to realists like Boyd and Putnum who are in favour of using inductive arguments, one of the earliest formulation of this argument can be found in the works of Popper who is neither an inductivist, nor of the view that realism is an empirical hypothesis. In his [1970b, now chapter 2 of his 1972/79, pp.39-40] Popper, putting forward various arguments for advocating realism, writes: "Although science is a bit out of fashion today with some people, ... We can ... assert that almost all, if not all, physical, chemical, or biological theories imply realism, in the sense that if they are true, realism must also be true. This is one of the reasons why some people speak of 'scientific realism'. It is quite a good reason. Because of its (apparent) lack of testability, I myself happen to prefer to call realism 'metaphysical' rather than 'scientific'. However one may look at this, there are excellent reasons for saying that what we attempt in science is to describe and (so far as possible) explain reality. ... There is a closely related and excellent sense in which we can speak of 'scientific realism': the procedure we adopt may lead ... to success, in the sense that our conjectural theories tend progressively to come nearer to the truth; that is to true descriptions of certain facts, or aspects of reality'.

37. " ... I claim that the success of current scientific theories is no miracle. It is not even surprising to the scientific (Darwinian) mind. 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 one which in fact latched on to actual regularities in nature." (op.cit), [1980], p.40.

38. Van Fraassen [1991], p.7. See also his [1970], [1980], [1987], and [1989].

Van Fraassen has explained his approach in the following terms: "To present a theory is to specify a family of structures, its models: and secondly, to specify certain parts of those models (the empirical substructures) as candidates for the direct representation of the observable phenomena. The structures which can be described in experimental and measurement reports we can call appearances: the theory is empirically adequate if it has some model such that all appearances are isomorphic to empirical substructures of that model." (van Fraassen [1980], p.64.)

Also, Patrick Suppes, whose works van Fraassen has frequently referred to as pioneering and trailblazing in semantic approach (see van Fraassen [1980], [1989], [1991]), has given the following formulation of this new views of theories, in the context of a measurement theory:

"Given an axiomatized theory of measurement of some empirical quantity such as mass, distance, or force, the mathematical task is to prove a representation theorem for models of the theory which establishes, roughly speaking, that any empirical model is isomorphic to some numerical model of the theory. The
existence of this isomorphism between models justifies the application of numbers to things.

We cannot literally take a number in our hands and apply it to a physical object. What we can do is to show that the structure of a set of phenomena under certain empirical operations is the same as the structure of some set of numbers under arithmetical operations and relations. The definition of isomorphism of models in the given context makes the intuitive idea of same structure precise. The great significance of finding such an isomorphism of models is that we may then use all our familiar knowledge of computational methods, as applied to arithmetical model, to infer facts about the isomorphic empirical model. A linguistic formulation of this central notion of an empirical model of a theory of measurement being isomorphic to a numerical model is extremely awkward and tedious to formulate. But in model-theoretic terms the notion is simple and in fact represents a direct application of the very general notion of isomorphism used throughout all domains of pure mathematics." (Suppes [1967], pp.58-9)

39. E.MacKinnon [1979, p.521] has drawn on van Fraassen [1971] and argued for the equivalence of the two approaches: "... van Fraassen [has] developed a compactness theorem which uses semantic entailment to give a systematization comparable to the systematization given by an axiomatic deductive system. ... With this correspondence one can use formal semantic to get the equivalent of an axiomatic system, i.e., a system in which every sentence deducible from the axioms by the rules is in the system. For the semantic equivalent, one defines a system as a set of sentences in a [formal language ] ⊆ closed under semantic entailment. A system that is finitely axiomatizable has finitary semantic entailment. In more intuitive terms, these equivalences show that the semantic notion of a formal system has the same organizing and inference supporting strength as an axiomatic deductive system."

Similarly M.Friedman [1982, p.277] has emphasised that, "... [T]his semantic relation — which, in the terminology of modern model theory ... is just the relation of T being model consistent relative to T; — has a very simple syntactic equivalent ... T is model consistent relative to T; in case every purely existential sentence consistent with T, is also consistent with T;." J.Worrall [1984, pp.71-3] has argued that 1) "[D]espite the impression which van Fraassen seems to try to convey, the primacy of the semantic approach cannot rest on logical consideration. So far as logic is concerned, syntax and semantics go hand-in-hand, they are complementary and not rivals — to every consistent set of first-order sentences there corresponds a non-empty set of models, and to every normal ('elementary') set of models there corresponds a consistent set of first-order sentences." 2) Despite van Fraassen's repeated claim that syntactic approach makes the notion of a theory unacceptably language-dependent, "he too must presuppose some system in which the isomorphism between structures that his approach requires can be stated and established."

40. In his [1970], van Fraassen writes: "There are natural interrelations between the two [the syntactic and the semantic approaches] an axiomatic theory may be characterised by the class of interpretations which satisfy it, and interpretation may be characterised by the set of sentences which it satisfies: though in neither case are the characterisations unique. These interpretations make implausible any claim of philosophical superiority for either approach. (p.236, emphasis added) See also van Fraassen [1971].

41. Edward MacKinnon (op.cit. [1979]) at the end of his analysis of van Fraassen's semantic approach concludes that, "Though [in van Fraassen's model-theoretic approach] ontic commitments are determined by the pragmatics of acceptance, they are manifested through the semantics. The key issues here are meaning, reference, and truth. Since van Fraassen's semantics does not involve referring to the atoms or articles, it might seem to avoid ontic commitments. Here, however, the surface appearance is a misleading guide to the depth ontology. This can be seen by considering the presuppositions involved both in semantic representations and in the treatment of truth. ... The efficacy and strength of the van Fraassen model is most strikingly shown in that it can supply a basis for an interpretation other than the anti-realist one van Fraassen himself favours. This is true even with regard to the rôle of the presuppositions we have been considering. Thus, van Fraassen argued that earlier accounts of scientific explanation were misguided because their starting points were generally deductive structures supplied by logicians rather than the practice of scientists. The scientists practice is to pose a question, "why P, rather than Q?" against a background of accepted, often tacit, presuppositions. (van Fraassen 1977b). This supplies the intended interpretation which a logical econstruction must reconstruct, or at least recognize. Functioning physics so presupposes the reality of particles, atoms and molecules that a reconstruction which omits and then denies them is like Hamlet without the Prince of Denmark." (pp.528-29)
Ronald Giere [1985] also has tried to show the availability of semantic approach for realists. "Empiricism, writes van Fraassen, "could not live in the linguistic form the positivists gave it." His own empiricist image of science, therefore, utilizes an alternative linguistic form for scientific theories. In liberating empiricism from its positivist shackles, however, van Fraassen has unintentionally also set free the realism he abhors." (p.75)

It should be noted that many realist writers who were not in favour of logical positivist approach, have long ago emphasised the rôle and significance of models in understanding the scientific enterprise. Among the better known works in this respect one can refer to the pioneering works of Norman Campbell [1952], M.Hesse [1966], R.Harré [1970].

42. [1980], p.65, [1989], pp.224-5.

43. Van Fraassen is full of praise for Suppes. The following quotations are quite typical:
"The first to turn the tide [of logical positivists' syntactical approach] was Patrick Suppes with his well known slogan: the correct tool for philosophy os science is mathematics, not meta-mathematics. This happened in the 1950s — bewitched by the wonders of logic and the theory of meaning, few wanted to listen." ([1989], pp. 221-2)

"When Patrick Suppes first advocated this sort of picture [i.e. semantic picture] of theories in his studies of mechanics (with the slogan that philosophy of science should use mathematics, and not meta-mathematics) he proposed a canonical form for the formulation of theories." ([1980], pp.65-6)

"Despite certain undoubted successes, the linguistic turn in analytic philosophy was eventually a burden to philosophy of science. The first to turn the tide was Patrick Suppes, ... . Suppes's idea was simple: to present a theory, we define the class of its models directly, without paying any attention to questions of axiomatizability, in any special language, however relevant or simple or logically interesting that might be."
([1991], p.6)

44. "This procedure [i.e., the semantic approach] is common in contemporary mathematics, where Suppes had found his inspiration. ... Suppes and his collaborators sought to reformulate the foundations of Newtonian mechanics, by replacing Newton's axioms with the definition of a Newtonian mechanical system. This gives us, by example, a format for scientific theories." ([1991], p.6. Italics in original)

45. [1980], p.67.

46. See C. Truesdell [1984], pp. 503-579.

47. Truesdell has pointed out that in 1952 he received an article written by McKinsey, Sugar & Suppes entitled "Axiomatic Foundations of Classical Particle Mechanics" for publication in his famous Journal. (This is by the way, the very article which van Fraassen names as a major work in developing the semantic approach.) However, since in his view the joint authors "had achieved virtually nothing of any value or relevance" (as far as the main theme of their article was concerned) he felt obliged to write back to them, mentioning their grave mistakes and advising them not to insist on the publication of the paper. At the end of his reply, Truesdell had informed the joint authors that if they wished, their article could be sent to Professor Hamel, an authority in axiomatic and conceptual foundations of mechanics and a member of the Journal's Editorial Board, for an independent assessment.

Correspondence between the joint authors, the editor and professor Hamel, who entirely endorsed the views of the editor of the journal and backed them up by further detailed analysis of the frequent shortcomings and inconsistencies of the authors' article lasted for almost a year. At the end, the joint authors agreed that their article to be published with the following remarks of the editor: "The communicator is in complete disagreement with the view of classical mechanics expressed in this article. He agrees, however, that strict axiomatization of general mechanics — not merely the degenerate and conceptually insignificant special case of particle mechanics — is urgently required. While he does not believe the present work achieves any progress whatever toward the precision of the concept of force, which always has and remains still the central conceptual problem, indeed the only one not essentially trivial, in the foundation of classical mechanics, he hopes that publication of this paper may arouse the interest of students of mechanics and logic alike, thus perhaps leading eventually to a proper solution of this outstanding but neglected problem." (ibid. p.527)
Having stated the background to the main contribution of Suppes et al to the foundation of mechanics, Truesdell next proceeds to discuss the conceptual defects he sees in the paper and then turns to the subsequent works by a number of writers, namely, Suppes, McKinsey, Stegmüller, Sned and Moulines whom he calls the Suppesians. (Incidently all of these writers are favourably quoted and recommended by van Fraassen in his various publications) At the end of his lengthy and detailed analysis of the works of these authors, Truesdell's has noted that they have, inter alia, the following common features:

1. Despite their claims, these authors, so far, have not been able to produce any viable axiomatic (set-theoretic or model-theoretic or semantic) system for any noteworthy physical theories. The system of axioms introduced in the article of Suppes et al [1953] and have exalted ever since as a paradigm by the Suppesians, has a central flaw and is insufficient to cover the problems treated even in Book I of Newton's Principia.

2. The basic content of the publications of these authors, which is usually heavily disguised in thick formal notations, is a repetition, over and over again, of a number of unsubstantiated and ill-argued claims plus frequent praises of each others and especially Suppes's works.

3. These authors have proved time and again, in their published works, that they do not hesitate in manufacturing the views of other writers or feigning the content of the work they cite, and have no difficulty in suppressing the evidence which is not favourable to their positions. (ibid. pp.568-69)

Truesdell has concluded his lengthy article by the following observation which deserve to be quoted in some length:

"What of all this? McKinsey's last two letters to me in 1952 left me astounded at what then seemed arrogance that deserved no reply. Stegmüller's books and other writings of the younger Suppesians correct this impression. Blathershakes who tie themselves into such knots in public are not arrogant. I began to wonder if they were all just charlatans. This suspicion was fostered by Suppesians' practice of fortifying their self-confidence by citing and quoting each other constantly and almost exclusively, an example of what Sussman & Zahler in demolishing Applied Catastrophe Theory call 'the redundant accumulation supportive statements':

"write a paper stating an unsupported theory, and this will not cause the theory to be believed. Write a second paper in which you refer to the theory of the first as 'well established', and the acceptance level will increase. Let a colleague of yours write a paper referring to your deep work and the level will rise still more. Multiply all this by two hundred, and you obtain something like Catastrophe Theory. By the time the whole thing reaches the average reader, it will be through articles in which the theory is taken for granted. The reader who wishes to pursue the matter further will be referred to more articles in which the same is done. Few will follow the thread all the way. Those few who do will require such an intellectual effort ... that when they reach the end and realize that the thread is ... tied ... only to itself, it will be hard for them to accept the truth, and to acknowledge that their effort has been in vain."

Andreski writes in his delightful Social Sciences as Sorcery,

"One of the most common ploys is a tacit exchange of praise. ... [I]n a field infested by charlatans it ... commonly occurs as an unprincipled collusion which enables the partners to circumvent the customary taboo on boasting."

On the contrary, the exhausting 'intellectual effort' I have put out in trying to penetrate the Suppesian miasma has disabused me of this idea, too, for charlatans claim to do what has not been done before or cannot be done at all: they will not expose themselves to the ridicule deserved by a club of grown men who, earnestly and in voluminous print, drawing only upon each other's works, struggle to learn in their own way what has been widely known for centuries." (ibid. p.569)

48. [1989], p.216.

49. P.Suppes [1961], p.165. Suppes has added that there is however, a difference in their use. But, as we shall discuss in the text, following E.McMullin [1968] and Redhead [1980], the difference in use, is substantial enough to warrant viewing the notion of model as different in these different disciplines. Van Fraassen, has been a bit more cautious in formulating Suppes's thesis, although at the end he comes close to the same position: "The use of the word 'model' in this discussion derives from logic and meta-mathematics. Scientists too speak of models ... In the scientists' use 'model' denotes what I would call a model-type. ... [N]evertheless, the usages of 'model' in meta-mathematics and in science are not as far apart as has sometimes been said." ([1980, p.44)
50. Tarski [1953], p.11, quoted in Suppes [1961], p.163.


54. R.Giere in a recent paper [1994] in defence of model-theoretic approach, has made this point even more explicit. In his view, the use of models would enable scientists to employ the resources in cognitive psychology to understand the structure of scientific theories. Drawing on the example of a pendulum, he has tried to show that how a family of models with various horizontal, vertical and radial structures, would enable both the novices and the experts to find out more about the similarities and differences of real systems: "Identifying the models in philosophers' model-theoretic accounts of theories with the concepts in cognitive scientists' account of categorization suggest a structure to families of models far richer than has commonly been assumed. Families of models may be 'mapped' as an array with 'horizontal' graded structure, multiply hierarchical 'vertical' structures, and local 'radial' structures. These structures promise important implications for how scientific theories are learned and used in actual scientific practice." (p.295)

55. This view, of course, does not clash with realism in either logic or maths, since according to realists, mathematical and logical entities, are supposed to exist in a non-physical realm. The point just mentioned, should also serve to rebut van Fraassen's possible reply that logicians, though using sentences, communicate in terms of propositions which correspond to possible state of affairs and not linguistic entities.

56. There are many examples of the scientific models, which though initially inconsistent, have been upheld by the scientists for their heuristic values. Planck's derivation of the black-body radiation law is a good case in point. The inconsistencies in Planck's model and derivation of his law, was first pointed out by Einstein. However, Einstein was adamant that despite these inconsistencies, Planck's law, is on the right track. In fact, taking Planck's quantization hypothesis, enabled Einstein to produce his own explanation for photo-electric effect. For details see Chapter Six of the present essay. For a fuller discussion of the issue of inconsistency in scientific reasoning see J.Smith [1990] and B.Brown [1990].

57. Van Fraassen [1989], p.8, italics added.

58. "I shall just conclude that it is, on the face of it, not irrational to commit oneself only to a search for theories that are empirically adequate, ... while recognizing that what counts as an observable phenomenon is a function of what the epistemic community is (that observable is observable - to - us)." ([1980], p.19)

"As to what is observable, that ... has both general and special limits. The most general limit is that experience discloses to us no more than what has actually happened to us so far. Hence, any observable structure is one which, in today's scientific world picture, fits inside the absolute past cone of some space-time point. In addition, the structure must be finite; indeed, on a cosmic scale, rather small. These are general limits that I take to apply regardless of who we (the epistemic community) are and which will therefore always remain. ... There are also much more special limits which derive from the *de facto* constitution of epistemic community. What these limits are is accordingly so very much an empirical matter that we cannot be entirely sure that we know what they are and, even less, what they will be. When I give examples, they always presume some suppositions about what we are like. For example, I always assume that we (the epistemic community) are all humans ..." ([1985], p.253). See also his [1989].

59. "... [B]ecause the amount of belief involved in acceptance [of a theory] is typically less according to anti-realists, they will tend to make more of pragmatic aspects." (*ibid.*, p.13)

60. *ibid.* pp.16-17.

61. G.Maxwell [1962]
62. [1980], p.11.


It seems van Fraassen has recently come to admit the validity of the notion of 'observable in principle'. In his reply to critics, van Fraassen himself has appealed to this very notion in order to rebut a realist argument: "Let realists and anti-realists alike bracket their epistemic and ontic commitments and contribute to the understanding of these conceptual enigmas. But, thereafter, how could anyone who does not say credo ut intelligam be baffled by a desire to limit belief to what can at least in principle be disclosed in experience?" ([1985], p.258, italics in original, emphasis added)

64. See G.L.Simon [1984], p.131.

65. This is of course a difference of a higher order between observing (seeing) that and observing (seeing) in ordinary cases such as when an Australian aborigine sees a computer without seeing that it is a computer.


67. A closer look at van Fraassen's claim for the significance of observability reveals a rather interesting point: it seems that he has identified observability with visibility and has not paid due attention to the rôle of other senses. It is noticeable that all his examples of observation are drawn from sight. However, as we have discussed in the text, practising scientists do not seem to be relying on sight as the exclusive source of observability.

68. See, M. Vasyliev and K. Stonowich [1970], pp.25-27. The authors have, among other cases, pointed out that ordinary human beings are totally 'blind' as regards the entire electromagnetic spectrum, save a tiny portion of visible wave-lengths. However, despite this fact, mankind have been able to successfully detect and establish the existence of many other invisible waves in the spectrum.

69. The following quotation, not from a realist but from a self-declared positivist physicist, namely, Heinz Pagels is a good case in point:

"A high-energy accelerator is essentially a microscope — a matter microscope — that is designed to see the smallest things we know exist — the quanta of elementary particle. The principle of the microscope and accelerator is the same. In an ordinary table microscope the beam consists of particles of light — photons — which scatter from the object we want to observe under the microscope. ... When the next generation of accelerators — the Cosmotron — ... began operating in 1952 and 1954 respectively, the modern voyage into matter began. What was revealed by these new matter microscopes was quite beyond the expectations of the physicists. The subnuclear world opened a vast uncharted ocean before the eager experimentalists, who, with their beams of high-energy protons discovered forms of matter never before seen. ([1984], pp.181-5, italics added)

70. This famous paradox can be stated in these terms, "Whenever you remove a grain of sand from a heap of sand, you will still have a heap of sand. ... Then start with a heap of sand and subtract the grains one by one. Eventually the heap dwindles to a singles grain. It must still be a heap!" Quoted from, W.Poundstone [1988], p.94.

71. "To delineate what is observable, however, we must look to science ... for that is also an empirical question. This might produce a vicious circle if what is observable is itself not simply a fact disclosed by theory, but rather theory-relative or theory-dependent. It will already be quite clear that I deny this; I regard what is observable as a theory-independent question. It is a function of facts about us qua organisms in the world and these facts may include facts about psychological states that involve contemplation of theories — but there is not the sort of theory that could cause a logical catastrophe here." ([1980], pp.57-8)

72. See M.Hesse [1970], op.cit.

73. We shall discuss a version of this argument in Chapter Five in the context of Hacking's philosophy of science.
75. P.Achinstein long ago, had emphasized that our classification into "observables" and "non-observables" depends on the purpose of the classification. The contrast between observables and non-observables is a context-dependent contrast. The appropriate response to the question "Is X an observable" is to ask the question to specify the kind of contrast one has in mind. Given that 'X' is used in certain context, which other terms - 'A', 'B', 'C',... - does the questioner take to be "non-observables"? Given this information, a comparison can be made. Consider the term 'virus-stained-and-viewed-under an-electron-microscope' (t). One might classify this term as 'unobservable' relative to the term 'diamond-viewed under an-electron-microscope' since what is 'observed' in the former case is not the virus itself but the heavy molecules attached to it in the staining process. But one might classify (t) as observable relative to the term 'virus-stained-and-viewed-by-X-ray-diffraction' since the electron microscope image is a likeness of the virus in a way in which the X-ray diffraction pattern is not. However the snag is that when the distinction is drawn in this way, then quite apparently where we draw it turns out to be a pragmatic matter, and while this is quite compatible with realist's position, it is not entirely consistent with van Fraassen's view who wants to attach epistemic significance to this distinction. (P.Achinstein [1968], pp. 160-72)

76. The following imaginary dialogue between a realist (R) and van Fraassen (V.F) is devised to dramatise the point discussed in the text and hence makes it clearer:
R: How do we decide what is observable and what is not?
V.F: We ask science.
R: But science issues all sorts of judgments all of which are theory-laden. That is to say, they are infected with the language of unobservables or vague cases.
V.F: We just go for those cases which are clearly testable.
R: If by that you mean pragmatically convenient or practically obtainable, that is fine with us. However, it does not entail any epistemic or ontologic significance. To assume that, is not only logically wrong (since we saw that we cannot sustain a meaningful distinction between 'practically testable' and 'in principle testable') but also deprives scientists from useful and interesting information. Eddington [1939] had suggested that an ichthyologist who trawls the seas using a fish-net of two-inches mesh concludes that the world contains no fish of smaller size. Similarly, an instrumentalistically interpreted science filters out a good deal of relevant information about the vague cases.

77. For the constructive empiricist, theory acceptance is a pragmatic matter. See note 19 above.

78. [1985], p.257.

79. [1985], pp.257-8, italic added.

80. The central theme of all conventionalist philosophies is that a theory may be exchanged for another one by altering the conventions to do with the meaning of basic terms. At every instance, the conventionalist regard the theory only as if it deals with the reality. The theory is regarded as a pattern, drawn not because it reflects reality but because it brings order of some sort into our observations; and this order is conventional because, if it does not produce adequate order, the conventionalist modify the pattern to make it usable. Here we have the central idea of truth as a convention, not as recording something about the world. In effect, theoretical terms on this view have no denotation. Perhaps the best example of this approach can be found in Poincré's philosophy according to which, say the Riemannian geometry is just as valid, as a system, as Euclidean geometry, but in it the term 'straight line' assumes the new meaning of 'geodesic '. For details see J.Wisdom [1987], J.Agassi [1975], J.Giedymin [1982].

81. Van Fraassen, as we have seen, has escaped the pitfalls of the earlier instrumentalistic approaches by admitting truth-value for theoretical terms. Conventionalists, on the other hand, only subscribe to an as if theory of truth and, like traditional empiricists, equate observability with existence. For them the world is as if a certain, currently favourite theory, is true. In this way, terms lose their denotations, because they are always subject to change of their meanings. Clearly, van Fraassen cannot go along this line and at the same time preserve his distinct brand of instrumentalism. However, as we shall see shortly he has made the unfortunate move and has (occasionally) embraced a conventionalist approach.
82. Van Fraassen, has resorted to ‘as-if’ language in explaining the constructive empiricists’ position concerning acceptance of theories. He explicitly declared that: "...adherents of a theory must talk just as if they believed it to be true. (pp.cit. [1980], p.202)

This methodological advice, as we shall see below (note 87), betrays constructive empiricism in yet another way apart from its conventionalistic outcome; it deprives van Fraassen of the distinction he wants to make between accepting a theory in the realist sense and accepting it in the sense of constructive empiricism.

There is also one other undesired outcome for van Fraassen’s recourse to as-if language. G.Rosen [1994] has produced a hermeneutic interpretation of van Fraassen’s constructive empiricism. His main argument is that van Fraassen’s use of as-if language has amounted to producing philosophical fictions or fictionalism in philosophy: "My thought, then, is that CE [constructive empiricism] is best seen as a fiction about science put forward not as true, but rather as adequate to phenomena of scientific activity.... Alternatively, we might say that in putting CE forward in this spirit the author asserts that science proceeds as if CE were a true description of the intentions that underlie it ... This fictionalist reading of constructive empiricism does justice both to the descriptive language of the texts and their probable falsity if taken literally. Much of what van Fraassen says is profitably seen as an effort to make good the claim that science proceeds as if scientists had the intentions he describes – that science “makes sense” when interpreted as the search for empirically adequate theories. It also discloses a pleasing symmetry in van Fraassen’s larger view. Just as science is portrayed as an as-if story about nature, so the author’s own philosophical remarks about science are to be taken as fictional assertions expressing a commitment to this theory as an adequate as-if story about the intentional features of science. (pp.153-54, italics in original, underline added.)

83. The rejection of realist ontological commitment, as noted earlier, renders van Fraassen’s model an undesirable model in the eyes of the scientists. In MacKinnon’s words, quoted earlier: “Functioning physics so presupposes the reality of particles, atoms and molecules that a reconstruction which omits and then denies them is like Hamlet without the Prince of Denmark.” (MacKinnon, op.cit. p.529)

84. “Scientific realism is the position that scientific theory construction aims to give us a literally true story of what the world is like, and that acceptance of a scientific theory involves belief that it is true. Accordingly, anti-realism is a position according to which the aim of science can well be served without giving such a literally true story, and acceptance of a theory may properly involve something less than belief that it is true.” ([1980], p.9)

85. op.cit. [1980], p.197.

86. ibid. p.72.

87. ibid. [1980], pp.68-69), emphasis added.

88. For van Fraassen, acceptance of a theory involves many things including, ”relying on the theory — e.g. to predict the weather or build a bridge; being conceptually immersed in the theory; using the language of the theory to ask and answer questions; being committed to a certain research programme; confronting future phenomena by means of the conceptual resources of the theory; and seeking explanation in terms of supplied by the theory”. ([ibid. [1980], pp.12, 88, 151,2)

Van Fraassen maintains that the believer should (would) convince himself that whatever the theory says about the world should be taken seriously and if the evidence testify against it, so much the worse for the evidence:

"Acceptance of theories (whether full, tentative, to a degree, etc) is a phenomenon of scientific activity which clearly involves more than belief... [it] involves not only belief but a certain commitment... A commitment to confront any future phenomenon by means of conceptual resources of this theory... There are similarities between this sort of commitment and ideological commitment.” (ibid. pp.12-13)

However, it seems if one accepts all of that which van Fraassen’s accepts (in the above sense) about a theory, and provided one is neither insincere nor engaged in self-deception, then one’s position will be practically indistinguishable from the position of those who would accept the theory as true. In other words,
if one accepts all the above points about a theory, then it must be concluded that one does believe in the theory in to to and not just its empirically adequate consequences. This is because there is no way to find out the contrary. It is interesting to see that even van Fraassen himself has observed that, "the breakdown of a long-entrenched, accepted theory precipitate a conceptual breakdown". (ibid. p.202) However, such a conceptual breakdown, is obviously incompatible with the position of a constructive empiricist who maintains that: "So far as empirical adequacy is concerned, the theory would be just as good if there existed nothing at all that was either unobservable or not actual." ([1980], p.197)

89. See Popper [1945/68, 1963/72], vol. 2, also his article in Lakatos and Musgrave (eds.) [1970].

Taking a cautious approach towards the end results of technological / applied activities is a sensible one, since here, refutability could mean a disaster. After all no one would wish a bridge to collapse as soon as it is built, or a plane crash as soon as it has taken off. In epistemological matters however, as Popper has shown, one should exercise a great deal of boldness in proposing new theories, which are falsifiable. As for the metaphysical blueprints and research programmes, although they are not directly refutable, but as we shall discuss in the last chapter, they should be structured in ways to give rise to proper, testable scientific theories.

90. A.Musgrave [1985, ch.9] has argued that: "Epistemological or not, the principle that one might as well hang for a sheep as for a lamb is a pretty sensible one. Given two criminal acts A and B whose risks of detection and subsequent penalties are the same but where A yields a greater gain than B, the sensible criminal will do A ... Suppose the realist tentatively accept a theory as true, while the constructive empiricist tentatively accepts it as empirically adequate. The realist does take a greater risk. But he takes no greater risk of being detected in error on empiricist grounds. So, given strict empiricism (the principle that only evidence should determine theory choice), it seems that we ought as well be hung for the realist sheep as for the constructive empiricist lamb."

J.Worrall [1984, p.59] writes: "But the point of the old maxim is surely that if a little and a lot are both available, and if the penalty is in either case the same, then one may as well go the whole hog and take the whole sheep. It is not that anyone is compelled to take the sheep, but that it seems rather perverse not to. If scientists, when they accept a theory, are to be considered as holding belief (that the theory saves all phenomena), which in the nature of the case cannot be justified, then why debar them from a little extra belief (that the theory is at any rate our present best guess as to the truth), which they anyway seem generally to hold, and which now fails to clash with any general principle?"

91. The nature of this heuristic advantage will be more fully discussed in Chs. 5 and 7. It should be noted that realists’s attitude does not make them more reckless, less sensible in epistemic matter. As we saw above, van Fraassen himself admits that both realists and constructive empiricists do go beyond the available evidence. However, realists would insist on a sensible non-dogmatic attitude towards introducing new conjectures and trying to refute both the established theories and the new proposed ones.

It is worth emphasizing that despite the fact that realist are better placed to claim an extra heuristic value for their position, nevertheless, as we shall discuss in later on, within the framework of empiricism they are not able to justify their claim.


94. For a recent study of the phenomenon of the emergence of new techniques and the impact of it on scientific fields see S.Hall [1992] and R.Crease [1992]. For the effect of technological development on advancement of science see N.Rescher [1978].

95. See N.Rescher, op.cit..

96. As Kuhn [1957] has pointed out one of the consequences of rotation of the earth around the sun is that each of the stars should seem slightly to change its position on the sphere of stars during the course of a year. This motion which is known as parallactic motion (parallax) was not seen even with telescope until 1838. (Kuhn [1957], p.162-3)
97. Needless to say, to substantiate the superiority of realist methodology an additional argument about the extra heuristic power of theories interpreted realistically as opposed to theories interpreted instrumentalistically is required. We have discussed one such argument in chapter 4. Here, I have tacitly relied on the validity of this argument.

98. Van Fraassen himself has noted the significance of this problem, "It is natural and appropriate for a philosopher to ask how sciences proceed, and then why they should proceed in just that way." ([1985], 259). We shall further discuss this important issue in the next chapter in the context of Laudan's philosophy of science.


100. Although Duhem freely employs the term 'evolution', unlike a number of his contemporaries, such as Herbert Spencer, he was not affected by Darwin's ideas in any substantial way. Stanly Jaki [1984, p.376] has noted that "Duhem emphatically rejected the portrayal of human history as seen through the inexorable struggle of the survival of the fittest which leaves no room for purpose". For an account of Duhem's conception of progress in science see B.Baigrie [1992].

101. It was stated in the first chapter that Duhem's views as expanded in the first part and the appendix of his [1954], makes it possible to give a semi-realist reading of his theory of science.


103. In criticising the instrumentalists' theory of knowledge, Popper [1963/1972, pp. 111-114] argues that instrumentalism cannot account for *scientific progress*, since it cannot explain why scientists attempt to develop theories beyond the limits of application. This point as far as classic instrumentalism is concerned is completely valid. However, in the case of Duhem (on a charitable reading) because of the introduction of the idea of natural classification, his (later) version of instrumentalism can be regarded as having come closer to realism and as such is more or less capable of producing an account of the progress of science. On a less charitable reading however, it can be argued that Duhem's ideal theory is just a theory which is instrumentalistically true to the observed facts and has nothing to say about the unobservable realm.


105. See note 37 above.

106. This latter issue can have different meanings. It may be regarded as a demand to explain how scientific progress comes about. In this sense it usually means a demand for describing the external factors which help the replacement of a theory with a more successful one. Alternatively, it may be taken as a quest for explaining why scientific progress takes place or why science is (to the extent it is) successful. Here, what is demanded is an account of the relative strength of the internal dynamism of the successor theory in comparison to its predecessor. Finally, it may be interpreted as asking for a prescription: how can scientific progress be achieved.

In his discussion of the problem of induction, Popper [1954/68, 1974] has observed that: "... [W]e must beware lest our theory of knowledge proves too much. More precisely, no theory of knowledge should attempt to explain why we are successful in our attempts to explain things." (Popper [1983], p.116, italics in original) Regardless of correctness or otherwise of Popper's observation, it should be clear that the point raised above is not similar to what Popper is prohibiting.

107. By *elegance*, I mean extra-empirical virtues such as simplicity, notational economy, unificatory power and the like.

108. Kepler's preference for the Copernican system, Faraday's preference for his field theory, and Einstein's refusal to endorse quantum theory despite its apparent success, are all good cases in point.
109. "Correlative to discussions of the relation between a theory and the world, is the question what is it to accept a scientific theory. This question has an epistemic dimension (how much belief is involved in theory acceptance?) and also a pragmatic one (what else is involved besides belief?). ([1980], p.4)

110. We have discussed the repercussions of van Fraassen’s appeal to pragmatic virtues at the end of this section.

111. The idea of verisimilitude was first introduced by Popper in his [1962b] and [1963/72] and later on was expanded in his [1972/75]. Invoking the two notions of truth-content and falsity-content of a theory, he put his basic idea in these terms, "Assuming that the truth-content and the falsity-content are comparable, we can say that t₂ is more closely similar to the truth, or corresponds better to the facts, than t₁, if and only if either, a) the truth-content but not the falsity-content of t₂ exceeds that of t₁, b) the falsity-content of t₁, but not its truth-content, exceeds that of t₂." ([1963], p.233, original text is in italics).

The snag with this intuitive idea is that apart from the difficulties involved in making clear logical sense of the basic notions of truth-content and falsity-content, it can be shown that neither cases of a) and b) are realizable. That is to say, if as in the case a) t₂ has more true consequence than t₁, then one can combine any statement belonging to the extra truth-content of t₂ (e.g., p) with any of the statements which belong to the falsity-content of t₁ (e.g., q) to produce a false statement (p&q) which belong to t₂ but not to t₁. This is a violation of a). Similarly, if t₁ has more false consequence than t₂, then by combining any two false statements (e.g., s, r) which are found in t₁ but not in t₂, one can produce true statements (e.g., s → r) which belong to t₁ but not t₂. Hence a violation of b). BJPS [1974], vol.25, no.2 contains a series of criticisms of Popper’s notion of verisimilitude.

112. The covering law model of explanation was popularized by Hempel and Oppenheim in an influential joint paper which published in 1948. The idea of course was not original. Popper in his German edition of the Logic of Scientific Discovery [1934] had already discussed this approach. According to this model of explanation, as Hempel and Oppenheim put it while commenting on the case of an oarsman’s observation that his oar is 'bent', “the question ‘Why the phenomenon happen? ’ is constructed as meaning ‘according to what general laws, and by virtue of what antecedent conditions does the phenomenon occur?’” The deductive pattern of explanation of a phenomenon takes the following form:

L₁, L₂, ... Lₖ General Laws
C₁, C₂, ... C Statements of Antecedent Conditions
E Description of Phenomenon

Van Fraassen has tried to develop a pragmatic model of explanation to replace the traditional covering law model. See his [1980], ch.5. For an assessment of van Fraassen’s model’s of explanation see P.Kitcher and W.Salmon [1987].

113. E.Nagel [1961, ch. 11] has given the fullest account of the idea of reduction according to the logical positivists. Nagel has distinguished two types of reduction. First, the homogenous reduction, in which a law subsequently is incorporated into a theory which utilizes substantially the same concepts that occur in the law. The absorption of Galileo’s law of free fall into Newtonian mechanics is a case in point. The second type of reduction consists in the deductive subsumption of a law by a theory that lacks some of the concepts in which the law is expressed. In many instances the law subsumed refers to macroscopic properties of objects whereas the reducing theory refers to micro-structure of the objects. An example to which Nagel devote some attention is the reduction of classical thermodynamics to statistical mechanics.

The idea of reduction served logical positivists as the main conceptual framework for explaining the notion of progress in science: Science establishes theories which, if highly confirmed, are accepted and continue to be accepted relatively free from the danger of subsequent disconfirmation. The progress of science consist in the extension of such theories to wider scopes (the first form of theory reduction), the development of new highly confirmed theories for related domains, and the incorporation of confirmed theories into more comprehensive theories (the second form of reduction). (cf. F.Suppe [1979]

It should be born in mind that for logical positivists theories were basically mathematical construct without truth-value. Van Fraassen does not want go along this line.

115. Max Planck [1909], (p.25)


118. H. Post [1968] has put this point in the following way: "... [N]o theory that ever 'worked' adequately turned out to be a blind alley. Once a theory has proved itself useful in some respect, has shown its semantic simplicity or explanatory power, it will never be scrapped entirely. Even the phlogiston theory had features that were useful scientifically in its day, and those features translate smoothly into present theory." (p.327) We shall further discuss this point in the context of Laudan's philosophy in the next chapter.

119. "Thus we accept that the task of science is the search for truth, that is, for true theories. ... Yet we also stress that *truth is not the only aim of science*. We want more than truth; what we look for is *interesting truth* — truth which is hard to come by. And in the natural sciences (as distinct from mathematics) what we look for is truth which has a high degree of explanatory power, in a sense which implies that it is logically improbable truth." (Popper [1963/1972], p.229)

120. Conjunction is one of the essential properties of truth. If T and T' are both true theories, their conjunction T" = T & T' is also a true theory. The same does not apply to theories which are only empirically adequate. Invariance under conjunction is only one, out of many, characteristics which distinguishes truth from other epistemic notions such as empirical adequacy. These other characteristics are as follows: "1) Truth is a primary dimension of assessment for beliefs and sentences that can express or report beliefs; 2) If x is true, then x will under favourable circumstances command convergence (agreement), and the best explanation of the existence of this convergence will either require the actual truth of x or be inconsistent with the denial of x; 3) For any x, if x is true then x has content; and if x has content then x's truth content cannot simply consist in x's being itself a belief, or in x's being something believed or willed or ...; 4) Every true belief (every truth) is true in virtue of something." (See D. Wiggins [1991]) We shall return to the topic of exclusive characteristics of truth in Ch.5.


122. Differences between pure and applied science and the two with technology, has escaped the attention of many of philosophers of science. While anti-realist tend to blur these distinction, realists would insist that, although overlap between the boundaries of these disciplines is inevitable, keeping them apart is important for analysis and critical evaluation of the developments in each of these fields. The following description seems to be a reasonable characterisation of each of these fields, "The distinction between pure and applied science seems too trivial to draw, since applied science, as the name implies, *aims* at practical ends, whereas pure science does not. There is an overlap to be sure, which is known as fundamental research and which is *pure* science in the short run but applied in the long run; that is to say, fundamental research is the search for certain laws of nature with an eye to using these laws. Still, this overlap shows that though the distinction is not exclusive it is clear enough. ... [T]echnology includes, at the very least, applied science, invention, implementation of the results of both applied science and invention, and the maintenance of the existing apparatus, especially in the face of unexpected changes, disasters, and so forth." Agassi [1975, p.282]. See also Agassi [1980].

We shall further discuss the case of equating pure science and technology in the subsequent chapters.

123. To restore virtues like simplicity and unity, van Fraassen has appealed to pragmatic considerations. But as discussed in the text, this tactic cannot help him out of the predicament.

124. Van Fraassen has introduced a brief account of theory construction in general though. We shall discuss his proposal in the last section (Section V below).

125. See Ch.2.
126. Popper’s view on verisimilitude was severely criticised by a number of writers (see BJPS [1974]). However, despite these effective criticisms, which succeeded in showing the inadequacy of Popper’s formal approach to the issue of truth-likeness, his intuitive idea has remained appealing. Popper himself, has emphasised this point. In his [1972/79, pp. 371-2] he writes: "... I am optimistic concerning verisimilitude. ... The idea seems to me a clear one, even if definition in purely logical terms should present great difficulties. The sequence of theories such as K (Kepler’s theory), N (Newton’s theory of gravitation), E (Einstein’s theory), seems to me to illustrate sufficiently what is meant by an increase in explanatory power and informative content; and if the increasingly severe tests of the successor theories should lead to positive results, these positive results seem to me to provide evidence favouring the conjecture that this is not an accident, but due to increased verisimilitude. ... I do think that we should not conclude from the failure of attempts to solve the problem that the problem cannot be solved. Perhaps it cannot be solved by purely logical means but only by a relativization to relevant problems or even by bringing in the historical problem situation”.

Despite Popper’s failure to provide a coherent account of verisimilitude, at present it constitutes the subject matter of a number of research programmes. In recent years a number of writers have tried to produce better formal approaches for representing Popper’s initial intuition. See especially G.Oddie [1986] and I.Niiniluoto [1987].

127. In his [1975/81] Popper writes: "From a biological or evolutionary point of view, science, or progress in science, may be regarded as a means used by the human species to adapt itself to the environment: to invade new environmental niches, and even to invent new environmental niches. ... We can distinguish between three levels of adaptation: genetic adaptation; adaptive behavioural learning; and scientific discovery which is a special case of adaptive behavioural learning. ... On the scientific level, the tentative adaption of a new conjecture or theory may solve one or two problems, but it invariably opens up many new problems; for a new revolutionary theory functions exactly like a new and powerful sense organ. If the progress is significant then the new problems will differ from the old problems: the new problems will be on a radically different level of depth. ... This, I suggest, is the way in which science progresses." (pp. 81, 83)

128. “Karl Popper [1981] correctly outlined, as far as I can see, the sole evolutionary parallel that can be drawn for the development of scientific theory.” (Van Fraassen [1985], p.261)

129. Popper [1963/72].

130. cf. Popper [1959/68], p.31.

131. This issue, which is closely related to the issue of providing a logic for scientific discovery, is absent most of the better known theories of science. This is indeed a serious defect, and as we shall discuss in the last chapter it has been responsible (to a great extent) for the impoverishment of many of the theories of science and the irrelevance of these theories to the actual scientific enterprise. Van Fraassen’s rather casual proposal for theory construction is discussed in the next section.

132. As pointed out in the first chapter, P.Duhem in his [1954, pp.199-200] put forward the idea that the physicist can never submit an isolated hypothesis to experimental test. Quine in his [1953] borrowed Duhem’s idea, put it into the context of his own philosophy and stated that "our statements about the external world face the tribunal of sense experience not individually, but as a corporate body." (p.41) This assertion which has subsequently come to be known as the Duhem-Quine thesis or ‘holism’, has extensively been used (or one might say misused) by almost all of sociologists of science, as well as a good number of philosophers.

133. See Newton-Smith [1978].

134. The so called problem of induction in fact, is not just one single problem but a cluster of inter-related problems. At the highest level of generality, the problem can be divided into the practical (concerning everyday decisions pertaining to actions or choices) and the theoretical (concerning knowledge garnering) ramifications. Among the different forms and formulations of the theoretical version of problem, the following three, are perhaps the more significant ones, namely: i) the problem of validating our theories on
the face of available data (is it rational to go beyond the available data?), ii) the problem of choosing the best theory from among a number of rival candidates which all account well for the existing evidence (underdetermination of theory by data), and iii) the problem of learning from the past experience (can one form an opinion concerning the future course of events on the basis of the past regular occurrences of events?). In all these cases and the other formulations of the problem of induction, the issue at stake boils down to the question of whether we are rationally entitled to make/adopt any universal view (either as a basis for action or conjectural knowledge) about the reality on the grounds of a limited evidence.

Ever since Hume in 1751, in pessimistic remarks on the pages of *An Inquiry Concerning Human Understanding*, expressed his critical opinion on the value of ‘inductive reasoning’, inductivists have tried, in many ways, to justify the validity of this type of reasoning. These attempts, as the vast literature on the subject reveals, have not produced the promised results.

For a recent criticism of the inability of empiricists, in general, and van Fraassen, in particular, to solve the problem of induction see N. Maxwell [1993].


136. Maxwell’s equations in differential forms are as follows:

\[
\begin{align*}
V \cdot E & = \rho/\varepsilon_0 \\
V \cdot B & = 0 \\
V \times E & = -\partial B/\partial t \\
V \times B & = \mu_0 (J + \varepsilon_0 \partial E/\partial t)
\end{align*}
\]

In the above equations \( E \) and \( B \) are the electric and magnetic fields respectively. \( \rho \) is the charge density and \( J \) the current density. The value of these two quantities will be zero in free space. \( \rho_0 \) and \( \varepsilon_0 \) are permitivity and permeability constants.

137. Steven Weinberg, one of the co-discoverers of weak-nucleic force, in his "Is Nature Simple?" has noted that: "We like to think that nature is fundamentally simple, that is governed by simple general laws. This may not be true, but it seems wise to assume so, as a guide to our work. Therefore, when we look at the current state of physics, and judge how simple nature seems in the light of our current understanding, we are not testing so much whether nature itself is simple, but how close we have come to working on the level of the fundamental laws of nature."

138. "... when we try to evaluate theories on the basis of their record even in the most assiduous testing, the ranking will reflect not only adequacy or truth, but also informativeness." ([1989], p.231)


139. Van Fraassen [1980], Chs. 4 & 5. Pragmatic measures, as we shall see shortly, are not the only non-empirical values used by van Fraassen. Tacit metaphysical assumptions are also exploited by him. However, since the choice of these assumptions are made on pragmatic grounds, ultimately in his view even these assumptions are part of the greater topic of pragmatics.

141. "Custom, then, is the great guide of human life. It is that principle alone which renders our experience useful to us, and makes us expect, for the future, a similar train of events with those that have appeared in the past. Without the influence of custom, we should be entirely ignorant of every matter of fact beyond what is immediately present to the memory and senses." (Hume [1777/1979], p.44-45)

142. cf. note 18 above.

143. "Pure logic is not the only rule for our judgment; certain opinion which do not fall under the hammer of the principle of contradiction are in any case perfectly unreasonable. These motives which do not proceed from logic and yet direct our choices, these 'reasons which reason does not know' and which speak to the ample 'mind of finesse' but not to the 'geometric mind,' constitute what is appropriately called good sense'. op.cit. Duhem [1954], p.217. See also ibid. p. 288 and passim.
144. Van Fraassen has introduced his new, and one should say significant, move towards relativism in the form of a qualification to his previous views: "... [T]his is a qualification of the description I gave in The Scientific Image of the theoretical virtues. My gloss on "we want informative theories" was that we want empirical adequacy, which I characterized as independent of pragmatic factors. The qualification is that, as with other virtues characterizable semantically, whether they are perceptible depends on the formulation of the theory we actually possess." [1991], p.8.


146. op.cit. [1980], p.73.

147. op.cit. [1989], p.225.

148. No philosophical view, no matter how anti-metaphysical can be devoid of metaphysical elements. Aristotle once said that we must philosophize if only to avoid philosophizing. What happened to the Vienna circle is a good case in point. During 1924-26, members of the circle were reading and discussing various points in Wittgenstein's Tractatus, which they regarded to contain highly useful and supportive ideas for their cause, namely, the elimination of metaphysics. During these meetings Neurath used to make frequent interjections, "metaphysic!" much to the irritation of Schlick who finally told him he was disrupting the proceedings too much. Hans Hahn, as conciliator, suggested to Neurath just to say "M" instead. After much humming, Neurath made another suggestion to Schlick, "I think it will save time and trouble if I say 'non-M' every time the group is not talking metaphysics". (Quoted from Otto Neurath: Empiricism and Sociology, pp.82-3, edited by M.Neurath & R.S.Cohen [1973])

The following confession by G.Ryle, in his autobiographical notes, is not untypical among many philosophers who have been sympathetic to positivist programme;

"There was a second quite unintended result of Logical Positivism. For by jointly equating Metaphysics with Nonsense and Sense with Science, it raised the awkward question 'where then do we anti-nonsense philosophers belong? Are the sentences of which Erkenntnis itself is composed Metaphysics? Then are they Physics or Astronomy or Zoology? What of the sentences and formulae of which Principia Mathematica consists?" We were facing what was in effect the double central challenge of Wittgenstein's Tractatus Logico-Philosophicus and the single central challenge of his future Philosophical Investigations. Neurath, Schlick, Carnap, Waismann, and for us above all others, Ayer had undeliberately raised a problem the solution to which was neither in the Logical Syntax of Language nor yet in the Tractatus. We philosophers were in for a near-lifetime of enquiry into our own title to be enquirers. Had we any answerable questions, including this one?" Ryle's notes are reprinted in O.P.Wood & G.Pitcher (ed) [1970]. p.10.

G.Bergman [1954] has discussed the metaphysical implications of logical positivism. See also Agassi [1975].

149. cf. Ch.2.

150. "I wish merely to remain agnostic about the existence of the unobservable aspects of the world described by science ..." ( [1980], p.72)

Van Fraassen's position is not exactly like that of Kant. Kant was of the view that the world of noumena is such that we can say nothing meaningful about it, except that it exists. Van Fraassen, on the contrary is ready to talk about the unobservable entities, however he does not want to commit himself to their existence.

151. Hume [1777/1979] writes, "Upon the whole, necessity is something, that exist in the mind, not in the objects; nor is it possible for us ever to form the most distant idea of it, conside'd as quality in bodies. Either we have no idea of necessity, or necessity is nothing but that determination of the thought to pass from causes to effects and from effects to causes, according to their experienced union." For van Fraassen's view see notes 24 and 25 above.

152. "When philosophers discuss laws of nature, they speak in terms of universality and necessity. ... Scientists, however, do not speak of law in terms of universality and necessity, but in terms of symmetry, transformations, and invariance." van Fraassen [1989], p.2. See also parts three and four of his [1989] for a thorough account of symmetry in science.
155. Similarly, drawing on a point first made by Bhaskar [1975/78] against Humean notion of constant conjunction, it can be argued that van Fraassen’s replacement of the notion of laws of nature by principles of symmetry, invariance and conservation does not render his system immune from similar criticisms. Bhaskar’s point concerns a distinction between open systems (i.e. nature) and the closed or isolated ones (i.e., controlled environment of laboratories). The difference between these two systems produces a great epistemic predicament which H.Poincaré [1902/1952, p.98] has succinctly summarized in the following way, “On the one hand laws are truths founded on experiment and approximately verified so far as concerns isolated systems. On the other hand, they are postulates to the totality of the universe and regarded as rigorously true.” Poincaré’s predicament can be paraphrased in terms of principles of symmetry, invariance and conservation. These principles are often arrived at and confirmed in the controlled situations, where the conditions for the applicability of ceteris paribus clause can be more or less observed. However, scientists have no qualm over the validity of these conjectures in the open systems (nature at large). Now, if as van Fraassen would have us to believe, there are no necessary connections in nature, then he should provide a satisfactory account as to why the basic scientific principles, verified in closed systems, are also valid for the open system. 

Van Fraassen has introduced two main arguments based on symmetry considerations, namely, “the structurally similar problems must receive structurally similar solutions”. (op.cit. [1989], p.10, & p.236) And, “An asymmetry must always come from an asymmetry”. (ibid. p.239) In view of the above points, it can be argued that these principles are the variants of the metaphysical principle of “the uniformity of nature”, and hence applicable to actual physical cases.

156. P.Roman [1966], p.368.


159. It should be noted that here, we are only interested in the so-called internal appraisal of the problem of “the emergence of new problem”. Surely, external factors like the availability of funds and political and social pressures also do have important rôles in encouraging the scientists to pursue certain lines of research.

160. The literature on this issue is relatively rich. Popper’s pioneering works now published in his [1963/72] and Agassi’s [1957b/1975] article, as a follow up of Popper’s main points, are among the best sources.

It should be emphasised here that van Fraassen’s alleged explanation of theory construction does not fare better than the accounts provided by his inductivist predecessors, whose theory is being dubbed by Popper [1972/79] as the bucket theory of mind. There are many points left unaccountable by this explanation. For example, it is not known why for more than 1000 years those who were using the Aristotelian world-view did not feel the pressure of new phenomena. In contrast, it is not clear why a scientist like Faraday who had at his disposal a successful theoretical framework, attempted to create a new theoretical scheme which remained, for a long time, a less plausible or less developed account than its more established rival. As noticed in the text, a main reason for theory construction is the change in metaphysical out-looks of the scientists. Within the framework of the new metaphysics, phenomena are seen under different light and new and fresh questions are being raised. The attempts to answering these new questions lead to formulation of new theories. Van Fraassen, however, due to his hostility towards metaphysics, cannot endorse this view.

In this context, it is interesting to note that van Fraassen, in his recent publications has tried to counter the charge of negligence of metaphysics, by qualifying his previous rather unqualified criticisms of metaphysics. His new stand is that he is opposing mostly the pre-Kantian metaphysics and then only if practised after Kant (van Fraassen [1989], p.viii.). Nevertheless, despite this qualification, he has not even attempted to produce criteria for distinguishing the sort of metaphysics which can be at the service of science from the rest.

161. op.cit. [1954], p.335, italics in original.
162. We shall discuss the significance of metaphysical blueprints for the advancement of more fully in the last chapter.
Chapter Four
The Growth of Knowledge, Reticulation and Normative Naturalism

I. The Idea of Progress

Popper once said that "the central problem of epistemology has always been and still is the problem of the growth of knowledge. And the growth of knowledge can be studied best by studying the growth of scientific knowledge".

This observation gives rise to a number of questions, each at the heart of contemporary philosophy of science and closely related to debates over realism versus anti-realism. One such important question is if scientific knowledge grows, then what is the proper rational criterion for measuring or evaluating its growth? Another basic question is; if scientific knowledge is a desirable thing to achieve, then, how can we rationally improve our (scientific) knowledge as well as our knowledge-garnering ability?

Philosophers of science throughout this century have devoted much time and energy to tackling these fundamental issues. The result has been a plethora of competing approaches. For example, in the case of the first issue, while realists, by and large, have accounted for the growth of (scientific) knowledge in terms of acquiring evermore (theoretical) truth about reality, cutting the world at its joints by means of our theories, or striving, as it were, towards a unified theory of everything, anti-realists, by and large, and to varying degrees, have tried to develop alternative criteria for progress of science, and its rationality. For older generations of anti-realists like logical positivists, growth of knowledge consisted (exclusively) in the growth of empirically verifiable (scientific) knowledge. In their view progress of science comes by means of reduction of highly successful theories into more general, more successful (i.e., more verifiable) theories. Scientific progress is therefore, achieved by cumulation, and there is, in principle, a
reasonable degree of correspondence between the superseded and superseding theories\textsuperscript{11}.

Some other anti-realist philosophers have rejected the growth of knowledge and have denied the rationality of science. Feyerabend, and radical sociologist of knowledge are among the better known representatives of this group of anti-realists\textsuperscript{12}. Still others have tried to introduce surrogates for truth, and have suggested alternative criteria for progress of science other than verisimilitude. Among the more influential older exponents of this approach one can name Toulmin\textsuperscript{13}, and Lakatos\textsuperscript{14}. As for more modern anti-realists, in Chapter three we noticed that neo-instrumentalists had difficulty in accounting for the progress of science. For them, the growth of knowledge would reduce to technological growth and the proliferation of ever more empirically adequate theories.

As for the second question, how to improve our knowledge and knowledge-garnering ability, a similar diversity of approaches can be exhibited\textsuperscript{15}. Thus for example, realists like Popper have advocated a methodology of conjectures and refutations and acquiring and improving knowledge through error elimination\textsuperscript{16}, yet they have rejected the idea of a logic for scientific discovery\textsuperscript{17}. Logical positivists, like Popper, were not interested in introducing methods for scientific discovery. However, unlike Popper whose interest was mainly in tackling cosmological questions, they were trying to improve knowledge through ‘logical analysis’ of language by giving precision to vaguely understood intuitive concepts and thereby solving philosophical perplexities and conflicts. In order to purge pseudo-knowledge from the realm of genuine knowledge, they adopted the so-called principle of verification, from Wittgenstein’s Tractatus and developed a verificationist methodology\textsuperscript{18}.

Some anti-realist philosophers of science like Lakatos have chosen to look at the scientific elite for discovering the best methodological rules for advancing knowledge\textsuperscript{19}.
Others, like Quine and his followers have asked for the naturalization of epistemology (methodology), a move which in effect would reduce epistemology to a sub-branch of descriptive psychology and thus deprives methodology of all its normative force. Quine, of course has not been the only writer who has had misgivings about methodology. Wittgenstein, for instance, was of the view that the search for the rules governing any practice, scientific or otherwise, is self-defeating or at best useless, since one needs rules telling one how to follow rules, and still higher-order rules for to follow those rules, \textit{ad infinitum}. Michael Polanyi maintained that scientific knowledge is governed by tacit knowledge and personal know-how which cannot be represented in the form of neat methodologies. Feyerabend was preaching that all methods are equally good (or rather equally bad).

One modern anti-realist writer who has, since the early 1970s, persistently worked to produce viable solutions to the major questions mentioned above, is Larry Laudan. Laudan has criticized both realist and relativist methodologies for not being able to tackle the fundamental issues of the progress, rationality and aim of science.

In what follows, we shall assess the force of Laudan’s criticisms of the realists’ position and appraise the credibility of his own alternative proposals, dealing mostly with those aspects which are more fundamental to his overall system.

II. Laudan’s Attacks on Realism

Laudan has directed his attacks on realism against two well-known realist interpretations of science, one advocated by Popper, and the other which has been chiefly defended by Boyd and Putnum among others. In this section we shall critically examine Laudan’s arguments against the above positions (II.C & II.D). We start by brief expositions of Laudan’s objection against each of these positions (II.A & II.B).
II.A. Laudan’s Misgivings about the Realist’s Aim

Laudan’s philosophy of science is, to a large extent, a point by point reply to Popper’s philosophy of science. His criticism of Popper’s methodology however, boils down to two major points. Firstly, he has cast doubt on the very aim Popper has defined for science and has objected to Popper’s approach to the issue of progress of science. Secondly, he has rejected Popper’s view concerning the normative nature of methodology and has produced a naturalistic methodology. In this section we shall only deal with Laudan’s first objection, leaving details of his second point to section III.B.

Perhaps the main feature of Popper’s version of realism is, as noted above, his idea of approaching the truth about the natural world (of which we are a part) via a (possibly infinite) series of conjectures and refutations. Popper maintains that achieving theoretical knowledge about reality is the most important goal of both science and philosophy:

All science is cosmology, I believe, and for me the interest of philosophy, no less than of science, lies in its bold attempt to add to our knowledge of the world, and to the theory of our knowledge of the world.26

... [T]he fact is that we too see science as the search for truth, .... Indeed, it is only with respect to this aim, the discovery of truth, that we can say that though we are fallible, we hope to learn from our mistakes. It is only the idea of truth which allows us to speak sensibly of mistakes and of rational criticism, and which makes rational discussion possible — that is to say, critical discussion in search of mistakes with the serious purpose of eliminating as many of these mistakes as we can, in order to get nearer to truth.27

Laudan, on the contrary, since his earliest writings, has displayed a conviction as to the unobtainability of theoretical truth28. In this, he has shared the view of a number of anti-realist philosophers who are influenced by some continental thinkers29, and has embraced a now dominant trend among many present-day writers of both anti-realist and neo-realist denominations30. Laudan maintains that the aim defined realists does not allow them to make rational sense of science. He has observed that:

Given the notorious difficulties with notions of ‘approximate truth’ — at both the semantic and epistemic levels — it is implausible that characterizations of scientific progress which view evolution towards greater truth-likeness as the central aim of sciences will allow one to represent science as a
rational activity. If rationality consists in believing only what we can reasonably presume to be true, and we define ‘truth’ in its classical, non-pragmatic sense, then science is (and will forever remain) irrational.

In Laudan’s view, historical records give rise to an argument which he has dubbed the historical gambit (otherwise known as the pessimistic induction or the meta-induction). Briefly put, it states that since our past successful theories have proved to be false, this provides good grounds for holding that our present successful theories will most probably be falsified. Thus we cannot dogmatically claim that scientific progress has been or will be towards truth, especially, since the very notion of truth, is fraught with seemingly unresolvable difficulties: "If scientific progress consists in a series of theories which represent an ever closer approximation to the truth, then science cannot be shown to be progressive." What strengthens the force of the historical gambit, according to Laudan, is the fact that history of science shows scientific progress is not cumulative, that is to say, the successive theories do not retain all the success of their predecessors, and the gains of scientific knowledge are always accompanied with losses.

Laudan has summarised his objections concerning the notion of truth, in the following way:

To retain the view that science aims at presumptively true theories, in the face of the admission that we would not know how to recognize a true theory if we had it, is to render science an irrational enterprise; for on any coherent account of what rational behaviour is, it is irrational to adopt a goal which (a) we do not know how to achieve, (b) we could not recognize if we had achieved, and (c) was such that we could not even tell whether we were gradually moving closer to achieve it. ’True scientific theories’ seems to be such a goal.

... Popper insists that in order for us to be able to show that a theory is progressive with respect to a competitor, we must be able to show that it entails every fact entailed by its competitor. In the absence of such an entailment, progress (in the Popperian sense) is impossible.

II.B. Laudan’s Criticisms of Convergent Realism

In his [1981d], Laudan launched a devastating attack on a brand of realism he dubbed "Convergent epistemological realism." Citing a number of episodes in the history
of science, Laudan has claimed that, contrary to the view of Convergent Realists:

A) Reference and success are not always connected\textsuperscript{41}.

B) Success and truth (or approximate truth) do not always go hand in hand\textsuperscript{42}.

In view of the above, Laudan has concluded that:

1) The convergentist’s assertion that scientists in a ‘mature’ discipline usually preserve, or seek to preserve, the laws and mechanisms of earlier theories is probably false. His assertion that when such laws are preserved in a successful successor, we can explain the success of the latter by virtue of the truthlikeness of the preserved laws and mechanisms, suffers from all the defects ... confronting approximate truth.

2) Even if it could be shown that referring theories and approximately true theories would be successful, the realist’s argument that successful theories are approximately true and genuinely referential takes for granted precisely what the non-realist denies, namely, that explanatory success betokens truth.

3) It is not clear that acceptable theories either do or should explain why their predecessors succeeded or failed. If a theory is better supported than its rivals and predecessors, then it is not epistemically decisive if it explains why its rivals worked.

4) If a theory has once been falsified, it is unreasonable to expect that a successor retain all of its content or its confirmed consequences or its theoretical mechanisms\textsuperscript{43}.

Laudan’s final conclusion, based on the above points, is;

V) ... It is not yet established ... that realism can explain any part of the success of science. What is very clear is that realism cannot, even by its own lights, explain the success of those many theories whose central terms have evidently not referred and whose theoretical laws and mechanisms were not approximately true. The inescapable conclusion is that in so far as many realists are concerned with explaining how science works and with assessing the adequacy of their epistemology by that standard, they have, thus far, failed to explain very much. Their epistemology is confronted by anomalies that seem beyond its resources to grapple with\textsuperscript{44}.

The above criticisms in II.A and II.B (notwithstanding their overlaps) if valid, pose a strong threat against various versions of realism\textsuperscript{45}. A number of convergent realists have tried to rebut Laudan’s charges. However, in doing so, they have modified, to varying degrees, their initial positions by weakening the main tenets of convergent realism\textsuperscript{46}.

Below we shall explore the possibility of defending the main features of minimal realism against Laudan’s attack. We start with Laudan’s objections to the notion of truth as a viable goal for science and the realist view of progress in science. We shall postpone until the next chapter our discussion of the difficulties which accompany the notion of truth (or more specifically the notion of correspondence truth)\textsuperscript{47}.
II.C. Are Laudan’s Criticisms of the Realists’s Aim Valid?

Laudan’s first criticism of the aim of science in II.A., namely, ‘the pursuit of truth is irrational because truth is unobtainable’, seems to be, on his own terms, unwarranted. To see this point more clearly, we should bear in mind that the issue at stake is the difference between the truth claims of $T_r$ (theory $T$ interpreted realistically) and $T_i$ (theory $T$ interpreted instrumentistically or phenomenalistically). What Laudan wants to establish is that since what $T_r$ says about the unobservable entities is unverifiable, opting for it rather than $T_i$, is not rational. However, this conviction flies in the face of many remarks made by Laudan concerning his own (allegedly) more prudent approach.

In the first place, he has claimed originality for introducing a distinction between the context of acceptance and the context of pursuit, and emphasised that the pursuit of seemingly unacceptable aims is not irrational. Secondly, Laudan regards science as basically a problem-solving pursuit which aims at generating theories which are increasingly reliable tools for prediction and control. Accordingly, he has made it clear that the main factor which makes a problem important and worth pursuing is the attention paid to it by knowing-agents.

Now, in view of the above it should be clear that the realist’s pursuit of theoretical truth cannot be branded as irrational: apart from Laudan’s own admission that the pursuit of seemingly unacceptable goals is not irrational, the very fact that a good many thinkers, throughout the ages and in different cultures, have decided that the problem of truth is an important one, and have tried to find a satisfactory solution for it, in itself, suffices to make truth a worth-pursuing problem and a rational aim according to Laudan’s own criteria for problem choice. To the above, it can be added that, as far as pursuing goals are concerned, the only obvious case for irrational behaviour is the pursuit of a goal that
we know to be unattainable, and yet we ignore this relevant knowledge. In all other cases of goal pursuing activities where this strict condition does not exist one cannot easily dismiss the pursuit as irrational. This is because in all these cases we simply do not know whether the stated aim is unattainable, and thus if we believe it is worthwhile enough, then (as Laudan has admitted) we cannot be branded as irrational if we make the attempt.

Laudan has gone to great length to hammer home a rather trivial point, namely, that if we pursue an unattainable goal we are acting irrationally: "If an agent comes to believe that a goal which he formerly espoused is in principle unrealizable, then continuing to hold that goal makes a nonsense of rational action." However, it seems that, to establish the claim that a goal is in principle unrealizable, is not as easy a task as Laudan wants us to believe. An agent's beliefs concerning his ability or the attainability of his goal can be influenced by many factors and thus the case for establishing the 'in principle unrealizability of the goal' may not even get off the ground.

The possibility or impossibility of achieving scientific goals is, among other things (e.g., the practical limitations or otherwise), very much dependent on the dominant metaphysical framework of science at each period. What may look like as an impossible goal for one generation may be the achievable target of the next. Thus for example, launching an artificial satellite into outer space was an impossibility for Aristotelians since according to their view there was a substantial difference between the objects below and above the moon. The former were made of the famous four elements, whereas the latter were crystalline spheres. Similarly, within an strict Newtonian metaphysics which viewed the universe as an aggregate of point particles, the idea of field was an impossibility,
hence the endeavour of Newtonians to explain field-like phenomena such as light in terms of a corpuscular theory. Likewise the phenomenological / instrumentalistic approach towards quantum mechanics meant that in their view, the development of any microrealistic account of quantum theory would have been an impossibility. Von Neumann’s famous theorem which prohibits all hidden-variable accounts of quantum theory, and Bohm’s development of a viable hidden-variable theory are both clear evidence of the influence of the general metaphysical consideration on the thinking of the scientists.

In relation to the above point, it is worth emphasising that Laudan’s dismissal of the positive values of pursuit of theoretical truth, far from being a constructive step forward for the enrichment of scientific enterprise, is likely to impoverish science and hinder its progress. To prescribe pursuing solely obtainable, tangible goals for science, as we have already pointed out, amounts to equating this enterprise with technology and engineering, which though closely related, are different disciplines from theoretical science. It is interesting to notice that while the aim defined for science by Popper enables scientists to pursue this enterprise in both pure and practical fields, Laudan’s prescribed aim can encourage them to focus only on finding solutions to (only) practical problems.

Notwithstanding the dependence of even this type of practical (technological) knowledge on the truth about the world, such an approach, as preached by Laudan, can soon render Faradayan or Einsteinian type scientists, who are not primarily concerned with the practical aspects of their speculations, an extinct breed, as scientists will change into engineers and technologists. The physicist, Victor Weisskopf, in a recent article has warned against the dangers of neglecting the theoretical aspects of science and concentrating on its practical aspects:

Fundamental science, based on the urge to know more about nature and ourselves, is in danger today. One need look no further for evidence than the offices of the National Science Foundation. The
Foundation’s original mission was to support basic science. Now even this leading funder of research ... directs an increasing proportion of its resources toward applied goals. ... The tendency to support science so that it might produce technological fixes is especially dangerous for those parts of basic science that are quite far removed from practical applications. ... These studies can be referred to as cosmic studies. ... Science is like a tree whose roots correspond to basic research. If the roots are cut, the tree degenerates. ... Science cannot flourish unless it is pursued for the sake of pure knowledge and insight.62

Laudan’s second charge against the realist aim, namely, ‘the pursuit of truth is irrational because we do not know how to achieve it’, seems to be more serious. However, if what he has in mind is an all-powerful algorithm or method which automatically would reveal the secrets of physical reality, then it appears that he demands something superhuman of scientists. Realists for sure, do not maintain that such a human-devised, all-powerful method exist63. Realists do not pretend that they should play God and opt for the whole truth, and nothing but the truth, in one go. They are much more modest than that. The claim of realists is that the ultimate truth should serve as an ultimate goal towards which we should proceed in a piecemeal fashion64. The way they proceed towards this goal is by opting for ever deeper, more unified, predictively more powerful and exact theories at each stage65. This task may well be never-ending. However, the criteria of depth, unification, predictive power, and exactness, will together ensure (provisionally) that we have moved one step further in acquiring knowledge (i.e., eliminating some of our mistaken views) about reality66.

The above also enable one to discard Laudan’s two other objections to truth as a viable aim for science, namely, ‘we could not recognize it if we had achieved it, and we could not tell whether we were gradually moving closer achieving it’. Our additional brief reply to these objections, in the light of the above argument will simply be that Laudan has misconceived the realists’ aim. If the realists’ aim is, as explained, that of making a modest move towards better understanding of the physical world, (that is to say, to achieve what Popper called the interesting truth67), then one has to say that, contrary to
Laudan, this goal has already been (partially) achieved.

Moreover, Laudan’s loss of faith concerning the lack of a satisfactory account of verisimilitude, seems to be rather exaggerated. This is because, although Popper’s formal approach has failed to capture his correct intuition, nevertheless, as pointed out in the last chapter, there are currently some more sophisticated and more promising accounts of truth-likeness being developed by a number of writers. This, of course, does not mean that these accounts are flawless, but only that they seem to be (in Lakatosian terminology) progressive research programmes which would gradually meet realists’ requirement for the notion of truth for the theoretical statements. Incidentally, it is not an extraordinary situation in man’s pursuit for knowledge.

There are and always have been concepts which for a long time have had no satisfactory definition or formulation. However, since the original intuitions in each case have been regarded as being on the right track, thinkers have kept them in the hope and expectation that they will be able (in due course and by acquiring better and better technical machinery as well as deeper insights into the nature of things) to make these initial hunches more accurate and elaborate. The notion of verisimilitude is of course no exception. Of course, it might well be the case, as Popper himself has noted, that one must introduce extra, non-logical considerations in order to provide a satisfactory answer. In the last chapter, and in the context of a more comprehensive theory of science we shall introduce a sound, though non-technical, account of verisimilitude and the idea of knowledge-garnering via falsifiable (or falsified) theories, which would withstand Laudan’s objection.

There is, however one point which can be used as a rebuttal to the defence put forward so far. An objector may raise the point that, in the face of the desirability of the
pursuit of a seemingly unobtainable goal like truth, how would realists differentiate between goals worthy of pursuit and the rest. Surely if all goals are said to be worthy of pursuit then one can hardly distinguish the realists’ position from Feyerabendian *anything goes*. This point, which is another version of the all-important issue of theory choice (partially discussed in the previous chapter in the context of van Fraassen’s model), is admittedly a strong objection. The problem, as Lakatos has shown long ago, cannot be solved in a satisfactory manner within the confines of Popperian methodology\(^72\). However, as Feyerabend has argued, Lakatos’s own solution, is no better either\(^73\). Laudan, on the other hand, has claimed that he has overcome this difficulty by means of his reticulational model. We shall assess the credibility of this claim in III.B. below.

**II.D. Can Minimal Realism Weather Laudan’s Objection Against Convergent Realism?**

Minimal realism, in comparison to convergent realism and some other versions of scientific realism, is a more modest thesis. While maintaining that the success of science provides good reasons for scientific realism\(^74\), it does not advocate the thesis that scientific realism should be upheld in all circumstances. Nor does it endorse the view that all past successful theories should be interpreted realistically, or that the main task of realism is to explain the success of science.

For a minimal realist the main task of realism is rather, as noted earlier, *making rational sense* of scientific enterprise: if past theories are all false, how can we talk of knowledge-garnering, of progress in science and of approaching the truth about the physical reality?\(^75\)

To see whether minimal realists can provide satisfactory answers to the above questions and rebut Laudan’s criticisms of convergent realism, we should, once again,
look at the main ingredients of minimal realism introduced in the first chapter. In the light of the third ingredient (M3), minimal realists would argue that explanatory theories that have obtained great empirical success throughout some domains of phenomena and that have been able to accommodate their past successful predecessors as their own limiting cases, are (at least) partially true. These successful theories, candidates for realistic interpretation, are ones "in which an increasingly finer specification of internal structure has been obtained over a long period, in which the theoretical entities function essentially in the equations of the theory and are not simply intuitive postulations of an 'underlying reality', and in which the original metaphor has proved continuously fertile and capable of increasingly further extension"^{76}. This means that, even if entities precisely like those postulated by these empirically successful theories do not exist, nevertheless, unobservable entities approximately like those postulated by them do exist^{77}.

Since Laudan has essentially based his attacks on convergent realism on historical cases, this way of looking at historical records^{78} will help us, without getting too much involved into the details of philosophy of language^{79}, to explore whether a reasonable correspondence relation can be established between the central terms and concepts of superseded theories and their more successful successors? This strategy has already been (successfully) developed by a number of realists. Historical research carried out by these writers all convincingly establish the very point Laudan is sceptical about, namely, that there is a good deal of correspondence between past successful theories and their present day acceptable successors^{80}.

Realists’ approach involves, among other things, taking a diachronic view towards the history of science while combining a certain degree of (constructive) scepticism in epistemological matters^{81} with a healthy dogmatism. This means that even when a theory
is in its heyday, realists would regard its ontological claims with caution and not with blind approval. Yet at the same time they emphasis that when theories are flourishing it is only sensible to have practical confidence in them despite their shortcomings. Popper has put this point in the following way, "I have always stressed the need for some dogmatism: the dogmatic scientist has an important rôle to play. If we give in to criticism too easily, we shall never find out the real power of our theories." Indeed, it makes sense to stick with a phenomenally successful theory because success implies that one has got something right. This is the very thesis H. Post in his forceful defence of the correspondence principle has encapsulated in the formulation: "No theory that ever 'worked' adequately turned out to be a blind ally." If however, the rough, tentative specifications of the theoretical posit is further developed and articulated and successfully applied to new areas, then we are justified in believing that the theoretical posit is representing a causally and explanatory significant feature of the world. Of course, in these cases the more detailed our specifications of the posit in question, the less likely that something exactly like it actually exists in the nature, and vice versa.

The above approach is complemented with the view that in cases where a research programme consistently fails to articulate the essential nature of its purported ontology, and the theoretical posits of its celebrated theory(ies) prove not to represent a real insight, and we have little understanding about the explanatory mechanism suggested by the theory(ies) then, until a better realist alternative can be found, an instrumentalist attitude is called for. This in turn means that the success of science need not always be explained in realistic terms.

This way of interpreting the theoretical posits of the past successful theories allows us to retain a good deal of the explanatory mechanisms of these conjectures during the
process of theory change. Laudan, on the other hand, has tried to cast doubt on this aspect and has regarded it as epistemically indecisive (i.e., irrelevant): "If a theory is better supported than its rivals and predecessors, then it is not epistemically decisive if it explains why its rivals worked." But this does not seem to be correct. The important point in this regard is that in the absence of this dual feature (namely, explanation/retention), not only can one not account for the all-important issue of growth of knowledge, but also one would run the risk of rendering the past scientists as irrational agents or endorsing the validity of incommensurability.

From the above it is also clear that contrary to Laudan, the best epistemic approach towards the history of science in the face of pessimistic induction, is not to adhere adamantly to a phenomenological interpretation. Such interpretations, which as we have already discussed do not provide scientists with either theoretical knowledge about physical reality or effective heuristic guides for advancing their researches, should be regarded as a last resort and made use of only when realistic interpretations (in the above sense) are not available.

One last point before turning to an assessment of Laudan’s own model: Laudan’s charge of circularity against convergent realists, namely, that they assume the truth of successful theories to account for their truth-likeness, looses its force against minimal realism. As noted earlier, according to minimal realists, the successful theories are not just false, but approximately true: The existence of a (diachronic) series of ever more general, more explanatory theories with increasing predictive power, each capable of accounting for the (partial) success of its predecessors (in the way discussed above), can be regarded as evidence for (or warrants belief in) the view that that series to be on the right track, i.e., truth-like. Of course the very fact that what warrants rational belief may change
historically does not entail that truth also changes (see below Sec. III.B).

III. Laudan’s Alternative Models

III-A. Progress and Problem-Solving

Laudan’s main objective, as stated above, has persistently been to provide a rather new account of rationality, and of progress of science. He has not been satisfied with the accounts provided by either realists or a number of anti-realists. On the one hand, he maintains that the classical (realist) connection between progress and rationality is wrongly conceived: the correct connection is the other way round. Moreover, he thinks that the traditional criteria of theory choice and content comparison, are irredeemably ineffective, and that (as we have already seen) the realist (i.e., Popperian) account which explains progress in terms of approaching truth cannot resolve the major difficulties raised against it. On the other hand, he holds that, while writers like Kuhn and Lakatos have rightly emphasised the significance of the more general theories rather than more specific ones, as primary tools for understanding and appraising scientific progress, and while they have correctly pointed out the rôle of external factors in the progress of science, their own models for scientific progress suffer from serious shortcomings.

In view of the above, Laudan’s primary problem has simply been, how, in the face of the fact that science is progressive, is it then possible to account for progress in science without falling into the trap of anarchism or extreme relativism. Laudan’s initial solution to this problem has been to measure progress in terms of problem-solving effectiveness.

In his [1977], which was his first major attempt towards an alternative theory of scientific progress and rationality, and in a number of subsequent publications, (e.g his [1981b, 1984c]), Laudan suggested that: “Science is essentially a problem solving activity”, and that: "the aim of science is to maximize its scope of solved empirical
problems, while minimizing the scope of anomalous and conceptual problems\textsuperscript{98}. Accordingly, he emphasised that: "progress can occur if and only if the succession of scientific theories in any domain shows an increasing degree of problem solving\textsuperscript{99}. In place of the more traditional ways of content comparison he introduced a set of new concepts including adequacy, acceptance, pursuit, promise, among others\textsuperscript{100}, all to elaborate his formulation of problem-solving effectiveness\textsuperscript{101}.

Laudan's problem-solving model suffers from a number of serious shortcomings. Apart from ambiguities over the very notion of problem\textsuperscript{102}, or the notion of adequate solution to a problem, the difficulties concerning the individuation of the problems prevented Laudan from producing a consistent and workable way to calculate the number of problems and to weigh their significance within each research tradition and between them\textsuperscript{103}. Laudan was also not able to solve the very basic problem which is at centre of the issue of growth of knowledge namely, the age-old paradox of continuity through changes\textsuperscript{104}. This meant that successive theories could not be compared, and the losses and gains in the contents of the succeeding theories could not be accounted for\textsuperscript{105}. This, naturally, brought the spectre of relativism, although Laudan had most emphatically tried to dissociate his views from it\textsuperscript{106}.

Laudan's first alternative model not only did not provide scientists with a useful method for theory choice, but it also represented / recommended a rather distorted image of science\textsuperscript{107}. Equally important, it was rather ironic that after all the troubles Laudan has led himself into to get rid of the notion of truth, his bid to produce an alternative (truth-free) scheme for theory comparison and progress of science, came to grief: not only does the notion of problem-solving squarely rest as much on the notion of truth, but his model is also every bit dependent upon this very notion\textsuperscript{108}.
III.B. Reticulational Model, Normative Naturalism and the Return of Relativism

In the absence of a satisfactory analysis of problem-solving effectiveness, Laudan, in his recent writings, (partly resting content with the pre-analytic, intuitionist conception of problem\textsuperscript{109}, and partly criticising his own previous views\textsuperscript{110}), has endeavoured to tackle the issues of rationality and progress by means of a new approach, one which he has dubbed the \textit{reticulational model}\textsuperscript{111}. Laudan has claimed superiority for this approach over the other existing methodological approaches, a claim which he has repeatedly defended in almost all of his recent numerous publications). In the rest of this chapter we shall attempt at a critical appraisal of this new model.

Laudan’s new model which is a set of meta-methodological rules\textsuperscript{112}, can be outlined as follows:

1. Methodological rules are hypothetical imperatives\textsuperscript{113}. As such they are at once both naturalized, and normative\textsuperscript{114}.

2. Methodological rules, in order to be able to adjudicate between aims, must be supplemented with axiological investigations. Like methodological rules, axiological researches are naturalized\textsuperscript{115}.

3. Facts/theories, methods and aims are, (a) interdependent, and (b) ever-changing\textsuperscript{116}.

The following diagram depicts, in a schematic way, the main features of Laudan’s reticulational model:
III.C. Appraisal of Laudan’s Reticulational Model

Perhaps the main feature in Laudan’s new model which can be considered as a (relative) improvement with regards to traditional hierarchical model\textsuperscript{117}, is point (3) above. However, even this feature, as we shall see shortly is not thorough enough, and as such, renders Laudan’s bid for production of an effective methodology, incomplete. We take Laudan’s points in turn.

(A) As for Laudan’s first point, the following objections can be raised: A-1) it is not the case that all (if any) of methodological rules are hypothetical imperatives or naturalized, i.e. based on the inductive reasoning from experience. In fact, Laudan’s naturalization of methodological rules seems to be a revival (in its methodological form) of a theory which Popper, dubbing it the bucket theory of knowledge, has long ago convincingly criticised\textsuperscript{118}. There are many methodological rules which are not and cannot be derived (in whatever diluted sense of derivation) from our past experiences\textsuperscript{119}. These rules are rather the outcome of our critical reflections on methodological matters. These rules are of course *rationally criticizable* but not *empirically refutable/verifiable*\textsuperscript{120}. The reason for this is not difficult to comprehend: methodological rules are, as Laudan has correctly observed, elliptic conditionals of the form ‘If you want to obtain x, do y’.
However, these conditionals, contrary to what Laudan claims, are not \textit{empirical} but \textit{metaphysical} and therefore they are only rationally criticizable\textsuperscript{21}.

Interestingly enough, Laudan, in discussing the methodological rules, has provided an effective counter-example to his own conviction that such rules are hypothetical imperatives. The case in point, is the rule of predesignation. Laudan writes:

\begin{quote}
Consider, for instance, a familiar and venerable methodological controversy: that between those who believed that the ability of a theory to make successful, surprising predictions counts for much more evidentially than its ability to explain facts already known. Prominent philosophers of science have been arrayed on both sides of this issue. Popper, Whewell and Peirce, for instance, thought that successful surprising predictions were probative; while the likes of Mill, Mach, and Keynes found them singularly unimpressive. ... \textit{Disagreement of this sort abound in methodological discourse.}\textsuperscript{122}
\end{quote}

If the empirical evidence (drawn from history of science) is so inconclusive, then the naturalists will be at a loss to find a sound methodological rule by examining the evidence available to them. However, there are \textit{rational} (though not empirical) ways for deciding whether the rule of predesignation is a pursuit-worthy rule. There are also rational ways to show that why scientists prefer prediction to accommodation\textsuperscript{123}.

(A-II), Laudan's naturalism, implicitly, assumes that the methods and rationales of contemporary science are to be preferred to those of the past sciences\textsuperscript{124}. However, this tacit principle betrays Laudan's own naturalist methodology in two significant ways. Firstly, it is not itself based on empirical evidence, and therefore, within the framework of Laudan's methodology it is a non-legitimate rule. But secondly, and more importantly, it renders the whole history of science utterly useless, and this despite all Laudan's insistence on the importance of learning from the history of science in methodological matters\textsuperscript{125}.

(B) Laudan's naturalization of axiology (study of aims and goals) does not fare better than his naturalization of methodological rules. In the first place, as Laudan himself rather surprisingly has acknowledged, his new methodological gambit cannot suggest any
preferred goal or aim. The choice of aims, in Laudan’s system is radically *underdetermined* by the methodological rules (apparently) conducive to those aims\textsuperscript{126}. But if Laudan’s model cannot help scientists to choose better aims from among a host of rival aims, then, precisely because of the interdependence of rules, aims, and theories / facts, (the third ingredient of Laudan’s model), the indeterminacy and underdetermination of aims would render the two other elements of the model equally indeterminate and underdetermined. Laudan’s only way out of this predicament has been to deny the significance of the issue of underdetermination for epistemological matters\textsuperscript{127}. However, as noted in the last chapter, underdetermination is an off-shoot of the problem of induction and as such it needs a rational solution. The denial of the existence of this problem (or cluster of problems) cannot be counted as a proper solution. Moreover, as we have already noted this problem(s) cannot be rationally solved within the framework of standard empiricism. But, standard empiricism (naturalism) is certainly a thesis that Laudan wants to uphold.

(C) Laudan, who has referred to the ever changing aims, methods and theories / facts, as a Heraclitean case\textsuperscript{128}, faces exactly the very problem that Heraclitus was grappling with, namely, how to account for continuity in an ever-changing world; in the absence of viable criteria for identifying the changing objects or subject-matters, the door will be wide open for a vicious relativism. Laudan, in his new model, however, has not produced any solution to this age-old problem. On the contrary, by replacing (or one might say conflating) the aim of science with those ever changing and widely different aims of scientists, Laudan has indeed rendered his own system vulnerable to relativism\textsuperscript{129}.

But what is the position of the minimal realism concerning the relation between aims, methods and theories/facts? As we have already noticed, many realists subscribe to
the hierarchical model. The great advantage of this model, over Laudan’s reticulational model, is that because of its reliance on one singular over-all aim for science (i.e. truth) it can ward off the threat of relativism\textsuperscript{130}. However, the great disadvantage of it is that, while the choice of theories and methods in this model are, in the final analysis, governed by the chosen aims, there is no higher and independent court of appeal to resolve the disputes over the aims. Here, as is evident in Popper’s philosophy, a tacit conventionalism settles the disputes\textsuperscript{131}. An advocate of (minimal) realism, clearly, then, cannot endorse either of these two models. However, it is unfortunate that, within the realm of received realist approaches there is no third alternative. Providing this alternative will be the task of the final chapter of this essay.
NOTES (Chapter Four)

1. K.Popper [1959/68], p.15. Italics in original. This is a major theme in Popper. The same idea can be found in many of his writings. See for example his [1963].

2. The question of whether scientific knowledge grows or not, depends on another prior question, namely, whether science aspires to knowledge? This question, as we noticed in the first chapter, lies in the heart of realist — anti-realist dispute. L.Laudan has put the typical anti-realist misgivings about science’s claim to knowledge in this way, "... the terse formula ‘science aspires to knowledge’ disguises a plethora of fundamentally disparate notions. Is the knowledge which science aspires to a knowledge of causes? In that case we see no agreement among either scientists or philosophers. Of essences? Or of appearances? Is science seeking knowledge that is useful and practical or theoretical and esoteric? Is science after knowledge that is certifiably true or knowledge that, while perhaps false, will allow us to save some phenomena?"
   (Laudan [1990d], p.49)

3. As noticed earlier (Ch. 3) there are four issues at stake here, namely,
   a) saying what scientific progress means;
   b) specifying a methodology for assessing scientific progress;
   c) providing justification for the methodology introduced; and
   d) accounting for or explaining scientific progress.

4. This question pertains to the third meaning of the fourth aspect of growth of knowledge mentioned in the last chapter. See note 105 in that chapter.

5. The pursuit of theoretical truth (i.e., truth about the unobservable aspects of reality) is something which is always challenged by anti-realists. In contrast, I take it to be a truism that truth about the observable aspects of the universe, is a goal for realists and anti-realists alike. Even technological activities, which anti-realists uphold (against theoretical pursuits) as the main aspect of scientific activities, in a way, are truth oriented: a bridge, for example, should remain true to the design and calculations of its designers and builders.

   In the next chapter, we shall discuss the case of a number of ‘realist’ philosophers of science who have broken rank with the more traditional version of realism, and contrary to the conventional wisdom among realists, have tried to dispense with the notion of truth and to suggest alternative ways to account for the growth of scientific knowledge.


7. See note 52, Ch.1.

8. The idea of progress in knowledge-garnering has a close link with the notion of rationality. Realists, generally, regard the two problems as intertwined and closely connected. For realists, science provides one of the best (if not the best) models (or paradigms) of rationality. It is by studying scientific enterprise (via the so called process of reconstruction, both in its present and past contexts) that, realists hope to be able to discover universal patterns of rationality. This view, as is mentioned in the text, has come under attack from different anti-realist writers. Among the self-proclaimed realists however, at least one writer, (namely, I.Hacking) has opposed the problem of rationality (in the above sense) and has advocated views not dissimilar to Feyerabend, a self-declared anti-rationalist. Hacking writes: "My own attitude to rationality is too much like that of Feyerabend ... Let there be no canon of rationality, no privileged class of good reasons, and no mind-binding preferred science or paradigm..." (Hacking [1984, p.14])
9. Logical positivists, in the spirit of good empiricists, were against theoretical knowledge. As noted earlier, one of their main research programmes was to give a general account of the structure of scientific theories, grounded in empirical basic statements with the help of a rigorous logic. The idea was to devise an empiricist language, into which scientific laws and theories might be translated. This programme was in a sense a culmination of the programme started by Russell. He wanted to give an empiricist account of knowledge by starting from the simplest elements of sensation and building upwards logically to give a full account of all our thoughts. This programme was in turn an elaboration of and an improvement on Mach’s phenomenalism. For a discussion of Russell’s views see M. Sainsbury [1979/85]. D. Pears [1972] is a general introduction to Russell’s logical atomism.

10. See note 113 Chapter Three.

11. Realists, too, subscribe to the theses of growth by cumulation and correspondence. However, contrary to positivists, they grant realistic status to theories. For an excellent account of correspondence from a realist point of view, see H. Post [1971].

12. See Ch.2.

13. See Ch. 2.

14. See Ch.2.

15. The following observations in text concerning the question of method are expansion of a theme pointed out by Laudan in his [1986b] and [1987a].


17. See Popper [1959/68]. Popper maintains that the issue of discovery belongs to the realm of psychology of innovation.

18. In its simplest form the principle of verification states that: “The meaning of a proposition is the method of its verification”. For an account of this principle in both Tractatus and the writings of the logical positivists see O. Hanfling [1981].

19. Lakatos had suggested that a condition on the acceptability of any proposed methodology of science was that its normative verdicts must coincide — by and large — with a larger class of judgements of the scientific elite about these matters than any of its rivals do. Lakatos writes: “Now let us consider the proposal that a rationality theory — or demarcation criterion — is to be rejected if it is inconsistent with an accepted ‘basic value judgement’ of the scientific elite. ... While there has been little agreement concerning a universal criterion of the scientific character of theories, there has been considerable agreement over the last two centuries concerning single achievements. While there has been no general agreement concerning a theory of scientific rationality, there has been considerable agreement concerning whether a particular step in the game was scientific or crankish, or whether a particular gambit was played correctly or not. A general definition of science thus must reconstruct the acknowledgedly best gambits as ‘scientific’: if it fails to do so, it has to be rejected”. (Lakatos [1971], pp. 110-111, italics in original).

20. "... epistemology merges with psychology, as well as with linguistics. This rubbing out of boundaries could contribute to progress, it seems to me, in philosophically interesting inquiries of a scientific nature." (Quine [1969], pp.89-90).

Quine has extended his relativism not only to scientific posits but also to scientific methods. He writes: "We have no reason to suppose that man’s surface irritations even unto eternity admit of any one systematization that is scientifically better or simpler than all possible others ... Scientific method is the way to truth, but it affords even in principle no unique definition of truth." (Quine [1960], p.23)

This despair over the possibility of coming nearer to truth, which is diametrically opposed to Popper’s approach of error elimination and learning by from our mistakes, is equally matched with Quine’s pessimism concerning the acquisition of further knowledge of the furniture of the world via scientific enquiry: "As an
empiricist, I continue to think of the conceptual scheme of science as a tool, ultimately, for predicting future experience in the light of past experience. Physical objects are conceptually imported into the situation as convenient intermediaries — not by definition in terms of experience, but simply as irreducible posits comparable, epistemologically to the gods of Homer. For my part I do, qua lay physicist, believe in physical objects and not in Homer’s gods; and I consider it a scientific error to believe otherwise. But in point of epistemological footing the physical objects and the gods differ only in degree and not in kind". (Quine, 1953), p.44, quoted in R.Trigg [1980])

Quine’s naturalising of epistemology with all its relativist and instrumentalist implications have opened the floodgate of relativism in 20th century philosophy. For a critique of Quine’s view see Trigg [1980].

21. "How can he know how he is to continue a pattern by himself? — whatever instruction you give him? — Well, how do I know? — If that means 'Have I reasons?' the answer is: my reasons will soon give out. And then I shall act, without reasons." Wittgenstein [1953/1978], p. 84*, and passim.

22. Michael Polanyi [1958].

23. Feyerabend [1975].

24. Judging by his published works, it can be said that Laudan, has, since his earliest essays, apparently tried to reject, (almost simultaneously), both realists’ methodology and a number of non-realist philosophies of science, most notably epistemic relativism (in its various guises and especially in its historicist form). For example, in his [1977] he has criticised the views of realists, positivists, Kuhn and Lakatos and in his [1984a] and [1984b] he has raised simultaneous objections against both realism and relativism. In his [1989b] he has attacked Feyerabend’s views on scientific methods and his [1990b] is a lengthy essay against (mostly) relativism. In recent years, Laudan has tried to elaborate his criticism against the thesis of underdetermination, which he (rightly) regards to be at the core of all relativists doctrines. See for instance his [1990c] and [1993].

25. Laudan [1981c, 1984a] has dubbed this second brand of realism convergent realism and has named Richard Boyd [1973], Hillary Putnum [1975a], William Newton-Smith [1978] and Ilkka Niiniluoto [1977] as the better known exponents of this view. These authors have expanded their views in a number of subsequent works. See for example, Newton-Smith [1980b], and Putnum [1978].


27. ibid. p.229.

28. In the Epilogue to his [1977] Laudan writes: "The preoccupation of classical philosophers of science has been with showing that the methods of science are efficient instruments for producing truth, high probability, or ever closer approximations to the truth. In this enterprise, they have failed dismally." (p.223). For further citations see below.

29. Laudan for example, freely admits the influence of Lakatos on his views. See his [1977] and [1986a]

30. This dominant trend, the so-called epistemologizing of truth, as the name implies, basically aims at replacing the traditional, realist (correspondence) notion of truth, with some more tangible (achievable) criteria like warranted assertibility.

Among the anti-realist advocates of empiristemologizing truth one can refer to Dummett [1978]) whose ideas can be traced back to the works of Wittgenstein, and also subtle pragmatist movements of the form defended in Davidson’s recent writings (e.g., his [1990]) which in turn can be traced to the works of Peirce as well as Hegel (see A.Hance [1992]).

In recent years, a number of realist philosophers have also been influenced by this trend and have abandoned the traditional realist view concerning the correspondence theory of truth. We shall discuss the case of these realists in the next chapter.

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Poincaré [1902/52, pp.11,12] has long before Laudan raised similar doubts about truth, though his attitude towards this notion is radically different from Laudan. He writes: "The search of truth should be the goal of our activities. But if truth be the sole aim worth pursuing, may we hope to attain it? It may well be doubted... Does this mean that our most legitimate, most imperative aspiration is at the same time most vain?". (Quoted from Watkins [1984], p.154)

32. *op.cit.* [1977], p.125, italics in original.


34. The pessimistic induction or the meta-induction has noted by a number of writers. Thus for example, H.Putnum ([1978], p.25) writes: "What if all the theoretical entities postulated by one generation (molecules, genes, etc., as well as electrons) invariably 'don't exist' from the standpoint of later science? This is, of course, a form of the old sceptical 'argument from error' — how do you know you aren't in error now... One reason this is a serious worry is that eventually the following meta-induction becomes overwhelmingly compelling: just as no term used in the science of more than fifty (or whatever) years ago referred, so it will turn out that no term used now (except maybe observation terms, if there are such) refers. It must obviously be a desideratum for the theory of reference that this meta-induction be blocked". W.Newton-Smith ([1978], p.269) after citing a number of falsified theories writes: "... Indeed, this graveyard of falsified theories provides inductive evidence for a generalization to the effect that any theory will be found to be false within, say, 200 years of first being propounded. I will call this the pessimistic induction". M.Devitt ([1984], p.145) has given the following formulation: "At any time \( t \) in the not too immediate past it would have been a mistake to infer Scientific Realism \((t)\) from what science at \( t \) (apparently) posited to explain observable phenomena, for it turned out not Scientific Realism \((t)\). So it is probably a mistake to infer Scientific Realism \((\text{now})\) from what science now (apparently) posits to explain observed phenomena".

35. Laudan [1977], p. 126. See also *ibid.* pp.125-7, [1981a], p.533, and [1982].

36. See for example, *op.cit.* [1977], pp.147-150. Also [1990a], ch.1. Elsewhere he writes: "Theory transitions are generally non-cumulative, i.e. neither the logical nor empirical content (nor even the confirmed consequent) of earlier theories is wholly preserved when those theories are supplanted by newer ones." (Laudan [1981c], p.144)

The same point is repeated again and again in almost all of Laudan’s publications.

37. Laudan writes: "They [Kuhn & Feyerabend] have also suggested that every gain in our knowledge is accompanied by attendant losses, so that it is impossible to ascertain when, or even whether, we are progressing". ([1977]. p.3.)

"As Kuhn and Feyerabend, and others have claimed, there are usually problem losses as well as problem gains associated with the replacement of any older theory by a newer one". (*ibid.* p.148)

Kuhn has discussed the idea of content loss in his [1962] (see especially p.169). For a rebuttal and critic of Kuhn’s view on ‘content loss’ see H.Post [1971]. In his more recent writings, Laudan, (due to insurmountable difficulties he has faced in developing a coherent alternative methodology) has tried to down play the significance of the notion of epistemic losses, (see for example, Laudan [1987b], esp. p.229).

38. Laudan [1981b], p.533. The same view can be found in his [1982,] and [1977, pp.125-7].

39. *op.cit.* [1977], pp.147-8

40. According to Laudan’s ([1981d], pp.219-20) the main ingredients of this brand of realism are as follows:

R1- Scientific theories (at least in the ‘mature’ science) are typically approximately true, and more recent theories are closer to the truth than older theories in the same domain.

R2- The observational and theoretical terms within the theories of a mature science genuinely refer
(roughly, there are substances in the world that correspond to the ontologies presumed by our best theories).

R3- Successive theories in any mature science will be such that they preserve the theoretical relations and the apparent referents of earlier theories, that is, earlier theories will be limiting cases of later theories.

R4- Acceptable new theories do and should explain why their predecessors were successful in so far as they were successful.

R5- Theses (R1) to (R4) entail that ('mature') scientific theories should be successful; indeed, these theses constitute the best, if not the only, explanation for success of science. The empirical success of science ... accordingly provides striking empirical confirmation for realism.

The basic idea of Laudan [1981d] paper can be found in Laudan’s earlier writings. See for example, his [1978], [1984a] and [1984c].

41. Laudan ascribes the following views to convergent realists with regard to the notions of success and reference;

S1) The theories in the advance a mature science are successful.
S2) A theory whose central terms genuinely refer will be a successful theory.
S3) If a theory is successful, we can reasonably infer that its central terms genuinely refer.
S4) All central terms in theories in the mature sciences do refer". (op.cit. [1981d], p.221).

He has then set out to refute the main elements in the above list:

"Are genuinely referential theories (i.e., theories whose central terms genuinely refer) invariably or even generally successful at the empirical level, as (S2) states? There is ample evidence that they are not. The chemical atomic theory in the eighteenth century was so remarkably unsuccessful that most chemists abandoned it in favour of a more phenomenological, elective affinity chemistry. The Proutian theory that the atoms of heavy elements are composed of hydrogen had, through most of the nineteenth century, a strikingly unsuccessful career, confronted by a long string of apparent refutations. The Wegenerian theory that the continents are carried by large subterranean objects moving laterally across the earth's surface was, for some thirty years in the recent history of geology, a strikingly unsuccessful theory ...") (ibid. p.223)

"What about (S3), the realist claim that success creates a rational presumption of reference? We have already seen that (S3) provides no explanation of the success of science, but does it have independent merit? The question specifically is whether the success of a theory provides a warrant for concluding that its central terms refer. ... A proper empirical test of this hypothesis would require an extensive sifting of the historical records that is not possible to perform here. What I can do is to mention a range of once successful, but (by present lights) non-referring, theories. ... for now we will concentrate on a whole family of related theories, namely, the subtle fluids and ethers of eighteenth — and nineteenth — century physics and chemistry.

Consider specifically the state of ethereal theories in the 1830s and 1840s. The electrical fluid, a substance that was generally assumed to accumulate on the surface rather than to permeate the interstices of bodies, had been utilized to explain inter alia the attraction of oppositely charged bodies, the behaviour of the Leyden jar, the similarities between atmospheric and static electricity, and many phenomena of current electricity. Within chemistry and heat theory, the caloric ether had been widely utilized since H.Boerhaave (by among others, A.L. Lavoisier, P.S.Laplace, J.Black, Count Romford, J.Hutton, and H.Cavendish) to explain everything from the role of heat in chemical reactions to the conduction and radiation of heat and several standard problems of thermometry. Within the theory of light, the optical ether functioned centrally in explanations of reflection, diffraction, and polarization. ... There were also gravitational (e.g., G.Le Sage’s) and physiological (e.g., D.Hartley’s) ethers which enjoyed some measure of empirical success. ... Indeed, on any account of empirical success which I can conceive of, non-referring nineteenth-century ether theories were more successful than contemporary, referring theories". (ibid. pp.225-6)

42. Laudan has discussed this case in two sections. In the first section entitled; "Approximate truth & success: the downward path", he has ascribed the following views to convergent realists: (T1) if a theory is approximately true, then it will be explanatory successful, and (T2) if a theory is explanatorily successful, then it is probably approximately true. His criticism in this section is focused on (T1) and is summarized in the following remark: "What can be said is that, promises to the contrary notwithstanding, none of the proponents of realism has yet articulated a coherent account of approximate truth which entails that the approximately true theories will, across the range where we can test them, be successful predictors. Further difficulties abound. Even if the realist had a semantically adequate characterization of approximate truth, and even if that semantics entailed that most of the consequences of an approximately true theory would be true, he would still be without any criterion that would epistemically warrant the ascription of
approximate truth to the theory. As it is, the realist seems to be long on intuitions and short on either a semantics or an epistemology of approximate truth". (op.cit. [1981d], pp.228-230)

In the second section entitled; "Approximate truth and success: the upward path", he has focused on (T2) and has produced the following list of theories which have been successful, but which are not approximately true (i.e., their central explanatory concepts have not been referential):

The crystalline spheres of ancient and medieval astronomy; the humeral theory of medicine; the effluvial theory of static electricity; catastrophist geology, with its commitment to a universal (Noachian) deluge; the phlogiston theory of chemistry; the caloric theory of heat; the vibratory theory of heat; the vital force theory of physiology; the electromagnetic ether; the optical ether; the theory of circular effluvia; and theories of spontaneous generation. (ibid. pp.230-1)

43. op.cit. [1981d], p.244.

44. ibid.

45. Anti-realists have, by and large, taken Laudan’s arguments as decisive. See for example, A.Fine [1984] where he bases his own criticisms of realism on the basis of the validity of Laudan’s observations. Van Fraassen [1985] has also endorsed Laudan’s arguments. It seems these arguments have played some rôle in Putnum’s disenchantment with realism. Putnum in his [1981] has turned against realism and put forward an anti-realist thesis, not dissimilar to that of Kant, which he has named internal realism. In fact, as we shall see in the next chapter, objections like these have made a number of realists change their minds over the rôle of the notion of truth in realism and water down their approaches to the extent that their positions are hardly distinguishable from anti-realist.

46. For defence of convergent realism see C.L.Hardin & A.Rosenberg [1982], R.Miller [1987] and N.Niiniluoto [1987]. One common feature of all these defence is that the defenders have tried to water down the claims suggested by Boyd [1973] and amplified by Putnum [1978]. Newton-Smith for example, in a recent paper entitled "Rationality and Scientific Method" (read at the biweekly seminars in the Department of History and Philosophy of science at UCL in November 1993) declared that he no longer entirely endorses his views as explicated in his earlier writing like his [1980]. Instead he wants to make rooms for two extra elements absent from his earlier considerations. Firstly, the impact of social factors on the opinions and views of scientists and the course of science. Secondly, the rôle of what Duhem called bon sens and Polanyi dubbed tacit knowledge. In Newton-Smith’s view this factor, which does not seem to be amenable to rational assessment, plays an important rôle in the advancement of science. Newton-Smith has claimed that Popper and many other methodologists including Laudan himself have not take this factor into consideration. Richard Miller too, in his [1987], has tried to water down the original position of convergent realism as stated in Boyd [1974] and Putnum [1978]. For example, he has insisted that arguments for or against realism cannot be resolved at a general level, but must appeal to topic-specific cases: "Standard defence of realism cannot account for the compelling force of their arguments. They cannot, I shall finally argue, because topic-specific truism plays an essential rôle when a case for unobservable entities is rationally compelling." (p.352)

47. Naturally a certain degree of overlap between the points discussed here and in the next chapter will be inevitable, though, I have tried to keep it to a minimum.

48. "Almost all the standard writings on scientific appraisal, whether we look to philosophical or historical discussion of science, have two common features: they assume that there is only one cognitively legitimate context in which theories can be appraised; and they assume that this context has to do with determinations of the empirical well-foundedness of scientific theories. Both these assumptions probably need to be abandoned: the first because it is false, the second it is too limited.

I shall be arguing that a careful examination of scientific practice reveals that there are generally two quite different contexts within which theories and research traditions are evaluated. ... [Namely, first], The context of acceptance ... it is clear that scientists choose to accept one among a group of competing theories and research traditions; i.e., to treat it as if it were true ... [and secondly], The context of pursuit ... there are many important situations where scientists evaluate competing theories by criteria which have nothing directly to do with the acceptability or 'warranted assertibility' of theories in question ... Scientists often
begin to pursue and to explore a new research tradition long before its problem-solving success (or its inductive support, or its degree of falsifiability, or its novel predictions) qualifies it to be accepted over its older, more successful rivals". (Laudan [1977], pp. 108-110)

"... Scientists can have good reasons for working on theories they would not accept ...". (Laudan [1977], p. 110, italics in original)

Laudan’s distinction between contexts of acceptance and pursuit is similar to van Fraassen’s distinction between belief and acceptance, respectively.

49. On this point see for example, Laudan [1977 passim], [1990a]. We shall discuss this point in the next section.

50. "To regard something as an empirical problem, we must feel that there is a premium on solving it. At any given time in the history of science, many things will be well-known phenomena, but will not be felt to be in need of explanation or clarification. ... [A] fact only becomes an 'empirical problem' when someone decides it was sufficiently interesting and important to deserve explanation ... A problem need not accurately describe a real state of affairs to be a problem; all that is required is that it be thought to be an actual state of affairs by some agent" ([1977], pp. 16-17, italics in the original, the emphasis added)

In his later writings, Laudan has made rationality more subject-oriented. For example, in his [1987a, p.21] he writes: "Whatever else rationality is, it is agent— and context— specific. When we say that an agent acted rationally, we are asserting minimally that he acted in ways which he believed would promote his ends."

The same theme is being repeated elsewhere: "The object of a theory of rationality is to link actions, beliefs and goals. Whatever it is, rational behaviour consists in selecting actions which we believe are conducive to our ends". (Laudan, [1987b], p.227)


52. J.Watkins [1984] has elaborated this point in the following way: "... I say an aim is infeasible if we know that it cannot be fulfilled. But we must be careful not to create an air of infeasibility by mis-portraying the aim in question. One’s aim may be: (i) to attain a certain goal; or (ii) to progress towards a certain goal without necessarily attaining it; or (iii) to progress in a certain direction without having an ultimate goal that one is progressing towards. ... A type (i) aim is infeasible if the goal is known to be unattainable, but type (ii) and type (iii) aims are infeasible only if progress in the intended direction is infeasible." (p.124)

53. Laudan [1987b], p.227, italics in original.

54. Somebody, a hero for example, may, against all odds, go ahead and make a great sacrifice in the hope that even if he cannot possibly achieve what he maintains as the goal for his cause, his endeavours may inspire others, or pave the way for them (in the general sense) and hence render a seemingly impossible objective a possible one.

Incidently, evolution has taught us that pursuit of seemingly unattainable goals is something which even the selfish genes are prone to engage in. Many recent studies have revealed that genes would go to extreme lengths, solely on the basis of the tiniest possibility that such a pursuit will probably increase the survival chances of their offsprings (and not even themselves). (See R.Dawkin [1989])

The very fact that we are living in an indeterministic universe where the feeble movements of a butterfly may produce huge, unpredictable effects, and bring about many new, unprecedented possibilities, suffices to remind us that logically possible (conceivable) goals cannot be easily rejected on the basis of unattainability.

55. See I.B.Cohen [1960/85]

56. See G.N.Cantor [1983].

57. See Chapter Six.
58. "The task of science is not confined to searching for purely theoretical explanations; it also has its practical sides: prediction-making as well as technical applications." (Popper [1972/79], p.352)

59. See note 5 above. We have further discussed this issue in the next chapter.

60. Popper, in many of his publications, has emphasised the value and significance of the cosmological speculations and conjectures, put forward by the thinkers in each generation, for improving man's understanding of his environment. In his [1963/72, p.141] for example, he writes:

"As to Presocratics, I assert that there is the most perfect possible continuity of thought between their theories and the later developments in physics. Whether they are called philosophers, or pre-scientists, or scientists, matter very little, I think. But I do assert that Anaximander's theory cleared the way for the theories of Aristarchus, Copernicus, Kepler, and Galileo. It is not that he merely 'influenced' these later thinkers; 'influence' is a very superficial category. I would rather put it like this: Anaximander's achievement is valuable in itself, like a work of art. Besides, his achievement made other achievements possible, among them those of the great scientists mentioned."

61. See note 107 below for further relevant arguments.


63. Belief in all-powerful methods has emerged from time to time. The *Ars Magna* of Ramon Lull, for example, in the 13th century, had such universal pretensions, as had such later works as John Dee's *Monas Hieroglyphica*, and Libniz's *De Arte Combinatoria*. (See D. Gjertsen [1989], p.88)

However, a proper reply to all such pretensions is a repetition of a remark made by Bacon with regard to Lull's book. Bacon ([1962], p.145) called it "a method of imposture". In our times despite all new developments in computer-aided problem-solving procedures, no one can claim that computers, or any other machines for that matter, can replace human ingeniousness and creative power. As D. Gillies [1992] has argued, the claim that computers can remake the discoveries of certain human scientists under the same initial conditions, if widely accepted, is likely to be harmful for the development of AI — particularly for the development of the rule-base expert systems: "Those who believe... and think that human scientific creativity can be adequately simulated by a few simple computer programmes will fail to realise that this human creativity is a wonderful resource which can be consciously used to produce improvements in the systems of artificial intelligence." (pp. 30-31)

64. Popper [1963/1972, p.236] has compared the realists' view of truth as a regulative principle, or an ideal goal, with the summit of a mountain towards which the climbers strive, though they may not reach it.

65. See Watkins op.cit.[1984]

66. The idea of knowledge-garnering by error elimination and from our past mistakes has best been explained by Popper in many of his writings. See for example, his [1963/72], [1972/79].

67. See note 119 Chapter Three.

68. Firstly, it can be argued that without a good deal of sound knowledge (true beliefs) about their environment, none of the living organisms could survive for long. The very fact that there exists such a developed species as human beings, is a convincing testimony for the knowledge-garnering ability (via correcting their errors or mistaken beliefs) among living species. (Another version of this argument is used in the next chapter.)

Secondly, (as Laudan would no doubt agree), even a cursory glance at the history of science would reveal a consistent growth in man's knowledge (in the sense of error elimination and learning from the past mistakes). The following cases are among the more celebrated general achievements of scientific enterprise as far as knowledge-garnering is concerned. (I) the successful establishment of the existence of previously unknown entities such as new planets and other heavenly bodies, basic constituents of matter, many live organisms, and so on. (II) The increase in precision of the measurements of experimentally determined quantities such as mass, charge, spin, modes of decay, and many more; the discovery of new constants of
nature, like the speed of light of and the Planck constant; and adding more entries to the inventory of the conservation laws and symmetries. And (III), The successful contribution of scientific theories in; shedding light on the problems, making valid predictions, or bringing about unexpected discoveries, not only in their own relevant fields, but also, in areas not directly related to their own realms. (See R.H.Schelagel [1988])

69. See for example, G.Oddie [1986] and I.Niiniiluoto [1987].

70. For a somewhat similar argument see Newton-Smith [1980a], [1980b].

71. See note 124 Chapter Three.

72. I.Lakatos [1970] criticised Popper’s methodology for being a mixture of naïve falsificationism and sophisticated falsificationism. In Lakatos’s view Popper had not distinguished between rejection and falsification (a charge which Popper [1974, p.1009] rejected). To amend Popper’s methodology, Lakatos introduced his own approach which he maintained to be the truly sophisticated falsificationism. In this approach the unit of theory appraisal was not single theories but series of interconnected theories known as "research programmes". Research programmes consisted of methodological rules, i.e., negative and positive heuristic which would tell the researcher which research route to avoid and which one to pursue. Lakatos distinguished between two types of research programmes; the progressive and the degenerative ones. The former consisted of a series of theories with ever greater empirical content and greater empirical success. According to Lakatos, it was rational to retain successful theories within progressive research programmes even in the face of occasional failures (falsifications).

73. P.Feyerabend [1970] argued that since according to Lakatos’s own account some degenerative research programme may, after a while, turn out to be progressive, this means that within the framework of Lakatos’s methodology one cannot decide the fate of various research programmes. Therefore his rules are of little or no use.

74. See Popper on “Realism” reprinted in his [1983].

75. The issue of ‘making rational sense of science ’, is a problem which realists and all those rational anti-realists, like van Fraassen and Laudan, who do not reject the notion of rationality as non-issue, must face. We shall discuss Laudan’s solution to this problem in the next section.


77. Two good cases in point are the atomic theory and the development of the concept of electric charge. See D.Roller & D.H.D.Roller [1954]

78. The historical cases can be divided into three main categories, namely, (I) cases like the early 20th-century atomism, which were on the right track and achieved enough evidential support (both phenomenal and explanatory) to warrant rational belief, (II) cases like circular inertia and effluvial theories, which, although on the right track, were initially crude so they could not obtain enough support, and (III) cases like phlogiston, caloric and ether theories, which although successful, were not on the right track.

79. A number of realists, who are influenced by the approach of philosophy of language, have tried to devise more suitable theories of reference to account for especially the cases of partial reference, (i.e., where from the perspective of contemporary science there is not one simple predicate for the same extension as was assigned by an earlier theory) and the cases of referent failure, (i.e., cases where there is no extension in the world for the central, explanatory terms of a theory, e.g., phlogiston, caloric and ether theories. See for example B.Enc [1976]), L.Roberts [1986] and D.Gummiskey [1992]. The common denominator of all these approaches is that they try to combine the better parts of the descriptive and causal theories of reference (to create a hybrid theory) and hence overcome their respective shortcomings in dealing with the theoretical terms as against the observational ones. (For an account of the shortcomings of each of these theories of
Apart from new theories of reference, some other realists, equally influenced by philosophy of language, have tried to produce reasonable defence of scientific realism in terms of more traditional (i.e. descriptive) theories of reference. For one such defence see P. Smith [1981].

The point to be noted is however that, philosophy of science need not to become subordinate to philosophy of language. (cf. Popper [1959/68], pp. 13-23). As we have pointed out in the text, a minimal realist can tackle the issue of ontological claims of past theories without resorting to any theory of reference.

80. H. Post in his [1971] which has obtained an almost classic status, and more recently F. Rohrlich and L. Hardin in their [1983], (among others) have drawn on many significant historical cases to show that there is a good deal of correspondence between successful succeeding theories and their successful predecessors.

81. Scepticism can both play a constructive as well as a destructive role in guiding the reason. As N. Maxwell [1984, p. 227] following Popper [1963/72, pp. 238-9] has noted, while nothing is immune to doubt, we need to be sceptical of scepticism itself. Whenever it can be shown that any application of doubt or scepticism can only hinder, and cannot aid, the growth of knowledge and understanding, then we are rationally entitled to abstain from this kind of doubt.

82. Realists would follow Popper’s advice in taking a critical approach towards all theories (conjectures) while exercising a certain degree of patronizing towards successful theories. Popper [1970, p. 55] writes: "I believe that science is essentially critical; that it consists of bold conjectures, controlled by criticism, and that it may, therefore, be described as revolutionary. But I have always stressed the need for some dogmatism: the dogmatic scientist has an important rôle to play. If we give in to criticism too easily, we shall never find out the real power of our theories."


84. H. Post op. cit. [1971].

85. Laudan [1984c]

86. The very notion of growth involves a paradox: a simultaneous coexistence of change and stability (continuity). Without the idea of retention, one cannot account for progress. Retention (preservation) of what is changing, is a necessary condition for progress. Laudan’s own failure to account for the growth of science, because of not invoking the notion of retention is a good case in point. See below sections III-A & III-B.

87. The fully-fledged rejection of the requirement that the later theories diachronically explain/retain the success of the past theories would lead to relativism.

88. As we shall see in the next chapter, some realists like R. Harré, and I. Hacking have upheld the same charge of circularity against inference to the best explanation.

89. In his [1977] Laudan stresses that: "Despite the attempted appropriations of epistemological issues by the professional philosophers, many of the classical questions about the nature of scientific knowledge still remain of broad, general interest: Does science progress? Are our ideas about nature really worthy of any credence? Are some beliefs about the world more rational than others? ... Confronted by the acknowledged failure of the traditional analysis to shed light on the rationality of knowledge, three alternatives seem to be open to us: 1. We might continue to hope that some as yet undiscovered minor variation in the traditional analysis will eventually clarify and justify our intuitions about the cognitive well-foundedness of science ..., 2. We might, alternatively, abandon the search for an adequate model of rationality as a lost cause ..., 3. Finally, we might begin afresh to analyze the rationality of science, deliberately trying to avoid some of the key presuppositions which have produced the breakdown of the traditional analysis... I am inclined to think that we should consider pursuing the third strategy. ... My basic strategy ... will involve the blurring, and perhaps obliteration, of the classical distinction between scientific progress and scientific rationality." ([1977], pp. 1-5)
90. Laudan, as noticed above, maintains that the aim defined by realists for science is unattainable. This has prompted him to opt for a more tangible and therefore obtainable goals. As for relativists, he is of the view that their approach has generated at least two undesirable consequences, namely, that science is not progressive and that science is not different from other belief systems including witchcraft and sorcery. His general criticism of various theories of science (including scientific realism) is that they all render science an irrational pursuit. The following (rather long) quotation fairly sums up his view on this issue:

"1. Philosophers of science, whose primary aim is to define what rationality is, have generally found that their models of rationality find few, if any, exemplifications in the actual process of scientific activity. If we accept the claim made on behalf of these models to the effect that they define rationality itself, then we seem forced to view virtually all science as irrational.

2. Attempts to show that the methods of science guarantee that it is true, probable, progressive, or highly confirmed knowledge, ... have generally failed, raising a distinct presumption that scientific theories are neither true, nor probable, nor progressive, nor highly confirmed.

3. Sociologists of knowledge have been able to point to several episodes in the recent (and distant) past of science which seem to reveal many nonrational, or irrational, factors decisively involved in scientific decision making.

4. Some historians and philosophers of science (e.g. Kuhn and Feyerabend) have argued, not merely that certain decisions between theories in science have been irrational, but that choices between competing scientific theories, in the nature of the case, must be irrational. They, especially Kuhn, have also suggested that every gain in our knowledge is accompanied by attendant losses, so that it is impossible to ascertain when, or even whether, we are progressing.

The scepticism to which such conclusions point has been reinforced by the general arguments of cultural relativism to the effect that science is just one set of beliefs among many possible ones ... all systems of belief, including science, are seen as dogmas, and ideologies, between which objective, rational preference is impossible." (Laudan [1977], pp.2,3)

As we shall see in the next chapter, R.Harré [1986, pp. 1-5] has appealed to essentially the same arguments in rejecting current theories (methodologies) of science.

91. "I am deeply troubled by the unanimity with which philosophers have made progress parasitic upon rationality. In part, my worry arises from a concern that it involves explaining something which can be readily understood (progress) in terms of something else (rationality) which may be far more obscure. More serious, however, is the absence of any convincing argument as to why we should explicate our concept of progress in terms of rationality. ... It will be the assumption here that we may be able to learn something by inverting the presumed dependency of progress on rationality. I shall try to show that we have a clearer model for scientific progress than we do for scientific rationality ..." (Laudan [1977], p.6)

"...It has normally held that any assessment of either rationality or scientific progress is inevitably bound up with the question of the truth of scientific theories. Rationality, it is usually argued, amounts to accepting those statements about the world which we have good reason for believing to be true. Progress, in its turn, is usually seen as a successive attainment of truth by a process of approximation and self-correction. I want to turn the usual view on its head by making rationality parasitic upon progressiveness" (ibid. p.125)

92. See note 105 below. For a discussion of the issue of content comparison see Popper [1959/1968]. Laudan [1977] has addressed his critical remarks mainly against this source.

93. Laudan, despite the similarity of many of his views on science with those of van Fraassen, (for example, his agnosticism towards the truth value of theoretical terms or his emphasis on pragmatic factors in theory choice), contrary to him maintains that non-empirical (i.e., metaphysical) doctrines play a major role in the progress of science. Following a lead from Kuhn (paradigms) and Lakatos (research programmes), he has called the unit for appraisal in science, research traditions (or R.T.'s). "A research tradition is a set of assumptions about the entities and processes in a domain of study and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain". ([1971], p.81). Every R.T. has a number of specific theories which exemplify and partially constitute it; some of these theories will be contemporaneous; others will be temporal successors of earlier ones. R.T.'s, contrary to the theories that constitute them, are neither explanatory, nor predictive, nor directly testable. They are global theories which their very generality, as well as their normative elements, preclude them from leading to detailed accounts of specific natural process. (see Laudan [1977], ch.3)
94. Laudan [1977], p.72.

95. On Kuhn’s model:

"... Kuhn’s model of scientific progress suffers from acute conceptual and empirical difficulties. For instance, Kuhn’s account of paradigm and their careers had been extensively criticized by Shapere ... Feyerabend and others have stressed the historical incorrectness of Kuhn’s stipulation that ‘normal science’ is in any way typical or normal ... Numerous critics have noted the arbitrariness of Kuhn’s theory of crisis ... There are other serious flaws as well ... the most significant of these are, 1) Kuhn’s failure to see the role of conceptual problems in scientific debate and in paradigm evaluation ... 2) Kuhn never really resolve the crucial question of the relationship between a paradigm and its constituent theories ... 3) Kuhn’s paradigms have a rigidity of structure which precludes them from evolving through the course of time ... 4) Kuhn’s paradigms, or ‘disciplinary matrices’, are always implicit, never fully articulated... 5) Because paradigms are so implicit and can only be identified by pointing to their ‘exemplars’ ... it follows that whenever two scientists utilize the same exemplars, they are, for Kuhn, ipso facto committed to the same paradigm. Such an approach ignores the persistent fact that different scientists often utilize the same laws or exemplar, yet subscribe to radically different ontology and methodology. (For example both mechanists and energeticists accepted identical conservation laws)." (ibid. pp.74-5)

On Lakatos model:

"... Lakatos’s model of research programmes shares many of the flaws of Kuhn’s paradigms, and introduces some new ones as well: 1) As with Kuhn, Lakatos’s conception of progress is exclusively empirical: the only modifications in a theory are those which increase the scope of its empirical claims... 2) The sort of changes which Lakatos allows within the mini-theories which constitute his research-programmes are extremely restricted... 3) A fatal flaw in the Lakatosian notion of research-programme is its dependence upon the Tarski-Popper notion of ‘empirical and logical content’ ... 4) Because of Lakatos’s idiosyncratic view that the acceptance of theories can scarcely, if ever, be rational, he cannot translate his assessments of progress ... into recommendations about cognitive action... 5) Lakatos’s claim that the accumulation of anomalies has bearing on the appraisal of a research-programme is massively refuted by the history of science... 6) Lakatos’s research-programmes, like Kuhn’s paradigms, are rigid in their hard-core structure and admit of no fundamental changes.” (ibid. pp.77-8)

96. Laudan’s treatment of the notion of problem and its central role in scientific progress draws much on works of Popper (see below), Duhem, Kuhn and Lakatos ( op.cit. [1977], passim.) among others. The end product is a view not dissimilar to that of Toulmin. (cf. Toulmin [1972]). E.McMullin [1979] has pointed out ‘obvious affinity’ between Laudan’s and Toulmin’s analyses. H.Siegel [1983] has provided a critical account of Toulmin’s and Laudan’s models. In these models, as we have already seen, science is viewed not as a truth-seeking process but as an activity aimed at solving practical problems.

97. Laudan [1977], p.11. As noted above (Ch.2) regarding science as essentially a problem-solving activity has been emphasised by many writers before Laudan. For example, Peirce, Dewey, Popper, Kuhn, and Toulmin. See McMullin [1979], Feyerabend [1981]. See also note 102 below.

98. ibid. p.66.

Empirical and conceptual problems are defined as follows: "Empirical problems are first order problems; they are substantiative questions about the objects which constitute the domain of any science" (ibid. p.15. italics in original).

"... a conceptual problem is a problem exhibited by some theory or other. Conceptual problems are characteristics of theories and have no existence independent of the theories which exhibit them, not even that limited autonomy which empirical problems sometimes possess. If empirical problems are first order questions about substantive entities in some domains, conceptual problems are higher-order questions about the well foundedness of conceptual structures (e.g. theories) which have been devised to answer the first order questions.” (ibid. p.48. italics, the first, in original, the second, mine.)

99. ibid. p.68.

Degree of problem solving or "the overall problem solving effectiveness" is determined "by assessing the number and importance of empirical problems which the theory solves, and deducting therefrom the number and importance of the anomalous and conceptual problems which the theory generates". ( ibid.)
100. "There is a broad spectrum of cognitive stances which scientists take towards theories, including accepting, pursuing, entertaining, etc. Any theory of rationality which discusses only the first two will be incapable of addressing itself to the vast majority of situations confronting scientists". (Laudan [1981], p.144)

101. Laudan has defined the above-mentioned concepts in a rather cumbersome way:

The **momentary adequacy** of a research tradition (R.T) is the sum total of problem-solving efficiency of the latest theories within the R.T. (p.106)

The **adequacy** or effectiveness of individual theories is a function of how many significant empirical problems they solve, and how many important anomalies and conceptual problems they generate. (p.106)

The **progressiveness** of an R.T is a temporal matter, i.e. it requires a historical survey and depends on two subordinate measures namely,

A) the general progress of a R.T which is determined by comparing the adequacy of the sets of theories which constitute the oldest and those which constitute the most recent versions of R.T.; and

B) the rate of progress of a R.T during any specified time span.

Obviously (A) and (B) may be widely at odds. Likewise the appraisals of an R.T based upon its progressiveness may be very different from those based on its momentary adequacy, hence two more modalities of appraisals are required, i.e.

**Acceptability** or warranted assertibility: scientists often choose to accept one among a group of competing theories and R.T.’s, i.e. to treat as if it were true; and

**Pursuability** or promise which is related to rate of progress and involves estimating the changes in problem-solving effectiveness over a period. (pp.107-114, 119)

Laudan emphasises that determinations of truth and falsity are irrelevant to the acceptability or pursuability of theories and R.T.’s. He also maintains that evaluations of R.T’s and theories must be made within a comparative context. (p.120)

102. Laudan has adopted, *inter alia*, the notions of problem and problem solving from Popper. Many writers, including Feyerabend [1981], and McMullin [1979] have argued that Laudan’s use of the term problem, far from an improvement on Popper’s initial introduction of the term, is only a degenerated usage, with no real relevance to actual business of science.

While Laudan’s adaptation of Popperian notion of problem-solving is a degenerate one, Popper’s own version, is also without its own difficulties. For a brief assessment of the difficulties of Popper’s notion of problem-solving, see J.Waterhouse [1984], pp. 380-8. Waterhouse has argued that Popper’s conception of problem solving and his conviction that each theory should be a solution to a problem, although intuitively appealing, cannot withstand the many counter examples which may be brought against it. He maintains that contrary to Popper’s suggestion, a theory can be both comprehensible and criticizable independent of any problem it is designed to solve.

103. In constructing a measure for problem-solving effectiveness, Laudan has made a number of risky assumptions, namely,

1) problems can unequivocally be recognized as problems;
2) one can tell when a problem has been solved;
3) one can *individuate* problems sufficiently sharply to count them;
4) one can assign relative weight to all kinds of problems;
5) one can deduct negative weights from positive ones;
6) one can assign weight to theories derivative from an R.T.

It should be added that Laudan’s model suffer from more *conceptual weaknesses* than listed above. For example, the notion of research traditions (R.T.’s) do not have a clear, unequivocal sense in Laudan. This, in turn, makes it almost impossible to make sensible comparisons between R.T.’s in terms of their fruitfulness or promise. For details see McMullin, *op.cit.* [1979].

104. This problem which is traditionally attributed to Heraclitus is simply stated as, How is change possible? How can a thing change without losing its identity? For an exposition and reconstruction of Heraclitus’ argument see Popper [1963/72], ch.5.
Laudan has tried to replace the more traditional accounts of content comparison, (e.g. degrees of confirmation, falsification or corroboration) with degree of problem-solving effectiveness. However, as noted in the text, due to insuperable difficulties surrounding the very notion of problem and its measurement, Laudan has not been able to offer scientists with a viable alternative account of theory choice.

For a realist approach to accounting for content losses and gains see H.Post [1971].

As pointed out above, Laudan, in almost all of his publications, has tried to distance his views from those of relativists. His latest book [1990a] is in fact a long essay against relativism. See also his replies [1988] to J.Worrall’s criticism [1988], where he criticises Worrall for misconstruing the threat from relativism. He writes: "The central claim of the epistemic relativist, at least where standards and methods are concerned, is not that those standards change but that — whether changing or unchanging — those standards have no independent, non-question begging rationale or foundation". (ibid. p.369)

However, as is evident from the point discussed in the text, since Laudan is unable to provide a non-arbitrary way for theory comparison, his own model collapses into the abyss of relativism.

Apart from the vulnerability of Laudan’s system to relativist intrusion, it also needs to be emphasised that Laudan’s arguments against relativists (as for example developed in his [1990]) do not and cannot rebut the more sophisticated versions of relativism. As P.Lipton [1992, p.189], has argued, Laudan’s two main objections against relativism, namely, that the relativist makes an unwarranted jump from anti-foundationism to scepticism, and that relativism is self-defeating, do not work. In the case of scepticism, a sophisticated relativist can respond to Laudan that, not only are the canonical rules of science not justifiable, but that science does not obey them (the impact of external factors). Secondly, in the case of self-reflexive objection, the sophisticated relativist may point out that our actual inferential habits may be psychologically irresistible, at least for a time and for a culture, so we cannot but follow them even as we undermine their warrant.

As noted in II.A., Laudan’s rejection of a goal such as truth implies that, in his view, science should only pursue tangible, achievable goals. This point plus the fact that Laudan’s conception of solutions is of basically practical solutions which enable the scientists to predict and control, invites the following observation. Laudan’s advice, if taken seriously by scientists, could not only reduce science to some sort of tool making, puzzle solving activity, but also could reduce scientists to mere puzzle-solver robots.

To see this, one only needs to think of Searl’s apt analogy namely the Chinese house (J.Searl [1984], pp.32-5). The attendant of the house, which may well be a mere robot, solves the problems given to him, by following a number of rules, without any need to understand those rules or the solutions or even the problems.

Now, in Laudan’s model, scientists are facing the possibility of being reduced to problem-solver robots: as long as their suggested solutions would work, they do not need to bother themselves about the understanding of what is going on beneath the level of appearances. It is not clear how (or even why), in Laudan’s philosophy of science, scientists could (should) break away from one puzzle solving tradition and join another one, provided what they have already subscribed to, is by and large, successful.

Science, of course, contrary to what Laudan implies, is not solely a problem-solving enterprise. Scientists, not only attempt to solve problems, but also spend a good deal of their times for collecting and processing data, making measurements, testing, assessing, describing phenomena and more important of all, explaining these phenomena.

The notion of truth props up in a number of crucial points in Laudan’s system;

Firstly, in the central notions of problem and problem-solving. Laudan writes:

"A problem needs not to describe a real state of affairs to be a problem; all that is required is that it be thought to be an actual state of affairs by some agent. For instance, early members of the Royal Society of London, convinced by mariners’ tale of the existence of sea serpents, regarded the properties and behaviour of such serpents as an empirical problem to be solved.” (ibid. p.16.)

"A theory may solve a problem as long as it entails even an approximate sentence of the problem; in determining if a theory solves a problem, it is irrelevant whether the theory is true or false, well or poorly confirmed ..." (ibid. p.209.)

But these two requirements without the support of truth, (and in the absence of any consistent algorithm for measuring problems and assessing solutions) will lay Laudan’s model open to the danger of relativism: all sorts of problems (pseudo, spurious, irrelevant, ... and of course really significant problems) can be
produced by all sorts of people (crank, charlatan, insane, ... and of course real genius), and all in the name of sound science. By depriving scientists of the basic notion of truth, Laudan is actually placing them in an impossible position: they will not be able to pursue their researches in a sensible way.

Secondly, in determining a viable aim for science. Laudan maintains that:

"raison d'être of science is to put us in a position to anticipate nature..." (Laudan [1982], p.80.)

"The main aim of science is to generate theories that are increasingly reliable tools for prediction and control." (Laudan [1990a]).

As noticed earlier, despite the appearance to the contrary, Laudan does not want to subscribe to old-school instrumentalism in which theories were denied truth-values. He would rather like to embrace a position like that of van Fraassen, concerning the truth content of theories. However, the difficulty with Laudan’s system is that it does not enjoy the relative consistency of van Fraassen’s. On the one hand, if he insists on the correctness of predictions, then this means that he has admitted that science is a truth-pursuing enterprise. On the other hand, if he drops the requirement for correctness of predictions, while this in itself is rather odd and contrary to the activities of scientists, it would once again opens up the relativism’s Pandora box: all non-scientific predictive devices, such as horoscopes, crystal balls, cards, etc. will be elevated to the level of scientific instruments, since they could have a very high problem-solving ability. (For details see Niiniluoto [1984], pp.167-8.)

Thirdly, in elaborating the notion of progress in science. Laudan insists that progress is not cumulative. But this, as we have shown in this chapter, once again pushes his position towards relativism.

109. See for example, Laudan [1990a] which is a lively debate between a realist, a relativist, a positivist, and a pragmatist. Laudan plays the rôle of pragmatist. In chapter one entitled 'Progress and Cumulativity' Laudan reiterates his previous views concerning the problem-solving function of science. However, this time he makes no attempt to refine the very notion of problem and rests entirely on its intuitive, pre-analytic meaning.

In this context it is interesting to note that Laudan in yet another of his recently written articles, (i.e., [1986a]) criticizes himself for having relied on the pre-analytic, intuitive views about historical cases and theory choices, and warns against such reliance. He writes:

"In brief, it will be the thesis of this paper that we need not — and that there are some good reasons for suspecting that we ought not — make a concordance between our pre-analytic intuitions (whether historical or not) and the verdicts issued by rival methodologies the test of soundness among those rivals". ([1986a], p.119)

"The demand that our methodological concepts (e.g. of warranted acceptance) must replicate or square with our pre-analytic, intuitive judgements (about which theories are acceptable) is, in effect, to see epistemology of science as nothing other than the ordinary language philosophy of a special linguistic community, viz, the users of scientific language. As such, it is open to all the familiar limitations of ordinary language philosophy as a genre." (Laudan [1986a], p.123)

McMullin (op.cit.) had long ago noticed that Laudan’s reliance on ‘pre-analytic’ intuitions of rationality is both problematic and contrary to the thrust of Laudan’s approach, which is to make rationality a function of problem-solving effectiveness and progressiveness rather than the reverse.

As stated in the text, in recent years Laudan has tried to play down the significance of his [1977] approach and instead concentrate on some other aspects of methodological matters.

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aim of science is to generate theories that are increasingly reliable tools for prediction and control [Laudan 1990a]. This is of course an instrumentalist aim, which reduces science to a practical problem-solving enterprise. Thus, Laudan’s instrumentalistic conception of science, plus his naturalizing of methodology, renders his methodological inquiry a pursuit of practical means for practical aims, with no room left for either conceptual means or goals.

113. "... methodological rules are thus hypothetical imperatives, of the form: 'If one’s goal is x, one ought to do y.' " ([1986b], p.349)

"I submit that all methodological rules should be construed not ... as if they were categorical imperatives, but rather as hypothetical imperatives. Specifically, I believe that methodological rules, when freed from the elliptical form in which they are often formulated, take the form of hypothetical imperatives whose antecedent is a statement about aims or goals, and whose consequent is the elliptical expression of the mandated action. Put schematically, methodological rules of the form: (0) 'One ought to do x', should be understood as having the form: '(1) If one’s goal is y, then one ought to do x '." (Laudan [1987a], p.24)

114. "If we conceive of methodological rules as prudential hypothetical imperatives, then we can both have our cake and eat it. Such rules retain all the normative force associated with any prudential rule of conduct, yet they derive their warrant from empirical information about how this particular world is constituted. One can thus 'naturalize' methodology (thereby avoiding the twin perils of treating methodology as either a priori or conventional), without being forced (with Quine) to believe that making it empirical and descriptive robs it of its normative force." (op.cit. [1986b], p.350)

"I am suggesting that we conceive rules or maxims as resting on claims about the empirical world, claims to be assayed in precisely the same ways in which we test other empirical theories. ... We thus have no need of a special meta-methodology of science; rather we can choose between rival methodologies in precisely the same way we choose between rival empirical theories of other sorts. (op.cit. [1987a], p.24)

115. "The methodology of inquiry has to be supplemented by the axiology of inquiry; by that phrase, I mean the study of the legitimacy of propounded aims or goals. ... I believe that axiology, like methodology, can be suitably naturalized, and thus provided with a non-arbitrary decision procedure." (op.cit. [1986b], p.352)

"I have said that a methodology is one key part of a theory of scientific progress. But there is another equally central part of a theory of cognitive progress. ... I suspect we all believe that some cognitive ends are preferable to others. Methodology, narrowly conceived, is in no position to make those judgements, since it is restricted to the study of means and ends. We thus need to supplement methodology with an investigation into the legitimate or permissible ends of inquiry. That is, a theory of scientific progress needs an axiology of inquiry, whose function is to certify or de-certify certain proposed as legitimate." (op.cit. [1987a], p.29).

"... If the aims of science differ significantly through the course of science, how can we avoid the thorough-going relativization of epistemology which seems to follow from the acknowledgement that, when the aims of science are concerned, it is a matter of 'different strokes for different folks'? What is needed in a comprehensive naturalized epistemology is not only an account of methodology but also a naturalistic axiology". (Laudan [1990d], p.47)

116. "Axiology, methodology, and factual claims are inevitably intertwined in relations of mutual dependency. ... [T]heories change, methods change, and cognitive aims shift." (Laudan [1984a], pp.63-4)

117. The hierarchical model was and still is one of the most influential methodological models. It was originally developed by positivist philosophers, who developed it in close parallel with their own understanding of the language levels in science: (cf. J.Loose, op.cit.)
In the hierarchical model the direction of justification is from the top to the bottom. This model, as Laudan has rightly observed (see his [1984a]), cannot account for the fact that aims and methods in science are ever-changing. In this model, factual/theoretical disputes are resolved by appeal to fixed methodological rules, and the disputes over these rules are adjudicated by appeal to axiology (i.e. the aims of science). The disputes over these fixed aims however, cannot be rationally resolved, since there is no other higher court of appeal.

118. See Popper [1972/79], ch.2.

It seems that Laudan is determined to recreate Popper's methodology in a rather degenerate form. We have already seen that Laudan's treatment of the Popperian view of science as a problem-solving enterprise amounted to presentation of a distorted image of science. Here, too, it seems Laudan has invested in some idea which Popper has long ago discredited. In his [1974] in reply to a famous objection, namely whether the principle of falsifiability can be falsified, Popper writes: "... The answer is that my theory is not empirical, but methodological or philosophical, and it need not therefore be falsifiable. ... In fact, I devoted a whole chapter — chapter 2 — of Logik der Forschung to the problem of the status of theory of scientific method. In this I took the view that methodology was not an empirical science... " ([1974], p.1010, emphasis added)

119. A number of such rules taken from Laudan's own historical researches are given below. This list can of course be extended considerably.

* prefer simple theories to complex ones.
* accept a new theory only if it can explain all the success of its predecessors.
* reject inconsistent theories.
* propound only falsifiable theories.
* avoid theories that postulate unobservable entities.
* prefer theories that make successful surprising predictions over theories which explain only what is already known.
* prefer theories that explain, or are confirmed by, a wide variety of phenomena distinct from those which they were initially introduced in order to explain. (cf. Doppelt [1990, p.11])

120. Popper [1963/72, ch.8] has argued the same point for philosophical theories. One way for testing (appraising) the credibility of a methodology is to compare the fruitfulness of the research programme it gives rise to with other rival programmes which are advocated by other rival methodologies. Agassi [1981] has long ago discussed a similar case for assessing the credibility of rival metaphysics.

121. Each methodological rule, while prescribing a certain course of action, is tacitly assuming that a certain state of affairs is actually obtained in nature. Thus for example the rule which prescribe the preference of simpler theories over more complex theories is assuming that our world is in its more fundamental level simple and thus can be best represented by simpler theories. However the view that nature is simple which provides the basis for the above methodological rule is not empirical. For detailed discussions of this point see Agassi [1981], Rosenberg [1985].

122. Laudan [1986a], p.123, italics added. See also his [1984a], pp.35-7.

123. One such rational evaluation has been suggested by P.Lipton [1991].

124. "We take science seriously precisely because it has promoted ends which we find cognitively important. More than that, it has become progressively more successful as times goes by. if you ask, 'Successful according to whom?' or 'Progressively according to what standards?' the answer, of course, is: successful by our lights; progressive according to our standards." (Laudan [1978a], p.28)

125. "I have said that the key rôle for history vis à vis methodology is that of providing evidence about ends/means connections." (Laudan [1987a], p.28 and passim). See also Laudan [1977], [1984a], [1986a]. (cf. Rosenberg, op.cit. for a somewhat similar argument)
126. "In short—and it is crucial—cognitive aims typically underdetermine methodological rules in precisely the same way that methodological rules characteristically underdetermine factual choices." op.cit. [1984a], p.35.

127. "To put it in a nutshell, I shall be seeking to show that the doctrine of underdetermination, and the assaults on methodology that have been mounted in its name, founder precisely because they suppose that the logically possible and reasonable are coextensive. Specifically, they rest on the assumption that, unless we can show that a scientific hypothesis cannot possibly be reconciled with the evidence, then we have no epistemic grounds for faulting those who espouse that hypothesis." Laudan [1990c], p. 449

"In this paper, we reject the supposition of empirical equivalence and inference from it to underdetermination. Not only is there no general guarantee of the possibility of empirically equivalent rivals to a given theory, but empirical equivalence itself is a problematic notion without safe application. Moreover, the empirical equivalence of a group of rival theories, should it obtain, would not by itself establish that they are underdetermined by the evidence. One of a number of empirically equivalent theories may be uniquely preferable on evidently probative grounds." Laudan & Leplin [1991], pp.450-1. See also Laudan & Leplin [1993].

128. Laudan [1984], p.64.

129. This conflation of the aim of science with the aims of individual scientists is a common feature of the majority of sociologists of science. We will further discuss this point in the next chapter. In the concluding chapter we shall argue that a sound version of reticulational model in which aims, methods and theories are ever-changing can be upheld within the framework of a more consistent and comprehensive realist theory of science.

130. For a defence of traditional view concerning fixed aims and methods see J. Worrall [1988], [1989]).

131. Popper uses the criterion of falsifiability or refutability, *inter alia*, to a) distinguish between scientific and non-scientific theories and b) to choose the more promising scientific theory among a number of competing ones. However, as Popper himself has admitted, in the end falsifiability relies on the personal *decisions* of scientists.

   In relation to (b) Popper writes in his [1959/68, pp.108-9]:
   "It may now be possible for us to answer the question, How and why do we choose one theory in preference to others? ... We choose the theory which ... not only has hitherto stood up to the severest tests, but the one which is also testable in the most rigorous way. ... From a logical point of view, the testing of a theory depends upon basic statements whose acceptance or rejection, in its turn, depends upon our *decisions*. Thus it is *decisions* which settle the fate of theories. To this extent my answer to the question, 'how do we select a theory?' resembles that given by the conventionalist ..."

   And in relation to (a) he points out that:
   "According to the conventionalist view, it is not possible to divide systems of theories into falsifiable and non-falsifiable ones; or rather, such a distinction will be ambiguous. ... These objections of an imaginary conventionalist seems to me incontestable, .... I admit that my criterion of falsifiability does not led into an unambiguous classification. ... The only way to avoid conventionalism is by making a *decision*: the decision not to apply its methods." (ibid. pp.81-2)

   Of course to fight conventionalism by means of a personal *decision* amounts to nothing but rejecting conventionalism by means of a convention!
Chapter Five
Entity-Realism: A Half Way House

I. From truth to praxis

Philosophy of science in recent years has witnessed, among other things, two major developments. The first one is a movement among the realist philosophers away from many of the views held by the realists of the past generations and towards increasingly less risky positions. This movement is apparently a result of sustained and elaborated attacks by modern anti-realists. Phrases like ‘modest realism\(^1\)’, ‘blind realism\(^2\)’, ‘fig leaf realism\(^3\)’ and the like are devised to capture and represent this new mood among realists. The second noticeable change is a healthy attention paid by various writers to the rôle and significance of practical activities in science. Titles like, "The uses of Experiment"\(^4\), "The Neglect of Experiment"\(^5\), "Experiment Right or Wrong"\(^6\), and "How Experiments End"\(^7\) are clear manifestations of this new trend.

Entity-realists are a group of realist writers who have positively responded to both of these new waves of change. Among the better known members of this group one can name I.Hacking\(^8\), N.Cartwright\(^9\), R.Harré\(^10\), and B.Ellis\(^11\). Despite differences in style and diversity of approaches, a common core can be discerned in the views of these modern scientific realists. This common core consists of a rejection of logical positivists’ (and their heirs apparent or allies\(^12\)) programme(s) for philosophy of science, and a change of emphasis away from the representational function of scientific theories and towards the epistemological significance of practical activities in science, i.e. observation, experimentation, measurement and technological intervention in the course of nature.

This change of emphasis has found its clearest expression in the opposition of the group to a version of realism known as *truth-realism* (i.e. the doctrine that scientific
theories aim to provide us with more or less true interpretations and/or representation of reality). In place of this more traditional version of realism, the members of the group have introduced *entity-realism* (the doctrine which states that many of the theoretical entities posited by more mature sciences actually exist, even though the descriptions provided by these sciences may not be quite accurate) and have claimed that realism will be more defensible, if realists, instead of bothering about the *truth* of scientific theories, focus on the *reality* of the theoretical posits.

Truth, of course, seems to have always been a perplexing issue for philosophers. Such difficulties had encouraged philosophers of the previous generations to introduce theories of truth other than the traditional correspondence theory. In recent times it seems that the general trend among many anti-realist philosophers has been a move away from the apparently troublesome notion of truth, towards seemingly more manageable concepts like ‘warranted assertibility’, or to dispense with the notion of truth altogether. On the other hand, it has been the conventional wisdom amongst philosophers that while anti-realists, generally speaking, can more easily put the notion of truth aside and resort to other surrogates for it, for realists, to settle for anything less than a correspondence notion of truth, and to aim for something else rather than the truth about physical reality, is to undermine their own position.

It is against this background that the claim of entity-realists in salvaging realism without resorting to theoretical truth gains prominence and significance. Entity-realists’ strategy is two-fold. On the one hand, they have tried to answer anti-realists’ two main possible objections to their programme, namely, 1) if all our scientific theories are born falsified then how can we claim that we *know* their theoretical posits exist? and 2) in view of the rapid and radical theory change, how can we talk about the *same* entity across a
large range of superseded theories? They have tried to respond to these difficulties by a tacit or explicit appeal to new (causal) theories of reference. On the other hand, they have produced a number of arguments to show that truth-oriented versions of realism are not tenable. Among these arguments, the better known are 'the phenomenological interpretation of fundamental laws', 'the epistemologizing of truth', and 'underdetermination of theory by data'. Apart from their onslaught on truth, entity-realists have produced a pragmatic theory of truth to replace realists' correspondence theory.

Since we have already assessed the force of argument from underdetermination against the minimal version of realism defended in this essay, here, in the present chapter we shall examine entity-realists' argument in defence of the primacy of action over speculation, their arguments against the veracity of fundamental laws of nature, their criticisms of the correspondence theory of truth, and their alternative pragmatic theory of truth.

The main task of the chapter will be to show that whilst the concerns of the entity-realists are genuine and their contributions welcomed, their proposed way of responding to the waves of change, despite all its positive aspects, is not entirely satisfactory and as such, from a methodological point of view, only amounts to a half-way house. I shall try to establish this basic point by arguing that 1) entity-realists' major arguments either for their own positions or against truth-realism do not hold; and 2) in eschewing truth-realism, entity-realists are in effect throwing out the baby with the bath water: entity-realism without truth-realism not only cannot reject the criticisms of anti-realists, but also itself degenerates into anti-realism.
II. The *Instrumentalistic* Defence of Realism

As discussed earlier, inference to the best explanation, has traditionally been and still is one of the powerful arguments at the disposal of realists to defend the truth of scientific theories and the reality of their posits. Entity-realists, as mentioned above, are not happy with the representational function of theories. They think scientific realism can only be defended by arguing for the reality of the unobservable scientific entities. Accordingly, they have produced an argument for establishing the reality of theoretical posits which, it is claimed, does not make use of the validity of the scientific theories or their explanatory powers but is based on the empirical exploitation of nature by means of scientific instruments. As such, this argument may be called the *instrumentalistic* defence of realism. Hacking has given a succinct version of the argument which he has dubbed 'the experimental argument for realism':

> We are completely convinced of the reality of electrons when we regularly set out to build — and often enough succeed in building — new kinds of device that use various well-understood causal properties of electrons to interfere in other more hypothetical parts of nature.

The best kind of evidence for the reality of a postulated or inferred entity is that we can begin to measure it or otherwise understand its causal powers. The best evidence, in turn, that we have this kind of understanding is that we can set out, *from scratch*, to build machines that will work fairly reliably, taking advantage of this or that causal nexus. Hence engineering, not theorizing, is the best proof of scientific realism about entities.

Experimental work provides the strongest evidence for scientific realism. This is not because we test hypotheses about entities. It is because entities that in principle cannot be "observed" are regularly manipulated to produce a new phenomena and to investigate other aspects of nature. They are tools, instruments not for thinking but for doing.

The *instrumentalistic* (or experimental) argument for realism, in a sense, is not an entirely new argument. M.Bunge for example, many years ago, drawing on Popper, has argued that, "experiment presupposes realism and confirms it." What gives this argument an air of novelty when used by the entity-realist is their insistence that this argument is an independent and self-sufficient argument. It invokes only the notion of praxis and does not fall back on theoretical considerations. As Hacking has put it:
"[E]ngineering, and not theorizing, is the best proof of scientific realism about entities."³¹

This conclusion however, seems to be rather hasty. It seems that, contrary to the conviction of the entity-realists, the Instrumentalistic argument can only be regarded as a back-up argument for realists' more traditional argument namely, inference to the best explanation. Alternatively, as a non-inductivist like Popper would put it, the Instrumentalistic argument would, at best, show that the experimentalists's rôle is to answer the questions put to them by theoreticians' conjectures.³² As Bunge has put it: "In short, experimental physics assumes the reality of the objects it manipulates, and it tests some of theoretical hypotheses made about the existence of physical system."³³ This same point can be put in the following way: in order to build sophisticated machines or instruments in order to invoke the causal powers of the theoretical entities which in turn provide good grounds for the existence of the entities in question, we need to rely on two types of theories one, which puts us in a position to be able to claim that we have well understood the causal powers of these entities. the other, which enables us to construct sophisticated machines, purpose built to manipulate these powers.³⁴

Contrary to what Hacking says, it is impossible to set out from scratch, to build machines which work reliably and make use of this or that causal power. Our knowledge about these causal powers comes from our theories, and unless what they tell us about the properties of the postulated entities are reasonably true, setting out to build machines which are to manipulate these very properties cannot be considered as a reasonable and justified act. Hacking, perhaps anticipating the above criticism, has tried to pre-empt it:
There are an enormous number of ways in which to make instruments that rely on the causal properties of electrons in order to produce desired effects of unsurpassed precision. The argument — it could be called the "experimental argument for realism" — is not that we infer the reality of electrons from our success. We do not make the instruments and then infer the reality of the electrons, as when we test a hypothesis, and then believe it because it passed the test. That gets the time order wrong. By now we design apparatus relying on a modest number of homely truths about electrons in order to produce some other phenomenon that we investigate.\(^5\)

However, a crucial point, which Hacking seems to have not paid enough attention to, is the following question: from where have we got these innocent-looking homely truths about electrons? Surely they cannot be regarded as innate or given. All the machines, instruments and technologies that we are using to probe deep into nature are themselves products of a considerable growth in our theoretical understanding of the secrets of nature and the functions of the mechanisms at work in it. While it sounds like a rather dull truism to say that the progress of modern science has been made possible due to a sort of dialectical relation between theoretical, practical and technological knowledge\(^6\), the point which requires attention and emphasis is that the rôles of the two latter kind of knowledge have been one of supporting and consolidating the first kind of knowledge, and not an independent one. To paraphrase Lakatos: technological and practical knowledge without theoretical knowledge are blind\(^7\). The unsuccessful experience of the so-called third world countries in copying the technological achievements of the industrial world, without being equipped with its scientific background, is a vivid and strong argument in favour of the point defended here: instruments and machines are themselves products of theories and as such cannot provide independent and sufficient scientific knowledge, including knowledge about the unobserved entities and mechanisms.

The instrumentalistic argument, as mentioned above, can serve realists as a back-up argument for the argument from the success of theory. In this sense, the findings of instruments, i.e. the practical exploitation of the causal power of the entity in question,
can provide further warrants for scientists’ belief in the existence of that entity. As Popper has put it: "We accept things as ‘real’ if they can causally act upon, or interact with, ordinary real material things." Gradually, with the introduction of more elaborate theories and construction of more powerful machines, one would reach a point where on could regard a hypothesized entity as a real one. E. MacKinnon in his [1972] has given two examples which show the kind of relation which exists between theory and experiment:

When Pauli postulated the neutrino in 1930 it was looked on as an ad-hoc hypothesis to save the principle of energy conservation in explanation of beta decay. After Fermi developed a mathematical theory of beta decay, in which the Pauli assumption played a key rôle, the existence of neutrino gradually came to be accepted. It was an assumption which was necessary to explain various types of particle decays ... and played a rôle in other conservation laws besides energy. Physicists accordingly, had little hesitation about including neutrino in lists of fundamental particles even prior to its experimental detection by Reines and Cowan.

Both mesons and anti-particles were first predicted on theoretical grounds, but not accepted as real until they were discovered experimentally. The case of anti-particles is rather revealing. In his original paper, on the relativistic theory of electron, Dirac said that he hoped to find some way of getting rid of the negative energy solution. When they proved to be an inescapable ontic commitment of the theory he devised his famous interpretation of an infinite sea of negative energy with anti-particles as holes in that sea. Few physicists took it seriously until Anderson discovered the positron experimentally in 1932.

These cases, which are typical of scientific activities, vividly show the rôle of the theory in guiding the experiment. Hacking seems (at least on the face of it) to endorse this observation:

Once upon a time it made good sense to doubt that there are electrons. Even after Thomson had measured the mass of his corpuscles, and Millikan their charge, doubt could have made sense. We needed to be sure Millikan was measuring the same entity as Thomson. More theoretical elaboration was needed. The idea needed to be fed into many other phenomena. Solid state physics, the atom, superconductivity; all had to play their role.

However, having conceded this much, and furthermore, having pointed out the important issue of determining the identity of the entity in question (a major point to which we come shortly) he goes on to state the real version of his argument:

Once upon a time the best reason for thinking that there are electrons might have been success in explanation. We have seen ... how Lorentz explained the Faraday effect with his electron theory. I have said that the ability to explain carries little warrant of truth. Even from the time of J.J.Thomson, it was the measurement that weighed in, more than the explanations. Explanations did help. Some people might have had to believe in electrons because the postulation of their existence could explain a wide variety of phenomena. Luckily we no longer have to pretend to infer from the explanatory success (i.e. from what makes our minds feel good) Prescott [and the team from the SLAC (Stanford
Linear Accelerator Centre] don't explain phenomena with electrons. They know how to use them.\textsuperscript{43}

However, contrary to what Hacking wants us to believe, this further elaboration does not provide extra ground for regarding the instrumentalistic argument as an independent argument for scientific realism. This is because Prescott and his colleagues, with all the advanced technologies at their disposal, can provide an argument (and certainly not an independent one) for the reality of electrons only if they know that what they are dealing with are electrons. Otherwise, if they do not know what the entity they are manipulating is, then they cannot even claim that they have produced an extra (let alone an independent) argument for the real existence of a certain specific theoretical entity.

As Hacking himself has observed, to be of any help to scientific realism about electrons, Prescott and colleagues must first determine whether their term 'electron' has the same referent as the theoretician's term. To show this in a fashion which fulfil entity-realists' claim about the independence of instrumentalistic argument, the experimenters must first decide what reference 'electron' would have solely on the basis of experimental intervention in nature; then secondly, decide what reference 'electron' would have solely on the basis of theoretical representation of nature; and thirdly, demonstrate that the two referents are identical.

However, any attempt on the part of entity-realists to show the co-referentiality of the two terms will result in establishing the case argued for so far — that the instrumentalistic argument is not an independent support for scientific realism. This is because either the entity-realist would succeed in establishing the co-referentiality of the two terms, in which case what they would show is that the theorists have been right in their insistence on the reality of the posited entity, or the entity-realists would fail to
establish the co-referentiality of the two terms, in which case, what they would discover is (possibly) a new theoretical entity which requires the help of theorists to produce an acceptable model of its behaviour and properties.

One of the moves on the part of entity-realists to uphold instrumentalistic argument has been a tacit or explicit appeal to modern theories of reference. N.Cartwright, for example, has pointed out that it is very much the case in science that scientists, while believing in the reality of a certain unobservable entity like electron or photon, may be using several different, often mutually inconsistent, models of that entity. She emphasises that none of these scientists take those models as truthful representation of reality. According to her, in their view, electrons and their ilk are not exactly what those models are telling that they are, but are whatever produce the effects they (scientists) bring about by means of their machines and instruments; hence a tacit allusion to the stability of referent through theory change.

R.Harré has taken this aspect of the instrumentalistic argument a step further and has explicitly linked it to the modern theories of reference. Drawing on a version of causal theory of reference due to L.Roberts [1986], he has argued that while theoretical representation of nature (i.e. our theories, models, and hypotheses) may (as often is the case) prove to be wrong we are still in a position to hold fast to entities introduced by these theories and whose causal power we exploit.

Appeal to the stability of referents through changing models by making use of the modern theories of reference, though arguably an improvement over the old arguments, is not sufficient to establish entity-realists' claim about the independence of the instrumentalistic argument and its superiority over the more traditional arguments used by the realists. There are a number of inter-related points which need to be spelled out. In
the first place, as Harré himself has noted, the standard accounts of causal theories of reference, mostly due to Kripke and Putnum, cannot provide a satisfactory basis for referent preservation in scientific discourse. Scientific theories, most of the time, refer to entities which are (even in principle) unobservable and therefore cannot be subjected to any baptism ceremony of the kind suitable for the ordinary, medium-sized objects.

Secondly, to rest content, as Cartwright seems to do, with rules like "whatever produces this or that effect is the entity in question" opens the way to the sort of objections levelled at operationalists. That is to say, since different instruments can bring about different aspects of the entity's causal power, experimenters will be facing a number of different effects. To relate all these different effects to one and the same entity requires additional argumentation, which would inevitably involve the theoretical considerations.

Thirdly, in science it is the theory which introduces new kinds of objects. It does so, partly by specifying the properties of the object in question and partly by defining the context in which the object plays its explanatory rôle. It follows that, if we are going to make use of theories of reference at all, then any theory of reference which is devised to secure a scientific (unobservable) object must account for these properties and their relevant contexts. In fact, Cartwright herself has emphasised that: "[D]esignating or picking out the individual does not tell us what it is." This means that the sort of theory of reference useful for science (if at all), is the one which combines the positive aspects of the descriptive and causal theories of reference. Harré has conceded this point and has gone some way to address it. However, to concede this much is to accept the point emphasised throughout this section, namely, that in securing the referents of scientific terms, a minimal amount of theoretical description (which is believed to be true of the essential properties of the referent in question) is necessary.
III. Cartwright and the Fundamental Laws of Physics

One rather distinct route to defend entity-realism is taken by N. Cartwright. She wants to advocate "a kind of operationalism" which while remaining true to the empiricists' outlook, opposes Hume's programme. This aim is to be achieved by arguing for the primacy of singular causal claims via introduction of the notions such as capacity. Another major aspect of her programme has been a systematic and sustained attempt to reject the realists' notion of fundamental laws in favour of more locally valid laws:

I say the laws of physics do not provide true descriptions of reality. This sounds like an anti-realist doctrine. Indeed it is, but to describe the claim in this way may be misleading. For anti-realist views in the philosophy of science are traditionally of two kinds. Bas van Fraassen is a modern advocate of one of these versions of anti-realism; Hilary Putnam of the other. ... But ... I have no quarrel with theoretical entities; and for the moment I am not concerned with how we know what they do. What is troubling me here is that our explanatory laws do not tell us what they do. It is in fact part of their explanatory rôle not to tell. Hilary Putnum ... also maintains that the laws of physics do not represent facts about reality. But this is because nothing — not even the most commonplace claim about cookies which are burning in the oven — represents facts about reality. ... I think we can allow that all sorts of statements represents facts of nature ... . It is just the fundamental explanatory laws that do not truly represent.

Cartwright has developed her programme in three stages. In her earlier writings (e.g. [1983/4]) she has strongly criticised the commonly held views among realists concerning the fundamental laws of nature. However, at this stage she was still reluctant to introduce notions like nature and capacity or generative mechanisms and structures.

At a later stage (e.g. [1989]) she has made her preference for dispositional properties as more basic ontological posits in contrast to laws, quite clear and explicit:

... the pure empiricist should be no more happy with laws than with capacities, and laws are a poor stopping point. It is hard to find them: why they have exceptions — big or little; why they only work for models in the head; why it takes an engineer with a special knowledge of real materials and a not too literal mind to apply physics to reality. The point of this [programme] is to argue that we must admit capacities, and my hope is that once we have them we can do away with laws. Capacities will do more for us at a smaller metaphysical price.

Quite recently (i.e. [1994]), in the third stage of her programme, Cartwright has further focused her attack on the notion of "universality" of the fundamental laws and has
urged for a new metaphysical picture of nature which she has dubbed "metaphysical nomological pluralism":

A number of years ago I wrote *How The Laws of Physics Lie*. That book was generally perceived to be an attack on realism. Nowadays I think that I was deluded about the enemy: it is not realism but *fundamentalism* that we need to combat. ... One cannot do positive science without the use of induction, and where those concrete phenomena can be legitimately derived from an abstract scheme, they serve as a kind of inductive base for that scheme. *How The Laws of Physics Lie* challenged the soundness of these derivations and hence of the empirical support for the abstract laws. I still maintain that these derivations are generally shaky, but that is not the point I want to make here. So let us for the sake of argument assume the contrary: the derivations are deductively correct and they use only true premises. Then, granting the validity of the appropriate inductions, we have reason to be realists about laws in question. But that does not give us reason to be fundamentalists. To grant that a law is true – even a law of ‘basic’ physics or a law about the so-called ‘fundamental particles’ – is far from admitting that it is universal, that it holds everywhere and governs in all domains.60

Metaphysical nomological pluralism is the doctrine that nature is governed in different domains by different systems of laws not necessarily related to each other in any systematic or uniform way: by a patchwork of laws.61

Cartwright’s account of entity-realism apparently challenges many versions of scientific realism. A good number of realists are not in favour of dispositional properties. Similarly, many realists do not endorse the causal account of explanation offered by Cartwright. They, instead, adhere to the standard Deductive-Nomological account.62

A minimal realist, in contrast, need not to have any qualm over invoking notions such as capacities and causal powers and their use in scientific discourse and investigations.63 Moreover, such a realist can whole-heartedly endorse Cartwright’s criticisms the shortcomings of the D-N model.64 Furthermore, a minimal realist would also endorse the significance of the experimental works for the advancement of science.65 However, despite all these common grounds, Cartwright’s position remains (at least on the face of it) incompatible with the position of a minimal realist. But need it be so?

In what follows, I shall argue that while Cartwright’s views have highlighted a major shortcoming in the works of realists, including the minimal realists, her own opposition to the fundamental laws are somewhat incompatible with her overall system.

Cartwright and other entity-realists’ emphasis on the rôle of practical activities in
science, is, no doubt, a valuable contribution. This emphasis, as the entity-realists themselves have pointed out, opens the way for a better acknowledgement of the importance of technological and engineering activities in the advancement of scientific knowledge. However, to conclude from here that science does not need the fundamental laws, involves at least two assumptions which cannot be accepted without further argumentation.

The first assumption, a typical anti-realist one, is to put scientific (theoretical) knowledge, on a par with the technological and engineering knowledge\(^{66}\). However, these two types of knowledge, despite their close relationship, are not one and the same thing (however, see below, note 68). For one thing, the aims pursued by the two are different. Whilst the aim of the theoretical knowledge is *understanding* the unknown aspects of the physical reality, the aim of technological and engineering activities is, by and large, control and prediction of phenomena.

As a result of the differences in aims, a second difference shows itself, namely, the difference in the criteria for judging the advance in each of these two fields. In theoretical science, as we have already discussed, the criterion of approaching the ideal of truth about the reality provides a rough (and admittedly not yet very well formalized) measure for progress. In technology and engineering, where, in contrast, the main concern is usually the advancement in making better, more effective instruments, pragmatic considerations gain more prominence\(^ {67}\).

Another noticeable difference between the two kind of knowledge is that scientific (theoretical) knowledge is, by and large, cumulative, whereas the same is not often the case for technological and engineering knowledge. Whilst the theoretical knowledge embedded in the fundamental physical theories can be retrieved from the more recent
theories which have those older refuted theories as their asymptotic or approximate versions, most of the tacit knowledge, or the know-how which has been in use by the previous generations of engineers and craftsman, is lost for ever. In the words of a minimal realist:

We risk losing the ability to operate ocean-going sailboats because of the advent of motor-powered boats, as is well-known: in small part we counteract this risk by the continued maintenance of some sailboats by sea scouts, by sports clubs, and by the navy. This is not to say that the art is totally preserved. Of course, it cannot be, since ocean-going sailboats belonged to an organized international network, subject to international law and customs and linked to sailor's folklore, not easily forgotten by readers of Joseph Conrad.

The British historian of technology Donald Cardwell ended his report of how much effort and learning went into his reconstruction of an early -Newcomen- steam engine saying: "Despite my Ph.D in physics and subsequent practical experience I would only just be qualified to be an engineman in 1712." Of course, this is not because an engineman two hundred years ago, knew more science than a professor of physics today, but because much of the detailed useful knowledge available to the engineman in question by tradition has been entirely forgotten.68

Cartwright’s second assumption is that "nature is governed in different domains by different systems of laws."69 This view which seems to bear some surface-semblance to the views of the advocates of the "complexity theory"70 is a purely metaphysical assumption which cannot be substantiated by a thorough empiricist who would regard as knowledge, only what can be measured71. It is evident that any attempt to justify this assumption on the basis of inductive evidence will raise the spectre of the so-called problem of induction.

Cartwright’s arguments against the fundamental laws do not seem to establish the point she has in mind. Using a battery of real life examples from the realms of science and engineering, Cartwright has summarized her arguments against the veracity and universality of fundamental laws in this way72:

1) The manifest explanatory power of fundamental laws does not argue for their truth.
2) In fact the way they are used in explanation argues for their falsehood. We explain by ceteris paribus laws, by composition of causes, and by approximations that improve on what the fundamental laws dictates. In all these cases the fundamental laws patently do not get the facts right.73

Not only do I want to challenge the possibility of downwards reduction but also the possibility of 'cross-wise' reduction.74

...We have no grounds in our experience for taking our laws –even our most fundamental law of physics– as universal. Indeed I should say ‘especially our most fundamental laws of physics ’,
these are meant to be the laws of fundamental particles. For we have virtually no inductive reason for counting these laws as true of fundamental particles outside the laboratory setting – if they exist there at all.\textsuperscript{75}

Cartwright's views invites a number of observations:

1) It seems that her objections to the fundamental laws would put her position in the same epistemological boat as those of Laudan's and van Fraassen's with regard to the denial of the possibility of acquiring theoretical knowledge (i.e., knowledge about the unobservable aspects of reality). In Laudan's view, since our past successful theories are falsified, we cannot speak of theoretical knowledge. After all, how can a number of false claims produce viable knowledge? In van Fraassen's view we should suspend judgement about the unobservable realm and rest content with the empirically adequate consequences of our theories.

Cartwright has two possible replies to this observation. First, that she has no qualm about the reality of the theoretical entities. And secondly, that she has no qualm about parochial, local knowledge produced by locally valid laws. What she does not accept is that our laws provide us with universally valid knowledge.

These responses, however, do not seem to be adequate. In the first place, as pointed out earlier, it is unclear how she can justify the claim that the same kind of entity is operating in a variety of circumstances, where quite different phenomenological laws will describe the behaviour of that entity. Jean Perrin for example, was able to determine Avogadro's number (the number of molecules in a certain volume of gas) in thirteen entirely different ways, including "Brownian motion", "Black-body radiation", "X-ray diffraction", "Alpha decay", and "Electrochemistry"\textsuperscript{76}. Cartwright commenting on Perrin's works writes:

For Many, Perrin's reasoning is a paradigm of inference to the best explanation ... I think this misdiagnoses the structure of the argument. Perrin does not make an inference to the best explanation, where explanation includes anything from theoretical laws to a detailed description of how the
explanandum was brought about. He makes a rather more restricted inference – an inference to the most probable cause. A well-designed experiment is constructed to allow us to infer the character of the cause from the character of its more readily observable effects.\textsuperscript{77}

Cartwright’s observation about the function of a well-designed experiment is of course quite correct. The question however, is that what warrants a scientist who has carried out a number of different well-designed experiments, each invoking certain different causal powers of some unobserved entity, to claim that he or she has been dealing with one and the same entity in all those different instances? The case of such a scientist is like the story of those people who were trying to find out about an elephant in a darkhouse with the palms of their hands. The story is narrated by the great Persian thinker of 13th century, Jalal-uddin-Rumi\textsuperscript{78}.

The Elephant was in the dark house: some Hindus had brought it for exhibition.
In order to see it, many people were going, everyone into that darkness.
As seeing it with the eye was impossible, each one, was feeling it in the dark with the palm of the hand.
The hand of one fell on its trunk: he said, ‘this creature is like a water pipe.’
The hand of another touched its ear: to him it appeared to be like a fan.
Since other handed its leg, he said, ‘I found the elephant to be like a pillar.’
Another laid his hand on its back, he said, ‘truly, this elephant was like a throne.’
Similarly, whenever any one heard a description of the elephant, he understood it only in respect of the part that he had touched.
If there had been a \textit{candle} in each one’s hand, the difference would have gone out their words...

Without the light of a unifying theory which links different causal manifestations of an unobservable entity to that very entity, those experiments will be of limited value.

Another equally important question to be asked is: why should certain phenomenological laws be able account for a large variety of cases which involve different material or elements? For example, Ritz in 1908 produced a general formula for spectral lines of many atoms which would incorporate Balmer’s, Paschen’s and Lyman’s formulae\textsuperscript{79}. Note that here the mere appeal to capacities and generative mechanisms without a tacit or explicit premise which ascribes similar inner structures (the unity of structures) to these different materials, cannot explain this very phenomenon. It is exactly

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such unity of structures or forms which allows scientists to produce laws which *unify* these disparate phenomenological laws. Bohr, for example, was able, on the basis of his atomic theory, not only to derive Ritz formula but also solve the puzzle of the Pickering series and explaining the Stark effect and Zeeman's normal effect.

The response to Cartwright's second point requires a more involved clarification. Cartwright is in favour of positing generative mechanisms and causally efficacious structures and tendencies and capacities. However, it seems that her particular interpretation of the rôle and function of natures and capacities and the ways their powers are being exercised has led her to an anti-realist reading of fundamental laws. In her [1983/4] she stated that "... the law of gravitation claims that two bodies have the *power* to produce a force of size Gmm/r². But they do not always succeed in the *exercise* of it". Again in her [1994] discussing the case of F = ma she has observed that: "Most of us, brought up within the fundamentalist canon, read this with a *universal quantifier* in front: for any body in any situation, the acceleration it undergoes will be equal to the force exerted on it in that situation divided by its internal mass. I want instead to read it, as indeed I believe we should read all nomological, as a *ceteris paribus* law: for any body in any situation, *if nothing interferes*, its acceleration will equal the forces exerted on it divided by its mass."

Minimal realists, as we have already pointed out, are in complete agreement with Cartwright about the difference between the closed systems (e.g. laboratories) where the *ceteris paribus* clause is operative and the open systems (nature at large). However, they would argue that to think of the exercise of the causal powers under the description of their fulfilment, is like claiming that Buridan's ass in order to be pulled in two ways, had actually to go in both directions. A realist position towards the issue is that any two
bodies of non-zero mass exert certain gravitational forces on each other, no matter what other forces are operative, and likewise for all other forces. The only difference which closed systems make is to allow us to measure these powers.

In her [1994], it seem Cartwright has endorsed this way of interpreting the capacities: "It is in the nature of a force to, produce an acceleration of the requisite size. That means that ceteris paribus, it will produce that acceleration. But even when other causes are at work, it will ‘try’ to do so. ... To ascribe a behaviour to the nature of a feature is to claim that behaviour is exportable beyond the strict confines of ceteris paribus conditions, although usually only as a ‘tendency’ or a ‘trying’. The extent and range of the exportability will vary. Some natures are highly stable; others are very restricted in their range."84 But then she has once again returned to her initial point and with somewhat less justification has claimed that: "The point is that we must not confuse a wide-ranging nature with the universal applicability of the related ceteris paribus law. To admit that forces tend to cause the prescribed acceleration (and indeed do so in felicitous conditions) is a long way from admitting that F = ma is universally true."85

But why is that? In fact, it does not seem to be an extravagant view to be held by a philosopher like Cartwright who subscribes to notions such as capacities and causal power, that the fundamental laws, which are based on the concepts of dispositional properties and generative mechanisms, are operative universally, even if in reality we face complicated situations which do not exactly match with the picture our fundamental laws portray. This fact can be simply explained by pointing out that since in the real situation there are many factors at work, a large number of them still unknown to us, the effects of the generative mechanisms we have postulated cannot be observed as neatly as one may wish. In other words, to account, by means of our theory, in an exact way, for a
complicated situation in the real world, we do need a lot of information about the boundary and initial conditions. This information however, most of the time is not available. Nevertheless, the very fact that these results are observed in the controlled situations, or under the auspicious of *ceteris paribus* clause, is indeed powerful evidence for the reality and ubiquity of these mechanisms and their dispositions\textsuperscript{86}.

This way of looking at the laws, enables the advocates of capacities and causal powers to explain the natural phenomena in a more plausible way. For instance, in her [1994] Cartwright discusses the example of a $1000 note flying in St. Stephen’s Square\textsuperscript{87}. She has argued that "Mechanics provides no model for this situation." We may explain the movement of the note by means of Fluid dynamics. "But this is not a subdiscipline of basic physics." It is part of engineering. Therefore, engineering and not fundamental laws, is a better tool for accounting such a phenomenon.

However, in the light of what mentioned above, it can be argued that the movement of the $1000 note is the outcome of a variety of operating mechanisms, namely, gravitational, thermodynamic, thermal, and perhaps some other factors. Of course, if we want to see the effect of each of these conjectured mechanisms, we have to eliminate the effects of other operating factors and this is why we can only test our conjectures concerning these mechanism by means of our well-arranged experiments within the confine of laboratory. However, this intervention in the course of nature, as we have already argued in the context of Hacking’s argument, is based on and guided by our fundamental laws which are, if true, universally true, whether inside and outside the laboratory.

Common-place practices in science like "approximation" and "idealization" are quite compatible with the view explained above. We use a basic law either directly or
indirectly, through one of its derivations. In this sense, most of the laws used by physicists have the basic laws as their backbones. We consider these two cases respectively.

a) We have a basic, fundamental theory and we are confident that it is on the right track. However, we also know that the exact effect of this law can only be shown in the closed systems. In applying this law to the real, messy situations in the open system, we make use of idealized models whose main features are the aspects we are interested in. Moreover, by using approximations we further remove those aspects which are not relevant or are less pertinent to our present purpose and interest. In this way we, in effect, try to imitate the conditions of the closed systems as much as possible. As a result, we obtain a version of our original theory which can be regarded as its "approximate derivation". The name is appropriate because although we make lots of simplifying amendments there is always enough common ground between the simplified (idealized) model and the original theory to warrant us to talk about refutation of the original theory should the predications of the modified model fail. Thus for example, to calculate the earth's period of revolution around the sun, we take the earth and the sun to be both point particles. We shall neglect the forces between planets, considering only the interaction between the sun and the earth (assuming that the earth has an acceleration towards the earth's centre and assuming a uniform field of gravity).

We take the orbit of the earth to be a perfect circle, with sun at its centre, and assume that

\[ r \]

\[ M \]
the plane of the revolution remains constant. Using the model of two spherical bodies of
masses $M$ and $m$ moving in circular orbits under the influence of each other’s
gravitational attraction in which the relation $GMm/(R+r)^2 = m\omega^2r$ holds, we can further
simplify our calculation by assuming that $R$ is negligible compared to $r$. This leads to the
equation $GMs = \omega^2r^3$, where $Ms$ is the mass of the sun. And this leads to desired relation,
namely, $T^2 = 4\pi^2r^3/GMs$.

b) In the second type of approximation we make alterations in the original model
to bring it nearer to the phenomenal reality. Here, contrary to the first case, to make the
model work, we may actually turn it into a much more complex system by adding
corrective factors. A good example of this type of alterations, which are quite common
in engineering and technological activities, is the case of the electronic amplifier,
discussed by Cartwright herself, at some length. It should be noted that although the
laws governing the operation of different parts of the amplifier, are regarded as
independent laws by engineers, nevertheless, their validity is based on the basic laws of
electromagnetism. In other words, although these phenomenological laws cannot be
reduced, in a strict sense, to the basic laws of electromagnetics, nevertheless, in an
important sense they are resting on these more fundamental laws in the same way that the
laws of chemistry are resting on the laws of physics, or the laws of biology are resting
on the laws of chemistry and therefore, physics.

My final observation about Cartwright’s approach, before turning to the case of
some other representatives of entity-realism, is that whilst her contribution to the current
debates in philosophy of science has had the welcoming effect of drawing attention to the
significance of technological and engineering activities in the advancement of science and
assisting the theorists in their pursuit of fundamental laws, her favourite metaphysical
picture seems to be heuristically less promising than the unified picture tacitly or implicitly embraced by traditional realists.

IV. The Epistemologizing of Truth

Realists subscribe to a rather modest correspondence theory of truth. Entity-realists have come to the conclusion that correspondence truth is of no use for realism and must be dispensed with. Brian Ellis for example, has emphasised that:

The truth theories most commonly accepted by scientific realists appear to be incompatible with their ontologies. The correspondence theory of truth, for example, make truth a relationship between eternal truth bearers and facts. But scientific realists have trouble identifying the entities required for this relationship to hold, even in the most elementary cases. In the more complex ones, the task seems hopeless. ... Therefore, it may be more in keeping with a scientific outlook to reject the classical [i.e. correspondence] concept of truth altogether, and replace it by an epistemic concept which is not absolute in this sense...

To reject correspondence, some entity-realists have, among other arguments, appealed to the tactic of epistemologizing the notion of truth. There are number of grounds for eschewing the correspondence notion of truth and invoking an epistemologized notion. These are: unobtainability of truth, inadequacy of ‘truth-conditions’ to account for the meaning of statements, and the implications of the notion of ‘language-games’. We shall consider, in three sub-sections below, each of these grounds which are somehow inter-related and which are supposed to provide one rather involved argument against the realists’ conception of correspondence truth.

IV.A. Paradox of Limited Knowledge

The first type of argument for epistemologizing of truth is based on the idea that truth, as defined by realists, is unobtainable. There are three slightly different versions of this argument. To state the first version, Harré has appealed to a paradox which he ascribes to Peirce. In Harré’s view, based on his reading of Peirce, any
defender of scientific realism must face the following dilemma:

On the one hand he wants to say that scientific statements are true or false by virtue of the way the world is, whether we know it or not, and that the existence of a state of affairs corresponding to the content of the statement in question is just what its being true is and the absence of one is what it is to be false. Yet we could never be in a position to know that a scientific statement was true.  

Another argument which usually accompanies the charge of unobtainability of the realist’s goal, is presented by an appeal to the principle of bivalence. Semantic anti-realists (and in their wake the entity-realists) usually emphasize the point that the principle of bivalence (that is the assumption that the statements of any given class of statements are determinately true or false) is a touchstone for a realistic interpretation of statements of that class. Having ascribed this to realists, anti-realists (entity-realists) try to show that for certain classes of statements the principle of bivalence does not hold and then use this conclusion to further undermine the correspondence theory of truth and strengthen their own alternatives. Harré for example, has argued that:

Many realists have based their defence of realism on the doctrine (one might almost say the dogma) of bivalence, the principle that most theoretical statements of a scientific discourse are true or false by virtue of the way world is, whether or not we, as limited human investigators, can know it. Other realists have persisted with the idea that the essence of scientific discourses and material practices, such as experimentation, is some abstract logical framework. Sceptics have had little difficulty in demonstrating that no real scientific research programme could come anywhere near realizing the bivalence principle in practice, nor have had much trouble in showing that real scientific thinking could make little use of logical schemata if its cognitive and material practices were made explicit. It looks as if the work of a scientific community can be rational only at the cost of being impossible, while on the other hand, the extreme special reaction offers nothing but a caricature of what we know the scientific achievement to have been. What has gone wrong?

Each party to the dispute has fallen into error. Contemporary sceptics have slipped into the commission of the ‘philosophers’ fallacy, the fallacy of high definition. By their defining, even only tacitly, such cognitive phenomenon as scientific knowledge in terms of truth and falsity, the demands placed on a community which has the task of accumulating some of ‘it’ are set in such a way that ‘it’ can never be achieved.

B.Ellis has given the argument from non-obtainability of truth a new twist and thus has produced its third version. He argues that:

Realistically interpretable truth-conditions for laws, theories, modals and conditionals, and hence for most of the propositions of science, cannot be specified, unless one is prepared to countenance an infinity of possible worlds other than the actual world; and, even then, there would be trouble in specifying adequate truth conditions for theories, arising from the openness of their fields of evidence. But belief in other worlds is incompatible with scientific realism, because: (a) it is contrary to the causal theory of knowledge to suppose that we could know anything at all about them; and (b)
we are not required to believe in them as theoretical entities, because ‘possible worlds’ theories are not causal process theories. The only reason we could possibly have to believe in other possible worlds is that we need them to save the semantic theory of truth. Without possible worlds, there is not enough reality for all of the truths we believe in to correspond to.\textsuperscript{102}

The moral of the above arguments is briefly that, since correspondence truth leads to either a dilemma or a circularity, it is better for realists to adopt other more manageable theories of truth, of which the pragmatic theory seems to be the most favoured candidate\textsuperscript{103}.

The first round of attack by entity-realists on truth (and correspondence theory), although strong and probably damaging for some versions of scientific realism, does not seem to affect the minimal version defended in this essay. Realism (correspondence) neither entails a knowledge of the exact truth value of all statements uttered, nor a commitment to the reality of all possible worlds or all the entities implied by our statements. It is more modest than this. However, its modesty does not mean that it is an uninteresting or un-illuminating philosophical position.

Realists’ basic claim is that we are indeed in a positions to discover facts about the physical reality, which they assume to be independent of man’s mental faculties. This in turn entails that at least some of our beliefs about this reality should be true of it (that is to say they should correspond to this reality or aspects of it). It is for example true that the 1992 Olympic games were held in Barcelona, true that water is thirst quenching, true that light polarization is caused by crystalline structure of matter, true that energy and matter are convertible, and false that America was discovered in 5th century B.C., false that there is no link between heavy smoking and lung cancer, and false that some materials release phlogiston upon combustion.

Note that endorsing all the above statements and many more like them (which we can, following Putnum, call them plain truths\textsuperscript{104}) does not require possession of a highly
advanced and technical account of correspondence theory of truth. We can, without being equipped with a technically sophisticated correspondence theory, recognize many clear cases of truth, as well as of falsity in the ordinary sense stated by the realists' rather mundane correspondence theory\textsuperscript{105}. Truth is a property of many sentences we utter and write — a characteristic we want those sentences (or to put it more precisely, those propositions) to have when we are not trying to deceive each other (or ourselves). In other words, truth, in a correspondence sense, is not — or would not ordinarily be held — unobtainable, as far as much common sense knowledge is concerned. The denial of this rather simple fact seems to lead to various sort of idealism, the doctrine which makes reality somewhat dependent on our cognitive abilities. In fact, as we shall see in the last section, entity-realists, by espousing the pragmatic theory of truth in place of the correspondence theory, have rendered their position susceptible to this undesirable outcome.

There is a second argument in defence of the minimal correspondence based on the fact that human beings, in their everyday and scientific life, do commit all sorts of errors or mistakes and on many occasions manage to rectify these errors. In fact, humans' (and other species' for that matter) ability to correct their mistakes and to learn from them, plays a significant rôle in their survival\textsuperscript{106}. The very idea of error, or of doubt (in its normal straightforward sense) implies the idea of an objective truth (that is truth as correspondence to reality).

Entity-realists seem to have no qualms over the use of conception of correspondence as far as observable phenomena are concerned\textsuperscript{107}. Nevertheless, they insist that even if correspondence at the observable level is not problematic the difficulties that accompany this notion in general (e.g., while dealing with the class of so-called
troublesome statements\textsuperscript{108}) render untenable realists’ claim about the general validity of the notion of correspondence truth.

However, it seems that if entity-realists have no objection to the so-called simple (correspondence) truths (or commonsensical truths, or plain truths), and their rejection of the realist conception of correspondence truth is solely based on the case of the statements which are evidence-transcendent, then ways can be found to sooth the anxiety of entity-realists and rebut their objection. The first step in this direction is to take into account a distinction which exists between the truth-value and truth-condition of the statements. The point to be noted in this respect is that there is nothing in realist correspondence theory which implies that the two should or would always coincide.

Correspondence theory is characterized by way of a relation between our mental or linguistic products — the potential bearers of truth (i.e., propositions) — and a world, which realists assume to be independent of everything mental\textsuperscript{109}. The dispute between realists and their opponents (and it is here that most of the conflations and confusions on the part of the opponents occur) is not over the question of whether a proposition is true or false, but what accounts for its truth or falsity. In other words, correspondence discloses what it is for a proposition (or a statement or a belief for that matter) to be true or false, but not whether it is true. This should make it clear that the criticism of some entity-realists (for example B.Ellis) who have rejected correspondence on the grounds that it cannot account for the value of truth\textsuperscript{110}, is misplaced. Correspondence implies neither that truth need obtain nor that it need not obtain, given our best grounds for it. Ellis’s charge of circularity is also not valid since all along we have regarded the correspondence theory of truth as a corollary of (metaphysical) realism and not, as Ellis has mistakenly assumed, as an input into the definition of (metaphysical) realism\textsuperscript{111}.
From the above discussions, it should also be clear that realists’ correspondence theory of truth is also not adversely affected by the principle of bivalence. The fact that there are many statements (members of the troublesome class) for which we may not ever be in a position to determine their truth values, in no way should or would affect realists’ position. Here, another distinction helps to clarify the matter. We must differentiate between, (1) what does it mean to assert p is true? and (2) how do we know p is true? The above distinction makes it clear that all grammatically correct statements have truth value, (that is to say, they may or may not correspond to the real state of affairs\textsuperscript{112}) regardless of the fact that we are / are not in a position to determine it\textsuperscript{113}.

Our argument so far has been devoted to clarifying the point that (contrary to entity-realists and semantic anti-realists) the main issue concerning the notion of truth is not to discover or propose a mechanism (or decision procedure, or algorithm, or method, etc.) to find the truth-values of the statements. Rather, it is over the nature of the truth in each instance. However, it may be objected that if the realists’ theory of truth does not give any clue as to how to find the truth / falsity value of different statements, then it may be a totally useless theory.

Realists’ reply to this charge is that any deliberation on the nature of truth does involve (albeit to varying degrees) talk of the truth / falsity values. However, the main admonition of the correspondence theory concerning assignment of the values of truth and falsity to statements of the troublesome class is that neither insistence on the discoverable values for all of the members of this class (as some hard-line realist may do), nor denial of truth-value for any of them (as modern anti-realists usually do), is a sound and rational attitude.

The most sensible way forward, so realists would argue, is to examine each and
every one of these statements separately and individually and decide its case on its own merit. Such an appraisal would lead to the following cases: it may be possible, for some of the statements of these troublesome classes, to declare them to be true not by virtue of their connection with the facts of a like kind, but by virtue of their connection to the statements of some other type, which in turn have connections with facts of the appropriate type. Thus for example, some troublemaking statements like some counterfactuals might be regarded as having determinate truth values on the basis of what actually has happened. Some other statements of this class may have a determinate truth value but are irreducible to other more basic statements. Others may not have a determinate truth value but a number of plausible accounts could be offered for their truth values.

Taking a lead from Stalnaker, one can go further and claim some extra heuristic value for the realists' attachment to correspondence. Suppose we represent a person's propositional attitudes and the content of his statements in terms of a space of alternative possibilities. The content of any statement is determinate relative to the relevant set of possibilities. It is, for example, characterized by a function from the points in this space to truth-values. This function, in effect, divides the space into two parts. It draws a sharp line between the points so that, for each possible world in the space, the proposition is true or false. But when we ask of any statement whether it is true in the actual world, we may find that the question has (for the time being) no answer for the following reason: at our present level of understanding of reality, there may be no facts which determine which of several points (some on each side of the sharp line) is the actual world. Our conceptual space of possibilities has cut things too finely, making distinctions to which nothing in our present understanding of reality answers. This can encourage us to revise
our knowledge of reality and delve into its deeper levels. In contrast, for those who deny truth values for these statements, such an incentive does not exist.

Statements and their truth values and truth makers aside, entity-realists have appealed to yet another argument for rejecting the correspondence theory of truth. This argument concerns the non-applicability of the notion of truth to the so-called cognitive objects. Harré for example has argued that:

Discourse is only one of many modes of the public display of cognition. I believe we need to make use of a wider class of informative entities which I shall call "cognitive objects". When Knowledge is expressed in the iconic mode as a diagram or model, representational accuracy and inaccuracy (faithfulness, etc.) replace 'truth' and 'falsity' as the main ways of expressing and assessing epistemic worth. A notion like 'representational quality' is obviously better adopted to a less strict dichotomizing of assessment than are 'truth' and 'falsity'. Likeness can be more or less faithful, drawings and diagrams more or less accurate portrayals of their subject matter. The qualified judgement 'more or less accurate' is certainly easier to analyze than the puzzling idea of degree of truth.118

The appeal to cognitive objects however, does not provide entity-realists with good grounds for abandoning the notion of truth. This is because, although beliefs need not be embodied in statements (they may be expressed by non-verbal actions or by cognitive objects), the contents of them can always be articulated in essentially the same way that statements are. And, just like the case of statements, non-verbal beliefs may be subjected to several interpretations (that is, the problem of underdetermination). As an example, consider the case of a stock-broker who uses some peculiar sign language to his colleague at the other end of the brokerage house. Different observers may interpret his hand's movement in different ways. In these cases we naturally seek further information by undertaking inquiries to resolve the indeterminacy. But if this cannot be achieved, for different reasons, we do not question the notion of correspondence, but the identity of the beliefs.119

IV.B. Truth Conditions vs. Warranted Assertibility Conditions

Entity-realists' second type of argument from truth epistemologized focuses on the
meaning of the statements. Realists are traditionally of the view that the meaning of statements consists in their truth conditions. In other words, on the realists’ account of meaning, to understand the meaning of a statement is to understand its truth conditions. Modern anti-realists claim that the meaning of a statement consists in the conditions under which it would be appropriate to assert it or to assent to an assertion of it\textsuperscript{120}. These conditions have been variously called ‘justification conditions’, ‘assertibility conditions’ and the like\textsuperscript{121}. In the view of anti-realists, since the meaning depends, ultimately, on use\textsuperscript{122}, we can use only those statements which we are warranted or justified in asserting. These are decidable, or verifiable statements (\textit{i.e.} their truth conditions are recognizable). Other classes of statements whose contents transcend the available evidence, though meaningful, do not have truth conditions. From here anti-realists conclude that truth conditions cannot account for our understanding of these seemingly troublesome classes of statements and should be abandoned. The alleged lack of truth conditions for certain classes of statements is then used by anti-realists to discredit correspondence theory of truth and replace it with other theories of truth.

The above argument, is fully endorsed by Harré and Ellis\textsuperscript{123} although Harré prefers to use the term plausibility in place of warranted assertibility:

There are no truth-conditions ... for the kinds of statements which are typical of theories, nor are there any such conditions for general statements, whether theoretical or not. ... But such statements are meaningful. It follows that their meaning cannot be identified with their truth-condition since they do not have any. However, there are conditions for warranted assertibility and rejectability of such statements. I shall be proposing quite a full account of these conditions under the names ‘plausibility’ and ‘implausibility’\textsuperscript{124}.

Since I have already argued that theoretical statements do have truth conditions, here, to counter-entity realists’ second type of argument, I will to show that whereas epistemic notions such as warranted assertibility are not sufficient for the meaning (understanding) of statements, the notion of truth is indispensable.
Plain truth, to which we referred above, as a semantic concept, has certain characteristics or marks, which are lacking in epistemological concepts like belief, certainty, proof, warrant, grounds, assertibility, justification, and knowledge. Moreover, all such epistemic notions are dependent upon the notion of truth and presuppose it and therefore cannot replace it.

In view of the above, to link the meaning of theoretical terms to use, i.e., to our (ever improving) ability to probe deeper into the reality, as some entity-realists seem to do, opens up the flood gate of relativism and make their own interpretation vulnerable to charges such as meaning incommensurability. Harré for example, writes:

Furthermore there is a good case to be made for the ontology of Niels Bohr, for deep physics. The universal ur-stuff manifests itself in this way or that depending on the kind of equipment that physicists choose to build. The idea that there is an electron at the North Pole now is not at all the same sort of idea as that there is a polar bear there. The ur-stuff at the North pole no doubt currently has the disposition to be shaped up by a suitable piece of apparatus but until such a one is set up there are not any electrons, only electron propensities, and we have no idea what occurrent properties they are grounded in.

However, the fate of the Operationalists' programme should provide a valuable lesson for anyone who wants to link the meaning of the theoretical terms to our ability of manipulating (using) them. Such moves bring about the threat of incommensurability for entity realists. The difficulty arises from the case of identical statements. According to realists, statements T and T' are identical iff they have the same truth conditions. This approach will serve realists to rebut any threat of meaning incommensurability. However, for entity-realists who do not admit truth conditions for statements of the troublesome class, and who link our grasp of the meaning of terms and statements to our ability to verify (justify) them, this would appear as a major difficulty.

The indispensability of the notion of truth for the meaning (understanding) of statements can be better illustrated by means of the notion of defeasibility. Roughly, a defeasible condition is one that forms a reasonably sufficient grounds for something, but
which can be overridden by further evidence favouring a contrary judgement. This contrary judgement too, in its turn may be overridden in the light of new evidence and so on\textsuperscript{130}. Most, if not all, of scientific statements are marked by defeasible assertibility-conditions, i.e. they are inconclusive\textsuperscript{131}. Now, while assertibility conditions may be defeasible, this is never so for truth conditions. If \( t \) makes \( S \) true, there is no further condition \( t' \) whose addition to \( t \) can render \( S \) untrue. If a statement is only warrantedly assertible, we are entitled to enquire in terms of what it is so? The final answer of this enquiry is invariably, 'in terms of our claim to the truth of the statement'. However, if a statement is regarded as true and not just warrantedly assertible, a similar question would not arise\textsuperscript{132}.

Entity-realists or anti-realists have tried to respond to this difficulty by constructing truth conditions out of justification (or assertibility) conditions. Thus for example Harré has stated that: "All I intend to show is that there is a defensible distinction between the idea of the truth of everyday as a defeasible pragmatic consensus, and the rigid relation of words and world that is implicit in the concepts of truth and falsity that figures in the bivalence formulation of scientific realism\textsuperscript{133}". But such a move is in fact equivalent to abandoning truth altogether and replacing it with assertibility. This would result in the denial of truth values for all statements whose assertibility conditions are defeasible. On this account, for any defeasible statement \( S \) which may be justified / assertible, the response of entity-realists or anti-realists towards questions like, "But is it true that \( S \)?" should be either that of declining to give any answer or resorting to agnosticism of the type similar to that of van Fraassen\textsuperscript{134}.

IV.C. Language Games and Sociological Turn

Entity-realists' last type of argument from epistemologized truth is based on the
Wittgensteinian notion of "language-games" combined with the insights introduced by modern sociologists of knowledge who in their own turn are inspired by Wittgenstein to a great extent. Language-games, as introduced by Wittgenstein, are either simplified fragments of an advanced language, or the whole of primitive (artificial) languages. A language-game among its other basic features, conveys a form of life, which amounts not to our passive observing of the game but our active participation in it. Hence entity-realists' emphasis on intervening rather than representing. Language-games are often presented to show that the meanings or justifications of a game's elements are determined by the rôle of those elements in the game as a whole. In other words, meaning determination and justification can only take place inside the language-game in question. On this view therefore, truth is something which is local to each language-game. We come to understand truth by way of understanding how we develop criteria for it, and those are determined by the relations of (true) statements generally to the language-games to which they belong.

Drawing on the above ideas and invoking the arguments of a number of sociologists of science, Harré has claimed that science is a particular language-game in which scientists are involved. Truth is not the paramount concern of the players of the game of science; it is rather the gaining of reputation, money and the like. Nor is truth the final arbiter for settling the disputes or justifying the claims. Such a rôle is played by the notion of trust, which contrary to the realists' notion of truth is more tangible and has the virtue of being achievable.

However, epistemologizing truth via language-games seems to be more available to relativists than self-confessed realists. Incidentally, Harré is not unaware of this point. Nevertheless, he seems to think that the model proposed by a number of sociologists of
science can be successfully adopted by realists\textsuperscript{141}.

Such a conviction however, does not seem to be well justified. There are a number of points which can be raised against the appeal to language-games and the sociological turn. Firstly, the notion of trust, as was briefly pointed out above, is among those epistemic notions which are linked to the notion of truth and which gain their currency and credibility from that notion\textsuperscript{142}. We rely on experts in both practical and cognitive matters. Practical issues, by and large, belong to realm of technology where success (i.e., control and prediction) is the main objective and workability, durability, precision and the like are the criteria. Cognitive problems, in contrast, belong to the realm of knowledge-garnering where everything is conjectural and hypothetical (to different degrees i.e. scepticism can never be completely ruled-out). The important point to bear in mind however, is that in both contexts, reliability is dependent upon truth. A reliable and trustworthy machine, theory, or person, is the one which is true to its design, predictions/explanations, or promises. It is interesting that even Harré himself is of the view (although perhaps somewhat inadvertently) that "Trust is maintained by telling each other what we honestly believe to be the truth."\textsuperscript{143}

Secondly, to view science as a particular language-game and to put undue emphasis on the social construction of knowledge, has led entity-realists just like their sociologists allies, to slide into the abyss of relativism. Harré, has explicitly revealed this fatal flaw in his ideas by stating that: "Science is a set of techniques, both cognitive and practical, for arriving at beliefs about what is invariant for all views. But there is nothing which is independent of any view."\textsuperscript{144}. In fact the logical outcome of viewing science as a language-game and upholding the idea of social construction of reality, will be that of embracing the idea that not only our language of the world, but even the world itself, is
a social construction. This leaves entity-realists with no independent criteria to judge their own claim to knowledge.

Thirdly, Harré’s point about the objectives which scientists pursue seems to be somewhat misplaced. While nobody would deny that scientists may have many motives apart from a love for discovering the truth about the world, there can be no denial of the fact that for them (as against say, the politicians) to realize their concealed motives, there is little choice but to put forward ideas which purport to be true. Gaining more reputation, money, etc. depends, to a great extent, upon producing ideas which, by and large, are believed to be true representations of reality. Scientific results are not produced and judged in terms of reputation or social influence and the authority of scientists, but in terms of their truthfulness or falsehood. In other words, even if the objectives of the scientists are those enlisted by Harré and the sociologists, the aim of science (if it is going to be progressive, in Lakatos parlance) cannot be but true representation of reality. To conflate the two kinds of goals amounts to committing a fallacy.

It is worth noting that the verdict of the scientists goes against the conviction of Harré and his sociologists colleagues. R. Feynman has summarised the issue in this way:

*We’ve learned from experience that the truth will out. Other experimenters will repeat your experiment and find out whether you were wrong or right. Nature’s phenomena will agree or they’ll disagree with your theory. And, although you may gain some temporary fame and excitement, you will not gain a good reputation as a scientist if you haven’t tried to be very careful in this kind of work.*

There are other pitfalls for entity-realists who want to embrace the sociologists’ approach. Sociologists do not seem to worry about the fact that their insistence upon the point that scientists are not truth-seekers and the aim of science is not truth, undermines their own claims as well: If Sociology is a science proper, as sociologists like to claim, and if the contents of scientific discourses in general are not the truth about different aspects of reality but bunches of rhetoric, produced solely for the purpose of gaining
fame, money, and position for their utterers, then the same must be true of the sociologists' scientific findings, including that very point concerning the aim of the sociology of science.

The difficulty with sociologists' account of science, which renders their conclusions untenable, of course lies in their method of approaching this particular subject-matter. That is to say, Sociologists, as it were, look at the activities of scientists from outside. They take scientists to be the members of an alien tribe and try to understand them, not by getting involved in their activities and learning their ways of doing things, but by observing the surface of their movements. Interestingly enough, Harré himself is not unaware of this fact. However, it appears that he does not regard it as a weakness in sociologists' approach. On the contrary, he maintains that this way of looking at science is superior to the known realist philosophies of science. According to Harré in this approach:

Laboratories are looked upon with the innocent eye of the traveller in exotic lands, and the societies found in these places are observed with the objective yet compassionate eye of the visitors from a quite other cultural milieu. There are many surprises that await us if we enter a laboratory and study a group of scientists in this frame of mind. The idea that the enterprise can be defined in terms of an idealized epistemology, whether that of experimentally based inductions or of the conjectures and empirical refutations of logicist philosophers of science, is quickly refuted. Logic, it seems, is not among the "idols of tribe". Where it appears it is an interest in the pursuit of rhetorical advantage in debate. The experimental control of thought, the official philosophy of science, is demonstrably remote from the considerations of those actually practice science as a way of life.¹⁴⁷.

However, the result of this sort of sociological or anthropological approach to science is to produce a kind of ‘knowledge’ about science which Feynman has aptly called ‘the cargo cult science’:

...In the South Seas there is a cargo cult of people. During the war they saw airplanes land with lots of good materials, and they want the same thing to happen now. So they’ve arranged to make things like runways, to put fires along the sides of the runways, to make a wooden hut for a man to sit in, with two wooden pieces on his head like headphones and bars of bamboo sticking out like antennas — he’s a controller — and they wait for airplanes to land. They are doing everything right. The form is perfect. It looks exactly the way it looked before. But it doesn’t work. No airplanes land. So I call these things cargo cult science, because they follow all the apparent precept and forms of scientific investigation, but they’re missing something essential, because the planes don’t land.¹⁴⁸.
V. Ellis’s Pragmatic Theory of Truth

In place of the realist correspondence theory of truth, entity-realists have turned to pragmatic theories. Among the entity-realists it is B. Ellis who has produced a fully-fledged account of such a theory. Ellis’s theory, in line with the general features of all pragmatic theories, is an evaluative theory:

As a truth theory, it belongs to the pragmatic tradition.... Roughly, the idea behind any pragmatic theory, or any other kind of evaluative theory, is that ‘truth ’ is a term like ‘rightness ’; it has a rôle in epistemology like ‘rightness ’ has in ethics, but is concerned with the evaluation of beliefs rather than actions. In other words, by the criteria appropriate to epistemic evaluation of beliefs, the truth is what it is right to believe.

Since the theory links the concept of truth to epistemic criteria, it is regarded as a value-based theory, in that truth is what satisfies our epistemic values such as consistency, conservatism, empirical corroboration, explanatory power, and unification.

Epistemic theories of truth in general, and Ellis’s particular variation of it suffer from a number of rather standard shortcomings. In the first place it can hardly provide a distinction between a realist who subscribes to it, from an anti-realist, say, an instrumentalist or one who enjoys a coherentist theory of truth. A theory may satisfy the epistemic values, and yet the entities it posits may be non-existent. For a correspondist, it is the very existence of the posited entities which is responsible for the success of theories – not the other way round. Concrete success does not distinguish truth from justified belief. The two do not always coincide. The same point can be emphasised by appeal to the case of defeasible justifications: when we change our minds in the light of further evidence, we might still hold that our original belief was justified but not that it was true.

The root of the difficulty for pragmatists of course, lies in the fact that they are
asking a rather misplaced question concerning the issue of truth. Instead of dealing with the problem of the nature of truth and asking 'what are the conditions by virtue of which a statement is true?' they ask 'what are the criteria (tests or standards) for a statement to be true?', and from here they raise the objection against realists that their correspondence theory would not give us the truth (falsity) values of the statements. But this desire to resort to "criteria" is surely an odd bed-fellow for the self-proclaimed realists. Entity-realists have been at pains to produce criteria which on the one hand make truth reside in an experience accessible to an individual cognizer, and on the other hand escape from a subjective conception of truth. But if something objective (i.e. about the world) supplies the grounds for truth, is it not more reasonable to take that thing (whatever it may be) as our truth-constituter rather than the experience (possible or actual) resulting from it?^151

Linking truth to epistemic values opens up the flood gate of relativism. If truth is what we, on grounds of available evidence, believe in, then it can be asked that why should such a belief command agreement for all other cognizers in other times and places. Different people may have different epistemic values and so may believe different propositions given the evidence. Or alternatively they may be in possession of different evidence, and again come to different judgments even if they share our epistemic values^152. The worst case, of course, can happen if a combination of different epistemic values and different evidence becomes possible. Interestingly enough, Ellis has conceded this point although in a slightly different context. While he tries (albeit not with evident success, as we shall see below) to reject relativism among human beings, he concedes that it is implausible to suppose that the human epistemic perspective is shared by all other intelligent beings^153. However, having conceded this much, he goes on to repair the
deficiency in his theory by claiming that:

... the argument for species-relativism concerning the nature of reality is inconclusive. For while it is possible to imagine species which operate with different systems of epistemic values, and have concepts of epistemic rightness similar to, but different from ours, we should not regard them as yielding rival conceptions of reality, unless the value systems on which they depend were more or less the same as ours. If they lacked one or more of our primary epistemic values, we should not regard their systems as belief systems in our sense.  

But this is rather unfortunate since it leads to a contradiction for Ellis’s theory. The contradiction arises because, Ellis, like many other good pragmatists, subscribes to Peircean realism and thus believes in intersubjective objectivity and the view that a proposition is true iff it would be in an epistemically ideal theory, i.e. a theory we would believe in after the application of our actual epistemic values to a totality of evidence. But then it can be asked, as it is the case with Peirce, albeit in a slightly different context, of what happens to truth if an alien species subscribes to a different ideal theory? Here one has no choice but to talk of different incompatible truths, and this is relativism pure and simple.

Species-relativism aside, it is hard to see how Ellis can avoid relativism, even within the human community. As noted above, for Ellis, like all other pragmatists, truth is objective, not in the strong realist sense of mind-independence, but in the weaker sense of intersubjective objectivity. This latter concept however, would not serve the same function and purpose as the realists’ strong objectivity does. It is because, once we take human fallibility seriously, then we cannot base our views about truth on what men think is true. Pragmatists maintain that truth is what wins the day and/or works successfully and this is what brings intersubjective agreement. But it should be asked 'why should working constitute truth?' After all, man’s history of intellectual advancement is littered with ideas which have been praised for their working-ability and success in practice, which after a while proved to be false. As Laudan has correctly pointed out, agreement may be about
non-existent entities. People may ascribe causal powers to what actually does not exist. While realists have no difficulty in rejecting these sort of commonly accepted opinions as false, entity-realists, because of their subscription to a pragmatist theory, have no choice but to bite the bullet and endorse these patently false ideas as true judgments.

A last objection to the pragmatic theory of truth stems from the fact that linking truth (of scientific claims) to the views held by the majority members of the scientific community, sows the seeds of a concealed dogmatism and discourages any dissent from popularly held views of the scientific community of the day. If the community is happy with its current theories and its understanding of nature, there is no incentive for a pragmatist to even attempt to produce a brand new idea, let alone to stand against all odds and defend some views which are unpalatable to the taste of the scientific community\textsuperscript{159}. Realism on the other hand, as we have already seen, encourages constant revision of our views and the reaching out for better and better judgments concerning the nature of reality. In fact, it has been the adoption of this attitude, rather than what is preached by pragmatism (or other brands of anti-realism for that matter) that has allowed men and science to progress. As Weber has observed:

"... All historical experience confirms that men might not achieve the possible if they had not, time and again, reached out for the impossible."\textsuperscript{160}
NOTES (Chapter Five)

1. Newton-Smith [1988].
2. R. Aldmer [1987].
5. A. Franklin [1986].
6. A. Franklin [1990].
7. P. Galison [1987].
8. I. Hacking [1983], [1984].
10. R. Harré [1985], [1986], [1990].
12. Among the philosophers whom the entity-realists regard (in one way or another) as the followers of logical positivists one can refer to Hempel, Nagel and Popper (as well as some of his students e.g. D. Miller). Harré [1970, pp. 2, 15] has called these philosophers the "deductionists" and has described their views in the following terms: "It is natural ... to combine extensionism in logic with positivistic empiricism, and phenomenalism in metaphysics and epistemology. This complex of doctrines I shall call 'deductivism'."


14. Harré has coined the terms referential-realism [1985, p. 57] and policy-realism [1986, p. 194] for his brand of realism. Hacking [1984, p. 27] has dubbed it realism about entities or entity-realism. Ellis [1991, p. 272] has named his realism naturalistic or Peircean. Cartwright [1984, p. 152] has used the term 'simulacram' to distinguish between her realism and the more traditional ones. The following quotations are some typical statements of the position defended by these new realists:

"Experimental physics provide the strongest evidence for scientific realism. Entities that in principle cannot be observed are regularly manipulated to produce new phenomena and to investigate other aspects of nature. They are tools, instruments not for thinking but for doing...

Discussions about scientific realism or anti-realism usually talk about theories, explanation, and prediction. Debates at that level are necessarily inconclusive. Only at the level of experimental practice is scientific realism unavoidable — but this realism is not about theories and truth. The experimentalists need only be a realist about the entities used as tools." (Hacking [1984], p. 154).

"There are two ways of defending realism in relation to the epistemological claims of science. The bivalence principle interprets scientific realism as the thesis that every scientific statement is true or false by the way the world is. Since we found good reason to think that no scientists or scientific research programme could ever tell whether any scientific statement was strictly true or false, a defence of realism based on the bivalence principle came to seem hopeless. The alternative approach was to build a more modest idea of scientific realism on the result of successful and unsuccessful acts of reference in which a real physical relation is established between an embodied person (acting for the linguistic community) and an exemplary thing, event etc. Referential-realism is developed on the basis of the thesis that many of the
referring expressions that occur in theoretical discourses have referents in the world that exist independently of human cognitive and practical activity, and that we have good reason to believe that." (Harré [1986], p.191).

15. Many philosophers have referred to the difficulties accompany the notion of truth. See for instance, G.Pitcher [1964, pp. 1-15]. Popper has recalled that: "... before I became acquainted with Tarski's theory of truth, it appeared to me safer to discuss the criterion of progress without getting too deeply involved in the highly controversial problem connected with the use of the word ‘true’. My attitude at the time was this: although I accepted ... the objective or absolute or correspondence theory of truth ... I preferred to avoid the topic. For it appeared to me hopeless to try to understand clearly this strangely elusive idea of a correspondence between a statement and a fact." (Popper [1963/72 p.223]) Although Popper, since he has read Tarski, has more confidently invoked the notion of truth, however, there are some philosophers who would cast doubt on Popper’s interpretation of Tarski’s approach. For one such criticisms see S.Haack [1978/1985].


Realists maintain that it is both legitimate and desirable to adhere to the notion of correspondence truth. Perhaps one of the oldest formulations of this theory can be found in the Aristotle: "To say of what is that it is not, or of what is not that it is, is false, while to say of what is that it is, or of what is not that it is not, is true." (Quoted in S.Haack [1978], p.88)

In modern times, Tarski has produced a crisp formulation for capturing this basic idea. Tarski’s formulation is represented by the following schema known as (T) schema: S is true iff p. Here p can be replaced by any sentences of the language for which truth is being defined and S is to be replaced by a name of the sentence which replaces p. An instance of (T) would be: ‘Snow is white ’ is true iff snow is white. (ibid. p.100)

Correspondence, as Vision ([1988], pp.27-9) has pointed out is the view that purports to supply the conditions constituting a statement’s truth (or the conditions making a statement true).

Correspondence theory answers the question ‘what are the conditions by virtue of which a statement is true?’ Truth can be framed in terms of a concept; true, or the predicate; ’is true’, or the operator; ’it is true’. Truth constitutor may be a fact, a situation, state of affairs, ordered aggregate of individuals, or a concrete chunk of spatio-temporal real estate. Truth constituters are not logical relationships between propositions (statements or judgements or beliefs or sentences), state of satisfaction, verification procedures, and approximation to theoretical ideals. Some truths may be about sentient beings or sentient doings and thus in a sense not mind-independent. But this is because of the subject-matter and not the nature of truth. One of the points, concerning the truth of our statements, which a realist would like to uphold is that the difference between "observational" and "non-observational" objects is just a difference in the sort of object one is observing, and has no bearing on what one means by "truth". (cf. ibid)

17. The term and the concept of 'warranted assertibility ' was first used by J.Dewey (cf. Haack, ibid. p.98) and later on used extensively by those who subscribe to non-correspondence theories of truth like pragmatic theory or redundancy theory. We shall discuss a version of pragmatic theory of truth in Sec.4, below. Next note contains few remarks concerning the redundancy theory. For useful surveys of the traditional and modern theories of truth see S.Haack [1978/1985], A.White [1970] and A.C.Grayling [1982].

18. Among modern theories of truth a family called variably (and sometimes interchangeably) as deflationary or, redundancy or, disquotational theories are developed by a number of writers like Ramsey, Austin, Strawson, Mackie and Belnap. (cf. Haack op.cit) A number of modern anti-realist (e.g. P.Horwich [1990] and R.Rorty [1979]) have also subscribed to this theory. According to Vision (op.cit) "redundancy theory of truth" involves three theses namely:

A) Logical Equivalence; a statement made with a sentence of the form ‘p’ is true iff one made with a sentence of the form ‘p is true ’, is true.

B) Semantic Equivalence; a sentence of the form ‘p is true’ means the same as one of the form ‘p’.

C) No Truth; there is no traditional theory of truth, or alternatively the only thing left of the theory of truth is that of the nature of judgement. This will amount to the claim that truth plays no rôle in our understanding of statements.

It seems that the advocates of the redundancy theory have, unnecessarily, stretched the correspondence
theory beyond its limits. There are a number of standard objections to redundancy theses. For example, it is easy to show that (B), which is regarded by many as the core of redundancy, is not at odds with correspondence theory, since it is dealing with a question (namely, "what is the meaning of phrases such as 'is true' or 'is it true that?’") which is different from the main concern of correspondence, namely, the question "what are the conditions by virtue of which a statement is true?"

Contrary to (C) it should be noted that not only do we need the notion of "truth" in the correspondence sense to talk meaningfully about the state of affairs, but also we need that notion to talk meaningfully about our statements (propositions) about the state of affairs. It can also be shown that attempts to establish (C) on the basis of (A) fail. One can ask what do 'p' and 'p is true' have in common? It seems the only reasonable answer is 'their truth-conditions'. But it is clear that the question 'what are these truth conditions?' does not have the same authority to dissipate the problem of what it is that makes them true, as that which we get when we turn from the question of truth-conditions to that of meaning. Instead the question sends us back to the main difference between correspondence and redundancy as mentioned in the case of (B). (For details cf. G.Vision [1988])

19. Modern anti-realists, as we have already observed, while neither perusing the programme of traditional idealists (who would deny that the world exists independently of mental) nor the programme of traditional sceptics (who would cast doubt on the possibility of knowledge-garnering), have, by and large, moved in the direction of Kant’s programme, i.e. to make a distinction between the world of noumena and the world of phenomena. According to this new generation of anti-realists, although there may be an independent reality, nevertheless we have no access to this reality as independently of our cognitive faculties. Our notions of reality are the results of our conceptual schemes, languages, conventions, social and economic influences, and the images portrayed by prevalent scientific theories. In this respect, theoretical truth is not a representation (however crude it may be) of an independent reality but either a mere coherence between our ideas, or whatever assists us to achieve our practical purposes, or even something totally superfluous and redundant.

20. As we have already seen in the previous chapters and shall argue further in this chapter, to argue for anything short of an objective, independent reality which our everyday as well as scientific statements purport to describe / explain, will cause realists to embrace notions of reality which are in one way or another related to, or dependent on, men’s cognitive faculties. Well known anti-realists’ surrogates for truth aside, even a notion such as ‘inter-subjective agreement / testability’ is not, as some philosophers with realist tendency like Quine [1960] would think, a good replacement for the notion of objective reality. For a detailed elaboration of this point see R.Trigg [1980].

21. This view is held by Cartwright but not shared by some other entity-realist. R.Harré ([1986], pp.289-91) for example, has criticized her on this point.

22. This argument is developed mostly by Harré and Ellis. We shall discuss this argument at some length in the text.

23. This is Hacking’s main argument against truth realism. In his [1983] he relates the case of Hertz’s Principles of Mechanics and concludes that:

"Hertz presents ‘three images of mechanics’ — three different ways to represent the extant knowledge of the motion of bodies... Hence within the best understood natural science — mechanics — Hertz needed criteria for choosing between representations. ... None of the traditional values — values still hallowed in 1983 — values of prediction, explanation, simplicity, fertility, and so forth, quite do the job. The trouble is, as Hertz says, that all three ways of representing mechanics do a pretty good job, one better at this, one better at that. What then is the truth about the motions of bodies?

The representations of physics are entirely different from simple, non-representational assertions about the location of my typewriter. There is a truth of the matter about the typewriter. In physics there is no final truth of matter, only a barrage of more or less instructive representations." (pp.142-6)

The same theme, namely underdetermination, has been picked up and amplified by Harré [1986] Cartwright [1983/84] and Ellis [1991].
24. In the last chapter, we shall try to show how many of aspirations of the entity-realists can be preserved and their concerns laid to rest within a more comprehensive model for realism.

25. Not all realists would resort to this argument. Popper, for example, due to his anti-inductivist stand, would not recognize this argument as valid.


29. Hacking [1983], p.263. Both Harré and Cartwright are in full agreement with the above instrumentalistic argument. Harré writes: "Hacking ([1983], chapter 16) develops a case which goes beyond Grene's comprehensive realism in a way with which I am in substantial agreement. Like Boyle, Hacking thinks in terms of projects which involve doing something with clusters of entities, the effects of which is 'to interfere in other more hypothetical parts of nature.' A hypothetical entity becomes real to us when we use it to investigate something else." ([1986], p.51)

   Cartwright in a similar way has pointed out that; "I agree with Hacking that when we can manipulate our theoretical entities in fine and detailed ways to intervene in other processes, then we have the best evidence possible for our claims about what they can and cannot do; and theoretical entities that have been warranted by well-tested causal claims like that are seldom discarded in the process of science." ([1983/4], p.98)

30. M.Bunge [1969], p.91. In my discussion of the instrumentalistic argument I have also benefitted from H.Holcomb [1988].

31. [1984], p.170.

32. Popper [1968], P.253.

33. op.cit. italics added.

34. We shall see shortly that even in the cases where experimentalists have discovered new entities for which no specific theory is yet formulated, they have to rely on the guide-lines provided by the theoreticians.


36. See Agassi [1980].

37. "Philosophy of science without history of science is empty; history of science without philosophy of science is blind" Lakatos [1971], p.91. Lakatos, on his part, had paraphrased Kant.


40. As noted above (note. 34) there are of course some other episodes in the history of science in which the experimenters have discovered some entities prior to the development of the theories which would explain them. However, as we shall discuss shortly, from an epistemological point of view, these cases are not essentially different from the ones in which theory precedes the experimental corroboration.

42. It is worth noting what does this acknowledgment on the part of Hacking entail. If it is further theoretical elaboration which puts us in a position to confidently recognize electrons and make use of them, then it is not our use or manipulation of electrons which have warranted our belief in electrons but the increase in our theoretical knowledge of electron; a knowledge, in its growth and development, experimentation and practical manipulation has no doubt played a significant rôle. However, the point is that experimentation and practical manipulation can only be aids to theoretical knowledge. On their own they are simply blind. This point is further discussed in the text.

43. op.cit. [1983], pp. 271-2.


45. op.cit. [1986], part two, pp.100-3.

46. As we have emphasised throughout this essay, philosophy of science should not be equated with philosophy of language. As such, there may not be any need for using the techniques used in philosophy of language, to tackle the issues in the field of philosophy of science.

47. ibid.

48. Kripke [1979], Putnum [1978]. Kripke has been more cautious in extending his causal theory of reference to the unobservable entities. Putnum however, has been quite forthcoming and explicit in this respect.

49. Against Operational definition advocated by Bridgman and his followers (see Ch.2) it has been argued that if we measure a physical quantity, say the distance between two points, by two different methods, e.g., by using a measuring tape and by using a theodolite, then according to the very method advocated by the operationalists, we are not dealing with one single concept, but two different concepts, the relation between which is not trivially known. As stated in the text, it seems the view advocated by Cartwright is vulnerable to a variation of this type of argument.

50. Cartwright & Mendell [1984], p.149.

51. op.cit. pp.100-107.

52. Cartwright [1989], p.3.

53. ibid.

54. Cartwright [1989] wants to argue against Humean interpretation of causal relations in nature. She has noted that for Hume 1) singular causal facts are true in virtue of generic causal facts, and 2) generic causal facts are reducible to regularities. This means that for Hume, causal relations are reducible, in the last analysis, to constant conjunctions between events.

55. "This book begins with a defence of causal laws, ... In addition to the notion of causal law, we also need the concept of capacity ..." [1989], p.141.

"... I want to focus on the special case of causes and capacities, and why we need them. They are part of our scientific image of the world, I maintain, and cannot be eliminated from it. ... I claim that laws of [physics] are laws about enduring tendencies or capacities." [1989], p.1. 

Empiricists, by and large, have been opposed to occult entities and processes. It is therefore interesting to observe that Cartwright, the self-proclaimed empiricist, wants to defend the reality of entities like capacities.

Cartwright of course, is aware of this limitation. Her justification however, is that capacities can be measured: "I maintain that the crucial question for an empiricist must always be the question of testing. So we must ask: can capacities be measured? But that question has been answered, ... I described how to use probabilities, treatment-and-control groups, and experiments constructed in the laboratory to test causal

57. Cartwright in many passages of her [1983/4] has emphasized her view concerning the veracity of phenomenological laws while denying the same value to the fundamental laws. "A long tradition distinguishes fundamental laws from phenomenological laws, and favours the fundamentals. Fundamental laws are true in themselves; phenomenological laws hold only on account of more fundamental ones. This view embodies an extreme realism about the fundamental laws of basic explanatory theories. Not only are they true (or would be if we had the right ones), but they are, in a sense, more true than the phenomenological laws that they explain. I urge the reverse." ibid. [1983/4] p.100.

58. In her earlier discussions of the non-facticity of fundamental laws, she had noted that: "We can preserve the truth of Coulomb's law and the law of gravitation by making them about something other than the facts: the laws can describe the causal powers that bodies have." (Cartwright [1983/4], p.61. Italics added)

Nevertheless, she went on to add that: "The introduction of causal powers will not be seen as a very productive starting point in our current era of moderate empiricism. ... If laws of nature are presumed to describe the facts, then there are familiar, detailed philosophic stories to be told about why a sample of facts is relevant to their confirmation, and how they help provide knowledge and understanding of what happens in nature. Any alternative account of what laws of nature do and what they must serve at least as well; and no story I know about causal powers makes a very good start." Ibid. pp.61-2. emphasis added.

59. ibid. p.8.


62. Cartwright has argued at some length against the D-N model of explanation in her [1983/84].

63. See thesis M2 in chapter 1.

64. As pointed out in the first chapter, the minimal realism does not subscribe to the D-N model of explanation. Instead, it endorses a causal theory of explanation not very different from the one discussed by Bhaskar [1975/79]. In his book Bhaskar has criticised the D-N model on the lines more or less similar to those found in Cartwright's.

65. Popper for example had long ago emphasised that: "The theoretician puts certain definite questions to the experimenter and the latter, by his experiments, tries to elicit a decisive answer to their questions, and to no others." (Popper [1968], p.253)

66. Cartwright has captured this view in her aphorism, "Science is measurement." op. cit., p.1.

67. Of course, pragmatic considerations, as we have already pointed out (Ch. 4) and shall discuss further in the next section, in the final analysis rely on the notion of truth for their credibility: an effective instrument is the one which remains true to its design, assuming that the design itself is a correct one, free from judgemental errors.

68. J.Agassi [1980], P.17.

It should be emphasised that despite the differences mentioned in the text between the theoretical and technological knowledge, the two are (especially in the modern time) increasingly inter-connected, and it may not always be possible to draw a line between the two. Nevertheless, it seems, for the sake of better analytical assessments, it is advisable to keep the fine distinction between the two realms.

The first serious challenge to the view that nature is being governed by the simple laws which are applied to the simple pattern underlying the complex and diverse phenomena came in the early 1980s from the Chaos Theory: the discovery that simple, nonrandom laws may lead to complicated, unpredictable, and chaotic behaviour. A standard requirement for the chaos is that the system should be nonlinear – its responses to change is not proportional to the size of those changes.

Taking a lead from the chaos theory, Complexity Theory is trying to find "order out of chaos"; While chaos theory shows that simple rules and laws can sometimes produce disorganised behaviour, complexity theory tries to find out how complicated rules and laws (i.e. phenomenological laws) can sometimes produce organised behaviour. Complexity theory, carries with it the tacit assumption of wholism: working on the complex, highly non-linear systems, for which the behaviour of the whole is assumed not simply be related to the behaviour of the parts, requires to look at a whole situation rather than looking in detail at individual aspect of it.

Although the advocates of the complexity theory try to describe the systems in terms of networks of interacting pieces which together give rise to local regular behaviour, they are not anti-physicalist in the sense that they have no qualm over describing the behaviour of the systems ultimately by appeal to some basic, simple underlying principles or patterns. However, they are not reductionists in that they admit that the basic laws of physics, on their own, cannot produce a complete description of chemical, biological, social, psychological and other systems which are at higher degrees of complexity than the fundamental level of physical constituents.

Murray Gell-Mann in his recent book : The Quark and the Jaguar" [1994] has put this point in this way: "A science at a given level encompasses the laws of a less fundamental science at a level above. But the latter, being more special, requires further information in addition to the laws of the former. At each level there are laws to be discovered, important in their own right. Investigating those laws at all levels, while also working, from the top down and from the bottom up, to build staircases between them." (p.112)

Cartwright share with the complexity theorists the view that complex, phenomenological systems are of great importance. However, it seems the resemblance between her views and complexity theorists almost ends here. She has emphasised the anti-reductionists thesis in a much more radical fashion: "Not only do I want to challenge the possibility of downwards reduction but also the possibility of 'cross-wise ' reduction." ([1994], p.281)

Moreover, she is not in favour of the wholistic approach: "It seems to me the wholism is far more likely to give rise only to ceteris paribus laws, whereas natures are more congenial to pluralism." (ibid. p.285)

Furthermore, she maintains that processes like approximation and idealization in science which help to improve the match between our theories and the reality do not work from top down but only from ground up: "But the improvements come at the wrong place for the defender of fundamental laws. They come from the ground up, so-to-speak, and not from the top down. ... What we do ... is to add a phenomenological correction factor, a factor that helps produce a correct description, but that is not dictated by fundamental law." ([1984], P.111)

Cartwright of course, can turn the table against the realists and argue, justifiably, that they too because of their subscription to the principle of empiricism cannot justify their preference for a unified pattern underlying the diverse phenomena on purely empirical grounds. We shall discuss this difficulty in the last chapter.

op.cit. [1983], p.3.

ibid. p.3.

Cartwright [1994], p.281.

ibid. p.291.

For an excellent account of Perrin’s work see M.J.Nye [1972].

Cartwright [1983/4], p.83.

79. Ritz formula which was known as "Principle of Combination" was, 
\[ V = R \left( \frac{1}{n^2} - \frac{1}{m^2} \right) \] 
where \( n = 1, 2, 3 \) and \( m \) is an integer > \( n \). (cf. d'Abro [1951]) For the other spectral formulae see next Chapter.

80. See next chapter.

81. ibid. p.61.


83. See Bhaskar, op.cit. p.100.

84. ibid. p.286-7.

85. ibid. p.286-7.

86. Incidentally, this way of interpreting fundamental laws is being endorsed by a fellow entity-realistic. R.Harré in his criticism of Cartwright's rejection of the "principle of superposition of forces" has noted that: "If the issue is indeed the realist reading of laws of physics and the deepest of these ascribe tendencies to physical fields on the basis of assignments of quantitative measures of potential, then the argument that there are cases, higher in the hierarchy of physical processes, such as those described by some set of the laws of hydrodynamics, for which there are no clear superposition rules, proves nothing of interest. But, if the joint dynamics of gravitational and electromagnctic potentials (together with the dynamical variables of the weak and strong interactions) are indeed the basis of all physical processes, then the principle of superposition, which are operative among the real tendencies these laws describe, are instances of the most general vector addition law. And they permit (though not guarantee) a realist reading of the basic laws of physics." (R.Harré op.cit. [1986], p.290.)


88. op.cit. [1983], pp.107-112.

89. Chemists for example, are concerned with different kinds of chemical bonds between atoms. They have produced many phenomenological laws which would help them to predict the behaviour of chemical reactions. At the same time efforts are made, by the theoretical chemists, to derive these laws, in "approximate ways" from QED.

90. As pointed out earlier in the context of van Fraassen's philosophy, traditional realists (including the minimal realists) despite their tacit or implicit subscription to a unified metaphysical picture, have hardly any justification for such a preference. In the last chapter we shall try to provide such a justification in the context of a more comprehensive philosophy of science.

91. See note 16 above.


93. "Truth epistemologized is truth so fashioned that its detection is custom-fitted to our practices with it or its available marks. Truth that can only be understood through conclusive justification, or through permissible moves within language-related practices (e.g. language games) introducing it, or as the upshot of various style of reasoning, or in terms of prevailing scientific theories, can never transcend (that is to say, be independent of) the evidence for it, and is thereby epistemologized." (Vision, op.cit., p.145)

The entity-realists' arguments from the epistemologized truth are fairly standard and quite similar to those arguments produced by well known anti-realists like Michael Dummett [1978] and Crispin Wright [1987] who have, in their turn, been inspired by Wittgenstein.

94. In elaborating the arguments in this section I have benefited from Vision [1988].
95. We have already discussed the objection to unobtainability of truth in Chapter 4. However, here we shall look at some other aspects of this objection, namely, the unobtainability of the truth-content of certain classes of propositions within the correspondence theory of truth.

96. Harré [1986, p.2] has quoted the following as Peirce's paradox: "Absolute truth is the settled belief of the community of enquirers, and 'indubitable propositions are the settled opinions of individual enquirers'. But no individual can be absolutely certain without 'the verdict of final scientific community' [and that, we, as historically situated human beings can never have]."

97. Ibid. p.32.

98. See Ch. 1.

99. See Dummett [1978].

100. R.Stalnaker [1987], pp.161-2). has noted that Dummett ([1978], using his own definition of realism, has posed the following dilemma to realists: "A realist about any class of statements must choose between naive realism — the view that statements in the class are barely or simply true — and reductionist realism — an explanation of the truth of the statement in terms of statements in some more basic class. Now a realist would be excessively naïve to be a naïve realist about ascriptions of character. If it is true that some person is brave or vain or complacent, it must be true in virtue of something more basic — either facts about the behaviour of the person or perhaps facts about the state of the person's brain. But if the reduction programmes also fails, then we must conclude that such statements do not make determinately true or false factual claims at all."

101. Harré [1986], pp.3-4. Apart from the general aspect of the argument from bivalence which will be discussed in the text, one particular point to be noted in Harré's version of this argument is that he seems to have conflated bivalence and correspondence, regarding them to be two sides of the same coin. He has defined the principle of bivalence as: "the principle that most theoretical statements of a science are true or false by virtue the way the world is." However, this definition can be divided into two distinct parts, i.e. i) the semantic principle of bivalence which simply says that all statements are either true or false, and ii) the correspondence theory of truth which states that true statements correspond to things or states of affairs in the world, whereas false statements fail to do so. Harré however, doesn't seems to be concerned about this distinction and thus throughout his latest book [1986] and his recent articles uses the term bivalence in the sense of bivalence and correspondence (cf. R.Nola [1987, p.576]).

In the text however, I have kept the two concepts apart to avoid undue confusion.


103. B.Ellis [1991] has produced an extensive account of a pragmatic theory of truth in line with the general approach of entity-realists. His views are explicitly endorsed by Harré [1985, p.58, 1986, pp. 40, 94-5]. It should however, be noted that despite overall agreement between entity-realists in adopting a pragmatic stand towards the issue of truth, differences of style in formulating this view, is nevertheless discernable in their works. Thus for example, while Harré, in his pragmatic approach has introduced the notion of 'plausibility' which is a variation of the notion of 'warranted assertibility', Ellis has opted for a kind of Peircean realism in which truth is a limit for the notion of reasonable belief ([1991], p.269). Nevertheless, in all these variation on the theme of pragmatism, the common core is always epistemologizing of the notion of truth.

104. Putnum [1991]. The argument in the text draws on Putnum’s article. Other writers have also developed similar arguments. See for example, E.J.Bond [1987], Vision [1988].

105. Tarski [1949, p.70] has put this point in an interesting way:

"... If we ask a high school boy, or even an adult intelligent man having no special philosophical training, whether he regards a sentence to be true if it agrees with reality, or if it designates an existing state of
affairs, it may simply turn out that he does not understand the question; in consequence his response, whatever it may be, will be of no value for us. But his answer to the question whether he would admit that the sentence "it is snowing" could be true although it is not snowing, or could be false although it is snowing, would naturally be very significant for our problem.

Therefore I was by no means surprised to learn (in a discussion devoted to these problems) that in a group of people we questioned only 15% agreed that "true" means for them "agreeing with reality", while 90% agreed that a sentence such as "it is snowing" is true if and only if, it is snowing. [Presumably 5% agreed to both!] Thus, a great majority of these people seemed to reject the classical conception of truth in its 'philosophical' formulation, while accepting the same conception in plain words." (Quoted from E.J.Bond [1987], pp. 79-80)

106. cf. K.Popper [1963], p.226

107. Entity-realists in a way not dissimilar to that of van Fraassen [1980] do admit correspondence between our discourse at the phenomenal (observable) level, while rejecting the veracity of the statements about unobservable entities and processes. Ellis [1991] writes:

"... there is, as I said ... something obviously right about the correspondence theory of truth. For it does give an account of what it is for some simple categorical statements to be true. For example, if I say that the cat is on the mat, then clearly what I say is true iff the cat and the mat both exist, and the cat stands in the appropriate relationship to the mat. I am not, of course, denying this. For this is precisely the claim that the statement makes, and I cannot see why anyone would wish to deny it. ... At any rate, this is not what I am doing. What I am saying is that there are many kinds of statements which we count as true or false which cannot be analyzed truth functionally, and they cannot be expressed in an extensional language for which realistically interpretable semantics can be given." (pp.157-8)

Harré, too, in a similar vein has observed that:

"Despite the arguments for relocating the strict sense of "true" and "false" and the tough sense of "knowledge", it would be absurd to deny that there is a sense of the pair "truth/falsity" by which scientists, like everyone else, comment upon their observations. It is obvious that that sense does not extend to the unobservable or the general, that is theories and laws." ([1986], p.93)

Harré has backed-up his objection to correspondence theory by introducing three realms of ontological depth and three corresponding levels of theories. These are, roughly speaking, as follows:

Type one theories which belong in Realm 1 (realm of actual and possible experience) and enable the constitution, classification and prediction of observable phenomena;

Type 2 theories represent the beings of Realm 2 (realm of unobservable but eventually observable beings);

Type 3 theories are cognitive objects with mathematical properties which enable the representation of the beings of Realm 3 (realm of in principle unobservable beings).

Harré claims that many singular statements and theories are not expressible in terms of truth and falsity, and that the principle of bivalence is not sensitive to ontological depth, viz, the notions of truth and falsity are not applicable to the types 2 and types 3 theories. ([1986], pp.70-1, p.38)

108. The common feature of the members of this class is that they are all verification-transcendent. Statements about past or future, generalizations (e.g. laws and theories), statements about possible worlds and/or counterfactuals, ascription of sensations to others are among the members of this class.

109. As noted earlier some statements may be about mental things or events (e.g. statements about one's ideas). But this sort of mind-dependence would not undermine the basic tenets of correspondence since it arises because of a particular subject-matter and not because of the nature of truth in that realm.

110. Ellis, not satisfied with the mapping relation (relied upon in the correspondence theory of truth) has argued that; "... physical correspondence theory of truth which seeks to identify truth with some kind of mapping of the world on the brain, cannot account for the value of truth." ([1991], p,9, see also pp. 160-4)

111. As noted in the previous chapters, realism, and not scientific realism, is the thesis which affirms the existence of a mind independent and self-subsistent reality. This thesis does not presupposes the correspondence theory of truth, whereas both correspondence theory and the doctrine of scientific realism
presuppose this thesis.

112. See also note 116 below and the relevant point in the text.

113. In the light of the above it could be argued that entity/anti-realists argument from bivalence, is in a sense a misguided venture. It has been suggested (Dummett [1978], pp.145-165 and elsewhere) that since one’s ontological commitments do not settle the issue between realists and anti-realists, it is therefore more appropriate and more promising to switch from the disputes over the existence of entities to the determinateness of truth-conditions for statements. This move, it is alleged, while preserving the basic tenets of the dispute, opens new avenues for successful advancement. This view is however, to say the least, debatable. The difficulty with the proposed programme (whatever its merits as an independent philosophical programme) is that as far as the issue of realism is concerned, it amounts to a change of subject and not just to a new strategy for tackling the same problems. (this point is incidently evident in van Fraassen’s remarks regarding Dummett’s definition of realism, see van Fraassen [1980], p. 37). We remember from the previous chapters that ontological issues cannot be settled by resorting to semantic or epistemic discussions. (See Devitt [1984]). Semantic properties and relations are themselves supervenient on the details of the physical properties and relations of person (s) and environment. That is to say, there could not be a difference concerning semantic properties and relations, unless there were a difference in the physical characteristics or context. This means that the full details about the physical situation entail the semantic properties and relations in question (see J.Bigelow & R.Pargetter [1990], p.441).

114. Russell’s analysis of Meinongian-type ontology is a case in point. See M.Sainsbury [1979/85].


116. To account for the truth in fiction, one can, a la D.Lewis [1983, 269], assume the fictional world to be as much like the actual world, or assume the fictional world to be in accordance as much as possible with the generally prevalent beliefs of the community in which the story originates. These accounts however, as K.Bach [1987, pp. 217-8] has pointed out, are not the only one which can be proposed. For example, perhaps what matters is not the beliefs that actually prevail but what the storyteller takes to be the beliefs that prevail among his intended audience; or maybe what matters are the beliefs of the storyteller himself (compare the case of describing a dream).

117. Stalnaker, op.cit. However, the point discussed in the text is not Stalnaker’s. His concern is to show, contra Dummett, that at least some counterfactuals do have un-reducible, determinable truth-values.

118. op.cit. [1986], p.80.

119. A rather similar argument due to Ellis [1991, ch.5] based on the notion of bearer of truth can be rebutted in a same fashion. Ellis, contra realists, holds that statements (propositions) are not appropriate candidate for truth-bearing. His favourite candidate are beliefs. However, as discussed in the text, since the content of beliefs are expressible in terms of statements (propositions), truth values can be assigned to them.

120. Colin McGinn [1979, p. 26] has distinguished two different arguments for understanding conditions in anti-realists literature, namely; First, the acquisition argument which states that we could come to learn a sentence only by connecting it with recognizable situations of a certain sort. Thus how we arrived at an understanding of a sentence places restrictions on what that understanding consists in. And Second, the manifestation argument which holds that one can ascribe language-mastery only to those who are capable of displaying it, and only circumstances in which the sentence one can accept or assert manifests what is affirmed can count as a sufficient display for ascribing mastery. (Vision op.cit., p.171)

121. S.Haack, op.cit.

122. Dummett [1978], p. xxi. The idea goes back to Wittgenstein [1953/78], "For a large class of cases — though not for all — in which we employ the word ‘meaning’ it can be defined thus: the meaning of a word is its use in the language." (ibid, p.20, no.43).
123. Harré, for example, freely acknowledges that "There are states of affairs, existing independently of human activities, which are denoted by theoretical and general statements". However, he emphatically denies that these state of affairs can be either the source of the meaning of such statements or the grounds for our acceptance or rejection of such statements. He maintains that the conditions for warrantable assertibility (or plausibility in his terminology) could be enough for the meaning with the proviso that we do not confine ourselves to realm of actual experience, since that would lead to a positivistic reduction. (op.cit. [1986], pp.32-3)

124. Harré [1986], p.32

125. For example, while truth under favourable conditions brings about agreement, an assertible valuation may fail even under favourable conditions to command agreement. Or again, while every plain truth is compatible with every other plain truth, i.e. the conjunction of plain truths is itself a plain truth, there is nothing in the assertibility property itself to guarantee that all one by one assertible evaluations are jointly assertible. Or again, while every truth is true in virtue of something, it is not clear that where there is disagreement there is always something or other at issue. For details see D.Wiggins ([1991], p.115-117).

126. See A.Goldmann [1986], [1988].

127. cf. E.J.Bond [1987], p.81. This fact is of course acknowledged by some of the entity-realists. Harré for example, in his discussion of epistemic notions writes: "... no combination of these conditions [e.g. assertibility-conditions] could replace the idea of truth exhaustively, since no epistemic property or combination of properties could be the equivalent of a semantic (ontological) property". This acknowledgment however, is not in accordance with Harré's subsequent attempts to epistemologize the notion of truth.


The following is a typical example of defeasible judgements. The unusual behaviour of a man in an uncrowded street may be a defeasible justification-condition for a neighbour, who happen to be a retired policeman, to assert, "the man is a drug-pusher". However, this assertion may be defeated when it become known to the neighbour that the man in the street is in fact an under-cover police officer involved in an under-cover operation. But the latter may be in turn defeated by the knowledge that the behaviour of the man does not accord with the accepted codes of conduct in these sort of operations, thereby re-establishing the case for the original assertion (statement). But, again, if the neighbour also learns that the officer's life is in real danger and he has to go to the extreme in his behaviour, this may be used to defeat all previous evidence for officer's misconduct, and so on.

131. The example of the classification of Acids (as told by Hacking [1984], pp.84-5) is a good case (out of many) in point. (Hacking's concern is of course different from mine. He has not used this example to highlight the defeasibility of theoretical statements but rather to show the inadequacy of Putnum's theory of meaning for the natural kind terms).

According to Hacking, Lavoisier and the community of the chemists around 1800 were of the view that acids are substances that taste sour in water solution, and change the colour of indicators such as litmus paper. They also react with many metals to form hydrogen, and react with bases to form salts, and also have oxygen in their structure.

The above beliefs concerning the properties of acids were partially defeated when in 1810 Davy showed that muriatic acid (i.e., hydrochloric acid HCl) is just an acid like any others with the exception that it does not have oxygen in its structure.

Dalton's revision, in its turn, gave way to further revisions (i.e. partially defeated). Today there are two sets of definitions (beliefs) for acids. On the one hand acids due to Brønsted and Lowry are members of a species that has a tendency to lose a proton (while bases have a tendency to gain one). On the other hand acids due to Lewis belong to a species that can accept an electron pair from a base by forming a chemical
bond composed of a shared electron pair.

132. The point discussed in the text deals with the asymmetry between the non-epistemic concept of truth and epistemic concepts such as warranted assertibility. Entity-realists/anti-realists cannot account for the defeasibility of our statements. This is because while all epistemologized concepts require the non-epistemic notion of truth as the concept which provide the final justification and explanation for those concepts, the same is not true for truth itself. Truth is not relying on any other concept to achieve justification or explanation. Similarly, truth-conditions cannot be constructed out of justification conditions.


134. It seems the above position is arrived at as a result of an unjustified distinction not dissimilar to the distinction made by anti-realists with regards to observable and unobservable realms. The distinction in question is made between two types of justification conditions with regards to their rôle in sentence mastery, namely, defeasible and non-defeasible justification conditions. Surely the onus is on entity-realists to explain what, within the resources of assertibility conditions and the connection of sentences meaning with use, enable them to determine which assertibility conditions are suitable as materials out of which to construct truth conditions. See Vision, op.cit. pp.188-9.

135. Wittgenstein has introduced the idea of language-game in his later works, namely after 1930. For example, in his Blue Books (p.17) he writes: “The study of language-games is the study of primitive forms of language or primitive languages. If we want to study the problems of truth and falsehood, of the agreement or disagreement of propositions with reality, of the nature of assertion, assumption and question, we shall with great advantage look at primitive forms of language in which these forms of thinking appear without the confusing background of highly complicated processes of thought”.

For a typical case of the influence of Wittgenstein on sociologists of science see D.Bloor [1976]. Incidentally Harré [1986] is in disagreement with Bloor and his colleagues (the so-called Edinburgh School). However, this disagreement does not concern the subscription of both parties to Wittgenstein’s later views. Harré, emphatically endorses the works of one other group of sociologists namely, the so-called Paris School whose main proponents are B.Latour and S.Woolgar.

136. For a typical adaptation of the notion of form of life and the concept of language game by sociologists of knowledge see B.Latour’s and S.Woolgar’s Laboratory Life [1979] which is the application of the above conceptual tool to the works of scientists.

137. G.Vision (op.cit. pp.218-19) has collected the main features of Wittgensteinian notion of language-games out of his scattered remarks in his various works. These features can be summarized as follows:

1) Language-games are sufficiently single-tracked to allow the point or purpose of an expression to emerge with clarity and ease.

2) They make manifest and concrete what counts as a justification of a use of an expression and in what context the evaluation is permissible.

3) We learn to participate in a language-game through training, imitation, example, i.e. by becoming full-fledged members of the language-user community.

4) Explanations are concrete, given by instances, examples, ostentations and so on.

5) Language-games are complete or autonomous. They do not allow a method of evaluation (or justification) or comparison (say, for effectiveness) with other language-games. Any criteria for evaluation must emerge from within the language-game itself.

6) There are normal conditions which make a language-game possible (boundary conditions). These are usually general features of nature. They may consist, for example, of the general rigidity of rods, or of our having certain interests in common.

138. “We can look upon it [the scientific discourse] as one of the many language-games that makes up this form of life. I propose in the light of these observations [i.e. the findings of certain sociologists of science whom Harré endorses] that we should reinterpret the activities of traditional philosophy of science... if we read the realist manifesto ‘scientific statements should be taken as true or false by virtue of the way the world is” as moral principle it would run something like this: “As scientists, that is members of a certain
community, we should apportion our willingness or reluctance to accept a claim as worthy to include in the corpus of scientific knowledge to the extent that we sincerely think it somehow reflects the way the world is". op.cit. [1986], pp.88-9.

139. The following quotations are typical examples of the views advocated by Harré, and endorsed, in varying degrees, by other entity-realists.

"... Science has a special status, not because it is a sure way of producing truths and avoiding falsehood, but because it is a communal practice of a community with a remarkable and rigid morality — a morality at the heart of which is a commitment that the products of this community shall be trustworthy." [1986, p.6]

"... What does scientific community produce? The naive answer is 'Truth '. But at least since the days of David Hume we know that answer will not do, unless the notion of 'truth ' is radically downgraded. Scientific method cannot produce 'Truth '. But turn an innocent eye on the doings of the science tribe and the answer is obvious. The community exists to produce writings. ... But what fuels this engine? The "symbolic capital" of the science tribe is 'reputation'. The 'logic ' of reputation is similar to that of the monetary capital that powers the engine of material production. If the product of a subgroup of the community is acceptable to the community at large, then the manufacturer's reputation is enhanced." ibid. pp. 9,10.

140. Harré writes: "The first impulse to the philosophy of science by the realization that science was the work of a community of persons, rather than the clean-cut product of the work of an abstract and perfectible 'logic engine ', was a shift towards relativism. What was to count as an established fact was seen to be relative to the established meaning of the terms in which it was expressed, which was itself historically conditioned by the beliefs of the community". (op.cit. [1986], p.8. italic added.)

141. As mentioned above Harré criticizes the views of the advocates of the so-called strong programme in sociology of science (the official view of the Edinburgh School). op.cit. [1986] p.14. However, he thinks the shortcomings in the works of these sociologists of knowledge can be avoided and in fact have been avoided by the approach of the writers like Latour & Woolgar ([1979] and Knorr-Cetina 1981). op.cit. pp.10, 85-92, and passim.

142. See A.Goldmann [1986] pp. 116 ff, and [1988]. Goldmann has dubbed epistemic notion such as reliable, rational, acceptable, warranted, trustworthy, assessable, probable, plausible, supportable (or supported), reasonable, justifiable (or justified) as the members of the reliability class.

143. op.cit. [1986], p.12. This is rather similar to what Popper has called intellectual sincerity or intellectual honesty which is a demand on the part of the scientists to represent as scientific ideas or results only those views/data which they sincerely hold to be true. See Popper [1963].

144. Harré [1990], p.307, italic added.

145. Even politicians cannot, in the long run, get away with falsity and lie.

146. R.Feynman [1985], p.342.

147. R.Harré [1982], p.viii.


149. Ellis [1991], p.3.

150. ibid p.119. Ellis contrasts his own value-based theory with other pragmatic theories which are rule-based. In these theories it is assumed that knowledge grows by a process which involves following rules. He claims that his theory while avoids the difficulties facing other theories of truth, has an important spin-off benefit namely, solving the age-old problem of induction (ibid. chs. 7&8). For a criticism of Ellis's claim of solving the problem of induction see J.Vollrath [1991], pp.257-62.

152. cf. Vision, op.cit. See also R.Trigg [1980].

153. ibid. p.277.


155. ibid. p.12.

156. ibid. p.12.

157. For a realist criticism of Peircean realism see R.Trigg [1980]

158. Ellis writes: "The concept of truth which derives from the naturalistic theory is objective in the intersubjective sense in which any naturalistic theory may be objective. That is, the theory makes truth and falsity relative to human nature. For, presumably, human beings have a certain epistemic perspective- an epistemic point of view, characterized by that specific system of human epistemic values used to construct our belief systems." op.cit. p.12.

159. See Popper's criticism of the "normal science", in Lakatos and Musgrave [1970/1977].

160. Quoted from J.Bell [1990], p.31. in A.Miller [1990].
Chapter Six
Quantum Physics: A Case For Anti-Realism?

Anti-realism, in its different guises, as we have seen so far, appears to be a less desirable position in comparison to the minimal realist stand, advocated in this thesis. It seems that, in classical physics as well as many other fields of empirical science, (for example, chemistry and biology), anti-realism does not fare as well as realism\(^1\). The reason, generally speaking, is that it furnishes scientists with a too conservative methodology to allow for the advancement of scientific knowledge\(^2\). However, it appears that anti-realists have taken heart from the discussions stemming from one of the most prestigious quarters of modern science, i.e. the realm of quantum physics.

Anti-realists have been quick to exploit the seemingly anti-realistic connotations of theories which deal with atomic and sub-atomic entities. Thus, for example, van Fraassen has argued that "the experimental violation of the Bell inequality is evidence against scientific realism. That is, if scientific realism does not work at the microlevel, then it cannot be generally valid."\(^3\)

To see whether quantum physics actually provides anti-realists with a compelling argument against a realistic interpretation of science, we should look at the issues involved in some detail\(^4\). The argument in what follows consists of three parts;

1. The orthodox version of quantum theory, developed out of the so-called Copenhagen interpretation, is neither the ultimate stage in interpreting the quantum realm, nor even a desirable one. In fact this version suffers from a number of fundamental defects which all stem from the inbuilt instrumentalistic ingredients of the theory.

2. Contrary to the conventional wisdom among anti-realists, the orthodox version of quantum mechanics, far from lending support to anti-realists conviction, provides a
rather neat *reductio ad absurdum* against anti-realism and in favour of realism in the quantum realm. This *reductio* argument can be summarised in this way: If anti-realism is a valid thesis then scientists should only develop and accept anti-realistic theories. However, orthodox quantum mechanics, as a paradigm case of anti-realistic theories suffers from fundamental defects which are all due to its basic instrumentalistic assumptions. In order to remove these defects, one has to develop a realistic quantum theory. Therefore anti-realism is not valid.

3. Micro-realistic interpretations of the formalism of quantum mechanics, which are at least as good as the standard account with respect to the empirical evidence, have already been developed. Moreover, the fact that currently many physicists and philosophers of science are trying (with varying degree of success) to develop more adequate micro-realistic schemes, is yet further evidence against the claim that the quantum realm is not amenable to realist interpretation. There is of course no absolute guarantee for the success of these endeavours. However, to reject them in an *a priori* fashion, as anti-realists do, would only amount to a dogmatic attitude towards science.

To set the stage I will start by looking at the roots of quantum mechanics.

I. The Historical Background

I.A. An Act of Desperation

Historians of science customarily trace back the origin of quantum physics to research on the *black-body (cavity) radiation* carried out in the later part of the nineteenth century. Introduction of a qualitative law by G.Kirchhoff in 1860 made the search for a theoretical explanation for the black-body radiation one of the major research programmes
in the latter part of nineteenth century.

In 1896 Wilhelm Wien formulated an equation for the energy distribution (density) of black-body radiation at temperature $T$ and frequency $v$. He showed that the energy density of thermal radiation at any one frequency, $u(v, T)$, does not depend on the frequency and temperature separately but on the ratio of the frequency to the temperature, namely,

$$u(v, T) = a \frac{v^3}{\exp(b \frac{v}{T})}$$

where $a$ and $b$ are constants. Wien's formula was well corroborated for radiation of relatively high frequencies and low temperatures. However for low frequencies and high temperatures the formula was not confirmed by the available data.

Max Planck, who had a great desire to discover universal laws and absolute relations, was also attracted to Kirchhoff's law, on which he commented, "This so-called normal energy distribution represents something absolute, and since the search for absolutes has always appeared to me to be the highest form of research, I applied myself vigorously to its solution." Planck examined the electromagnetic radiation that is enclosed within a cavity, assuming that a black-body may be treated as if it were a collection of linear, harmonic electromagnetic oscillators, each with characteristic frequency $v$ of oscillation. He also assumed that the oscillators were damped. They could only absorb or emit radiation in the neighbourhood of their natural frequency $v$. Planck argued that the oscillators emit electromagnetic energy into the cavity, which is assumed to have ideally reflecting walls, and absorb electromagnetic energy from it. Thus, it should be possible to deduce the characteristics of cavity radiation from those of the oscillators.
with which it is in equilibrium. On May 1899, Planck presented his result at a meeting of the Prussian Academy of Science;

\[ u(v, T) = \frac{8\pi v^2}{c^3} U(v, T) \]

where \( U(v, T) \) is the equilibrium energy of each oscillator and the factor \( \frac{8\pi v^2}{c^3} \) gives the number of radiation modes per unit volume per unit frequency.\(^{11}\)

Planck's formula had enabled the physicists to link the cavity radiation energy \( u \) to the energy of one oscillator \( U \). The problem was now to find out the exact mathematical form of \( U \). In June 1900, Rayleigh, applying a well known, but controversial theorem of thermodynamics, namely the equipartition theorem, to the formula obtained by Planck, suggested a definite form for \( U \). The formula, which came to be known as the Rayleigh–Jeans radiation law, stated the energy density, (of the black-body radiation), at frequency \( v \) and temperature \( T \) is proportional to the absolute temperature of the cavity, \( T \):

\[ u(v, T) = \frac{8\pi v^2}{c^3} kT. \]

This new formula agreed with all experimental data in the region of extremely low frequencies, just where Wien’s law failed. However, it was clear that this formula cannot work for high frequencies. Contrary to experience, it would assign no maximum to \( u(v, T) \) which meant that as the frequency becomes large, the theoretical prediction goes to infinity.\(^{14}\) The grossly unrealistic behaviour of the prediction of classical theory at high frequencies was later on dubbed by Ehrenfest as the *ultraviolet catastrophe*.\(^{15}\)

To remedy this discrepancy, Planck, who had derived Wien’s formula from his own formula by relying on his rather particular approach to *entropy*,\(^{16}\) suggested that if
Wien’s formula were modified in a simple way it would prove to fit the data far better\(^{17}\).

Planck announced his formula to the Berlin Physical Society on 19\(^{th}\) October 1900;

\[
\frac{u(v,T)}{\exp(b\frac{v}{T})-1} = a \frac{v^3}{\exp(b\frac{v}{T})-1}.
\]

This formula agreed with the data in the low frequency and high temperature range and reduced to Wien’s formula in the high frequency and low temperature region.

Despite the empirical adequacy of his formula, Planck was not satisfied with what he would call a 'lucky guess'\(^{18}\). To promote the status of his formula to the rank of a statement with real physical significance, he once again turned to Boltzmann, imitating one of his postulates\(^{19}\), and in a slightly muddled way, and with a minor mathematical gaffe\(^{20}\), he eventually arrived at the final form of the equation known as ‘Planck’s energy distribution law’, i.e.

\[
\frac{u(v,T)}{c^3} = \frac{\frac{8\pi h v^3}{c^3}}{\exp(\frac{hv}{kT})-1}.
\]

This formula, which was announced at the meeting of the German Physical Society on 14\(^{th}\) December 1900, heralds the coming of age of the quantum theory\(^{21}\). The crucial point in the above formula is the assumption that the fictitious oscillators which comprise the black-body (cavity) absorb and emit energy only in discrete bundles of value \(e=\hbar v\), where \(\hbar\) is the same as constant \(b\) in Wien’s formula, a constant whose value Planck had already established as \(6.55\times10^{-27}\) erg seconds\(^{22}\). In this way the notion of energy quantisation was introduced into physics\(^{23}\).

Planck, as he himself has mentioned in his autobiography, was dissatisfied with his approach for resolving the black-body energy distribution problem. He had tried
repeatedly, albeit unsuccessfully, to accommodate the factor $h$ into the framework of classical mechanics$^{24}$. In a letter to his friend R.W.Wood, Planck described the introduction of the postulate of energy quanta as 'an act of desperation', done because 'a theoretical explanation had to be supplied at any cost$^{25}$. Such a desire for understanding the inner mechanisms of phenomena was quite typical of Planck. At the end of his life he pointed out that:

My vain attempts to somehow reconcile the elementary quantum with classical theory continued for many years, and cost me great effort. Many of my colleagues saw almost a tragedy in this, but I saw it differently because the profound clarification of my thoughts I derived from this work had great value for me$^{26}$.

I.B. The Dual Nature of Light

Einstein was the first physicist to take seriously the idea of discreteness of energy$^{27}$. He realized that the inconsistencies in Planck’s equation are of fundamental significance and cannot be handled by mere mathematical adjustments$^{28}$. Einstein’s reflections on Planck’s law soon convinced him that energy quantisation is a quite general result affecting all of mechanics. In Einstein’s view, classical electromagnetism, with its built-in notion of a continuous energy spectrum, could work only for phenomena which are time-averaged (e.g. reflection and diffraction) and not instantaneous values. In the latter area, e.g. cases like photo-electric effect, or photo-luminescence and photo-ionisation, one has to rely on the notion of discrete packages of energy, which Einstein had dubbed "light quanta" in his paper of 1905$^{29}$.

According to Einstein, such bundles of energy remain localized as they move away from a light source with velocity $c$. He assumed that the energy content $E$ of each light quantum (or photon, as it came to be called later), is related to its frequency $\nu$ by the equation $E=h\nu$. By invoking his light quantum postulate, Einstein produced a reasonable explanation for the photo-electric effect$^{29}$: individual light quanta are completely absorbed
by individual electrons in a photocathode. Having absorbed the kinetic energy of photons, electrons are ejected from the atoms of the metal and produce electric current. Einstein also predicted that the maximum kinetic energy of the ejected electrons is a linear function of the frequency of light, i.e. \( K_{\text{max}} = hv - w_p \), where \( w_p \), a characteristic energy of the metal called the work function, is the minimum energy needed by an electron to pass through the metal surface and escape the attractive forces that bind the electron to metal.

Einstein’s light quantum hypothesis was widely rejected, even by physicists like Planck who were familiar with the problems of black-body radiation\(^1\). The reason for this unsympathetic reaction was that Einstein’s equation had a paradoxical nature: it was relating a discrete quantity (\( E \) energy of a photon) to a continuous quantity (\( v \) frequency of the photon\(^2\)). Despite a number of experimental verifications, including Millikan’s famous experiments\(^3\), the recognition of his great conjecture, came very late, almost two decades later in 1923\(^4\). In the meantime, main stream physics had produced its own interpretation of the photo-electric phenomenon: Philipp Lenard explained this effect in terms of the wave model as a resonance phenomenon\(^5\) and when this model could no longer account for the experimental results, many physicists, including Bohr, toyed with the idea of sacrificing the conservation of energy and momentum, in order not to accept the light quanta hypothesis\(^6\).

Einstein, himself, in 1908-10 tried strenuously to resolve the problem of the dual nature of light by developing a new electrodynamics. However, all his attempts failed to produce the desired results\(^7\). The sad memory of these failed efforts remained with Einstein until later years of his life. In his *Autobiographical Notes* he writes\(^8\):

> All my attempts, however, to adopt the theoretical foundation of physics to this [new type of] knowledge failed completely. It was as if the ground had been pulled under one, with no firm
I. That an atomic system can, and only can, exist permanently in a certain series of states corresponding to a discontinuous series of values for its energy, and that consequently any change of the energy of the system, including emission and absorption of electromagnetic radiation, must take place by a complete transition between two such states. These states will be denoted as the 'stationary states' of the system.\textsuperscript{43}

II. That the radiation absorbed or emitted during a transition between two stationary states is 'unifrequentic' and possesses a frequency $\nu$, given by the relation $E' - E'' = h\nu$, where $h$ is Planck's constant and where $E'$ and $E''$ are the values of the energy in the two states under consideration.\textsuperscript{44}

Bohr's theory, despite its \textit{ad-hoc} appearance\textsuperscript{50} and despite the fact that it left many important questions unanswered\textsuperscript{51}, proved to be highly successful. It managed not only to
produce Balmer’s formula for the line spectrum of hydrogen and Rydberg’s formula for
the spectrum of heavier elements52, but also made several novel predictions. For example,
it predicted that the Pickering series of lines belonged not to hydrogen, but to helium53.

The unexpected success of the theory however, gave rise to two important
questions among others. First, can Bohr’s proposal be generalized? In other words are
there rules which can be applied for the quantization of any mechanical system and not
solely to the hydrogen atom and the oscillators in a cavity? Secondly, can a general theory
be formulated which describes physical processes in discontinuous steps and yet includes
classical mechanics as a limiting case?

Arnold Sommerfeld took the next step in developing quantum theory. In 1915 he
introduced a general rule of quantization which, in spite of its limitations, could account
for a number of cases, including the fine structure of the line spectrum of hydrogen54, the
normal Zeeman effect55 and the Stark effect56 and thus partially solved the first problem
mentioned above.

I.D. The New Quantum Theory

Despite remarkable success in accounting for a large number of unaccounted
phenomena, the old quantum theory was suffering from a number of severe defects57, chief
among them were:

1. Lack of coherence: as noticed above, in order to solve the problems, it had to
make piecemeal application of classical mechanics and electrodynamics suitably modified
by quantum conditions58.

2. The theory treated only periodic systems. Non-periodic phenomena could not
be analyzed by it59.

3. The theory did not provide a reliable method to calculate the rate at which
transitions between stationary states, (different energy levels), take place.

4. After 1922 the theory faced serious problems, with regards to determining the energy states of the helium atom, and the anomalous Zeeman effect. The theory could not be used for multi-electron atoms.

In the summer of 1925 Heisenberg discovered the basis of a seemingly more coherent and certainly more comprehensive quantum theory, in which the position coordinates and momenta of bound electrons are treated mathematically as matrices. His paper had a clear anti-realist tone. This, as we shall see shortly, was a symptom of a strong positivistic attitude which was to be built into quantum mechanics and soon came to be known as the orthodox version. Within a few months Heisenberg’s new approach was elaborated by Born, Jordan, and Heisenberg himself, into what has become known as matrix mechanics. Their method was based on a refinement and deeper interpretation of the correspondence principle joined to the use of matrices for the representation of kinematic variables. The basic tenet of this method was calculating the non-commuting quantities $p$ and $q$ which were related via the following equation:

$$\sum_n (q_m a_n - q_n a_m) = -i\hbar \delta_{mn}$$

where $\delta_{mn}$ (the Kronecker delta symbol), is equal to 1 if $m=n$, but otherwise is equal to zero.

In 1926 Erwin Schrödinger introduced his version of atomic physics which was dubbed wave mechanics. The theory take its lead from a suggestion by Louis de Broglie in 1923 to the effect that atomic particles might have a wave-like aspect to their behaviour. De Broglie had linked the momentum of the particles to the wave length of those ‘matter waves’ by $p=h\lambda$.

Based on this assumption, Schrödinger’s wave mechanics specifies the laws of
wave motion which the particles of any microscopic system obey. This is done by specifying, for each system, the equation which controls the behaviour of the wave function, and also by specifying the connection between the behaviour of the wave function and the behaviour of the particles. For a particle of mass \( m \) that moves in a potential field \( V \) the Schrödinger’s wave equation is

\[
-\frac{\hbar^2}{2m} \nabla^2 \psi + V \psi = i\hbar \frac{\partial \psi}{\partial t}.
\]

Generally, \( V \) is a function of both space and time, \( V(r,t) \). In the above equation \( \nabla^2 \) is the Laplacian operator and \( \psi \) is a wave function subject to usual classical type boundary conditions.

Soon after introducing his wave mechanics, Schrödinger showed that his own formalism and Heisenberg’s matrix mechanics are mathematically equivalent despite the obvious disparities in their basic assumptions, mathematical apparatus, and general tenor.

While Heisenberg’s representation was regarded as a purely mathematical tool, Schrödinger’s formalism was taken to be a generalization that includes Newtonian’s theory as a special case (in macroscopic limit), much as Einstein’s theory of relativity was a generalization that included Newtonian’s theory in the low velocity limit. In view of such an expectation, the question of how to interpret the wave function \( \psi \) was raised as the first conceptual problem for wave mechanics. The situation is well described in this quatrain by a young physicist:

Erwin with his psi can do
Calculations quite a few.
But one thing has not been seen
Just what does psi really mean?

Schrödinger first considered the wave function as a physical reality, i.e., the electron is actually a wave. But this soon led to a difficulty. A wave may be partially
reflected and partially transmitted at a boundary, but an electron cannot be split into two parts for transmission and reflection. The difficulty was removed by Max Born who proposed a statistical interpretation of the \( \psi \)-function. Born argued that since what we observe ought to be real and \( \psi \) is a complex function, therefore it is \( |\psi|^2 = \psi^* \psi \), where \( * \) denotes a complex conjugate, that has physical significance. Consequently, Born put forward the following postulate: The wave density \( |\psi(\mathbf{r},t)|^2 \) does not represent an actual charge density of the electron, it rather represents the *probability density* \( P(\mathbf{r},t) \) for a particle to be located at point \( \mathbf{r} \) at time \( t \). Thus \( |\psi(\mathbf{r},t)|^2\,d\tau \) is the probability it will be in the infinitesimal volume \( d\tau \) at time \( t \).

The introduction of the statistical interpretation meant a severe blow to the classical views concerning causality and determinism. In his paper of 25 June 1925, Born noted that: "One obtains the answer to the question, not 'what is the state after the collision' but 'how probable is a given effect of the collision' ... Here the problem of determinism arises. From the point of view of our quantum mechanics there exists no quantity which in an individual case causally determine the effect of a collision ... I myself tend to give up determinism in the atomic world."  

**I.E. Uncertainty and Complementarity Principles**

In 1927 the final stages were set for a version of quantum mechanics which is known as the Copenhagen interpretation. This was largely due to the introduction of two principles that express in qualitative terms the physical content of quantum mechanics. The first principle, the principle of uncertainty, developed by Heisenberg in February that year, stated that, it is impossible to specify precisely and simultaneously the values of both members of particular pairs of physical variables that describe the behaviour of an atomic system. The members of these pairs are canonically conjugate to each other in the
Hamiltonian sense: a rectangular co-ordinate $x$ of a particle and the corresponding component of momentum $p_x$, the energy $E$ of a particle and the time $t$ at which it is measured, etc. Put more quantitatively, the uncertainty principle states that the order of magnitude of the product of the uncertainties in the knowledge of the two variables, must be at least Planck's constant $\hbar$ divided by $2\pi$;

$$\Delta x \Delta p_x \geq \hbar/2\pi = \hbar$$

$$\Delta E \Delta t \geq \hbar/2\pi = \hbar$$

The uncertainty principle, as Heisenberg first formulated it, was therefore meant to be an epistemic principle: it would lay down the limits to what we can know. On this view quantum mechanics becomes an indeterministic theory simply because the data required for deterministic predictions in the sense of classical mechanics are unobtainable. As noticed above, during this period Heisenberg was sympathetic to a positivistic theory of science, and hence he believed that the question whether quantum mechanical objects possess exact simultaneous position and momentum is meaningless: "When one wants to be clear about what is to be understood by the words 'position of the object', for example of the electron (relative to a given frame of reference), then one must specify definite experiments with whose help one plans to measure the 'position of electron'; *otherwise this word has no meaning*."^^

The uncertainty principle could be given an ontic interpretation (or a semantic variant of the ontic interpretation): Objects possess (or can be said meaningfully to possess) only observable properties; since the exact simultaneous position and momentum of a quantum mechanical object is unobservable, no such objects possess (or can be meaningfully to be said to possess) such a property.^^

Another step towards the fully-fledged positivist interpretation of quantum theory
was taken by Bohr. The experimental confirmation of the photon hypothesis, and the existence of two different mathematical schemes for dealing with atomic and sub-atomic phenomena, had caused Bohr to take the issue of wave-particle duality more seriously and to try hard to find a way to resolve the apparent paradox which had first been pointed out by Einstein. Heisenberg’s work on the principle of uncertainty prompted Bohr to give further attention to the problems of interpreting the formalism of quantum mechanics and the conditions for proper use of descriptive concepts. The result of this intellectual endeavour was a new principle, which Bohr called *complementarity*. This principle which occupied a significant place in Bohr’s future philosophical deliberations on quantum theory was officially introduced in his lecture at the Como conference on 16 September 1927:

... On the one hand, the definition of the state of a physical system, as ordinarily understood, claims the elimination of all external disturbances. But in that case according to the quantum postulate, any observation will be impossible, and, above all, the concepts of space, and time, lose their immediate sense. On the other hand, if in order to make observation possible we permit certain interactions with suitable agencies of measurement, not belonging to the system, an unambiguous definition of the state of the system is naturally no longer possible and there can be no question of *causality* in the ordinary sense of the word. The very nature of the quantum theory thus forces us to regard the *space-time coordination* [i.e., particle behaviour] and the claim of *causality* [i.e., wave behaviour], the union of which characterizes the classical theories, as *complementary* but exclusive features of the description, symbolizing the idealization of observation and definition respectively. ...

The problem of the nature of the constituent of matter presents us with an analogous situation. ... Just as in the case of light, we have consequently in the question of the nature of matter, so far as we adhere to classical concepts, to face an inevitable dilemma which has to be regarded as the very expression of experiment. In fact, here again we are not dealing with contradictory but with complementary pictures of phenomena, which only together offer a natural generalization of the classical mode of description.

In a sense, the principle of complementarity together with the principle of uncertainty sanctioned a totally phenomenological / instrumentalistic approach to quantum world. According to this approach, quantum theory is basically a theory about the outcome of measurement. Quantum mechanical phenomena should be defined and described in terms of experimental arrangements, and the description of the experimental arrangement is given in the language (i.e. using the concepts) of classical physics.
From 1927 onward, the formalism of the new theory was developed very rapidly and its predictive power went from strength to strength. In the same year Dirac gave a quantum mechanical description of the electromagnetic field and Pauli introduced spin into quantum mechanics. Dirac gave a relativistic theory of quantum mechanics in 1928. In 1932 von Neumann formulated quantum mechanics as an operator calculus in Hilbert space, and soon other formalisms such as the algebraic approach and the quantum logical approach, were suggested by a number of physicists. These developments laid the foundation of a powerful predictive tool known as the Orthodox Quantum Theory (OQT). Applications of this theory, which had the Copenhagen interpretation as its backbone, to many branches of physics, met with remarkable success. These developments however, do not concern us here. We would rather concentrate on the bearing of the Copenhagen interpretation and OQT on the realism/anti-realism dispute.

II. The Shortcomings of Orthodox Quantum Theory and the Copenhagen Interpretation

The many successes of the orthodox quantum theory (OQT) have had profound effects on the course of scientific thought in the twentieth century: due to the strong inbuilt instrumentalism/positivism of the theory, a view has taken shape among the majority of working physicists that successful physical theories should be built along the same instrumentalistic line of thinking as advocated by the members of the Copenhagen school. As recently as 1986, a physicist commenting on the impact of the Copenhagen approach, pointed out that, "Most modern undergraduate courses in modern physics seem to be aimed at conditioning students to think in this rather positivistic way."

Perhaps one of the reasons for encouraging Bohr to take an instrumentalistic
approach has been his initial success in his introducing ad-hoc principles in order to resolve the difficulties facing the investigations at the atomic level\textsuperscript{89}. Another reason for the slide of members of the Copenhagen school towards anti-realism, seems to be their failure to fully appreciate the importance of upholding the postulate of independence and self-subsistence of reality\textsuperscript{90}. Apparently, the outstanding success of the theory's formalism in predicting/accounting for experimental results, combined with the (seemingly) insurmountable difficulties of the wave-particle paradox, led Bohr and Heisenberg to the conclusion that physics should concern only with \textit{phenomena}, and \textit{phenomena}, should only be understood as the outcome of measurement\textsuperscript{91}.

Heisenberg, as he himself has made it clear, became convinced that he should only save the phenomena, "forgoing the space-time description and objectification"\textsuperscript{92}. This was the very reason why he developed a version of quantum mechanics which was based only on measurable quantities. Bohr's principle of complementarity too, was an effort to fudge the fundamental concern about the nature of reality by means of a philosophical doctrine\textsuperscript{93}. This in turn, inevitably meant that, for the members of the Copenhagen school the only acceptable reality at the quantum level was the \textit{empirical reality}, that is to say, a reality in the creation of which the experimenter himself plays a crucial rôle\textsuperscript{94}, and that quantum mechanics did not ascribe underlying \textit{causes} for the correlations between phenomena. This point was emphasised by Heisenberg, in 1927, in the following way; "One may be led to the presumption that behind the perceived statistical world there still lies a \textit{real} hidden world in which causality holds. But such speculations seems to us, to say it explicitly, fruitless and senseless. Physics ought to describe only the correlation of observations."\textsuperscript{95}

On this view the quantum world is at best phenomenally objective but without objects\textsuperscript{96}.

This instrumentalistic / positivistic approach gradually hardened among the
followers of Bohr and Heisenberg's views. Von Neumann's grand work on the mathematical foundations of quantum mechanics was of great significance in this respect. Using a not well-justified postulate, von Neumann, commenting on the possibility of hidden variables, claimed that, "Not only is the measurement [of these variables] impossible but so is any reasonable theoretical definition, i.e., any definition which, although incapable of experimental proof, would also be incapable of experimental refutation." Soon, the advocate of the orthodox interpretation of quantum theory claimed that this interpretation is the only possible interpretation of the formalism of the theory. Pascual Jordan for example, writes, "There is only one interpretation which is capable of conceptually ordering the ... totality of experimental results in the field of atomic physics."

While anti-realists have naturally rejoiced over the impact of the theory, many realist writers have pointed out that OQT, notwithstanding its predictive success and the consistency of its formalism, is not a satisfactory theory, in that it suffers from a number of acute conceptual deficiencies and as such cannot be regarded as a constructive paradigm for physics.

In fact, as it is shown below, all these conceptual defects are due to the instrumentalistic outlook which has been imported into OQT via the Copenhagen interpretation. This fact in itself produces a strong reductio ad absurdum against the anti-realism in physics: the orthodox interpretation does not fulfil the fundamental aim of science, that is, it does not provide us with the knowledge and understanding of the realm of quantum entities. The theory does not tell us what sort of entities are electrons or quarks or photons or their ilk. Moreover, it is unable to resolve the wave-particle dilemma in a satisfactory way. It only explains away this crucial problem by recourse to the
principle of complementarity. Anti-realism leads to instrumentalism and instrumentalism forbids understanding and knowledge.

The epistemological anti-realism inherent in the theory has also led to an ontological anti-realism; being primarily a theory about the outcome of measurement, OQM lacks a characteristic physical ontology. The lack of a specific ontology forces the Copenhagen interpretation to embrace the view that there is no deep reality. Hence Bohr's remark that: "There is no quantum world. There is only an abstract quantum physical description. It is wrong to think that the task of physicist is to find out how nature is. Physics concerns what we can say about nature." Alternatively, if the theory does not want to deny the existence of quantum entities, it must refrain from ascribing any dynamical attributes to them in the absence of measurement. Thus Robert Oppenheimer has claimed that:

If we ask, for instance, whether the position of the electron remains the same, we must say 'no'; if we ask whether the electron is at rest, we must say 'no'; if we ask whether it is in motion, we must say 'no'.

This view has currently found a fresh and enthusiastic advocate in van Fraassen. Van Fraassen's view, among other things, leads to an explicit agnosticism towards quantum attributes.

The above unpleasant and contrary to common sense consequences are not the only undesired outcomes of the instrumentalistic outlook of the theory. This unhealthy attitude has produced a number of other significant defects in the theory:

1) OQT is an ad-hoc theory in that it is a mixture of classical and quantum mechanics. The purely quantum mechanical part deals with the behaviour of the quantum objects, whereas measuring instruments are subject to the laws of classical mechanics. However, in the absence of the measurement, the theory cannot say anything about the
future course of behaviour of the quantum objects. As a result it can only issue in conditional or counterfactual predictions about what would be the case if a measurement were to be performed.

2) A further snag is that the theory cannot draw a clear cut-line between the realms of validity of each of its components. It is not clear where the realm of applicability of measuring device (which is treated by means of classical mechanics) ends, and the realm of applicability of purely quantum mechanics starts. This aspect which has rendered the theory imprecise and ambiguous, as we shall see in the next section, has given rise to a purely subjective approach due to von Neumann and E.Wigner\textsuperscript{110}.

3) OQT cannot be generalized to include gravity (general relativity). This is because in order to quantize general relativity, space-time itself would need to be quantum states. This in turn requires to postulate preparation and measurement devices external to space-time, which is an impossible task.

That there is something unsatisfactory about OQT was realized by Einstein from the early stages of development of the theory. Einstein initially was of the view that the scheme is inconsistent. However, later on he concluded that the theory is consistent but incomplete\textsuperscript{111}. Einstein’s discontent with the theory led him to embark on a long life project of refuting OQM. His persistent efforts, as Bohr has explicitly acknowledged, contributed a great deal to pinpoint certain conceptual problems which present themselves, once one is in the quantum realm.\textsuperscript{112}

III. Einstein vs. Bohr

Many of the conceptual (philosophical) aspects of the orthodox quantum theory have been discussed between Bohr and Einstein. The standard view regarding the stance
of each side in this debate is that while Einstein has been an ardent realist, the members of Copenhagen school, have all been staunch anti-realists. For instance, a distinguished physicist like Franco Selleri as lately as 1990 has vehemently attacked the anti-realistic connotations of the views advocated by Bohr and his collaborators. He argues that Bohr’s response to the three central questions about physics, namely, "1) Are the basic entities of atomic physics, such as electrons, protons, photons, and the atoms themselves actually existing independently of the human beings and the observations they are able to perform? 2) Is it possible to comprehend the structure and evolution of atomic objects and processes in terms of mental images formed in correspondence with reality? and 3) Should one formulate the physical laws in such a way that at least one cause can be given for any observed effect?" are all negative, and he concludes that Bohr is therefore undeniably an anti-realist.

This rather conventional view has recently been challenged by a number of writers. On the one hand, some, like A.Fine, have tried to cast doubt on Einstein’s realist credentials by claiming that far from being a realist, Einstein should be regarded as a constructive empiricist like van Fraassen. On the other hand, others have tried to argue that Bohr and his friends, after all have been realists.

But how can one adjudicate between these rival views? We mentioned earlier the positivist tone of Heisenberg’s original paper which gave birth to modern quantum mechanics. It should be emphasised that this choice of approach came about only after many unsuccessful attempt to preserve a realistic approach. Ironically, Einstein himself, though inadvertently, had played a rôle in this unhappy turn.

As we have already pointed out, on occasions where finding a realist interpretation for a successful theory proves to be difficult, it is quite legitimate, and in fact
methodologically sound, for scientists to opt temporarily for an instrumentalistic approach. However, it seems that the members of the Copenhagen school instead of regarding their instrumentalistic position as a temporary stage, and doing their best to develop a thoroughly micro-realistic account of the quantum realm, gradually became convinced that their interpretation is a complete one and not in need of modification. In this respect, the responsibility for taking such a stand rests mostly on Bohr's shoulders. After all, he was the philosophical mentor of the group. However, from a methodological point of view, such an inflexibility with which Bohr's prohibited even asking certain question about quantum theory, is harmful. According to realists: "Setting dogmatic limitations on scientific theorizing, on the basis of obscure philosophical preconceptions, is a dangerous prejudice ... to the nature of scientific activity."

Perhaps the answer to the question of why Bohr and his colleagues, despite of their initial enthusiasm for preserving realism at the micro-level, radically changed tack, whereas Einstein, who had for a while entertained a positivist attitude, fought in the realist corner to the end, lies in the attitude of the two main players of this drama towards the basic aim of science. Einstein once had said:

I want to know how God created this world. I am not interested in this-or-that phenomenon, in the spectrum of this-or-that element. I want to know His thoughts, the rest are details.

For Bohr, on the other hand, to describe an independently existing physical reality was not the main aim, but to bring greater order to our sensory and experimental observations, to coordinate our experience, and to reduce it to order:

In our description of nature the purpose is not to disclose the real essence of the phenomena but only to track down, so far as it is possible, relations between the manifold aspects of our experience.

That the basic point of difference is over the issue of the aim, is something that Einstein himself quite explicitly pointed out: "What does not satisfy me in that theory (i.e.
quantum theory), from the standpoint of principle, is its attitude towards that which appears to me to be the programmatic aim of all physics: the complete description of any (individual) real situation (as it supposedly exists irrespective of any act of observation or substantiation\(^{123}\))". The difference over aim, quite naturally, reflected the difference in the philosophical attitudes of the two scientists\(^{124}\). While Einstein insisted upon a thoroughly objective approach towards micro-entities, Bohr opted for a typical anti-realistic approach, namely, transposing ontological problems to epistemological one; he was no longer interested in the question whether knowledge gained by a physical theory, in particular by quantum mechanics, pertains directly to a mind-independent reality; he was convinced that such a knowledge pertains only to the appearances of such a reality in human experience, taking in physics the form of experiment and measurement.

Einstein's notion of realism was, initially, perhaps a bit too strong and less flexible. He not only believed in the independence of reality (of all things mental), but also in its continuity, and locality.

The concepts of physics refer to a real external world, i.e. ideas are posited of things that claim a 'real existence' independent of the perceiving subject (bodies, fields, etc.), and these ideas are, on the other hand, brought into as secure a relationship as possible with sense impressions. Moreover, it is characteristic of these physical things that they are conceived as being arranged in a space-time continuum. Further, it appears to be essential for this arrangement of things introduced in physics that, as a specific time, these things claim an existence independent of one another, in so far as these things 'lie in different parts of space'.\(^{125}\)

Moreover, he was adamant that God has made the physical reality in a deterministic fashion and that indeterminism is not possible. In a letter to Born on 7 September 1944, Einstein wrote: "We have become Antipodean in our scientific expectations. You believe in the God who plays dice, and I believe in complete law and order in a world which objectively exists, and which I, in a widely speculative way, am trying to capture. I firmly believe, but I hope that someone will discover a more realistic way, or rather more tangible basis than it has been my lot to find. Even the great initial
success of quantum theory does not make me believe in the fundamental dice-game, ...\textsuperscript{126}.

However, later on, Einstein realised that the main issue is not determinism but realism. In his later correspondence with Born, Einstein was not emphasising on determinism, but rightly insisting that realism as a framework for sound scientific research should be preserved:

\ldots the ‘real’ in physics is to be taken as a type of programme, to which we are, however, not forced to cling \textit{a priori}. No one is likely to be inclined to attempt to give up this programme within the realm of the ‘macroscopic’ (location of the mark on the paper strip ‘real’). But the ‘macroscopic’ and the ‘microscopic’ are so inter-related that it appears impracticable to give up this programme in the ‘microscopic’ alone.\textsuperscript{127}

This sentiment was not shared by Bohr. In fact Bohr had shown, from the outset, with his new model of the atom, that he was ready to resort to ad-hoc measures in order to solve conceptual difficulties. A case in point, as we have already seen, was his readiness to jettison the time-honoured principles of conservation of energy and momentum for individual processes, and to regard them as only statistically valid for ensembles of particles.\textsuperscript{128}

Another area of difference between Einstein and Bohr was the issue of \textit{common sense} and its rôle in the advancement of science. While for Einstein, notions like ‘phemonenon’, ‘causality’, ‘being’, and ‘to know’ were to be understood in their usual common sensical ways, and while in his view scientific concepts were nothing more than a ”refinement of everyday thinking”\textsuperscript{129}, Bohr was of the view that at the quantum level all these notions should be radically redefined in order to become compatible with the sort of conditions which prevail there\textsuperscript{130}. According to Bohr, in the quantum domain, even ”words like ‘to be’ and ‘to know’ lose their unambiguous meaning”\textsuperscript{131}. Bohr would regard the ambiguities resulting from uncritical use of language as partly responsible for his differences with Einstein: "Surely, in a situation like this, where it has been difficult
to reach understanding not only between philosophers and physicists but even between physicists of different schools, the difficulties have their roots not seldom in the preference for a certain use of language suggesting itself from the different lines of approach\(^1\). In fact an appeal to this very difference over the meaning attached to the concept of physical reality was Bohr’s main rebuttal against Einstein’s ingenious argument of 1935 (briefly discussed below).

It seems Bohr’s preoccupation with language and the way it helps the process of concept formation, has played a decisive rôle in encouraging him to give priority to epistemological problems over the ontological ones\(^2\). This move however, as we have already discussed in previous chapters, opens the floodgate to all sorts of non/anti realistic interpretations. It thus appears that Bohr’s philosophical inclination has encouraged him to move along the dangerous path of idealism and to play a risky game with reality\(^3\). The full impact of his move towards putting the epistemological horse before the ontological cart, can be seen in the following remark,

"There is no quantum world. There is only an abstract quantum physical description. It is wrong to think that the task of physicist is to find out how nature is. Physics concerns what we can say about nature."\(^4\)

This very attitude towards language and epistemological issues, as noted above, can be seen in Bohr’s reply to the famous paradox proposed by Einstein and his colleagues to challenge the completeness of quantum theory. It is to this paradox we now turn.

Between 1927 (the fifth Solvay conference) and 1934, Einstein produced a number of thought experiments which were mainly devised to criticise Bohr’s thesis of kinematic-dynamic complementarity in its epistemic form. However in 1935, Einstein turned his attention to the ontic interpretation of the thesis\(^5\). The result was a new and powerful argument, known in the literature as the EPR thought experiment\(^6\). As N.Rosen,
In view of the rôle of the probability concept in the quantum theory, Einstein regarded the theory as having a statistical character, i.e. as describing an ensemble of systems and not a single system. As such, he considered the theory to be incomplete in the sense of not providing a complete description of a single system. However, it was desirable to provide a convincing demonstration of the incompleteness, and this was the purpose of the EPR paper. The idea was to show that the description of the system by the formalism of the quantum mechanics failed to tell us everything about what the system was really like. In other words it was necessary to show that not all the elements of the physical reality associated with the system entered into its description by quantum mechanics.

The strategy of Einstein and his colleagues was to show that the acceptance of the claim that quantum theory is a complete theory would lead to a contradiction. The challenge was put in the form of dilemma: either the description of reality given by the wave function in quantum mechanics is not complete or those physical quantities described by non-commuting operators cannot have simultaneous reality.

The argument of the EPR paper hinged upon what Einstein considered to be a reasonable definition of physical reality: "If, without in any way disturbing a system, we can predict with certainty (i.e. with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity." There were also three main assumptions which formed the backbone of the arguments, namely, realism: there are some properties of the world that are independent of the human observers; completeness: every element of the physical reality must have a counter part in the physical theory, and separability: a measurement made with one instrument cannot influence the result of a measurement made with a second spatially separated instrument. A more restrictive form of this assumption, known as locality, forbids such influences only if it is simultaneous with the original measurement. In other words, locality forbids that the influence would have to propagate faster than light.

Having stated their premises, the authors then suggested a thought experiment. Two quantum systems, I and II, whose initial states are supposed to be known, are
allowed to interact from time $t = 0$ to $t = T$, after which it is supposed that there is no longer any interaction between the two systems. According to the calculus of quantum mechanics (i.e., Schrödinger’s equation) the state of the combined system $\psi_{i,ii}$ can be calculated at any subsequent time.

We can measure the position of the first particle, say $x_1$. Using this datum and the data about the initial state of the two particles we can easily calculate the position of the second system, $x_2$. In a similar way, we could have calculated the momentum of the second one, $p_2$, by measuring momentum of the first system, $p_1$. However, since our measurements on the first system do not affect the physical properties of the second system, it follows that the second system has had the properties $x_2$ and $p_2$ all along. But position and momentum are non-commutable properties (quantities), and as such cannot be represented by a single wave function. This obviously contradicts the claim of the quantum theory to completeness. Insistence on the completeness of the theory gives rise to the other horn of the dilemma, namely, denial of simultaneous reality for non-commuting quantities. Having shown the incompleteness of the quantum theory the authors warned about one possible manoeuvre for evading the above conclusion, "One could object to this conclusion on the grounds that our criterion of reality is not sufficiently restrictive." It is exactly such an objection that forms the essence of Bohr’s reply.

Bohr’s reply to EPR’s paradox is basically an argument from language; it rests on the claim that Einstein’s criterion of physical reality is ambiguous and in fact does not apply properly to the realm of quantum mechanics. Bohr has summed up his reply by reiterating his main objection:

Form our point of view we now see that the wording of the above mentioned criterion of physical reality proposed by Einstein, Podolsky, and Rosen contains an ambiguity as regards the
meaning of the expression "without in any way disturbing the system". Of course there is in a case like that just considered no question of a mechanical disturbance of the system under investigation during the last critical stages of the measuring procedure. But even at this stage there is essentially the question of an influence on the very conditions which define the possible types of predictions regarding the future behaviour of the system. Since these conditions constitute an inherent element of description of any phenomenon to which the term ‘physical reality’ can be properly attached, we see that the argumentation of the mentioned authors does not justify their conclusion that quantum mechanical description is essentially incomplete.

Although it is the received view among the majority of physicists that it was Bohr who came triumphant out of the controversy with Einstein, recent research by a number of philosophers of science have shown that this does not reflect the whole truth of the matter. Fine and Beller for example, have argued that Bohr’s reply actually missed the main points in the EPR paper, namely, the assumptions about reality and separability. Bohr’s main concern in his reply was to rebut the charge of inconsistency, whereas, as was pointed out, it was the completeness of the theory which was at stake. Moreover, the authors have shown that Bohr’s attempt to find a physical fault with the EPR argument was not successful and that is the reason why in his subsequent discussions of this paper (e.g., his contribution to Schilpp volume), Bohr concentrated more on the philosophical/epistemological aspects of his [1935] reply rather than its physical reasoning.

Experimental tests of EPR’s experiment (see section V below) have established that while (contra Bohr) it is reasonable to talk of an independent quantum reality, this reality (contra Einstein) is not local. This means that the two main contenders of the philosophical aspects of quantum theory, have both been partially right or partially wrong.

IV. Alternative Interpretations

The Copenhagen interpretation, despite all the conceptual difficulties referred to
above (Sec.II) was, as far as predictions and calculations were concerned, a successful scheme. This fact helped to produce an environment in which the majority of physicists who were mostly interested in practical problems used the scheme without questioning its conceptual validity. However, the endeavour to find alternative interpretations have continued by a number of philosophers and philosophically minded scientists ever since the Einstein — Bohr controversies. The snag however, has all along been that there has been hardly any consensus among these writers.

The situation is aptly described by N. Herbert, "Quantum theory resembles an elaborate tower whose middle stories are complete and occupied. Most of the workmen are crowded together on top, making plans and pouring forms for the next stories. Meanwhile the building’s foundation consists of the same temporary scaffolding that was rigged up to get the project started ... Physicists’ reality crisis consists of the fact that nobody can agree on what’s holding the building up. Different people looking at the same theory come up with profoundly different models of reality ...".

Although most of these interpretations have covert or overt anti-realistic leanings, however this fact should not bring comfort to anti-realists since on the one hand, as we shall see, these anti-realist schemes suffer from considerable conceptual deficiencies. On the other hand, the very fact that there are a number of different interpretations, realist and anti-realist alike, is indeed an argument against the conviction of those anti-realists who would regard the orthodox interpretation as complete and final. The following models are among the better known interpretations.

**IV.A. Consciousness-created Reality:** This interpretation was prompted by von Neumann who had concluded that if the predictions of quantum mechanics are correct, then the world cannot be made up of ordinary objects possessing unobservable or hidden attributes.
Being an ardent advocate of the OQT, von Neumann believed that there is a definite separation between the measurement device and the quantum objects, and that the wave function collapse occurs in some vague neighbourhood between the two. He decided to calculate the size of this neighbourhood.

However, to his surprise, it turned out that the collapse, as far as ordinary experiments are concerned, can virtually occur anywhere at all. As a result of this consequence, von Neumann started thinking of the human consciousness, as a part of the long chain of measurement. However, while von Neumann himself did no more than allude to the role of the conscious mind in bringing about the collapse of the wave packet, this possibility was taken seriously by a number of physicists, chief among them Eugene Wigner. Wigner and others, have dramatised the situation by proposing a paradox in the form of a thought experiment which draws on Schrödinger’s famous cat paradox.

The experiment involves a sealed and insulated box containing a radioactive source. The source has a 50-50 chance of triggering the Gieger counter during the course of the experiment, thereby activating a mechanism that causes a hammer to smash a flask of prussic acid, thereby killing the cat. An Observer has to open the box in order to collapse the wave function into one of the two possible states (cat = dead, cat = alive). A second observer (Wigner’s friend) is then needed to collapse the wave function of the larger system comprising the first observer, the cat, and the equipment. The problem here is that now the original observer, Wigner’s friend, and the equipment plus the cat, constitute a new system, which may itself require an ‘Acquaintance’ to collapse its wave function, and so on.

Wigner’s own solution is that due to interaction between living minds and inanimate nature, the state of the original system changes from an indefinite one into a
definite one as soon as *any* mind would become conscious of the outcome of a measurement upon the original system. For Wigner, the conscious mind is the basic reality, and things in the world are no more than useful constructions built out of one's past experiences, somehow coded into one's consciousness.

Wigner's proposal, has not been met with great enthusiasm among the majority of physicists\textsuperscript{150}. An exception is however, the American physicist, John Archibald Wheeler, who has taken Wigner's approach one step further and has declared that, "no elementary phenomenon is a real phenomenon until it is an observed phenomenon"\textsuperscript{151}. Einstein, once wrote, "I cannot believe that a mouse can change the world by simply looking at it". Bohr, as we have seen before\textsuperscript{152}, tried to distance himself from this sort of interpretation. However, his equivocal remarks, have no doubt contributed to the rise of this overtly subjective approach\textsuperscript{153}.

Apart from strong idealistic connotations of this approach (remember Berkeley's tree in the Quad\textsuperscript{154}), there are a number of internal conceptual difficulties which undermine the soundness of it. In the first place, Wheeler has restricted the act of creating reality by observation only to elementary particles, and has denied it in the case of medium size and large size objects. But such a restriction seems to be quite arbitrary, with no convincing rationale. In fact as other physicists with the same persuasion as Wheeler (e.g. D.Mermin\textsuperscript{155}) have claimed all entities — cats, oranges, rainbows, even moon and stars — are not real until somebody looks at them. This is of course, a very natural conclusion, which directly derives from Wheeler's basic assumption.

The other difficulty with this scheme is that its advocates do not agree on what counts as an observation. Some of the followers of this school, including Wheeler himself, are of the view that the essence of the measurement is *the making of a record*, and this
can be done even by a robot. Others believe that only a conscious observation counts as a measurement. Here again, due to the arbitrariness of the distinctions and lack of objective criteria, no progress has been made.

IV.B. The Many-World Interpretation: This approach due to H. Everett\(^{158}\), states that in any act of measurement, while one of the many possibilities latent in the wave function actualizes for the observer, the rest also are simultaneously actualized in the worlds parallel to, but inaccessible from, that of the observer. The important point in Everett’s scheme, which by the way makes it attractive to some physicists, is that, according to him no wave reduction takes place, and since there is no collapse of wave function, there is no measurement problem. Another important feature of this scheme is that its interpretation seems to arise naturally out of the mathematical formalism, whereas the other approaches require additional assumptions associated with the distinction between the quantum system and the measurement apparatus\(^{159}\).

In Everett’s interpretation any isolated system is described by a wave function that changes only as prescribed by Schrödinger’s equation. If this system is observed by an external observer then, in order to discuss what happens, it is necessary to incorporate the observer into the system, which then becomes a new isolated system. The new wave function, which now describes the previous system plus the observer, is again determined for all times by the Schrödinger equation\(^{160}\).

However, the scheme suffers from the following rather serious conceptual inconsistency\(^{161}\). As noted above, according to Everett, the reality is a wave function which always contain all possible outcomes and a conscious observer is capable of demanding a particular result, and thereby selecting a ‘branch of world’ in which he exists. But this poses a problem, because on the one hand the wave function — with all
its components — corresponds to the whole reality, and on the other hand there exist a
number of conscious observers over and above this whole reality which can cause the
process of ‘branching’.

There are other reasons for doubting the credibility of the many world model.
These arise principally because it is not clear from the theory just when the alleged
branching takes place. It is sometimes said that it happens whenever a ‘measurement
like’ interaction between a quantum system and a measuring apparatus occurs, but if this
is the case then the many-world model has clearly failed to solve the measurement
problem! Alternatively, branching may occur whenever any kind of interaction takes place
between two component parts of the universe. But this means, among other things, that
the electron and proton in a hydrogen atom are continually interacting and creating
infinities of universe! The formalism of many-world model does not provide clarifications
over these difficulties162.

IV.C. Hidden Variable interpretation: Among the realist physicists who took Bohr’s
views seriously, David Bohm is to be mentioned. Bohm’s attention was drawn to the
notion of indivisibility between the quantum domain and the large-scale domain according
to the Copenhagen interpretation. Time and again Bohr had emphasised that, "... [T]he
fundamental difference with respect to the analysis of phenomena in classical and in
quantum physics is that in the former the interaction between the objects and the
measuring instruments may be neglected or compensated for, while in the latter this
interaction forms an integral part of the phenomenon. The essential wholeness of a proper
quantum phenomenon finds indeed logical expression in the circumstances that any
attempt at its well-defined subdivision would require a change in the experimental
arrangement incompatible with the appearance of the phenomenon itself"163.
Bohm, although sympathetic with this account, was uneasy about its apparent contradiction. In his inaugural lecture at Birkbeck College he noted that, "In reality, they are only one indivisible system. Yet, our very language asserts that they are two. Hence, there is a contradiction between our common language and the facts of the case. It is this contradiction that is at the root of our inability to find a single conceptual model of the movement and behaviour of the observed system."164

In Bohm's view, Bohr and Heisenberg had resorted to a sort of conventionalism and arbitrariness as a way out of this contradiction. They had suggested "a purely imaginary 'cut', at some place where classical physics is still adequate. The precise place is not significant, as long as it is still in the classical domain. On the large-scale side of the 'cut' it is evidently adequate to go on using our ordinary classical concepts. On the other side, we apply the laws of quantum mechanics, whose sole experimental meaning is however now the prediction of probable results on the observable classical side of the 'cut'."165

Bohm, who was "dissatisfied with the self-contradictory attitude of accepting the independent existence of the cosmos while one was doing relativity and, at the same time, denying it while one was doing quantum theory"166, decided to produce an alternative micro-realistic interpretation of the formalism of quantum mechanics. His guiding thought was that, contrary to the claims of Copenhagen school, a wave function, far from presenting a complete description of reality, captures "only certain aspect of what happens in a statistical ensemble of similar measurements, each of which is in essence only a single element in a greater context of the overall process."167

A meeting with Einstein made Bohm interested in finding out whether a deterministic extension of quantum mechanics could be found168. To this end, Bohm
reformulated quantum mechanics in a language which is closer to that of classical physics. He wrote the complex wave function in the form, $\psi = R \exp \left( iS/h \right)$ and obtained two real equations, one which is essentially a classical equation of motion, the other a potential term called by Bohm "the quantum mechanical potential"\textsuperscript{169}.

This second wave would act as a pilot wave, spreading out at super-luminal velocity and coming towards where the quantum object is found, telling it how to move. The idea of a pilot wave which guides the quantum object was originally introduced by de Broglie in the Solvay conference of 1927 but rejected by the advocates of the Copenhagen school\textsuperscript{170}. It was deemed to be a real but in principle unobservable entity which serves the function of residing in the environment and reporting its finding back to the particle which is detectable. The particle then acts in accordance with the information provided by its associated pilot wave.

Bohm had called his version of quantum theory a hidden variable theory. However, he later on came to regret the choice of the term\textsuperscript{171}. Despite the fact that Bohm’s version did agree precisely with OQM in all its empirical predictions, physicists, by and large, did not look at it sympathetically. The reason, apart from the quasi-ideological dominance of the views of Copenhagen school, has been the fact the it has not accounted for at least one experiment which is not accounted for by OQM. The following quotation depicts the standard attitude of many present-day physicists towards this theory: "In the absence of experimental distinguishability between [Bohm’s version and OQM], the former becomes a substructure to OQM that is scientifically gratuitous. It would be based completely on philosophical grounds rather than empirical grounds."\textsuperscript{172}

Another problem with this scheme is that the pilot wave should travel faster than light to serve its purpose. But this is clearly contrary to the special theory of relativity.
Some more positivistically inclined physicists have also objected that the fact that the pilot wave is not even in principle detectable makes its existence spurious.

Bohm’s partial answer to these difficulties has been that the pilot wave is not a wave of matter, but just a wave of active information. Its effects depend only on its form, not upon its magnitude; consequently, unlike matter waves whose effects diminish with distance from the source, the pilot wave can have big effects at long distances (non-locality).

Perhaps Bohm’s greatest achievement, notwithstanding the cool response from the physics community, has been to put a successful challenge to the seemingly absolute injunction against this sort of model, imposed by von Neumann. In fact, it was exactly this interpretation, which was later on re-named by Bohm as the causal interpretation, that led John Bell to develop his famous theorem.

In subsequent years Bohm developed his ideas concerning the underlying reality responsible for quantum effects still further. He was particularly attracted to the rôle of language in forming our conceptions, and to the significance of the notion of "order" for shaping our scientific ideas. Thinking on the ways of reconciling the theory of relativity, which replaces "the concept of a permanent extended object by that of a continued structure of similar and related events, constituting a process taking place in a more or less tube-like region of space-time", and quantum theory, that "denies the notion of a continuous and exactly specifiable process-structure, because the particle movement is always being disturbed by its interaction with the environment through indivisible quantum links associated to what would classically be its continuous field", Bohm came to appreciate a new notion of order which he dubbed the implicate order.

In this new metaphysical-scientific model, the notion of extensionless point
particles is replaced by an undivided seamless whole whose enfoldment and unfoldment gives rise to the *explicate order*. This seamless whole has a super-quantum potential (hence the link with Bohm’s former model, namely, the causal interpretation) and a wave function is assumed for the whole universe. "The general picture that emerges out of this is of a wave that spreads out and converges again and again to show a kind of average particle like behaviour, while the interference and diffraction properties are, of course, still maintained. ... The whole universe not only determines and organizes its sub-wholes, but also ... gives form to what has until now been called the elementary particles out of which everything is supposed to be constituted. What we have here is a kind of universal process of quantum potential as to give rise to a world of form and structure in which all manifest features are only relatively constant, recurrent and stable aspects of this whole."\(^{179}\)

As is apparent, in this theory, the quantum attributes are not *localized* in the quantum entity itself but reside in ‘the entire experimental set up’ which may have to include not only the activities in the immediate vicinity of the entity’s actual detector but action arbitrarily remote in time and space from the detection site. Ultimately the whole universe may be implicated in a simple measurement. In Bohm’s view, the quantum potential of ordinary physical systems should be regarded as the first implicate order, while the super-quantum potential is called the second implicate order (or the super-implicate order). In principle, according to Bohm, there could be an infinite series of implicate orders with growing degrees of subtlety and generality\(^ {180}\).

Bohm’s implicate order, notwithstanding its possible heuristic merits, has not been developed into a full mathematical model. This is perhaps one of the main reasons that the theory has not been taken enthusiastically by the physics community.

**IV.D. Propensity Interpretation:** Another attempt to produce a realistic interpretation of
quantum mechanics has been made by Popper. In his [1967] he has produced thirteen theses which summarise his views on this issue. Popper, following Einstein, maintains that quantum theory is essentially a statistical theory which gives statistical accounts of the behaviour of ensembles of quantum systems and does not deal with the cases of individual quantum entities. However, unlike Einstein, Popper is of the view that: "the interpretation of the formalism of quantum mechanics is closely related to the interpretation of the calculus of probability." This way of looking at the issue has led Popper to both his main objection against the Copenhagen school and his own proposed solution to the apparent difficulties of the orthodox interpretation.

In Popper's view the proponents of the Copenhagen school have committed a "great quantum muddle" which consists in "taking a distribution function, i.e. a statistical measure function characterizing some sample space (or perhaps some 'population' of events), and treating it as a physical property of the elements of the population." Popper maintains that this same muddle is behind many confused talks about wave-particle duality. Many physicists according to Popper have taken the ψ-function as a physical property of the elements of the population, whereas, Popper says, "the wave shape (in configuration space) of the ψ-function is a kind of accident which poses a problem to probability theory, but which has next to nothing to do with the physical properties of the particles." It is as if someone were called a 'Gauss-man' or a 'non-Gauss-man' in order to indicate that the distribution function of his living in a certain location has Gaussian or non-Gaussian shape. For Popper, on the contrary, the ψ-function is only a probability distribution function, whereas it is the element in question which has the properties of a particle.

The reason behind the great quantum muddle in Popper's view is the appeal of
quantum physicists to a subjective theory of probability. In fact, as his third thesis, Popper clearly states that, "[I]t is this mistaken belief that we have to explain the probabilistic character of quantum theory by our (allegedly necessary) lack of knowledge, rather than by the statistical character of our problems, which has led to the intrusion of the observer, or the subject into quantum theory."\textsuperscript{189}

To remedy this great misunderstanding, Popper has proposed an objective theory of probability which he has dubbed the propensity theory\textsuperscript{190}. Briefly stated, it is a theory for the application of the probability calculus to a certain type of "repeatable experiment" in physics and related fields such as biology\textsuperscript{191}. In Popper's view probability statements (as against the statistical statements) should be taken as statements about "some measure of a property (a physical property, comparable to symmetry) of the whole experimental arrangement"\textsuperscript{192}. This is a measure of a virtual frequency (i.e. infinite sequences of well arranged experiments), while the statistical statements correspond to frequencies in actual (i.e. finite sequences of such) experiments.

Propensities, according to Popper, are thus some kind of abstract physical properties related to the whole experimental setups. Every experimental arrangement is liable to produce, in the course of frequent repetition, a sequence with frequencies dependent on that arrangement. These virtual frequencies or propensities are probabilities. On this approach, quantum theory is seen as a theory not about the dynamic processes in time but a probabilistic propensity theory that assigns weight to various probabilities. For example, to assert that the probability of a photon's passing through a semi-transparent mirror is one-half, is to say that the entire experimental arrangements here have a propensity of letting the photon pass through the mirror in 50\% of the cases.

To show that his propensity interpretation solves the problem of the relationship
between particles and waves, Popper has resorted to an analogy between a pin board and a quantum system. Having explained the change in probability distribution of those balls which actually hit a certain pin due to the change in experimental arrangement (e.g. lifting one corner of the board), Popper then goes on to claim in his ninth thesis that, "In the case of the pin board, the transition from the original distribution to one which assumes a ‘position measurement’ (whether an actual one or a feigned one) is not merely analogous, but identical with the famous ‘reduction of the wave packet’. Accordingly, this is not an effect characteristics of quantum theory but of probability theory in general."

Applying this approach, Popper, has reasoned that Heisenberg’s interpretation of the famous example of photons passing through a semi-transparent mirror which was first suggested by Einstein, is misguided. According to Heisenberg, if we find that the photon is reflected, "Then the probability of finding the photon in the other part of the packet immediately becomes zero. The experiment at the position of the reflected packet thus exerts a kind of action (reduction of the wave packet) at the distant point occupied by the transmitted packet, and one sees that this action is propagated with a velocity greater than that of light." However, according to Popper, this apparently reveals a conflation on the part of Heisenberg. The relative probabilities namely,

1) \[ p(a,b) = p(-a, b) = \frac{1}{2}, \text{ and} \]

2) \[ p(a, -a) = 0, \quad p(-a, -a) = 1, \]

where \( a \) refers to photon passing through the mirror and \(-a\) to a reflection event, and \( b \) to the experimental arrangement, are independent of each other; each belongs to a certain experimental arrangement entirely different from the other. As Popper has put it, "No action is exerted upon the wave packet \( p(a,b) \), neither an action at a distance nor any
other action. For $p(a,b)$ is the propensity of the state of the photon relative to the original experimental conditions. This has not changed, and it can be tested by repeating the original experiment.

Popper’s approach, though not without some intuitive appeal, suffers from a number of shortcomings. Apart from lack of precision and rigour with the technicalities of probability calculus and quantum mechanics, the major difficulty with his account is that it does not provide a truly micro-realistic interpretation of quantum mechanics. This is because, in Popper’s approach, propensities are attributed to the whole experimental arrangements and not to quantum entities. This in turn means that on the one hand, Popper’s propensities are macro properties, and on the other hand, they cannot be regarded as a somewhat natural generalization of the notion of dispositional properties which are prevailing in all branches of science. This is because, these dispositions (e.g. fragility), are properties of the entities themselves and not the features of experimental arrangement.

Moreover, to attribute propensities to the experimental setups imports an element of arbitrariness as to what should be regarded as the proper setup in question. This arbitrariness in a way resembles the very arbitrariness in the Copenhagen interpretation as to where to draw the line between the macro world and the micro system. Furthermore, since in Popper’s account, the problem is being shifted from the domain of quantum physics to the realm of probability calculus, even if it can account for the issue of measurement, it has no satisfactory reply to the issue of non-locality of reality which has become apparent from the results of Aspects’ experiments\textsuperscript{193}.

\textbf{V. Bell’s Inequality and its Implications}

In his 1951, D.Bohm had formulated an alternative version of the EPR experiment
that paved the way for experimental verification of hidden variable theories\textsuperscript{198}. In Bohm’s experiment, a molecule is supposed to contain two atoms in a state in which the total spin is zero and that the spin of each atom is $\hbar/2$. This means that the spin of each particle points in a direction opposite to the other. If the molecule is disintegrated by some process the atoms will begin to separate and will soon cease to interact appreciably. However, the total spin angular momentum of the system will remain unchanged, because by hypothesis, no torques have acted on the system. Since the spin components are measurable, it is possible to preform experiments to verify the validity of the conclusions of EPR experiments.

In 1964, J.\textit{Bell} in a theorem\textsuperscript{199} demonstrated that for any variant of the quantum theory that preserves \textit{determinism} and \textit{locality} (i.e. assumes hidden variable and separability) there are fixed limits to the extent to which the properties of pairs of quantum particles can be correlated. The equations relating the magnitudes of the correlations to their upper and/or lower limits are known as Bell’s inequalities\textsuperscript{200}. Under certain circumstances, these limits can be exceeded by the prediction of quantum theory, allowing direct experimental tests to be made for a class of hidden variable theories.

Ever since the appearance of Bell’s theorem, there have been a flurry of technical or otherwise presentations of the theory as well as numerous experimental attempts to test its prediction. In 1981-82 A.\textit{Aspect} and his colleagues performed a series of experiments on the correlation between the polarization orientations of pairs of photons emitted in rapid succession from exited $4p^2S_0$ state of calcium atoms that had been prepared in an atomic beam by two photon laser excitation\textsuperscript{201}. The results of these experiments were in excellent agreement with the predictions of quantum theory and in clear violation of Bell’s inequalities. But what should we conclude from the outcome of such seemingly conclusive
experiments like that of Aspect et al.\textsuperscript{202}. The opinions of physicists and philosophers widely differ on this issue. An interesting case in point is a recent anthology in honour of the sixtieth birthday of J.Bell entitled \textit{Philosophical Consequences of Quantum Theory: reflections on Bell's Theorem} [1989]\textsuperscript{203}, in which a number of writers with different philosophical persuasions have put forward a wide range of interpretations and glosses over the theorem. An anti-realist like van Fraassen has argued that the realist try to explain significant correlations by appeal to a common cause, which more often than not turns out to be unobservable. But, he goes on to say, Bell's theorem and the experimental results which violate it, have shown that there are significant correlations (i.e. spin correlations), for which no common cause can be posited. These correlations are just a brute fact\textsuperscript{204}.

Another anti-realist, Asher Peres, has concluded that any attempt to inject realism in physical theory is bound to lead to inconsistencies. Interestingly enough, a realist like Ernan McMullin, has lost any hope of rescuing realism at the quantum level, and thus has suggested that "Because of its many features, mechanics is quite unsuitable as a paradigm of science generally ... Rather than being the paradigm of natural science, much of physics becomes, at least in the context of this issue [i.e. realism] an anomaly". Instead he has suggested that the realist cause can be best defended by invoking the notion of \textit{structural theories} (his term for causal explanation and/or inference to the best explanation) in fields like chemistry, geology, astrophysics, and genetic leaving aside for the time being the pursuit of realism in the quantum domain\textsuperscript{205}.

In contrast to these rather negative attitudes, some other advocates of realism have managed to come up with a number of fairly convincing arguments and plausible interpretations in defence of realism in the micro-world. The gist of these arguments is
that the moral of Bell’s theorem is neither to abandon realism, nor to embrace an
instrumentalistic interpretation of quantum physics\textsuperscript{204} but that all naïve notions of reality,
and all brands of naïve realism, be it modern or classic, must be eschewed. The notion
of physical reality should (and incidently easily and naturally can be) generalized to
accommodate the results of experiments inspired by Bell’s theorem. The generalization
consists in recognizing that \textit{a new modality of reality} is implicit in quantum mechanical
description of the world. This new modality is defended and expanded by a number of
physicists and philosophers.

To see how the realist option can be defended, it is instructive to use a
representation of Bell’s theorem, invoked by A.Shimony\textsuperscript{205} and a number of physicists /
philosophers of science\textsuperscript{206}. In an EPR type experiment we define the following:

\[ \psi \] is the complete specification of the properties of 1+2 when they leave the source and
move towards the analyzers, whose respective adjustable parameters are shown by \( a \) and
\( b \). The channels from which the particles would emerge are labelled + and −. We define
the following:

- \( x_a = \) the outcome of analysis of particle 1, which can be either + or −.
- \( x_b = \) the outcome of analysis of particle 2, which can be either + or −.

\[ P_{\psi}(x_a, x_b \mid a, b) = \] the probability of joint outcomes \( x_a, x_b \).

The above definitions, would allow one to define the probability of single
outcomes, and also conditional probability in terms of \( P_{\psi}(x_a, x_b \mid a, b) \):
\[ P^1_{\psi}(x_a | a, b) = P_{\psi}(x_a, + | a, b) + P_{\psi}(x_a, - | a, b), \]
\[ P^2_{\psi}(x_b | a, b) = P_{\psi}(+, x_b | a, b) + P_{\psi}(-, x_b | a, b), \]
\[ P^1_{\psi}(x_a | a, b, x_b) = P_{\psi}(x_a, x_b | a, b)/P^2_{\psi}(x_b | a, b), \]
\[ P^2_{\psi}(x_b | a, b, x_a) = P_{\psi}(x_a, x_b | a, b)/P^1_{\psi}(x_a | a, b). \]

On the basis of the expressions, Shimony, following Jarrett\(^2\), has defined two distinct independence conditions, namely;

1. **Parameter Independence:**
   \[ P^1_{\psi}(x_a | a, b) \text{ is independent of } b, \]
   \[ P^2_{\psi}(x_b | a, b) \text{ is independent of } a. \]

2. **Outcome Independence:**
   \[ P^1_{\psi}(x_a | a, b, x_b) = P^1_{\psi}(x_a | a, b), \]
   \[ P^2_{\psi}(x_b | a, b, x_a) = P^2_{\psi}(x_b | a, b). \]

As Jarrett has shown the conjunction of 1&2 is equivalent to Bell’s locality condition, namely:

\[ P_{\psi}(x_a, x_b | a, b) = P^1_{\psi}(x_a | a)P^2_{\psi}(x_b | b) \]

The negative result of Aspect’s experiment means that either 1 or 2 should be rejected. But the repercussions of the two are quite different;

If 1 fails, e.g., because \[ P^2_{\psi}(x_b | a, b) \neq P^2_{\psi}(x_b | a', b) \] this means that at a moment when the particles 1 and 2 are about to impinge upon their respective analyzers, an experimenter can make a choice between parameter values \( a \) and \( a' \). This in turn means that he can affect the probability of the outcome \( x_b = + \) for the analysis of particle 2; and if an ensemble of pairs of 1+2 is prepared in a sufficiently short interval of time, then the frequency of \( + \) outcome will be affected with certainty by the choice between \( a \) and \( a' \).

This means that the experimenter can inform another experimenter who is observing the outcome of the second analyzer by transmitting information with the speed faster than that of light. This in turn amounts to a violation of special theory of relativity.

If however, 2 fails it means that the notion of locality, i.e. reality consists of
localised stuff, must be abandoned. It has been shown that quantum mechanics does not violate Parameter Independence, which means that this theory is not a local theory. However, being a non-local theory does not amount to incompatibility with the special theory of relativity. In fact the two theory can have a peaceful coexistence.

A caveat however, is in order. As Shimony has pointed out it is tempting to regard $\psi$ as merely a description of the state of the scientist’s knowledge of the two photons, or alternatively, as the description of an inhomogeneous ensemble of photon pairs, the individual members of which have definite properties that are not described by $\psi$. If however, we concede that $\psi$ is a complete description of the polarization of the pairs of photons, then we must accept the indefiniteness of the polarization of each with respect to a x-y axes as an objective fact, not as a feature of the knowledge of one scientist or of all human beings collectively. We must also acknowledge the objective chance and objective probability, since the polarization analysis of each photon is a matter of probability. Such an objective probabilistic reality is dubbed differently by different writers. Shimony has summarized the philosophical conclusion which can be drawn from Bell’s inequality in the following way:

The work indicated by Bell has the consequence of making virtually inescapable a philosophically radical interpretation of quantum mechanics: that there is a modality of existence of physical systems which is somehow intermediate between bare logical possibility and full actuality, namely the modality of potentiality.

Bell’s theorem and the results of Aspect’s type experiments, in fact confirm the view advocated by sophisticated realists that adopting radically new conceptions of reality need not, and in fact does not in any way, undermine the realists’ basic principle, namely the objectivity and independence of reality.

The remaining task before the realists is to show that it is possible to produce a version of quantum theory, based on the new conception of reality, which is at least as
successful as OQM. This task has already been carried out by a number of writers, many of whom have put forward promising ideas along more or less similar lines\textsuperscript{215}. The very fact that such new lines of research and interpretation have been suggested is in itself a strong piece of counter evidence (if not a refutation) of anti-realists' claim that quantum world is not amenable to realistic theories. In the remaining part of this chapter we shall briefly introduce one such proposal, which is in line with the general approach of this thesis, due to N.Maxwell\textsuperscript{216}.

The main point of this proposal is that fulfilment of the basic aim of science, namely understanding of the fundamental structure of nature, requires the development of a micro realistic version of quantum theory. Such a version should be exclusively about micro entities and their interactions. Macro systems, and in particular measuring instruments should not be lurking, in however concealed a fashion, in the background as far as the basic postulates of the theory are concerned\textsuperscript{217}. The key to developing such a version is the realist conviction that micro entities exist independent of the human perceivers, or in other words it is not the case that \textit{esse est precipe}. To fulfil this requirement, any viable micro-realistic explanatory theory must have a definite, characteristic ontology of its own.

As mentioned in the first chapter, a realist guiding principle for probing the nature of unobservable entities (including quantum posits), is the general methodology of conjectural essentialism\textsuperscript{218}. In line with this methodology two sensible assumptions, which set the basic framework of a micro-realistic approach, need to be introduced;

1) In speaking of the \textit{properties} of fundamental physical entities (such as mass, charge, spin) we are in effect speaking of the dynamical laws obeyed by the entities – and \textit{vice versa}. Thus if we change our ideas about the nature of dynamical laws, we thereby,
if we are consistent, change our ideas about the nature of properties and entities that obey the laws, and

2) The quantum world is fundamentally probabilistic in character, that is, the dynamical laws governing the evolution and interaction of the physical objects of the quantum domain are probabilistic and not deterministic.\(^{219}\)

The main conceptual tool of the proposed approach is the notion of dispositional properties.\(^{220}\) Physical entities, macro objects or micro objects, possess properties which are dispositional in character: Their properties simply imply something about how the respected objects change, resist change, or affect change in other objects, in certain circumstances. From this point of view, the main difference between micro entities and macro entities is that while the latter have deterministic dispositional properties which are accounted for in classical physics\(^{221}\), the properties of quantum entities are probabilistic. These kinds of properties are called propensities\(^{222}\) and the objects which possess them are called propensitons.\(^{223}\)

Two kinds of probabilistic laws, namely, continuous and discrete probabilistic laws are considered by Maxwell. Corresponding to these two laws, two kinds of propensitons, continuous and discrete propensitons are introduced. Maxwell has suggested that quantum entities (e.g. electrons, photons, ...) are varieties of the second kind of propensitons called discrete propensitons. As long as the physical conditions for probabilistic actualization of the propensities of these entities are not realized, they evolve in space and time deterministically. When these conditions are realized, they suffer an instantaneous, probabilistic change of state, determined probabilistically by the value of relevant propensities at the instant in question. In order to specify the nature of these propensitons (i.e. the nature of the propensities they possessed) three things need to be specified: i) the
deterministic dynamical laws of evolution and interaction; ii) the precise propensiton condition for probabilistic event to occur; and iii) probabilistic laws governing instantaneous probabilistic transitions\textsuperscript{224}.

This task has been carried out by Maxwell in a number of publications. The final result is a new version of quantum theory called propensiton quantum theory, or PQT for short\textsuperscript{225}. PQT, retains the dynamical equations of orthodox quantum theory (OQT) but rejects Born probabilistic interpretation of $\psi$. Instead of interpreting $\psi$ as containing information about values of observables and about the outcome of performing measurement on the system (or ensemble of systems) in question, PQT interprets $\psi$ as specifying the actual physical state of the individual quantum system in physical space and time, even in the absence of preparation and measurement\textsuperscript{226}. PQT also regards all measurements to be no more than special cases of a kind of probabilistic process occurring naturally throughout the universe. According to PQT, what exists potentially in one spatial region at an instant depends, in this way, on what exists, potentially, elsewhere – a feature of the quantum world (i.e. non-locality) not encountered within classical physics, and confirmed by the outcomes of the experiments on testing Bell’s inequality. PQT’s explanation for this fact is that $n$ interacting particles do not have $n$ distinct quantum states, but only have a joint, quantum entangled state as a whole. This whole undergoes probabilistic transitions, when the condition for such transition (collapse) is achieved. This way of describing the quantum reality prepares the ground for an experimental test which can decide between the two rival versions, namely, OQT and PQT.

For OQT, as we have already seen, all quantum mechanical phenomena (or physical systems) are to be regarded as wholes, consisting of the measuring instruments
and what is being measured. What is being measured, whatever it is, always exists in a superposition of states, until an act of measurement, which causes a particular wave collapse, is carried out. As a real example, consider a rearrangement collision between spinless particles a, b, and c, with the following two channel outcome:

\[(a + b) + c \rightarrow (A) \text{ or } (B), \text{ where;}\]

\[(A) = (ab) + c \text{ and}\]
\[(B) = a + b + c\]

where \((ab)\) is the bound state. According to OQT, the outcome of the interaction is a superposition of the two channel state, \((A)\) and \((B)\), and only on measurement one of the two states can be detected. According to PQT however, the superposition of \((A)\) and \((B)\) collapse spontaneously and probabilistically (provided the condition for such a collapse is achieved) even in the absence of the act of measurement.

To examine the validity of his claim, Maxwell has suggested a crucial tests which he hopes will conclusively establish the superiority of PQT over OQT. Although this test has not been carried out yet, it is in principle possible to perform it. The proposed scheme, as a physicist has put it, provides an interesting research programme for advancing a realist theory of quantum world.
NOTES (Chapter Six)

1. For an assessment of realism vs. anti-realism dispute in biological science see B.Kanitscheider [1988], R.Burian [1987].

2. Many writers have observed that the anti-realist approach is unhelpful for the advancement of science. See for example, K.Popper [1959/68], [1963/1972]. R.Trigg ([1980], p.x) has observed that "the repudiation of realism in whatever context can lead to a debilitating nihilism."


4. In his rather brief review of E.MacKinnon’s *Scientific Explanation and Atomic Physics*, Alan Franklin complains that; "One last problem, which seems to afflict almost all work in the history and philosophy of science, is the almost total absence of experimental results. Although [some] experiments which showed the discrepancy between Wien’s law and experiment and led to Planck’s introduction of quantization, are mentioned, we never see a graph or drawing of experimental results compared to theoretical predictions ... If science is empirical, as virtually all scientists and philosophers of science agree, and if empirical adequacy is an important criterion in the evaluation of theories, then, surely, ... we deserve a few glimpses of what the results that these scientists were concerned about looked like." (A.Franklin [1984], p.483)

Point taken! To avoid this objection, in this chapter I shall try to produce charts and graphs as well as formulae and equations where required.

5. "Generally speaking, the detailed form of the spectrum of the thermal radiation emitted by a hot body depends somewhat upon the composition of the body. However, experiment shows that there is one class of hot bodies that emits thermal spectra of a universal character. These are called black-bodies, that is, bodies which have surfaces that absorb all the thermal radiation incident upon them. The name is appropriate because such bodies do not reflect light and appear black.

An important example of a black-body, can be found by considering an object containing a cavity which is connected to the outside by a small hole. Radiation incident upon the hole from the outside enters the cavity and is reflected back and forth by the walls of the cavity, eventually being absorbed on these walls. If the area of the hole is very small compared to the area of the inner surface of the cavity, a negligible amount of the incident radiation will be reflected back through the hole. Essentially all the radiation incident upon the hole is absorbed; therefore the hole must have the properties of the surface of a black-body. Most black-bodies in laboratory are constructed along these lines." (R.Eisberg, R.Resnick [1974], pp.4-6).


7. In 1860 Gustav Robert Kirchhoff introduced an important law: the spectral density (the amount of energy per unit volume for each radiating frequency) of a black-body depends only on the frequency and the temperature. This law prompted many physicists (both experimentalists and theorists) in the second half of the 19th century to discover the exact quantitative relation stated in the law. The first major breakthrough was made in 1879 when Josef Stefan conjectured from an analysis of experimental data that this energy should be proportional to forth power of temperature, namely, \( \Phi \propto T^4 \). In 1884 Ludwig Boltzmann demonstrated theoretically that Stefan’s guess holds strictly only for the energy emitted by a black-body. The next important step was taken by Wien. (cf. T.Kuhn [1978], P.Stehle [1994])

8. See note 21 below.


12. This theorem due to Maxwell and Boltzmann stated that for a system of gas molecules in thermal equilibrium at temperature $T$, the total kinetic energy of the system is on average equally distributed among all its degrees of freedom. The average kinetic energy of a molecule per degree of freedom is $kT/2$, where $k = 1.38 \times 10^{-23}$ joule/K is called Boltzmann's constant. The law actually applies to any classical system containing, in equilibrium, a large number of entities of the same kind. For each of the imaginary oscillators in the cavity, we must assume two degrees of freedom. Hence the average energy of each oscillator will be $U = kT$. (See A.d’Abro [1951], p.455.)

At that time the validity of this theorem was much debated. The problem with the theorem was that although it could be proven rigorously on the basis of classical mechanics, in some cases it could not account for the actual phenomena. For instance, the internal degrees of freedom of an atom did not manifest themselves in the specific heat, and a monatomic gas had a specific heat as if the atoms were points, which they certainly are not. Although these problems troubled the great statistical mechanicians of the day (people like Boltzmann, Kelvin, Rayleigh) none of them suspected that the root of the problem might have lain in the inadequacy of classical mechanics itself. (See E.Segrè [1980], p.66.)

13. Since Einstein too, was involved in deducing this formula, some writers have called it Rayleigh - Einstein - Jeans (REJ) formula. See for example, A.Pais [1982, p.372-3, 1991, p.84].

14. The following plot compares the predictions of Rayleigh - Jeans formula with empirical data.

![Plot comparing Rayleigh - Jeans formula with experimental data.]

The step which was taken by Rayleigh was quite simple. He simply used classical equipartition theorem for a resonator inside the cavity, namely, $U = kT$, in Planck’s formula.

The views of historians concerning the question why Planck himself did not take this easy step is divided. Some (e.g. Jammer [1966], p.14. Hermann [1971], p.8) maintain that at the time (namely, 1899) he was not aware of the existence of this theorem. One of the main reason for this unfamiliarity, it is claimed, has been Planck’s aversion to Boltzmann’s statistical methods. According to this group of historians, Planck only started a systematic adaption of Boltzmann works in late 1900.

Kuhn [1978] on the other hand, has rather convincingly argued that Planck began his use of Boltzmann’s method in 1988. According to Kuhn, Planck was, in effect, trying to develop an electromagnetic equivalent of Boltzmann’s $H$-Theorem. This theorem was obtained during Boltzmann’s attempts to find the probability of a gas being in some specific macroscopic state, i.e., in certain statistical equilibrium state. Boltzmann made a direct study of the collisions of gas molecules, which were assumed to be perfectly spherical and elastic. He showed that as a result of the collisions a certain magnitude, which he called $H$, would tend to decrease until it had attained a minimum value $H_\infty$. The rate of change of $H$ was also investigated. The value of the difference $H - H_\infty$ was shown to measure the departure of the actual macroscopic state of the gas from the state of statistical equilibrium. $H$ is in effect equal to the entropy $S$ with its sign reversed. That is to say, $-H = S = k \log W$ where $W$ is the probability of a given macroscopic state of a given mass of gas, and $k$ is a constant. For details see E.MacKinnon, op.cit. See also note 19 below.

16. Entropy was a long-life source of fascination for Planck. After studying a paper of Rudolf Clausius, the young Planck became convinced that besides the energy principle, the second law of thermodynamics with its bearing on the concept of entropy, also possesses a fundamental significance. The relation between the two can be stated in these terms: a natural process that starts in one equilibrium state and ends in another will go in the direction that the entropy of the system plus environment to increase.

In his *Scientific Autobiography* [1949], Planck has explicitly stated that, "... my previous studies of the Second Law of Thermodynamics came to stand me in good stead now, for at the very outset I hit upon the idea of correlating not the temperature but the entropy of the oscillator with its energy. It was an odd jest of the fate that a circumstance which on the former occasions I had found unpleasant, namely, the lack of interest of my colleagues in the direction taken by my investigations, now turned out to be an outright boon."

In the course of his studies on entropy, Planck came to a new definition of this principle, namely, "The process of heat conduction cannot be completely reversed by any means." This definition posed a challenge for Planck who maintained that the fundamental task of physics was to reduce unidirectional changes to conservation laws.

After putting aside his initial misgivings about Boltzmann's statistical method, (which in his view, because of its use of random ordering on individual events, was not suitable for explaining unidirectionality), Planck obtained his formula for the energy of a resonator, \( u(v,T) = (8\pi v^3/c^3)U \). However, at this stage he did not yet have a satisfactory solution to the problem of irreversibility. To get this he needed a function which is determined by the instantaneous states of the system and which changes only in one direction. Planck defined the entropy of an individual resonator as \( S = - (u/v) \log(U/e) \), in which a and b are yet undetermined natural constants, and e the base of natural logarithms.

Next, Planck argued that at equilibrium the total entropy must be constant. By considering a virtual transference of energy between resonators of different frequencies, Planck derived a formula for the energy of a resonator, namely, \( U = bv \exp(-av/\theta) \). By using his 1899 formula, linking the energy of a resonator to a radiant energy, Planck derived the formula \( u = (8\pi bv^3/c^2)\exp(-av/\theta) \). With the appropriate identifications of constants, this formula is equivalent to Wien's formula. In identifying \( \theta \) with the absolute temperature \( T \), Planck had again used Boltzmann's method in developing the \( H \)-theorem. For details see Kuhn [1978], MacKinnon [1982].

17. In his [1949], Planck writes, "Since for the irreversibility of the exchange of energy between an oscillator and the radiation activating it, the second differential quotient of its entropy with respect to its energy is of characteristic significance, I calculated the value of this function on the assumption that Wien's law ... is valid. ... I got the remarkable result that on this assumption the reciprocal of that value, which I shall call here \( R \), is proportional to the energy. ... [D]irect experiments established two simple limits for the function \( R \): for small energies, \( R \) is proportional to the energy; for larger energy values \( R \) is proportional to the square of the energy. ... The problem was to find such a formula for \( R \) which would result in the law of the distribution of energy, ... Therefore, the most obvious step for the general case was to make the value of \( R \) equal to the sum of a term proportional to the first power of the energy and another term proportional to the second power of the energy, so that the first term becomes decisive for small values of the energy and the second term for the large values."

18. In his Nobel prize address in 1920, Planck said:

"But even if the radiation formula proved to be perfectly correct, it would after all have been only an interpolation formula found by lucky guess-work and thus would have left us unsatisfied. I therefore strived from the day of its discovery to give it a real physical interpretation." (E.Segrè [1980], p.72. italics added).

Planck has reiterated this point in his [1949], :"But even if the absolutely precise validity for the radiation formula is taken for granted, so long as it had merely the standing of a law disclosed by lucky intuition, it could not be expected to have more than a formal significance. For this reason, on the very day when I formulated this law, I began to devote my self to the task of investing it with a true physical meaning."

19. As noticed in note 14 above, Boltzmann had suggested the formula \( -H = k \log W \), for the statistical interpretation of any molecular system in a given state. Here \( W \) is, as Boltzmann himself put it, "the number of 'complexions' or possibilities of permuting the molecules without changing the state of the system." (Boltzmann [1877], quoted from Mehra and Rechenberg [1982], p.48). \( k \) is a natural constant now called
the Boltzmann constant. In modern thermodynamics the above formula is used as a mathematical definition of disorder and $W$ is called the disorder parameter.

Planck has said this about his use of Boltzmann's formula in his [1949], "This quest [i.e., the quest for a physical meaning for his 19 October formula] automatically led me to study the interrelation of entropy and probability — in other words, to pursue the line of thought inaugurated by Boltzmann. Since the entropy $S$ is an additive magnitude but the probability $W$ is a multiplicative one, I simply postulated that $S = k \log W$, where $k$ is a universal constant; and I investigated whether the formula for $W$, which is obtained when $S$ is replaced by its value corresponding to the above radiation law, could be interpreted as a measure of probability. As a result, I found that this was actually possible, and that in this connection $k$ represents the so-called absolute gas constant, referred not to gram-molecules or moles, but to the real molecules. It is understandably, often called Boltzmann's constant." (Planck [1949])

20. In his paper, Planck employed two concepts whose interrelations is not altogether clear. The first is the idea of monochromatic resonators which helps him to link $S$, the entropy of an individual resonator (or alternatively $S_N$, the entropy of a collection of $N$ individual resonators, $S_N = NS$) to $W$, the number of ways the total energy may be distributed among $N$ resonator, via the formula $S = k \log W + \text{const}$. The second idea is that energy is not treated as a continuously divisible quantity, but is composed of a well-defined number of equal parts, namely $E = Pe$ where $P$ is an integral number. Planck, in both his papers of October, and December, 1900 had ascribed the notion of energy discreteness to groups of oscillators, and had not talked of discrete energy levels for individual oscillators.

With these not quite clear assumptions, Planck, adopting Boltzmann's method, argued that the number of ways in which $P$ units of energy can be distributed among $N$ oscillators is

$$R = \frac{(N + P - 1)!}{(N - 1)! P!}.$$ 

Planck mistakenly took the above formula to be a first approximation to Sterling's formula, namely, $N! = N^N$. The actual first approximation of Sterling's formula is $N! = 2\pi N(N/e)^N$. Even at relatively low values of $N$ the discrepancy between Planck's and the true approximation is extreme. Thus for $N = 50$, $N! = 3.041 \times 10^{64}$. The true first approximation gives $3.036 \times 10^{64}$ which is off by $0.2$ percent; whereas Planck's formula gives $8.88 \times 10^{64}$ which is off by a factor of $2 \times 10^{30}$. This extreme discrepancy however, does not introduce a significant error. On the assumptions $P \gg N \gg 1$ Planck's approximation produces almost the same results as the true approximation. cf. MacKinnon [1982, p.136].

21. This is the date of birth of quantum mechanics according to the majority of historians of science. Some, (e.g., Jammer [1966, p.45-46]) however, maintain that, the birthday of quantum mechanics should be regarded as 19th October 1900.

In the following diagram the outcomes of Wien , Rayleigh-Jeans, and Planck's formulae are compared.

22. Planck [1949] has said this about the introduction of the notion of quantum into physics. "... [A]s for the magnitude of $W$, I found that in order to interpret it as probability, it was necessary to introduce a constant, which I called $\hbar$. Since it had the dimension of action (energy x time), I gave it the name,
elementary quantum of action."

23. As for Planck's appreciation of the assumption of discrete quanta of energy, there are three different accounts in the literature. According to the popular account (which was certainly wrong) Planck discovered that radiant energy comes into little bundles of value $e=\hbar v$. As we have seen above, in his papers Planck introduced the idea of discrete energy values, not for radiant energy (this was done later on by Einstein) but for fictitious oscillators in equilibrium with this radiation. According to the standard reconstruction which is due to Martin Klein [1962] Planck's breakthrough came in two stages. First, in October 1900, he presented a new radiation law as a modification of Wien's law. When this was found to fit the data remarkably well, Planck spent three months of intensive work and then presented a theoretical justification of the new law. The crucial point in this justification was to ascribe the discrete values of energy $e=\hbar v$ to each of the fictitious oscillators which together, it was assumed, made up the black-body.

Thomas Kuhn [1978] however, has argued that Planck did not quantize the energy absorbed or emitted by individual oscillators. This discreteness was simply a mathematical device applied to the collection of oscillators. For individual oscillators Planck still believed in continuous energy distribution. The idea that energy comes in discrete units was suggested by Einstein.

E.MacKinnon [1982, p.129] has noted that Planck in his autobiography does not claim credit for introducing the idea that energy comes in discrete units. This he attributes to Einstein. What he does claim credit for is the discovery of the law that bears his name and for the two constants $\hbar$ and $k$. After considering the arguments concerning Planck's introduction of the concept of energy quantisation, MacKinnon concludes that, "As for the concept itself, Planck definitely did not hold that radiant energy comes in discrete units of value $e=\hbar v$. There is no disagreement about that. He did hold that groups of oscillators are quantized. He never explicitly discussed individual oscillators. If the propositions he reflects were made explicit, they would manifest an underlying incompatibility. When he speaks of the distribution of the amplitudes and energy of the resonators in a group, he is implicitly presupposing that energy is a classical function of amplitude. This allows a continuous distribution of energies. Yet, when he associated with them the energy $e=\hbar v$, he has an assumption which cannot lead to the same energy distribution. Planck probably paid little, if any, attention to this issue before Einstein and others made it crucial." (ibid., pp.139-40) See also note 28 for Einstein's view of Planck's formula.

24. "While the significance of the quantum of action for the interrelation between entropy and probability was thus conclusively established, the part played by this new constant in the uniformly regular occurrence of physical processes still remained an open question. I therefore, tried immediately to weld the elementary quantum of action $\hbar$ somehow into the framework of the classical theory. But in the face of all such attempts, this constant showed itself to be obdurate." (Planck, op.cit., [1949])


27. T.Kuhn op.cit., [1978].

28. The arguments which Planck used to formulate his famous formula were shown to be inconsistent by Einstein in his famous paper of 1905 (in which he had put forward his revolutionary proposal of the light quanta). The inconsistency arises because in formulating his equation Planck had unwarrantedly, combined two incompatible theories. Planck's final celebrated formula consisted of two parts, 1) $u(v,T) = (8\pi^2/c^3)U$, and 2) $U = \hbar v [\exp(hv/kT) - 1]$. The first part, which states a relation between the radiative energy density $u$ and the oscillator energy $U$, was obtained from Maxwell's theory and a well entrenched assumption of classical physics: an oscillator's energy is a continuously variable quantity. The second part however, represented a statistical interaction among oscillators of different proper frequencies, and was based on the unprecedented assumption that energy is a discrete quantity, capable of assuming only values which are multiple of $\hbar v$.

In his paper of 19(5, Einstein put his criticism rather mildly and diplomatically. Referring to the relation derived by Planck for the dynamical equilibrium between the fictitious oscillators and the radiation present in the cavity by assuming that the radiation is a completely random (i.e., statistical) process, he then remarked that, "If the radiation of energy of frequency $v$ is not continually increasing or decreasing, the
following relations must be obtained
\[ U(v, T) \equiv \frac{R}{N} T = \left( \frac{c}{8\pi v^2} \right) u(v, T) \]
\[ u(v, T) = \left( \frac{R}{N} \right) \left( \frac{8\pi v^2}{c^4} \right) T. \]
[here \( R \) is the gas constant, \( N \) Avogadro's number, and \( \frac{R}{N} = k \) the Boltzmann constant]

These relations, found to be the conditions of dynamic equilibrium, not only fail to coincide with experiment, but also state that in our model there can be no talk of a definite energy distribution between ether and matter. The wider the range of wavelength of the oscillators, the greater will be the radiation energy of the space, and in the limit we obtain

\[ \int_0^\infty u dv = \infty. \]

In a later paper published the following year, Einstein put his finger more sharply on this inconsistency and remarked that "If the energy of a resonator can change only discontinuously, the usual theory of electricity cannot be applied for the calculation of the average energy to such a resonator in a radiation field. Planck's theory has, therefore, to assume that, although Maxwell's theory of elementary resonators is not applicable, the average energy of such a resonator, surrounded by radiation, is equal to that which would result from the calculation on the basis of Maxwell's theory of electricity ... Such an assumption would be plausible provided \( e = \hbar \nu \) were small throughout the observable spectrum compared to the average energy \( U \) of the resonator; but this is not the case". Einstein saw in this inconsistency an indication that the foundation of the traditional radiation theory, based on Maxwell's electromagnetic theory, had to be revised.

Years later in his Autobiographical Notes [1949] Einstein, analysing Planck’s derivation of his formula in a more explicit way, remarked that, "If Planck had drawn this conclusion, he would probably not have made his great discovery, because the foundation would have been withdrawn from his deductive reasoning." (1949/1982, p.43) See also note 79 below.


30. The photoelectric effect was first observed by Heinrich Hertz during 1886-87 when performing experiments that first confirmed the existence of electromagnetic waves and thus corroborated Maxwell’s electromagnetic theory of light propagation. Hertz discovered that an electric discharge between two electrodes occurs more readily when ultraviolet light falls on one of the electrodes. Soon after, Philipp Lenard showed that the ultraviolet light facilitates the discharge by causing electrons to be emitted from the cathode surface. This phenomenon, i.e. the ejection of electrons from a surface by the action of light is called the photoelectric effect.

There are three major features of the photoelectric effect that cannot be explained by the classical (Maxwell’s) wave theory of light;

1. Wave theory requires that the photoelectric effect should occur for any frequency of the light, provided only that the light is intense enough. However, experiments had shown that there is a minimum, (cutoff or threshold) frequency \( \nu \), of radiation, characteristic of the surface, below which no emissions takes place, no matter what intensity of the incident radiation, or how long it falls on the surface.

2. According to the wave theory the kinetic energy of the photoelectrons should increase as the light beam is made more intense. Experiments failed to confirm this prediction; the emission of photoelectrons were found to depend linearly on the frequency of the radiation and to be independent of intensity.

3. If the energy required by a photoelectron is absorbed from the wave incident on the metal plate, the "effective target area’ for an electron in the metal is limited, and probably not much more than that of a circle having about an atomic diameter. In the classical theory the light energy is uniformly distributed over the wave front. Thus, if the light is feeble enough, there should be a measurable time lag between the time when light starts to impinge on the surface and the ejection of electrons from the surface. During this period, electron should, supposedly be absorbing energy from the beam until it has accumulated enough energy to escape. However no such time lag was detected. (For details see Halliday and Resnick [1966])

31. Before Millikan's complete experimental validation of Einstein’s light quanta hypothesis in 1914, Einstein was recommended to membership in the Prussian Academy of Science by Planck and others. Their early negative attitude toward the photon hypothesis is revealed in their signed affidavit, praising Einstein,
in which they wrote: "Summing up, we may say that there is hardly one among the great problems, in which modern physics is so rich, to which Einstein has not made an important contribution. That he may have sometimes missed the target in his speculations, as, for example, in his hypothesis of light quanta, cannot really be held too much against him, for it is not possible to introduce fundamentally new ideas, even in the most exact sciences, without occasionally taking risk". (Jammer[1966], pp.43-44).

32. The paradoxical nature of Einstein equation can be seen better if we consider the energy and momentum of a photon. (Incidentally, Einstein published his formula for the momentum of the photon in the same volume of the journal Annalen der Physik where he had published his photoelectric conjecture. The fundamental equations for a photon can be written in this way:

\[ E = hv, \quad P = hv/c = h/\lambda, \]

where \( E \) is the energy of the photon, \( P \) its momentum, \( v \) the frequency and \( \lambda \) its wavelength. Now, since the angular frequency is \( \omega = 2\pi v \) and the wavenumber \( k = 2\pi/\lambda \), the above equations can be re-written in this form:

\[ E = \hbar \omega \quad \text{and} \quad P = \hbar k. \]

(Where \( \hbar = h/2\pi \)) These two equations encapsulate the dual nature of radiation. The left-hand sides of the equations refer to energy and momentum, both particle-like properties. To be sure, waves carry both energy and momentum but such quantities would have to refer to some specific volume of radiation-filled space whereas here they refer to single entities, namely, photons. In contrast the right-hand sides of the equations refer to frequency and wavenumber, both wave-like properties.

33. In 1914, Millikan, in a series of experiments, vindicated Einstein’s bold conjectures. However, the value of Millikan’s empirical data has recently been challenged. Historians of science have shown that Millikan’s data have largely been obtained through deliberate adjustments and corrections in the light of Einstein’s hypothesis. It is worth-mentioning that, Millikan, in spite of the fact that his results were in agreement with Einstein’s predictions, did not regard his experiments as confirming Einstein’s equation. He did not believe in the light quanta hypothesis and would regard it as wholly untenable. Like most of his contemporaries, Millikan believed the photoelectric effect to be a resonance phenomena and thought that Einstein’s hypothesis was a case of an erroneous theory that has led to the discovery of empirical relations of the greatest importance. For the background to Millikan’s experiments and the way he handled them see A.Franklin [1986 & 1990], W.Broad and N.Wade [1983].

34. The particle-like nature of radiation received dramatic confirmation in 1923 from the experiments of Compton. He allowed a beam of X-rays of sharply defined wavelength \( \lambda \) to fall on a graphite target. For various angles of scattering, he measured the intensity of the scattered X-rays as a function of their wave length. As shown in the figure below, although the incident beam consists essentially of a single wavelength \( \lambda \), the scattered X-rays have intensity peaks of two wavelength; one of them is the incident wavelength, the other, \( \lambda' \), being larger by an amount \( \Delta \lambda = \lambda - \lambda' \) known as Compton shift.

![Intensity Peaks of Compton Scattering](image_url)

The presence of scattered wavelength \( \lambda' \) cannot be understood if the incident X-radiation is scattered as a classical electromagnetic wave. In the classical model the oscillating electric field vector in the incident wave of frequency \( v \) acts on the free electrons in the scattering target and sets them oscillating at that same frequency. These oscillating electrons, like charges surging back and forth in a small radio transmitting antenna, radiate electromagnetic waves that again have this same frequency \( v \). Hence in the classical picture the scattered wave should have the same frequency \( v \) and the same wavelength \( \lambda \) as the incident wave.

Compton interpreted his experimental results by postulating that the incoming X-ray beam was not a wave of frequency \( v \) but a collection of photons, each of energy \( E = hv \), and that these photons collide with the electrons in the scattering target as in a collision between billiard balls. In this view, the ‘recoil’ photons emerging from the target make up the scattered radiation, which have a lower energy \( E' \), and a lower frequency \( v' = E'/h \) and a longer wavelength \( \lambda' = cv' \) (which satisfies Compton shift \( \Delta \lambda = \lambda - \lambda' \)). For details see Halliday & Resnick [1966].

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35. In Lenard's explanation it was assumed that electrons vibrating in resonance with the radiation of the same frequency are ejected with an energy which is proportional to the vibration frequency; the incident radiation thus functions merely as a trigger for the release of electrons, which already possess within the atom the energy required for their release. However, the essential difference between this type of explanation and the light-quanta theory lies in the continuous and progressive character of the energy accumulation in the metal; consequently, photovoltaic emission, instead of being instantaneous, cannot take place before the metal has received energy \( h\nu \). If one operates with sufficiently fine metallic particles, this minimum delay between the start of the irradiation and the onset of the emission can be made sufficiently long to be detectable experimentally. Experiments for this purpose were conducted by Meyer and Gerlach in 1914 on metallic dusts. In every case they observed the emission of electron right at the start of irradiation. Experiments such as these convinced physicists to abandon Lenard's explanation and seek for alternative models. cf. Jammer [1966], A.Messiah [1962].

36. Einstein himself was perhaps the first eminent physicist who thought of rejecting the principle of conservation of energy in order to solve the riddle of radiation problem. In 1910 he wrote to a friend, "At present, I have high hopes for resolving the radiation problem, and that without light quanta. I am enormously curious as to how it will work out. One must renounce the energy principle in its present form". However, a few days later he was disenchanted. "Once again the solution of the radiation problem is getting nowhere. The devil has played a rotten trick on me". cf. A.Pais [1982], p.418. Mackinnon [1982].

Other physicists, however, were apparently attracted to this line of thought.

In 1916 Nernst published an article 'On an attempt to revert from quantum mechanical considerations to the assumption of continuous energy changes’, in which he proposed that energy is conserved only statistically, that is, when averaged over an assembly of individual processes of a specific kind. This paper influenced Bohr. In 1919 Darwin wrote to Bohr, "At present I consider the case against conservation of energy quite overwhelming." In 1922 Darwin published a paper in which he emphasised that, "The speculations connected with [the quantum theory] have as their basis the law of conservation of energy ... [however] there seems no reason to maintain the exact conservation of energy. In the same year, Bohr in his Nobel-prize lecture emphasised that, "The hypothesis of light-quanta ... is not able to throw light on the nature of radiation."

The efforts to uphold the wave theory of light at the expense of the laws of conservation of energy and momentum came to its culmination in 1924. In January that year, after the publication of Compton's experimental results, a rather unfortunate paper was published by Bohr and two of his colleagues, Kramers and Slater, entitled, 'The Quantum Theory of Radiation’. In this paper, which was effectively written by Bohr and was more of a research programme than a detailed research report, the authors started by noting that, "In the attempt to give a theoretical interpretation of the mechanism of interaction between radiation and matter, two apparently contradictory aspects of this mechanism have been disclosed. On the one hand, the phenomena of interference ... claim an aspect of continuity of the same character as that involved in the wave theory of light ... On the other hand, the exchange of energy and momentum between matter and radiation ... claim essentially discontinuous features." The authors had then proceeded to introduce their own solution to this puzzle: "As regards the occurrence of transitions, which is the essential feature of the quantum theory, we abandon ... any attempt at a causal connexion between the transition in distant atoms, and especially a direct application of the principles of conservation of energy and momentum, so characteristic of the classical theories. ... Not only the conservation of energy ... but also conservation of momentum [reduce to] a statistical law.” Regarding Compton results, the authors had noted that his experiments had only confirmed energy-momentum conservation averaged over many individual process, and not the conservation of these properties for individual processes.

A few years later, Heisenberg, referring to Bohr, Kramers, Slater joint paper, which soon after publication turned out not to be on the right track, commented that, "This investigation represents the actual culmination of the crisis in the [old] quantum theory." It is worth-mentioning that in this joint paper three main distinct ideas had been presented, namely,

1) Slater's idea of a 'virtual radiation field’,
2) statistical conservation of energy and momentum,
3) statistical independence of the processes of emission and absorption in distant atoms.

Out of these three main themes only the first one was correct. Slater, who initially had proposed the existence of a virtual field, (a field which was obeying Maxwell's law and the function of which was to guide corpuscular quanta), had to abandon this sound idea under the pressure from Bohr who was against
the light quanta postulate. Slater, in his later years bitterly regretted this episode. For details cf. Bohr, Kramers, Slater [1924], reprinted in Waerden [1967], see also A.Pais [1982], [1991].


39. For a detailed historical account of the development of a model of the atom, from the time of the Greeks to the introduction of Schrödinger’s theory, see F.Cajori [1929]. S.Weinberg [1984] provides a detailed history of the search for sub-atomic particles.

40. The main difficulty with Rutherford’s planetary model of atom was its theoretical instability: according to the laws of electromagnetics revolving electrons should emit energy and collapse into the nucleus almost immediately! For a thorough assessment of the difficulties with Rutherford atom. cf. Sin-Itiro Tomonaga [1962], pp. 90-93.

41. The papers entitled "On the Constitution of Atoms and Molecules" were published in the Philosophical Magazine and Journal of Science [1913].

42. The quotation in the text is from Bohr [1918]. The reason for choosing this paper rather than the original paper of 1913 for citation is that Bohr’s [1918] was a major paper, summing up his work until then in a more systematic way.

In his paper of 1913, Bohr put his basic ideas in this way:

"Returning to the simple case of an electron and a positive nucleus ..., let us assume that the electron at the beginning of the interaction with the nucleus was at a great distance apart from the nucleus, and had no sensible velocity relative to the latter. Let us further assume that the electron after the interaction has taken place has settled down in a stationary orbit around the nucleus. We shall, for reasons referred to later, assume that the orbit in question is circular; this assumption will, make no alteration in the calculations for systems containing only a single electron.

"Let us now assume that, during the binding of the electron, a homogeneous radiation is emitted of a frequency \( \nu \), equal to half the frequency of revolution of the electron in its final orbit; then, from Planck’s theory, we might expect that the amount of energy emitted by the process considered is equal to \( h \nu \), where \( h \) is Planck’s constant and \( \nu \) an entire number. If we assume that the radiation emitted is homogeneous, the second assumption concerning the frequency of revolution suggests itself, since the frequency of revolution of the electron at the beginning of the emission is 0. The question, however, of the rigorous validity of both assumptions, and also of the application made of Planck’s theory, will be more closely discussed in § 3." (N.Bohr [1913], pp.4-5) Bohr’s paper of 1918 is reprinted in 'The Sources of Quantum Mechanics', Van Der Waerden [1967]. In the introductory part of this rather extended paper, which he had dedicated to "the memory of [his] venerated teacher Professor C.Crisiansen", Bohr had noted that the paper will be in four parts. However, the last part, which was supposed to "contain a general discussion of the theory of the constitution of atoms and molecules based on the application of the quantum theory to the nucleus atom", never appeared and the third part, "discussion of the questions arising in connection with the explanation of the spectra of other elements", was published only in 1922.

43. This assumption which is known as quantum condition or quantum postulate or quantizing condition leads to the quantization of angular momentum, that is to say, \( L = mrv = nh/2\pi = n\hbar \), where \( m \) is the mass of electron, \( v \) its velocity, \( r \) its radius of the circular path, and \( n \) is a positive integer which is called the quantum number. It is the same number which Bohr had shown in his [1913] paper by the Greek letter \( \tau \).

44. This assumption is usually called the frequency condition or the frequency rule. Combining his assumptions with the known laws of mechanics and electrostatics enabled Bohr to calculate the radius of an orbit characterized by the quantum number \( n \), as well as the velocity and the energy of the electron in this orbit. He argued that for an electron in a circular orbit the centripetal force must equal the Coulomb force between the nucleus and the electron, \( m\nu^2r = Ze\hbar^2 \), where \( m \) and \( e \) are the mass and the charge of electron, respectively, \( Z \) is the atomic number and \( r \) the radius of the orbit. From here, he deduced the following equation for the energy of the orbit,
\[ E_n = -\frac{2\pi^2 m^2 e^2 n^2}{\hbar^2} \quad n = 1, 2, 3, \ldots \] (E. Ikenberry [1962])


46. The concept of ‘adiabatic change’ or ‘adiabatic process’ was first developed in thermodynamics; it was applied to processes during which no heat flows into or out of the system. Because the flow of heat is slow, any process can be made adiabatic if it is performed quickly enough. This idea was later on extended to the realm of mechanics, to such processes during which the motions determining coordinates of the system were not directly affected by the external forces.

Ehrenfest applied this same idea to the quantum realm. In a paper published in 1913, he said, "If a system be affected in a reversible adiabatic way, allowed motions are transformed into allowed motions". Ehrenfest’s starting point was Wien’s formula. In a later paper, [1916], he argued that the reason for the validity of Wien’s formula (which had been wholly derived from classical foundations) in the "region of the quanta", has been the fact that this formula, in essence, relates two adiabatic invariants, namely \( \varepsilon / \nu \) and \( v/T \), where \( \varepsilon \) is the energy of each proper vibration of frequency \( \nu \), and \( T \) is the temperature. The validity of Wien’s formula in quantum theory was assured because the ratio \( \varepsilon / \nu \) for the sinusoidal vibrations before the adiabatic change equals the ratio \( \varepsilon' / \nu' \) for the modified sinusoidal vibrations after the adiabatic change. Referring to these states as ‘undeformed’ and ‘deformed’ states or motions respectively, Ehrenfest pointed out that, "Wien’s law implied that in the course of an adiabatic transformation an allowed (or stationary) undeformed motion changes into an allowed deformed motion while the adiabatic invariant retains its initial value." (cf. Jammer [1966], p.98).

Bohr, in his famous paper of 1918, put Ehrenfest’s adiabatic principle (which he dubbed the principle of mechanical transformability) into good use (see next note). The name adiabatic hypothesis is due to Einstein, as Ehrenfest states in his 1916 paper.

For an abridged version of Ehrenfest influential paper of 1916, see Van Der Waerden [1967], pp.79-94.

47. Having the adiabatic principle in mind, Bohr in his paper of [1918] wrote: "... In the above considerations we have by an atomic system tacitly understood a number of electrified particles which move in a field of force which, ... possesses a potential depending only on the position of the particles. This may more accurately be denoted as a system under constant external conditions, and the question next arises about the variation in the stationary states which may be expected to take place during a variation of the external conditions, e.g. when exposing the atomic system to some variable external field of force.

Now, in general, we must obviously assume that this variation cannot be calculated by ordinary mechanics, no more than the transition between two different stationary states corresponding to the constant external condition. If, however, the variations of the external condition is very slow, we may from the necessary stability of the stationary states expect that the motion of the system at any given moment during the variation will differ only very little from the motion in a stationary state corresponding to the instantaneous external conditions. If now, moreover, the variation is performed at a constant or very slowly changing rate, the force to which the particles of the system will be exposed will not differ at any moment from those to which they would be exposed if we imagine that the external forces arise from a number of slowly moving additional particles which together with the original system form a system in a stationary state.

From this point of view it seems therefore natural to assume that, with the approximation mentioned, the motion of an atomic system in the stationary states can be calculated by direct application of ordinary mechanics, not only under constant external conditions, but in general also during a slow and uniform variation of these conditions. This assumption, which may be denoted as the principle of the ‘mechanical transformability’ of the stationary states, has been introduced in the quantum theory by Ehrenfest and is, ... , of great importance in the discussion of the conditions to be used to fix the stationary states of an atomic system among the continuous multitude of mechanically possible notions.” Bohr [1918], reprinted in Van Der Warden (ed) [1967], pp.101-2.

48. In his first paper in Phil. Mag. 26, [1913], Bohr, discussing the spectrum of Hydrogen atom (pp. 8-9), derived the following formula for the frequency of the radiation corresponding to the jump of electron from \( \tau_2 \) to \( \tau_1 \).
\[ v = 2 \pi^2 m e^4 \left( \frac{1}{\pi_2^2} - \frac{1}{\pi_1^2} \right) h \]

and he proceeded: "We see that this expression accounts for the law connecting the lines in the spectrum of hydrogen. If we put \( \tau_2 = 2 \) and let \( \tau_1 \) vary, we get the ordinary Balmer series. ... " (For Balmer series see note 51 below)

On page 15 of the same paper, continuing his general considerations of the relations derived in the paper, Bohr stated that, "... we shall for a moment return to the question of the significance of the agreement between the observed and calculated values of the constant entering in the [above] expression for the Balmer series of hydrogen spectrum. From the above consideration it will follow that, taking the starting point in the form of the law of the hydrogen spectrum and assuming that the different lines correspond to a homogeneous radiation emitted during the passage between different stationary states, we shall arrive at exactly the same expression for the constant as that given in [above], if we only assume 1) that the radiation is sent out in quanta \( h \nu \), and 2) that the frequency of the radiation emitted during the passing of the system between successive stationary states will coincide with the frequency of revolution of the electron in the region of slow vibrations."

Bohr regarded this asymptotic relation between classical electrodynamic and the quantum theory as 'the most beautiful analogy'. He continued alluding to this principle in his [1918] paper (see next note). The name 'Korrespondenzprinzip', however, first appeared in a paper published in 1920. In a later paper published in 1923, Bohr discussed anew the fundamental principles of quantum theory in connection with the principle of correspondence. For details cf. Van Der Waerden [1967]. See also D.Murdoch [1987, p.38].

49. In the high quantum number region, the quantized energies and orbits are so close that they may be treated as being continuous. As a result, classical mechanics may be valid. Thus, there exists a correspondence between the classical and the quantum laws in a region where \( n \) is large enough to be regarded as a continuous variable, and the quantum laws in the high quantum number region may be guessed at from the classical laws. Once the quantum laws in the high quantum number region are obtained in this way, they are assumed to apply equally well in the low quantum number region; a complete quantum theory may thus be established.

In the second section of his famous paper of 1918, Bohr notes that, as far as the frequencies are concerned, there exists a close relation between the ordinary theory of radiation and the new theory for large quantum numbers \( n \). He then proceeds, "In order to obtain the necessary connection, mentioned in the former section, to the ordinary theory of radiation in the limit of the slow vibrations, we must further claim that a relation, as that just proved for the frequencies, will in the limit of large \( n \), hold also for the intensities of the different lines in the spectrum. Since now on the ordinary electrodynamics the intensities of the radiations ... are directly determined from the coefficients \( C_n \), ... we must therefore expect that for large values of \( n \) these coefficients will on the quantum theory determine the probability of spontaneous transition from a given stationary state for which \( n = n' \) to a neighbouring state for \( n = n' = n - \tau \)." (Bohr [1918] reprinted in Waerden [1967], p. 110.)

This close relation between the classical and the quantum theory in the limit of large quantum numbers is what Bohr intended by the principle of correspondence. The developed form of the principle, phrased in a more modern language, can be put like this:

1) the predictions of the quantum theory for the behaviour of any physical system must correspond to the prediction of classical physics in the limit in which the quantum numbers, \( n \), specifying the state of the system, become very large.

2) A selection rule [i.e. the rule which specify the transitions between allowed orbits, namely \( n_{de} - n_{ge} = \pm 1 \)] holds true over the entire range of quantum number concerned. Thus any selection rules which are required to obtain the required correspondence in the classical limit (large \( n \)) also apply in the quantum limit (small \( n \)). (op.cit Eisberg & Resnick [1974], p.128).

50. Bohr's assumptions did a thorough job of mixing classical and non-classical physics in a rather ad-hoc or arbitrary manner. For example, the very hypothesis that only circular orbits are allowed is arbitrary. Moreover, that an electron moving in a circular orbit is assumed to obey classical mechanics while having quantized angular momentum, also seems to be ad-hoc. Likewise, that the electron was assumed to obey one feature of classical electromagnetic theory (Coulomb's law) and yet not to obey another feature
51. In Bohr’s theory, the interaction between matter and radiation remained mysterious. Why does not the atom emit radiation, when it is in the ground state? What really happens when an atom passes from one stationary state to another? What laws determine the probabilities of these transitions? These were some of the most relevant questions for which the theory had no explanation. For a discussion of the shortcomings of Bohr’s initial theory cf. L.de Broglie [1953], pp.141-4.

52. Work on atomic spectra was one of the thriving branches of physics and later chemistry in late 19th century. As a result of numerous experimental efforts lots of data had been gathered which required organization as well as theoretical explanation. Before Bohr’s model a number of empirical formulations had been suggested by different people. These formulae would account for the data in certain ranges. The following table represents the more established formulae for the spectral lines of Hydrogen atom:

<table>
<thead>
<tr>
<th>Names,</th>
<th>Wave length</th>
<th>formula</th>
<th>n = 2,3,4, ...</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lyman</td>
<td>Ultraviolet</td>
<td>$\kappa = R_H(1/n^2-1/n^3)$</td>
<td>n= 3,4,5, ...</td>
</tr>
<tr>
<td>Balmer</td>
<td>Near ultraviolet &amp; visible</td>
<td>$\kappa = R_H(1/2^2-1/n^3)$</td>
<td>n= 4,5,6, ...</td>
</tr>
<tr>
<td>Paschen</td>
<td>Infrared</td>
<td>$\kappa = R_H(1/3^2-1/n^3)$</td>
<td>n= 5,6,7, ...</td>
</tr>
<tr>
<td>Brackett</td>
<td>Infrared</td>
<td>$\kappa = R_H(1/4^2-1/n^3)$</td>
<td>n= 6,7,8, ...</td>
</tr>
<tr>
<td>Pfund</td>
<td>Infrared</td>
<td>$\kappa = R_H(1/5^2-1/n^3)$</td>
<td></td>
</tr>
</tbody>
</table>

In above formulae, $\kappa$ is the wave number which is the reciprocal of wave length $1/\lambda$, and $R_H$ is Rydberg constant which is equal to 10967757.6 ± 1.2 m⁻¹.

For alkali element atoms (Li, Na, K, ...) the series formula (Rydberg formula) has the same general structure:

$$\kappa = 1/\lambda = R[1/(m-a)^2 \cdot 1/(n-b)^2]$$

where $R$ is the Rydberg constant and $a$ and $b$ are constant for each series. (See Eisberg & Resnick [1974])

The following diagram depicts the energy level of the hydrogen atom:
53. In 1896 Charles Pickering from Harvard had observed a series of lines in the spectrum of starlight which he attributed to hydrogen, although this could only be accounted for by a modified form of Balmer's formula, namely, \( \kappa = cR(1/2 \lambda - 1/(m+1/2)) \). Bohr pointed out that "we can account naturally for these lines if we ascribe them to helium, singly ionized helium, that is one electron system with \( Z = 2 \)." He further showed that the modified Balmer's formula and some others can be generalized by the following formulæ: \( \kappa = 4cR(1/4 - 1/k^2) \) and \( \kappa = 4cR(1/3 - 1/k^2) \). Bohr's formulæ produced data which were in his own words "in exact agreement with the experimental value". cf. A. Pais [1991] p.149, and Jammer [1966], pp.82-4.

54. Sommerfeld augmented Bohr's theory in two ways. In the first place, he allowed for the elliptical orbits and thus removed the restriction imposed by Bohr on his own model in which electron was allowed to move only in circular stationary states. For elliptical orbits there are two generalized momenta: the radial momentum \( p^r \) and the angular momentum \( p^\theta \). There are also two quantum numbers, namely, radial \( n' \) and azimuthal \( n^\theta \). The energies of the elliptical orbits are,

\[ E = - \frac{2\pi^2 m^2 Z^2 e^2}{(n' + n^\theta)^2}. \]

By regarding \( n = n' + n^\theta \) as the principal quantum number, the above formula will become identical to Bohr's result (note 43 above).

Sommerfeld's next step was to introduce relativity into the quantum theory. According to the special theory of relativity, the mass of a particle increases with its velocity, \( M = M_0 / \sqrt{1-(v/c)^2} \). The velocity of the electron in the first Bohr orbit is \( c/137 \). The corresponding change of mass is small but detectable. This change of mass gives rise to small changes of orbit; a detailed mathematical analysis shows that the orbit is no longer a closed curve but may be approximated by a precessing ellipse, similar to the advance of perihelion of the planetary orbits. Sommerfeld showed that the energies of the \( n \) 'ellipses' with the same principal quantum number \( n \) are no longer the same. Thus each level in the energy diagram of the hydrogen atom splits into a number of closely spaced levels: the ground state does not split, the first excited state \( (n = 2) \) splits into two, the \( n^n \) level into \( n \) levels. As a result each line in the spectrum splits into a number of lines of nearly the same frequency. Sommerfeld was able to account for this spectrum split (which is known as the fine structure of spectral lines) in hydrogen atom. For details cf. Fong [1962]

55. Lorentz, on the basis of his electronic theory, had predicted that if a radiating atom were placed in a magnetic field, each spectral line which the atom emitted in the absence of the field would be split into three components.

This phenomenon, experimentally established by Zeeman in 1897, is called the normal Zeeman effect, as against the anomalous Zeeman effect. The former phenomenon occurs only when the line is a singlet. The anomalous effect however, occurs whenever the split lines belong to a group of multiplets. In this case usually more than three components appear which are different from those of normal one.

Sommerfeld's augmented model could only account for the normal Zeeman effect. In doing so he introduced space quantization, i.e., the orientation of an elliptical orbit in space is quantized and can take
on only a finite number of specified orientations with respect to an external field. cf. d'Abro [1951]

56. In 1913 Stark discovered that an electric field splits the lines of the Balmer series into a number of components. In other words, under the influence of the electric field, additional periodicity appears in the electron's motion, and the number of energy levels (and therefore the number of spectral lines) increases. While the normal Zeeman effect can be explained by classical theory, the Stark effect cannot. Therefore, the success in explaining the Stark effect was considered a major triumph of the old quantum theory. op. cit. d'Abro.

57. Bohr fully recognized the imperfections of his theory and reckoned that, at best, it could only represent a temporary stage before a more consistent theory was found. In 1921 Bohr observed: "the incomplete character of the theory can be recognized in two obvious ways – not only in the working out of individual details, but also in connection with the grounding of a general point of view." (Quoted from J.Honner [1987], p.36.)

A.Pais in his [1991] notes that "there are hundreds of pages of unpublished notes in the Bohr Archives ... [which reflect] Bohr's deep conviction that all of [the old] quantum physics should for the time being be considered provisional." (p.178) In fact he was much more cautious and critical of the type of quantum theory based on models that existed before 1924, than other distinguished physicists, such as Sommerfeld. W.Heisenberg in his [1967, p.95] describing his first serious discussion with Bohr notes that, "For the first time I understood that Bohr's view of his theory was much more sceptical than that of many other physicists – e.g. Sommerfeld – at that time, ..."

F.C.Hoyt in his interview with T.Kuhn has remarked that, "He [i.e. Bohr] thought that in every fine point that came up Sommerfeld was wrong." (ibid)

58. cf. note 49.

59. One can refer to collision phenomena as a large group of non-periodic phenomena. The old quantum theory could shed no light on these types of phenomena. Thus, for example, while Franck's and Hertz's experiment, (dealing with collisions between electrons and mercury atoms), provided experimental confirmation for Bohr's postulate of the discrete nature of bound-state energy levels of atoms, the theory could not predict the trajectory actually taken by electrons or describe in detail the inelastic nature of the collision of electrons and the target atoms. In their 1914 experiment, Franck and Hertz observed that if the electron kinetic energy is less than 4.9 eV, the collisions are elastic. When the energy reaches 4.9 eV many collisions become completely inelastic, the electron gives up its entire kinetic energy to the atom. Above 4.9 eV many electrons still give 4.9 eV to the atom, then continue with an energy less by that amount. For details see A.Messiah [1962]

60. See note 54 above.

61. Although in his paper of July 1925 Heisenberg had hit on the main idea of non-commutative quantities and had unwittingly used the matrix method, he was not as yet aware of the existence of calculus of matrices previously discovered by the English mathematician A.Cayley. He gave his paper to Born, asking him to submit it for publication if he approved. It was Born who invoked the matrix calculus for formulating Heisenberg's ideas. See note 64.
The basic idea of the matrix mechanics is that the co-ordinates and momenta associated with the electrons in the atoms are to be treated as matrices rather than as quantities having definite numerical values. The matrices involved in quantum mechanics are infinite, i.e., having an infinite number of rows and columns. The Heisenberg matrix elements are quantities which are intimately connected with the concept of transition probability in the old quantum theory, and alternatively with Fourier coefficients in classical mechanics. A complete version of Heisenberg paper can be found in van der Waerden [1967]. For a thorough account of Heisenberg's discovery of matrix mechanics see Mehra & Rechenberg [1982, vol.2]. Heisenberg himself has given vivid accounts of how he succeeded in developing the basic ideas of matrix mechanics in his [1958] and [1974].

62. "The present paper seeks to establish a basis for a theoretical quantum mechanics founded exclusively upon relationships between quantities which in principle are observable.

It is well known that the formal rules which are used in quantum theory for calculating observable quantities such as the energy of the hydrogen atom may be criticized on the grounds that they contain, as basic element, relationships between quantities that are apparently unobservable in principle, e.g., position and period of revolution of the electron. ... In this situation it seems sensible to discard all hope of observing hitherto unobservable quantities, ... . Instead it seems more reasonable to try to establish a theoretical quantum mechanics, analogous to classical mechanics, but in which only relations between observable quantities occur." (Heisenberg, op.cit. note 60, reprinted in Waerden [1967], p.262. italics in original, emphasis added)

63. As we shall see (section 3 below), Bohr played a major rôle in importing this positivistic tendency into the Copenhagen interpretation. His move towards an anti-realistic interpretation of quantum mechanics, was gradual and not without intellectual efforts to preserve realism at the micro level. However, it seems his approach right from the beginning has been more of a problem-solving (in a phenomenological fashion) than theoretical understanding. In fact, it can be argued that Bohr's very first papers (his trilogy), most probably unbeknown to him, paved the way for all the subsequent positivist ingredients built into the orthodox quantum mechanics.

In his first paper he had implied that it is meaningless to talk of the status of electron in the stationary state or during its probabilistic jumps. The gist of Bohr's implicit argument was that inside certain intervals, measurements cannot be carried out, therefore it would be meaningless to ascribe definite status to electron in theses intervals or to demand definite values for its dynamical attributes (e.g., position, momentum). This enthusiasm for solving practical problems, was, perhaps under the pressure for accounting for numerous empirical data, consolidated among all members of the Copenhagen school in later years.

It is however interesting that both Bohr and Heisenberg in a number of publications have tried to reject the charge of positivism. For example, in his [1958, p.133] Heisenberg writes, "It should be noticed at this point that the Copenhagen interpretation of quantum theory is in no way positivistic. For, whereas positivism is based on the sensual perceptions of the observer as the element of reality, the Copenhagen interpretation regards things and processes which are describable in terms of classical concepts, i.e., the actual, as the foundation of any physical interpretation." However, as it is clear from this very quotation, and as we shall further discuss in the text, Heisenberg is defining positivism in a very narrow sense.

The insistence of the Copenhagen interpretation to adhere to observable quantities, places it comfortably in the camp of positivists and instrumentalists. P.Feyerabend, while still a realist, made this remark on Heisenberg's claim, "This [Heisenberg's claim] is true. However this "foundation" is again assumed to be "given" in the sense that it cannot be further analyzed or explained, an attitude which to a certain extent still justifies the term "positivism"." (Feyerabend [1981], p.226) See also Bohr's criticisms of positivism in his dialogue with Heisenberg and Pauli, in Heisenberg [1971, pp.205-217].

According to Bohm and Groenewold in private communication with Feyerabend [1962, p.259], Bohr has always claimed to be a realist and has been somewhat critical of Heisenberg's positivism. Feyerabend however, having assessed Bohr's various comments, has concluded that Bohr's point of view can be christened as a "positivism of a higher order." ([1958], p.82, italics in original)

Some writers maintain that apart from Bohr, Born, who was Heisenberg supervisor for a while, was also responsible for encouraging Heisenberg towards taking a more positivistic approach to the quantum realm. Encyclopedia Britannica [1964] has put the point in this way, "The impetus to this approach was Born's repeated emphasis that the reason the old quantum theory was then (1925) failing was that it sought to use the same kinematical concepts of space and time within the atom as in ordinary measurable large-scale
events. After all, the concepts of space and time have a meaning only when we tell how they can be measured, and obviously at atomic distances we cannot use ordinary measuring rods or clocks. Guided by this philosophy based on the so-called operational viewpoint, Heisenberg discovered the matrix mechanics. (1964, p.926)

64. The old quantum theory had relied upon a whole set of notions for which there were no experimental foundation. The notion of electronic orbit is an example of such a notion. This serious shortcoming was one of the main reasons for Heisenberg and his colleagues such as Born and Jordan to develop their matrix mechanics by starting exclusively from physically observable quantities such as the frequencies and intensities of the radiation emitted by atoms. In matrix theory each observable quantity is associated with a matrix. The equations of motion of the dynamical variables of a quantized system are thus equations between matrices. Following the correspondence principle, the authors of the theory assumed that these equations are formally similar to the equations of the corresponding classical system, with the difference that the quantum quantities, contrary to the classical quantities, obey a non-commutative algebra.

Born, in his Nobel prize lecture, has referred to this development in this way: "In Göttingen we also took part in the attempt to distil the unknown mechanics of the atom out of the experimental results. The logical difficulty became ever more acute. ... The art of guessing correct formulas, which depart from the classical formulas but pass over into them in the sense of correspondence principle, was brought to considerable perfection. ... This period was brought to a sudden end by Heisenberg, who was my assistant at the time. He cut the Gordian knot by a philosophical principle and replaced guesswork by a mathematical rule. The principle asserts that concepts and pictures that do not correspond to physically observable facts should not be used in theoretical description. When Einstein, in setting up his theory of relativity, eliminated the events at different places, he was making use of the same principle. Heisenberg banished the picture of electron orbits with definite radii and periods of rotation, because these quantities are not observable." (M.Born [1954] quoted in Born [1970], p.91. italics added.)

65. Born, in his Noble prize lecture, has noted that: "[In his paper] he [Heisenberg] demanded that the theory should be built up by means of quadratic arrays .... Instead of describing the motion by giving a co-ordinate as a function of time x(t), one ought to determine an array of transition probabilities x^\prime. To me the decisive part in his work is the requirement that one must find a rule whereby from a given array

\[ x_{11}, x_{12} \ldots \]
\[ x_{21}, x_{22} \ldots \]

the array for the square,

\[ (x^2)_{11}, (x^2)_{12} \ldots \]
\[ (x^2)_{21}, (x^2)_{22} \ldots \]

may be found (or, in general, the multiplication law of such arrays).

By consideration of known examples discovered by guesswork he found this rule and applied it with success to simple examples such as the harmonic and an-harmonic oscillator. This was in summer 1925. ... The significance of the idea was immediately clear to me, and I sent the manuscript to the Zeitschrift für Physik. Heisenberg’s rule of multiplication left me no peace, and after a week of intensive thought and trial, I suddenly remembered an algebraic theory that I had learned from my teacher, Rosanes, in Breslau. Such quadratic arrays are quite familiar to mathematicians and are called matrices, in association with a definite rule of multiplication. I applied this rule to Heisenberg’s quantum condition and found that it agreed for the diagonal elements. It was easy to guess what the remaining elements must be, namely, null; and immediately there stood before me the strange formula \( pq - qp = \hbar/2\pi \). This meant that co-ordinates p and momenta \( p \) are not to be represented by the values of numbers but by symbols whose product depends on the order of multiplication – which do not ‘commute,’ as we say.” Born [1970], pp.91-2.

67. The similarity of motions in corpuscular mechanics and wave propagation, (namely, the Maupertuis’s Principle of Least Action which states that from among all the possible trajectories a particle could take between A and B in the given interval of time, the actual path is that for which the action = momentum x distance is minimum, and the Fermat’s Principle of Least Time which states that light moves between two points by the route that is quicker than any other routes nearby), strongly suggested to de Broglie that there might exist a close relation between the concepts of particles and wave.

Since radiation was known to have a dual property that is like a wave in interference and diffraction phenomena, and like a particle in photoelectric and Compton effects, de Broglie reasoned that matter might also have a dual property. A wave of frequency \( \nu \) and wavelength \( \lambda \) might be assumed to be associated with a moving particle of energy \( E \) and momentum \( p \). In strict analogy to the Einstein relations for a photon, namely \( (E = h\nu, p = h/\lambda) \), he assumed the similar relations between \( \nu, \lambda, E, p \) to be \( E = h\nu, p = h/\lambda \). These are known as the de Broglie relations.

While dual property of light had been well established by 1924, the dual property of matter was merely a bold speculation at that time. This speculation however, was experimentally verified independently and in two different ways in 1927. Davisson and Germer showed the wave properties of matter by investigating the reflection of electrons from the face of a crystal. G.P. Thomson showed the same properties by analysing the transmission of electrons through a thin foil of polycrystalline material.

68. Schrödinger conjectured that quantum mechanics stands in the same relation to ordinary classical mechanics that physical optics does to geometrical optics. Just as the ray-tracing characteristics of the latter fail in the explaining the phenomena of diffraction and interference, so ordinary mechanics cannot explain atomic phenomena, the reason in each case being that the dimensions are not large compared to the wavelength. Hence he sought to establish a procedure analogous to that used in physical optics. (cf. Schrödinger [1928], First lecture)

69. In an attempt to resolve the wave-particle problem, Schrödinger conceived of the possibility of a particle being in reality a bunch of waves, or a wave packet. (See note 72). A wave packet is a mixture (superposed) of different wavelengths. Heisenberg [1930, p.13] has defined the wave packet in this way, "By wave packet is meant a wavelike disturbance whose amplitude is appreciably different from zero only in a bounded region. This region is, in general, in motion, and also changes its size and shape, i.e., the disturbance spreads."

70. Heisenberg’s matrix mechanics stemmed from the particle dynamics of Hamilton (Hamiltonian dynamics), whereas Schrödinger’s wave mechanics was based on Hamilton’s wave dynamics. In the case of classical physics, the Hamiltonian \( H \) for a ‘one dimensional’ particle of mass \( m \) moving in a conservative field of total energy \( E \) can be written as \( H = P^2/2m + V(x) = E \), where \( P \) is the momentum and \( V(x) \) is the potential energy. Schrödinger suggested replacing the independent variable by itself and also replacing the momentum \( P \) by the differential operator -i\( \hbar \)d/dx. A quantum mechanical Hamiltonian \( \mathcal{H} \) is then defined by \( \mathcal{H} = -\hbar^2/2m \cdot d^2/dx^2 + V(x) \). Here \( \mathcal{H} \) is an operator. The Schrödinger time-independent equation can be written, \( \mathcal{H}\psi = E\psi. \)

Schrödinger also realized that the main result of Heisenberg’s theory, namely, \( P_x x - xP_x = -i\hbar \) follows in this way; \( P_x = -i\hbar d/dx \) and \( xP_x = -i\hbar d/dx \).

In August 1926, Dirac showed how Schrödinger’s extraction of the Heisenberg matrices could be performed in a simpler and more comprehensive way. He thus produced an equivalence between the two formalism by showing that they are two particular formulations of a theory which can be presented in very general terms.

The equivalence between the formalism of quantum mechanics gained further clarification when John von Neumann, a few years later, showed that quantum mechanics can be formalized as a calculus of Hermitian operators in Hilbert space and that the theories of Heisenberg and Schrödinger are merely particular cases of this calculus.


72. Schrödinger believed that point particles did not exist. He felt that the underlying reality was a continuously distributed field or wave, whose properties and behaviour allowed one to sometimes get the impression that particles existed. His view was compatible with the fact that the continuous classical
The electromagnetic field possesses inertial and mechanical properties.

The Schrödinger matter wave was a true classical continuous wave. He regarded electron as a continuous distribution of charge, the density of which \( \rho \), was related to the wave amplitude \( \psi \), by the relation \( \rho = |\psi|^2 \). This interpretation would imply a completely deterministic behaviour for the waves.

Schrödinger's interpretation was tenable only as long as \( \psi \) would remain confined within an atom. In free space however, according to Schrödinger's equation, the wave must spread out rapidly over all space without limit. On the other hand, the electron is always actually found within a comparatively small region of space, so that its charge density cannot in general be equal to \( \rho = |\psi|^2 \).

73. Contrary to Schrödinger's view, Born believed that point particles were the basic reality and that \( \psi \) wave was only a measure of where a particle was likely to found.

74. In his Nobel prize lecture, entitled,"Statistical Interpretation Of Quantum Mechanics", Born has noted that: "Wave mechanics enjoyed much greater popularity than the Göttingen or Cambridge version of quantum mechanics. Wave mechanics operates with a wave function \( \psi \), which — at least in the case of one particle — can be pictured in space, and it employs the mathematical methods of partial differential equations familiar to every physicist. Schrödinger also believed that his wave theory made possible a return to deterministic classical physics; he proposed ... to abandon the particle picture entirely and to speak of electrons not as particles but as a continuous density distribution \( |\psi|^2 \), or electric density \( e |\psi|^2 \).

To us in Göttingen this interpretation appeared unacceptable in the face of the experimental facts. At that time it was not possible to arrive at a clear interpretation of the \( \psi \) function by considering bound electrons. I had therefore been at pains, as early as the end of 1925, to extend the matrix method, which obviously covered only oscillatory processes, in such a way as to be applicable to aperiodic processes. I was at that time the guest of the Massachusetts Institute of Technology in the U.S.A., and there I found in Norbert Wiener a distinguished collaborator. In our joint paper we replaced the matrix by the general concept of an operator and, in this way, made possible the description of aperiodic processes. Yet we missed the true approach, which was reserved for Schrödinger; I immediately took up his method, since it promised to lead to an interpretation of the \( \psi \) function. Once more an idea of Einstein's gave the lead. He had sought to make the duality of particles (light quanta or photons) and waves comprehensible by interpreting the square of the optical wave amplitudes as probability density for the occurrence of photons. This idea could at once be extended to the \( \psi \) function: \( |\psi|^2 \) must represent the probability destiny for electrons (or other particles)."

M.Born [1970], p.94. italics added.

75. Quoted from A.Pais [1991], p.286.

Einstein, however, as we shall see in the text, was not prepared to endorse this view concerning causality and determinism.

76. Heisenberg [1927] reprinted in Wheeler & Zurek [1983]. In the abstract of his paper Heisenberg states that, "First we define the terms velocity, energy, etc. (for example, for an electron) which remain valid in quantum mechanics. It is shown that canonically conjugate quantities can be determined simultaneously only with a characteristic indeterminacy. This indeterminism is the real basis for the occurrence of statistical relations in quantum mechanics. Its mathematical formulation is given by the Dirac-Jordan theory. Starting from the basic principles thus obtained, we show how microscopic processes can be understood by way of quantum mechanics. To illustrate the theory, a few special gedankenexperiments are discussed."

A succinct account of this principle can be found in Heisenberg [1930].


"We turn now to the concept of 'path of the electron'. By path we understand a series of points in space (in a given reference system) which the electron takes as 'positions' one after the other. As we already know what is to be understood by 'position at a definite time', no new difficulties occur here. Nevertheless, it is easy to recognize that, for example, the often used expression, the '1s orbit of the electron in the"
hydrogen atom’s, from our point of view has no sense. In order to measure this 1s ‘path’ we have to illuminate the atom with light whose wavelength is considerably shorter than 10⁻⁶ cm. However, a single photon of such light is enough to eject the electron completely from its ‘path’ (so that only a single point of such a path can be defined). Therefore here the word ‘path’ has no definable meaning.” (ibid. p.65, italics added)

78. D.Murdoch [1987, p.47].

79. In 1909 Einstein was asked to give a talk at the Salzburg meeting of the German Physical Society. Although he was expected to discuss his theory of relativity, he started with the Planck law and instead of calculating the entropy as he had done before, he calculated the mean square fluctuation of the energy per unit volume of radiation with frequency between \( \nu + \Delta \nu \), namely, \(<(\nu - \langle \nu \rangle)^2>\). Einstein showed that this term consists of two parts. One was of the form to be expected from the fluctuation in the number of particles per unit volume in a gas or in any set of independently moving particles. The other was a new term and was shown to be of the form expected for the interference of waves of this frequency with random amplitudes and phases. This analysis, which shows the simultaneous presence of wave-like and particle-like fluctuations in the energy density of black-body radiation, was the first clear statement of wave-particle duality. For details see P.Stehle [1994], p.141. See also note 28 above.

The wave particle duality can be best represented by the so-called two-slit experiment. When photons or elementary particles (e.g. electrons) from the source impinge upon the first screen, the distribution pattern on the second screen will be radically different if only one of the two slits (rather than both) be open. In the first case, the distribution pattern shows a particle-like behaviour, whereas in the second case it suggests a wave-like interference.

80. In May 1926, Heisenberg, for the second time, came to Copenhagen to succeed Kramers as lektor. He stayed there until June 1927. During this period, one of the major problems which was constantly debated between him and Bohr, was the wave-particle duality. The two principles of uncertainty and complementarity are the fruits of the (sometimes heated) discussions of this rather short time span. As Heisenberg has recollected, "[During these discussions], we discovered that the two of us were trying to resolve the difficulties in rather two different ways. ... He [Bohr] was not so much interested in a special mathematical scheme. Especially he was not so willing to say, 'Well, let us take for instance matrix mechanics and let's just work that out, then we must find all the right answers'. He rather felt, 'Well, there is one mathematical tool — that's matrix mechanics. There is another one — that's wave mechanics. And there may still be other ones. But we must first come to the bottom in the philosophical interpretation.'" Heisenberg [1963] quoted from Pais [1991], p.302.

The difference in style, prevented them from coming to a common conclusion, "In the end, shortly after Christmas, we both were in a kind of despair. In some way we couldn't agree and so we were a bit angry about it. ... Both of us became utterly exhausted and rather tense. Hence Bohr decided in February 1927 to go skiing in Norway, and I was quite glad to be left behind in Copenhagen, where I could think undisturbed about the hopelessly complicated problems." Heisenberg [1971], p.77.

During this period, Heisenberg developed his uncertainty principle while Bohr was groping with his (would be) complementarity principle. After his return from Norway, in March and spending some months
with Heisenberg, trying to crystallize his views concerning the principle of complementarity, he was at last able to present the first full version of this principle at the Como conference on 16 September 1927.

81. Bohr [1927] reprinted in Wheeler & Zurek [1983], pp.89-91. Bohr’s lecture did not impress the physicists at the conference. Léon Rosenfeld later said about it, "There was a characteristic remark by Wigner after the Como lecture, ‘This lecture will not induce any one of us to change his own meaning about quantum mechanics’. " See Pais [1991, p.315]. It also should be emphasised that in the course of time Bohr tried again and again to further elaborate his conception concerning the exact meaning of complementarity. On one occasion he even changed the term to reciprocity, thinking that it is "more efficacious and pedagogical". However, a month later he declared that the new name to be a blunder. (ibid. p.426). We shall further discuss in the text the significance of this principle for Bohr’s philosophy.

Heisenberg in his [1930, p.65] has summed up his discussion of Bohr’s complementarity principle in the following diagrammatic way:

<table>
<thead>
<tr>
<th>CLASSICAL THEORY</th>
<th>QUANTUM THEORY</th>
</tr>
</thead>
<tbody>
<tr>
<td>Causal Relationships of Phenomena</td>
<td>Phenomena described in terms of space and time</td>
</tr>
<tr>
<td>Described in Terms of Space and Time</td>
<td>(Alternatives related statistically)</td>
</tr>
<tr>
<td>Either</td>
<td>Or</td>
</tr>
<tr>
<td>Phenomena described in terms of space and time</td>
<td>Causal relationships expressed by mathematical laws</td>
</tr>
<tr>
<td>But</td>
<td>But</td>
</tr>
<tr>
<td>Uncertainty Principle</td>
<td>Physical description of phenomena in space-time impossible</td>
</tr>
</tbody>
</table>

82. See M.Redhead [1987], pp.49-51. The main postulates of the Copenhagen interpretation can be summarized as follows;

I. The Completeness principle: quantum mechanical wave function gives a complete specification of what can be known concerning quantum states.

II. The Superposition principle: a linear combination of two quantum states is itself a quantum state.

III. The Uncertainty principle: observables represented by noncommuting operators cannot simultaneously be measured with arbitrary exactness.

IV. The Probability Interpretation: the amplitude of wave function corresponds to a probability amplitude and its absolute square corresponds to a probability density.

V. The principle of inseparability: in quantum mechanics, the object under investigation and the apparatus for making measurement make an in-principle inseparable ensemble.

VI. The principle of Complementarity: the wave and particle models are complementary. Which model we use is determined by the nature of measurement.

VII. The Correspondence Principle: quantum mechanics must converge to classical mechanics in the limit where \( n \) (quantum number) is large. (Quoted from M.Stuart [1991]) Stuart has claimed that the Copenhagen interpretation is logically inconsistent. In his view the following pairs of postulates are logically incompatible and contradictory, \((V,VI),(IV,VI),(IV,V),(II,V)\), and \((I,V)\). Strong logical inconsistency is certainly not approved by many of other critics of the OQM. We shall discuss the shortcomings of the Copenhagen interpretation in the next section.

83. cf. M.Jammer [1974].

84. Or alternatively the Orthodox Quantum Mechanics (OQM).

85. It should be emphasised that despite the frequent lip-services paid to the Copenhagen interpretation by those physicists who were in favour of this school, the subsequent developments of OQT were much less in the light of some of the basic postulates of this interpretation (namely, the principle of complementarity...
and the correspondence principle. (See Feyerabend [1962], p.255).

86. Quantum mechanics is dubbed as the most successful physical theory. Among its considerable achievements the following can be mentioned: a quantum theory of the chemical bonds has been formulated, a quantum theory of solid state has also been developed, quantum theory shed lights on the phenomena associated with the interaction of matter and wave, it also has predicted the existence of positron and antiproton, etc., etc.

87. In a letter to Schrödinger in 1928, Einstein wrote, "The Heisenberg - Bohr tranquillizing philosophy – or religion? – is so delicately contrived that, for the time being, it provides a gentle pillow for the true believer from which he cannot very easily be aroused. So let him lie there." Einstein [1928] reprinted in K.Przibram(ed) [1967], p.31.

For the impact of OQT on the physics community see also Selleri [1990].

88. cf. A.Rae [1986], p.53.

89. Bohr's initial model of atom, as we have seen already, solved many of the problems of the earlier models in a rather ad-hoc way. Einstein has commented on Bohr's way of resolving the difficulty of the atomic models in this way, "That this insecure and contradictory foundation was sufficient to enable a man of Bohr's unique instinct and tact to discover the major laws of the spectral lines and of the electron shells of the atoms together with their significance for chemistry appeared to me like a miracle — and appears to me as a miracle even today." (Quoted from A.Pais [1982], p.416)

90. For a discussion of the significance of upholding this postulate see R.Trigg [1980].

91. Bohr in his later years has extensively discussed the meaning and significance of the concept 'phenomenon' for the quantum physics and has tried to find a refined formulation for this term. In 1929 he wrote, "The finite magnitude of the quantum of action prevents altogether a sharp distinction being made between a phenomenon and the agency by which it is observed" (Bohr [1929], English translation 1961, p.1, his italic). This formulation, which was a reminiscent of his Como lecture and had obviously quite unpalatable epistemological consequences (see next note), was later on refined furthermore.

In 1938 Bohr put forward the following definition of the term phenomenon, "It is certainly more in accordance with the structure and interpretation of the quantum mechanical symbolism, as well as with the elementary epistemological principles, to reserve the word "phenomenon" for the comprehension of the effects observed under given experimental conditions." (Bohr [1938] reprinted in New Theories in Physics [1939], p.11)

In his contribution to Schilpp volume he ascribed the term phenomenon to the whole experimental setup, "As a more appropriate way of expression I advocated the application of the word phenomenon exclusively to refer to the observations obtained under specified circumstances, including an account of the whole experimental arrangement." (op.cit., [1949], pp.237-8)

92. In his Nobel Prize lecture, in 1932, Heisenberg stated that: "The areas of validity of classical and quantum mechanics can be marked off from each other as follows: Classical physics represents the striving to learn about Nature in which essentially we seek to draw conclusions about objective processes from observations and so ignore the consideration of influence which every observation has on the object to be observed; classical physics, therefore, has its limits at the point from which the influence of the observation on the event can no longer be ignored. Conversely, quantum mechanics makes possible the treatment of atomic processes by partially forgoing their space-time description and objectification." (Quoted in French & Kennedy [1985] 'Niels Bohr; A Centenary Volume ', p.25)

93. Feyerabend ([1962], pp.192-3) has pointed out that compatibility of the complementarity principle with experimental results should not be taken as a sign of its correctness but as a possible indicator of it being devoid of empirical content. Furthermore, the vagueness in the formulation of this principle has enabled the founders and the followers of the Copenhagen school to take care of objections by "development rather than by reformation, a procedure which will of course create the impression that the correct answer has been there all the time and that it was overlooked by the critics."
94. Bohr was unhappy about this unpalatable consequence. However, he himself was largely responsible for this undesired outcome. In his Como lecture he had said that: "Our normal [classical] description of physical phenomena is based entirely on the idea that the phenomena may be observed without disturbing them appreciably". (Bohr [1927], reprinted in Wheeler and Zurek [1983])

Schrödinger has commented on this unfortunate influence of Bohr in a letter to Sommerfeld, "... I think the influence erroneous and regrettable which he [Bohr] has himself exercised, owing to his tremendous authority on this more recent development, above all by inventing some catchwords such as complementarity, direct influence of the observer on what is observed, blurring the limit between subject and object... etc. The above mentioned complex of catchwords has been dragged on for two decades already... and [no one notices] that it has not led to any single tangible success... In this respect, I always remember Anderson’s fairy tale about the ‘King’s new cloths’." Schrödinger [1949] quoted from U.Roseberg [1990]

Bohr, later on in his life, tried to warn against drawing such a conclusion for the orthodox interpretation. For example, in his contribution to Schilpp’s volume on Einstein, after pointing out the importance of the question of terminology in the quantum realm, he writes:

"In this connection I warned especially against phrases, often found in physical literature, such as 'disturbing of phenomena by observation' or 'creating physical attributes to atomic objects by measurements' such phrases, which may serve to remind of the apparent paradoxes in quantum theory, are at the same time apt to cause confusion, since words like 'phenomena' and 'observations' just as 'attributes' and 'measurements' are used in a way hardly compatible with common language and practical definition." [1949, p.237].

However, despite these cautionary notes, Bohr could not produce a more acceptable approach which would avoid the inherent inconsistencies of the Copenhagen interpretation.


96. N.Herbert [1985, p.162] has given the rainbow as an example of a phenomenon which is objective but not an object. It is objective, e.g. it can be photographed, yet it is not an object; it has no end, and it appears in a different place for each observer.


98. The ill-conceived assumption is illustrated below; suppose quantities X and Y can be measured separately on a system, and that it is also possible to measure X+Y directly. Then the assumption was that the average value of X+Y over any collection of identical systems (ensembles) was equal to the average value of X + the average value of Y. Since, in general, the variable X+Y is of a different kind, measured by a different type of apparatus, from either X or Y, there is no reason why such an equality should hold. (See E.Squires [1986, p.78]).


100. Quoted from Feyerabend, op.cit.

101. See for example D.Bohm [1957], K.Popper [1967], [1983], B.d'Esagnat [1979/83] [1981/83].

It should be born in mind that the formalism of the theory and different gloss of that formalism are not one and the same thing. Each theory T consists of a formalism F, a set of correspondence rules R, and an explanatory model M. The correspondence principle and the model together (or if the former is included in the latter, only the model) are called the interpretation or the semantic of the theory. Now while no pure formalism can be regarded as a physical theory, the semantics which serve to link the formalism to the outside world, may turn out to be incorrect. That is to say, the entities it has postulated may be non-existent, or the envisaged correspondences may be misplaced. From a realist point of view, science by and large progresses by making the formalism F, of an empirically successful theory T, part of, or at least isomorphic with, a more comprehensive formalism F* of a more comprehensive theory T*. The more comprehensive theory T* may in turn retain part of the semantics of the older theory, or produce a totally new account of
the physical reality while providing proper explanation as to where the old interpretation has gone astray.

As we shall see below there are a number of rival interpretations for the present formalism of quantum theory. One can also find radically conflicting accounts even within the frame work of OQM, and in the name of Copenhagen interpretation itself. As M.Stuart [1991, p.601] has pointed out, Rohrlich [1983] maintains that the Copenhagen interpretation has always upheld a realist ontology, whereas Peres [1985] is of the view that the cost of realism within that interpretation is paradoxical.

It is also worth noting that in the case of the Copenhagen interpretation, that the predictive success of the theory was established before the finalization of the Copenhagen scheme.

102. The theory is not essentially about the ways in which definite kinds of physical objects evolve and interact in physical space and time irrespective of whether the objects are undergoing measurement. In fact, as we have seen, OQT requires us to presuppose the existence of measuring instruments in its basic axioms. Therefore, it cannot explain the macro-domain as arising solely as a result of interaction between micro entities. The state vector \( \| \) of OQT cannot be interpreted as specifying the actual physical state of the individual quantum system in physical space and time, because there is no solution to the wave/particle dilemma; rather \( \psi \) is to be interpreted as containing probabilistic information about the results of measuring diverse quantum observables, such as position, momentum, energy, spin.


104. Dynamic attributes are time-varying properties of a quantum object, like position, velocity, energy, and spin axis orientation. Mass, charge, and spin which do not change over time are called static attributes.

105. Quoted in N.Herbert [1985], p.159.

106. Van Fraassen [1991].

107. We have already (Ch.3) discussed van Fraassen's agnosticism as regards scientific theories in general.

108. Many writers have pointed out the deficiencies in the orthodox account of quantum mechanics. See for example, N.Maxwell who, in a number of articles (See the bibliography) has produced an impressive list of criticisms against the orthodox view. The points mentioned in the text are essentially based on his views.

109. Schrödinger time-dependent equation, \((-\hbar^2/2mV^2\psi + V\psi = i\hbar \partial \psi / \partial t)\) specifies the ways in which the quantum function \( \psi \) evolves in time deterministically, while no measurement is taking place.

110. See section 4.

111. Until the sixth Solvay conference in 1930, Einstein was trying to show the inconsistency of Copenhagen interpretation by means of a number of thought experiments. His last attempt in this respect was an ingenious one. He considered a box which has a hole in one of its walls that can be opened or closed by a shutter controlled by a clock inside the box. The box is filled with radiation. The box is being weighed. Then the shutter is opened for a small interval during which a single photon can escape the box. Now if one weighs the box, one (in principle) has found to arbitrary accuracy both the photon energy and its time for passage. This result is in conflict with the energy-time uncertainty principle.

Bohr however, used the following experimental set up to reject Einstein's objection. The initial weighing is performed by recording the position of the pointer attached to the box relative to the scale attached to the fixed frame. The loss of weight resulting from the escape of the photon is compensated by a load (hung underneath the box) that returns the pointer to its initial position with a latitude \( \Delta q \). Correspondingly, the weight measurement has an uncertainty \( \Delta m \). The added load imparts to the box a momentum which can be measured with an accuracy \( \Delta p \) delimited by \( \Delta p \Delta q = \hbar \). Now, \( \Delta p < \hbar / \Delta m \), where \( \Delta m \) is the time taken to read the mass on the scales. Then the shutter is opened for a small interval during which a single photon can escape the box. Now if one weighs the box, one (in principle) has found to arbitrary accuracy both the photon energy and its time for passage. This result is in conflict with the energy-time uncertainty principle.
search for inconsistencies in the theory and concentrated instead, on its incompleteness. Bohr has discussed this problem in his contribution to Schilpp volumes on Einstein. For the historical background to the issue cf. A.Pais [1982], ch.25.

112. In his contribution to Schilpp's volumes on Einstein Bohr has emphasised, "When invited ... to write an article for this volume in which contemporary scientists are honouring the epoch-making contributions of Albert Einstein to the progress of natural philosophy and are acknowledging the indebtedness of our whole generation for the guidance his genius has given us, I thought much of the best way of explaining how much I owe to him for inspiration." Bohr [1949], in Schilpp [1949/82], p.201. For another first hand account of Einstein's influence on Bohr, see A.Pais's recollection in S.Rozental (ed) [1967].

113. F.Selleri [1990], ch.1. see also the last chapter.

114. Fine, in his latest book, The Shaky Game [1986], despite a rather thorough research on materials and documents (some yet unpublished) concerning Einstein's work, has come to a rather unconventional conclusion, namely, that Einstein, despite his passion for realism should in fact be regarded as a constructive empiricist of the type van Fraassen claim to be: "If we understand Einstein in the way that he takes us to, his own realist-sounding language maps out a position closer to constructive empiricism than to either 'metaphysical realism ' or 'scientific realism " [1986, p.108] (italics added).

As for Bohr's philosophical inclination, Fine's advocates the traditional view that Bohr has been a positivists. See A.Fine & M.Beller [1994], "Bohr's Response to EPR", paper read at the meeting of BSPS, 17 January 1994.

115. The majority of the writers on the philosophical aspects of quantum mechanics in the past few decades have upheld the view concerning the anti-realist tendencies of the members of Copenhagen school. One early exception to this trend has been (the later) Feyerabend [1967/1981] who (contrary to his earlier views in 1950s and early 1960s), has claimed that Bohr's approach is closer to the realists like Popper.

In recent years a number of works have appeared on the scene challenging the traditional view. Among these publications, the essays by A.Shimony, and E.Mackinnon, and some recent books on N.Bohr, (e.g., J.Honner [1987] and D.Murdoch [1987] are worth mentioning.

Some of these writers, in their enthusiasms for rehabilitating those founding fathers have apparently gone too far. For example, H.Krips [1987, p.1] writes: "Neither Heisenberg nor Bohr were anti-realists in the metaphysical sense of denying the existence of an objective external reality lying behind the 'veil of perception '; nor did they eschew the scientific realist's commitment to describing that reality within science. In particular they shared with Einstein and the 'realists ' a belief in the objective reality of atoms, as well as putting forward atomic theories of matter within science."

Some other writers, (e.g. H.Folse [1986]), in their attempts to make a realist of the authors of Copenhagen version of quantum mechanics have tried to make use of more recent developments in realism/ anti-realism debates. Folse has claimed that Bohr and his colleagues should be called, (in modern jargon),
entity-realist, "His [Bohr's] position parallels Cartwright's realist defense of phenomenological laws, and, like Cartwright he eschews building a model to interpret the formalism and then establishing a representational correspondence between the properties of this model and the properties of a reality that lies behind the phenomena." (bid. p.99) This view however, does not seem to be correct given the fact that Cartwright, as we have already seen, advocates the notion of capacity and power for micro-entities and does not subscribe to a Kantian distinction between the noumena and phenomena as Bohr did. See p.21 and note 118 below.

116. Bohr's unsuccessful attempt in 1924 to preserve the traditional picture of light, (i.e. his joint paper with Kramers and Slater), is a good indicator of his concern for preserving the classical picture of reality. His relentless efforts to solve the wave-particle duality, which preoccupied him for a long time is another indicator of his desire for preserving realism. However, it seems that during the year 1927 and as a result of his discussions with Heisenberg, and especially after the latter produced his uncertainty principle, Bohr came to the conclusion that realism cannot be saved at quantum level. At this stage, he introduced his principle of complementarity, claiming that at the micro-level both wave and particle pictures should be preserved as complementary forms of reality. From this date Bohr's attention was mostly shifted towards the rôle and function of language in depicting the world as it appears to us. Heisenberg in his [1958] and [1971] has given vivid accounts of Bohr's and himself groping with the issue of realism.

A. Miller [1984/86], [1991] has argued that Bohr's and Heisenberg's efforts to accommodate realism into the scheme of the new theory led them to develop new understanding of notions such as visualizability and intuition. Heisenberg redefined intuition with the mathematics of quantum mechanics while demarcating between "to be understood intuitively" and the visualization of atomic processes. The new concept of intuition had no visual component. Thence Heisenberg used the terms visualizable and intuitively to denote the visualization given by mathematics of the quantum mechanics, and declared that we should no longer regard the quantum mechanics as unintuitive.

Bohr however, disagreed with Heisenberg's basic assumption of focusing on nonvisualizable particles with their essential discontinuities and presented instead his complementarity view. Bohr argued that since Planck's constant links the wave and particle modes of light and matter, it places restrictions in the atomic domain on our language and visual imagery which cannot describe or depict, respectively, quantities that are simultaneously continuous and discontinuous. Consequently the wave and particle modes are neither contradictory nor paradoxical, but complementary.

Later on however, through the efforts of Heisenberg, who moved beyond Bohr's formulation of complementarity, and the developments in nuclear physics (1932-1949), and finally Feynman's diagrams, visualizability was, to a sufficient extent, restored. See A. Miller [1991], pp.40-1, [1984/86], pp.127-183. The same theme has been discussed in further length in Miller's other articles, see the Bibliography.

117. Like many physicists of his age or a bit more senior than him, Einstein had been under the influence of Mach's positivists philosophy. This influence, which in the case of Einstein started in early years of 20th century, continued until the later years of the second decade of this century. It was only in 1922 that Einstein openly renounced Mach's philosophy as a "deplorable one". However, his dissent towards this philosophy must have been started some years earlier. See G. Holton [1973].

Heisenberg in his [1971] narrates a lively conversation with Einstein in the spring of 1926 shortly after he had introduced his matrix mechanics. After a talk in University of Berlin on the new quantum mechanics, Einstein had admonished him about the positivistic connotations of his approach:

"But you don't seriously believe," Einstein protested, "that none but observable magnitudes must go into a physical theory?"

"Isn't that precisely what you have done with relativity?" I asked in some surprise. "After all, you did stress the fact that it is impermissible to speak of absolute time, simply because absolute time cannot be observed; that only clock readings, be it in the moving reference system or the system at rest, are relevant to the determination of time."

"Possibly I did use this kind of reasoning," Einstein admitted, "but it is nonsense all the same. Perhaps I could put it more diplomatically by saying that it may be heuristically useful to keep in mind what one has actually observed. But on principle, it is quite wrong to try founding a theory on observable magnitudes alone. In reality the very opposite happens." (Heisenberg [1971], p.63)
118. In his reply to the famous paper by Einstein, Podolsky, Rosen [1935], *Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?*, Bohr writes, "It is shown that a certain 'criterion of reality' formulated in a recent article with the above title by A.Einstein, B.Podolsky and N.Rosen contains an essential ambiguity when it is applied to quantum phenomena. In this connection a viewpoint termed 'complementarity' is explained from which quantum-mechanical description of physical phenomena would seem to fulfill, within its scope, all rational demands of completeness." (Bohr [1935] originally in *Physical Review*, 48, reprinted in S.Toulmin [1970], pp.130-142, italics in original, emphasis added)

119. Many writers, (some among Bohr's own colleagues and former students) have emphasised the influence of Bohr's personality on the views of his collaborators and scientific partners. See for example, Heisenberg [1958], [1971], and the articles in S.Rozental (ed) [1967/1985]. A good case in point of Bohr's influence on his colleagues is the rôle played by Bohr in forcing Slater to change his mind and go along Bohr's line of thought in their joint paper of 1924. (See note 35).

120. M.Redhead [1987], p.51.

121. E.Salaman [1976], p.22.

122. Bohr [1961].


124. The influence of non-realists philosophers like Kant, Kierkegaard and William James, upon Bohr's idea have been noted by many writers. An interesting exception to this norm, is A.Pais [1991, pp.423-5] who has tried to reject (or at least minimize the effect of) such influences. In Pais's opinion, Bohr's familiarity with philosophical writings of philosophers like Spinoza, Hume, Kant, and even his fellow countryman Kierkegaard has been scanty and superficial. Other writers, on the contrary have maintained that Bohr had been influenced, to a great deal, by the philosophical teachings of his teacher and family friend, the Danish Philosopher Harald Hoffding. According to these writers Hoffding has had introduced the ideas of the great past masters to Bohr. This view, incidently, gains support from Bohr's own references to Hoffding's influence on himself. For the impact of other philosophers on Bohr's way of thinking see among others, C.A.Hooker [1972], J.Honner [1982], E.Mackinnon [1982], D.Murdoch [1987].

As for Einstein's familiarity with different philosophical schools, the unanimous verdict among various writers is that he was far better educated in philosophical matters than Bohr. See for example, Pais [1982], [1991].


126. M.Born [1971], p.149.
Two years later, in yet another letter on 4 december 1926, Einstein had remarked that, "Quantum mechanics is certainly imposing. But an inner voice tells me that it is not yet the real thing. The theory says a lot, but does not really bring us any closer to the secret of the 'Old One'. I, at any rate, am convinced that *He* is not playing at dice." (ibid., p.91)

127. A.Einstein [1949], p.674. See also Einstein's letter of 5 April 1948, in which he had enclosed a short article explaining his latest views on quantum mechanics to Born.

128. See note 36 above concerning Bohr, Kramer, Slater [1924] paper. Einstein, in a letter to Born on 29 April 1924 had commented that, "Bohr's opinion about radiation is of great interest. But I should not want to be forced to into abandoning strict causality without defending it more strongly than I have so far. I find the idea quite intolerable that an electron exposed to radiation should choose of its own free will, not only its moment to jump off, but also its direction. In that case I would rather be a cobbler, or even an employee in a gambling-house, than a physicist." (Born [1971], p.82.)


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130. See MacKinnon [1982], Murdoch [1987], and Miller [1984/86].


In fact Bohr’s preoccupation with the problems concerning the use of language and the ways in which the theoretical terms can be applied to physical situations, as MacKinnon[1982] has noticed, led him to contribute to the part of theory of language called pragmatics, which specifically studies the circumstances of the language user.

133. That Bohr regarded epistemology as more important than ontology is confirmed by writers. Even those writers like A.Shimony, E.MacKinnon, D.Murdoch, and J.Honnerr who would regard Bohr as a realist, albeit a weak one, and would not rank him among anti-realists, have endorsed this view. These writers however, are at pains to reconcile Bohr’s priority for epistemological-semantic issues over the ontological ones with a true realist approach. For an account of Bohr’s groping with language see A.Petersen [1968].

134. This refers to a phrase used by Einstein in a letter to Schrodinger, on 22, Dec, 1950. Talking of quantum theorists, Einstein says “Most of them simply do not see what risky game they are playing with reality”. Quoted from Fine [1986, p.2].

We have already argued that sensible defence of realism is only possible if one clearly distinguishes between ontological and epistemological issues and settle the former before tackling the latter.


136. Murdoch, ibid. ch.8.

137. Einstein, Podolsky, Rosen [1935] originally published in Physical Review 47, reprinted in S.Toulmin [1967], pp.122-130. The intent of the paper, in Einstein’s view, was to present a simple, yet compelling argument for the incompleteness of quantum theory. But as Einstein soon after the publication of the paper confided to Schrödinger that, “For reasons of language [the paper] was written by Podolsky after many discussions ... But, it still has not come out as well as I really wanted; on the contrary, the main point was, so to speak, buried in erudition.”

In a recent article, Robert Delete and Reed Guy [1991] have examined the EPR paper from this point of view and has suggested a reconstruction of Einstein’s own incompleteness argument based on the material researched by others in Einstein’s archive. Their final verdict however, is that Einstein’s own argument thought valid, is nonetheless unsound, in that it cannot distinguish, unambiguously, between different quantum systems.

For sympathetic and informative expositions of Einstein’s interpretation of Quantum mechanics L.E.Ballentine [1970], [1972].


139. Einstein, Podolsky, Rosen [1935], reprinted in, S.Toulmin [1970], p.124. The authors had emphasised that their criterion, “while far from exhausting all possible ways of recognizing a physical reality, at least provides us with one such way, whenever the conditions set down in it occur. Regarding not as a necessary, but merely as a sufficient condition of reality, this criterion is in agreement with classical as well as quantum-mechanical ideals of reality.” (ibid.)

140. ibid., p.130.


142. A.Fine & M. Beller [1994].

The criterion of reality which Bohr had made as the main target of his criticism was used by Einstein and his colleagues only to clarify that, "when a value is inferred on the unmeasured system, that value constitute an element of reality (i.e., that it must be included in a complete description)." (ibid.)
143. Bohr and Heisenberg have considered the uncertainty relation as a prohibition not merely on simultaneous measurability, but on the simultaneous existence of sharp values for conjugates variable. In this case, the EPR assignment of simultaneous sharp values for both P and Q would simply be inconsistent with the uncertainty relations. (See ibid. p.6)

144. ibid., pp.36-8.

145. On this aspect of the Copenhagen interpretation which almost played the rôle of ideology among physicists see Feyerabend [1965].

Popper [1967] has noted that: "Most physicists who quite honestly believe in it [i.e. Copenhagen Interpretation] do not pay any attention to it in actual practice." (p.8)

146. N.Herbert [1985], pp.157-197.


148. Schrödinger presented his cat paradox in his [1935].


150. An eminent physicist like Frederic Belinfante has this to say about Wigner’s proposal: "Though Wigner has invoked the law of action and reaction for justifying this suggestion (remarking that, as our mind is influenced by our surrounding, it is only natural to assume that our surrounding will be influenced by our mind) most physicists have rejected Wigner’s suggestion as too far fetched, and as unnecessary." F.J.Belinfante [1975], pp. xiv-xv.

151. J.A.Wheeler [1979-1981], reprinted in Wheeler & Zurek [1983], pp.182-216. For Wheeler the essence of reality is meaning, and the essence of meaning is communication defined as the joint product of all the evidence available to those who communicate. In this view meaning rests on action, which means decisions, which in turn forces the choice between complementary questions and the distinguishing answers. These amount to the basic idea of generating reality by act of measurement.


153. op.cit. note 94.

154. Ronnie Knox in his limerick exposed the bizarre nature of Berkeley’s view:
There once was a man who said ’God
Must think it exceedingly odd
If he finds that this tree
Continue to be
When there’s no one about in the Quad’.
Berkeley’s approved answer is equally interesting:
Dear Sir, your astonishment’s odd;
I am always about in the Quad.
And that is why the tree
Will continue to be,
Since observed by Yours faithfully, God. (Quoted from J.L. Polkinghorne [1986, p.66).


158. E. Squires [1986], p.69.


160. See, Rae, ibid. pp.81-2.

161. Bohr [1958], p.72. Italics added. It should be born in mind that for Bohr the very concept of "phenomenon" refers only to the observations obtained under circumstances whose description includes an account of the whole experimental arrangement.

162. D. Bohm [1963], p.10.

163. ibid. p.10.

164. Bohm [1987], p.34.

165. Bohm [1987], p.34.

166. Bohm [1987], p.35.

167. See Bohm [1952]. Bohm began his new interpretation with the one-particle Schrödinger equation, namely,

\[ \frac{i\hbar}{\partial t} \psi = -\frac{\hbar^2}{2m} \nabla^2 \psi + V(x)\psi. \]  

He wrote the complex function \( \psi \) in the form

\[ \psi = R \exp(iS/\hbar) \]  

where \( R \) and \( S \) are real. It follows that

\[ \frac{\partial R}{\partial t} = -\frac{i}{2m} [R\nabla S + 2VR - VS], \]  

\[ \frac{\partial S}{\partial t} = -\frac{1}{(V\nabla)}/2m + V(x) - (\hbar^2/2m) \nabla^2 R/R. \]  

It is convenient to write \( P(x) = R^2(x) \), or \( P = P^2 \) where \( P(x) \) is the probability density. We then obtain

\[ \frac{\partial P}{\partial t} + \nabla \cdot (P\nabla S/m) = 0, \]  

\[ \frac{\partial S}{\partial t} + (\nabla S)/2m + V(x) - \hbar^2/4m [\nabla^2 P/P - 1/2 (V\nabla)^2 P] = 0. \]  

Bohm noted that in the limit (\( \hbar \rightarrow 0 \)) the function \( S(x) \) is a solution of the Hamilton-Jacobi equation. He then considered an ensemble of particle trajectories which are solutions of the equations of motion. Using a well-known theorem of mechanics that if all of these trajectories are normal to a given surface of constant \( S \), then they are normal to all surfaces of constant \( S \), and \( \nabla S(x)/m \) will be equal to the velocity vector, \( v(x) \) for any particle passing through point \( x \), Bohm stated that equation (5) can be re-written as

\[ \frac{\partial P}{\partial t} + \nabla \cdot (Pv) = 0. \]  

According to Bohm, "This equation indicates that it is consistent to regard \( P(x) \) as the probability density for particles in our ensemble. For in that case, we can regard \( Pv \) as the mean current of particles in this ensemble, and equation (7) then simply expresses the conservation of probability." He then made his basic move, namely, to assume that in the case of (\( \hbar \neq 0 \)) each particle is acted upon, not only by a "classical" potential, \( V(x) \) but also by a "quantum mechanical" potential, in the form of

\[ U(x) = -\hbar^2/4m [\nabla^2 P/P - 1/2 (V\nabla)^2 P] = -\hbar^2/2m [\nabla^2 R/R] \]  

In this way, as Bohm observed, equation (6) can still be regarded as the Hamilton-Jacobi equation for the ensemble of particles, and \( \nabla S(x)/m \) as the particle velocity, and eq. (5) as describing conservation of the probability in the ensemble. (For details see D. Bohm [1952] reprinted in A. Wheeler & H. Zurek (eds.) [1983], pp.369-402. See Also Bohm [1957] for a shorter account of his proposed model.)

For a survey of hidden variable theories see F. J. Belinfante [1973].

168. In the fifth Solvay conference, de Broglie delivered a paper entitled "The new dynamic of quanta" in which he presented in an incomplete and diluted form a simplified version of his original ideas concerning a wave-pilot theory. However, this theory did not receive with enthusiasm among the audience. De Broglie himself admitted that it was partly due to this unfavourable reaction that he abandoned his own ideas and espoused the Copenhagen interpretation from then on. Twenty-five years later, however, Bohm's paper, and certain other developments in the general theory of relativity revived his interest in his original causal approach. cf. Jammer [1964], p.357.
169. Hiley & Peat in their introduction to their anthology on Bohm [1987] have noted that Bohm’s emotive use of the term “hidden variable”... led to the implicit view that to mention the term “hidden variable” was in some sense to commit a cardinal heresy. Even today the term often provokes a sceptical, if not irrational and agnostic response. Bohm now admits it might have been a mistake to call his theory a “hidden variable theory”. After all, it only uses positions and momenta, whereas the real drive for the hidden variable approach was to find “additional” parameters to describe the underlying process.” (p.8)

170. Fritz Rohrlich [1983], p.1252. In the last chapter we shall deal with the issue of the relevance of philosophical (metaphysical) conjectures to the advancement of science.

Bohm himself has referred to the indistinguishability of his version and the OQM as the main reason for its unfortunate fate. In one of his later publications he writes, "Perhaps the main objection was that the theory gave exactly the same predictions for all experimental results as does the usual theory. I myself did not give much weight to these objections. Indeed, it occurred to me that if de Broglie’s ideas had become the accepted interpretation, then, if someone had come along to propose the current interpretation, one could equally well have said that since, after all, it gave no new experimental results, there would be no point in considering it seriously." ([1987], p.39)

171. “[T]he quantum potential [does] not depend on the intensity of the wave associated with [the quantum objects]; it depends only on the form of the wave. And thus, its effect could be large even when the wave has spread out by propagation across large distances.” ([1987], p.36)

It is worth noting that Bell’s theorem and its experimental confirmation (see next section) does not refute Bohm’s version of hidden-variable approach. This is because his version is non-local and as such the faster than light speed is allowed according to it. Bell’s theorem however, refutes the local hidden-variable theories. See A.Aspect’s comments in Davies & Brown (eds.) [198], p.44.

172. Von Neumann in his [1955] after presenting his theorem against the hidden variable theories had emphasised that, "It should be noted that we need not go any further into the mechanism of the hidden parameters since we know that the established results of quantum mechanics can never be rederived with their help” (p.324). As we have noted before, although von Neumann’s proof was mathematically impeccable, he had made a harmless looking technical assumption. Bohm of course had not used this assumption and thus was able to produce a viable version of hidden variable theory. It was John Bell who in 1964 clarified the issue and removed the imposed restriction.

173. Bohm [1957].

174. See next section.

175. See Bohm [1963] and [1971]

176. op.cit, [1963], p.12.

177. ibid.

178. See Bohm [1981] for a thorough account of his new theory.

179. op.cit. [1987], p.43.

180. ibid. pp.43-4.

181. Popper has discussed his views concerning the problems of quantum mechanics in a number of publications including [1959/68], [1967], and [1983].

182. Popper [1967], pp. 7-44.
183. ibid., theses 1 and 2. This point is in sharp contrast to the view held by the creators of Copenhagen interpretation who maintained that $\psi$ should be interpreted not as the distribution function for an ensemble, but as the variable associated with individual quantum systems. On this point see for example, Heisenberg [1930].

184. ibid., thesis 8. Popper has made the following distinction between the probability statements and the statistical statements. The former are statements about frequencies in virtual (infinite) sequences of well characterized experiments, whereas the latter are about frequencies in actual (finite) sequences of such experiments. In probability statements, the "weights" attached to the possibilities are measures of these (conjectural) virtual frequencies, to be tested by the actual statistical frequencies. See Popper [1967], p.32.

185. ibid. p.19.


187. ibid. p.17.

188. Popper has discussed his propensity approach in a number of his publications. See for example, his [1959/68] and [1959b].

189. Popper [1967], p.31.

190. ibid. p.32.

191. ibid. pp.34-35.


193. See next section.


196. The following version of Bell's inequality is due to d'Espagnat [1979].

In an EPR type experiment, we measure the spin components of a photon along any one of three arbitrary chosen axes, $A$, $B$, and $C$. If the spin component along axis $A$ is found to be plus, it is labelled $A^+$; if the component along axis $B$ is minus, it is given as $B^-$. An important point to notice is that the signs of spin components for each pair of elementary particles / photons along any axis, are always opposite. That is to say if for example the spin of one member of a pair along $A$ axis is $A^+$, the spin of the other member is $A^-$. Now Bell's inequality can be presented in the following way: if a particle/photon has been found to have the properties $A^+$ and $B^-$, then it must be a member of either the class $A^+ B^- C^+$ or the class $A^- B^+ C^-$. Hence if $N(A^+ B^-)$ represents the number of such particles, it must be equal to the sum $N(A^+ B^- C^+) + (A^- B^+ C^-)$. In a similar way it can be shown that $N(A^+ C^-)$ is equal to $N(A^+ B^- C^+) + N(A^+ B^+ C^-)$, from which it follows that $N(A^+ C^-)$ is greater than or at least equal to $N(A^+ B^- C^-)$. The same reasoning leads to the conclusion that $N(B^+ C^-)$ is greater than or at least equal to $N(A^+ B^- C^-)$. These three relations can now be combined to yield the following inequality: $N(A^+ B^-) \leq N(A^+ C^-) + N(B^+ C^-)$. The same relations holds if all signs are reversed to give the inequality $N(A^- B^+) \leq N(A^+ C^-) + N(B^+ C^-)$. The last two inequalities can be added to yield a relation among all individual particles for which two properties have opposite values.

We now extrapolate from the case of single particles for which two properties are known to that of pairs of particles, each particles of which is tested for one property. Because, as mentioned earlier, there is a strict negative correlation between respective properties of a pair of particles, if one particle in a pair is found to be $A^+$ the other must have the property $A^-$. Because of this correlation, if one particle in a pair is found to be $A^+$ and the other is found to be $B^+$, it is possible to deduce the properties of both particles. The doubly positive test result can arise only if one particle has the two properties $A^+ B^+$ and the other has the
properties $A' B'$. Hence the number of such doubly positive test results, which can be designated $n[A' + B']$, must be proportional to the number of particles with the properties $A' B'$ and $A' B'$.

Similar proportionalities can be derived for the number of doubly positive results observed when pairs of particles are tested for properties $A$ and $C$ and $B$ and $C$, these are quantities $n[A' C']$ and $n[B' C']$. The constant of proportionality depends only on the numbers of pairs submitted to each set of test and on the total number of pairs, and so the constant is the same in all three cases. It therefore follows that:

$$\frac{n[A'B']}{N(A'B') + N(A'B')} = \frac{n[A'C']}{N(A'C') + N(A'C')} = \frac{n[B'C']}{N(B'C') + N(B'C')}$$

$$N(A'B') + N(A'B') \leq N(A'C') + N(A'C') + N(B'C') + N(B'C')$$

$$n[A'B'] \leq n[A'C'] + n[B'C']$$

which is Bell's inequality.

197. Aspect’s experimental arrangement is shown below. Pairs of photons from the source S travel 6 meters to the acousto-optical switches. The route of a photon beyond the switch determines which of the differently oriented polarizers it will encounter. The photons are detected using photo-multipliers (PM) and coincidences between the various channels are monitored electronically. For details see A. Aspect, J. Dalibard & G. Roger [1982], p. 1804.

198. Here "conclusive" means what can be reasonably established in the realm of science. Philosophers of science like Duhem and Popper have taught that nothing in science is hundred percent fault proof, and there are always ways for rejecting undesired outcomes. But, at any given time, the dominant rationality criteria of the day would compel one to regard something as reasonably established or otherwise. Here we are appealing to this notion of conclusiveness. In the case of Aspect’s experiment, as it is pointed out in the text, there are some escape routes for those who do not want to bow to the inevitable. These alternatives are of course not very satisfactory.

199. J. Cushing & E. MacKinnon (eds) [1989].


204. cf. Cushing & MacKinnon [1989].


207. *ibid.* p.291.

208. For this kind of interpretation of $\psi$ see E. Squires [1990].

209. H. Margenau for example has called it latency while Heisenberg dubbed it potentia. In late 1950's, Heisenberg, in an attempt to move away from the phenomenological approach towards quantum mechanics, introduced the notion of potentia: "Now, the theoretical interpretation of an experiment starts with the two steps that have been discussed. In the first step we have to describe the arrangement of the experiment, eventually combined with a first observation, in terms of classical physics and translate this description into a probability function. This probability function follows the laws of quantum theory, and its change in the course of time, which is continuous, can be calculated from the initial conditions; this is the second step. The probability function combines objective and subjective elements. It contains statements about possibilities or better tendencies ('potentia' in Aristotelian philosophy), and these statements are completely objective; they do not depend on any observer; and it contains statements about our knowledge of the system, which of course are subjective in so far as they may be different for different observers." (Heisenberg [1958/1989], pp.40-1) See also A. Miller [1984/86, 1985, & 1990].

210. *ibid.* p.292. Italics added. Shimony notices that in line with the above notion, one can easily define the entanglement states of n-particle system. Such a state rests upon the fact that a quantum mechanical state of a system is a network of potentialities, the content of which is not exhausted by a catalogue of the actual properties which are assigned to the system.

211. A survey of a number of these proposals is given in A. Shimony [1986].

212. Maxwell has introduced his proposal in a number of publications. See the bibliography for a list of relevant essays.

213. Maxwell [1976], p.275. At the outset of his article, Maxwell has made it clear that in his approach, macro systems arise simply as the outcome of interaction between a vast number of micro systems. His main concern is to show that such an interpretation is in principle, viable. That there is a practical problem in calculating the structure of macro systems out of the configuration of micro systems does not undermine the validity of the model.

214. See Chapter One, p.20. We shall further discuss this thesis in the next Chapter.


216. We shall further discuss dispositional properties in the next chapter.

217. Note that classical statistical mechanics is not a fundamentally probabilistic theory: it presupposes that the dynamical laws are deterministic. Probabilism enters into classical statistical mechanics via probabilistic distribution of initial and boundary conditions in relevant ensembles of physical systems. (see Maxwell [1988, p.12]).

218. The notion of propensity was first introduced by Popper [1957]. Any propensity $P$ has associated with it a number of possible outcomes $O_1...O_n$. In specifying the value of a the propensity $P$ at any instant we specify the probability $P_r$ that outcome $O_r$ will occur should the propensity be actualized through the occurrence of a probabilistic event at the instant in question, with $r = 1...n$, and
Maxwell's notion of propensity, differs from that of Popper. The basic difference boils down to the point that for Popper propensity is a characteristic of the entity + the whole experimental set up, whereas for Maxwell, it is only a characteristic of the entity itself. (See Maxwell [1976, pp.283-6])


221. See for example, Maxwell, [1988], [1993b], [1994].

222. Margenau [1963, p.6] has defined the distinction between preparation and measurement in the following way:

"In general, a preparation of state is any physical operation which assures that, if a system has been subjected to the operation, it will afterward be in a specifiable quantum state (ψ, or perhaps a mixture of ψ's). A measurement, on the other hand, will be taken to be a physical operation which yields a number (within the penumbers of an error range) that refers to the state present before the operation."

M. Redhead [1987, p.52] has noted that measurement can be used as a backward-looking process, telling us what observable value Q the system S possessed before interacting with the measuring apparatus A. However, if we use the measurement interaction as forward-looking device to tell us what value the observable Q will have after the interaction of S and A, then the measurement is functioning as state-preparation device, preparing the state of S such that Q will possess a certain value.

Landé [1965 (quoted in Maxwell [1976, p.664]) has called such a device which ensures that the system will have a certain observable value, a filter. Examples of filters are: a screen with a slit in it; a Polaroid film which only allows light of a specific polarization to pass through; a momentum selector which only allows system of a specific momentum to pass through.

223. It should be born in mind that according to OQT, the measuring device itself, is also subject to the formalism of quantum mechanics if it is itself the object of observation by another piece of macroscopic apparatus.

224. For details see Maxwell [1988], [1993b], [1994].

Chapter Seven
Scientific Realism and the Aim-oriented Empiricism

I. The basic problems again

In the first chapter we emphasised that any viable theory of science should be able to specify i) a (number of) basic, essential aim(s) for scientific enterprise; ii) a set of methodological rules which are conducive, in a reasonably effective way, to the defined aim(s) and are governing choice of the best theory from among a number of rivals, and iii) a demonstration to show that the proposed aim(s) and methodological rules are better than the alternative candidates.

Modern anti-realists, as we have already seen, have been of the view that science should aim at empirical adequacy. They were adamant that scientific knowledge consists in the empirically adequate knowledge. In their view, scientific theories are either abstract equations, implicit definitions, or other highly sophisticated intellectual constructs. These products of human imagination need not be regarded as mere fictions or instruments (as was the case with the previous generations of anti-realists). Nevertheless, these constructs do not provide us with knowledge of the underlying, unobservable reality. Their main function is to provide scientists with a neat way of organizing and systematizing the empirical data and to facilitate the knowledge at the level of phenomena.

The arguments in the past chapters have served to establish the point that the proposed aim and methods of the celebrated modern anti-realist theories of science are not tenable. Modern anti-realists are not able to produce viable criteria for objective theory choice. The phenomenon of progress of science also poses a great difficulty for modern anti-realist theories. Furthermore, the all-important problem of induction remains unsolved, even untouched, within these anti-realist frameworks. These shortcomings on the part of
anti-realists' programmes have rendered them unable to provide viable images of science and have pushed them towards the slippery slope of becoming mere intellectual games, with little relevance to the real scientific enterprise.

As for the realist writers, it was noted that in the light of the criticisms levelled at them by modern anti-realists, many of their stronger claims were shown to be too vulnerable to be defensible. These criticisms indeed have played a significant rôle in the emergence of the break-away group of entity-realists. Nevertheless, it was argued that whereas it is difficult to sustain the views of either the entity-realists or the advocates of the stronger versions of scientific realism, the aim and methods suggested by minimal realism are, relatively speaking, more viable than those of their rivals or counter-parts. Minimal realists have produced, at least intuitively acceptable, measures for verisimilitude and progress of science and have also taken steps towards solving the problem of induction.

Nevertheless, notwithstanding the plausibility of their position, minimal realists have not been able to justify their approach. Verisimilitude, as we have seen, was, despite all its intuitive appeal, fraught with logical difficulties. It has also been pointed out that Popper's anti-inductivist method relies tacitly on inductive reasoning. Furthermore, it was noted that realists' willingness to resort to extra-empirical values while limiting themselves to the confines of standard empiricism, renders their position inconsistent. In fact, as noted in the previous chapters, within the confines of standard empiricism none of the major problems of rationality, e.g., problem of induction, can be solved satisfactorily.

What makes the case even more embarrassing for realists who intend to make rational sense of science, is that it seems that working scientists, in deciding the fate of
rival theories, as a matter of routine, resort to extra-empirical values, taking them as epistemically significant. Such a wide-spread practice among scientists, i.e., scientists’ persistent preference for more unified, more explanatory and simpler theories, in itself is not only a powerful *reductio* against standard empiricism but also a damning verdict against realists’ claim that they are better placed to make sense of science.

The question which therefore needs to be answered is that of whether a more rational and less restrictive framework can be found which would make a better sense of science and would provide satisfactory solutions for the above fundamental problems? To answer this question we should consider three further basic and inter-related questions.

The first such question is: "What must the world be like for scientific knowledge not only to be *possible* but also to have the greatest chance of *progress*?" The second question is: "What aim and structure must science have to be *successful*, i.e., to give us knowledge of the observable as well as unobservable aspects of the physical universe?" And the last question is: "How must the methodology be like to maximize the success-rate of science?" We shall consider these questions in sections II, III, and IV below.

**II. The Comprehensibility of Nature**

The possibility of scientific knowledge both presupposes the intelligibility of nature and vindicates it. The *fact* that universe *is* comprehensible, is indeed, as Einstein aptly put it, quite incomprehensible. However, the argument *for* its intelligibility is not a complicated one. It is an argument from the possibility of (conjectural) empirical science: If the universe were not intelligible, science would not be possible. But science is possible. Therefore, the universe is intelligible. In a totally unintelligible universe, even if we allow for the continuation of life, improving knowledge of the universe will not be
possible.

The above argument can at most establish the point that for science to be possible, the universe must be somehow intelligible. However, our question was: "What must the world be like for scientific knowledge not only to be possible but also to have the greatest chance of progress?" Therefore, what we should seek to clarify is that of exactly what sort of comprehensible universe will allow maximum progress for science? The intelligibility of the universe of course, can mean many, if not infinitely many, different things. The following are a number of such possible interpretations:

- 'The universe is comprehensible' means only partial comprehensibility. This thesis, in turn, can take different forms, e.g:
  - Only the phenomenal world is intelligible. The noumenal world is forever out of the reach of human beings;
  - The universe is only intelligible in certain space-time regions or under certain conditions;
  - The universe is only periodically comprehensible. There are periods of intrusion of incomprehensibility (what ever that may mean).
- The universe is comprehensible in the sense that each phenomenon, process, event, entity and thing has a guardian goddess, and only a (mystical?) unification with that goddess renders that phenomena, etc., intelligible.
- The universe is intelligible in the sense defined by occasionalists, i.e. direct and constant intervention of a Supreme Being enables man to see some regularities; otherwise there is no real lawfulness in the universe.

The question to be answered is that of in which of these and other rival universes would science acquire its greatest chance of progress? Following N.Maxwell [1984], I will
suggest that to give science the best chance of progress, it is essential to assume that the physical universe is maximally lawful (i.e. the basic structure of the universe is simple, unified, causally connected, and expressible in the language of mathematics) and maximally knowable (for the human beings). It is not difficult to reason why such a universe gives the science a better chance of flourishing.

The total comprehensibility, is, from a methodological point of view, superior to other hypotheses which advocate partial comprehensibility; the chances of progress in scientific knowledge are much greater in a universe in which scientists are not barred (on \textit{priori} grounds) from understanding the building blocks of that universe. Among the partially intelligible universes two are of particular interest. One is the Humean universe in which there are no causal connections only constant conjunctions between successive events. In this universe the comprehensibility is reduced to the phenomenological comprehensibility. And the other is the quasi-Kantian universe (advocated by anti-realists like van Fraassen) in which the noumena, as posited by scientific theories, though real, nevertheless are forever out of the epistemic reach of mankind.

If the universe is \textit{actually} only partially comprehensible, as these hypotheses (and their ilk) would assert, a scientist who subscribes to the thesis of maximum comprehensibility of the universe will not lose anything. On the contrary, by formulating testable theories which are designed to account for the phenomena in a larger domain (as regards time & space) he will give himself a better chance of progress, in the sense of noticing his mistakes. In contrast, in the case of partial comprehensibility, there is always far greater room for \textit{ad-hoc} manoeuvres which are entirely consistent with the general drive of the accepted comprehensibility assumption; any failure of the proposed conjectures for explaining the putative phenomena can be ascribed to the
incomprehensibility of this particular aspect of nature rather than the shortcomings of the theory. Any particular conjecture of partial comprehensibility thesis, can therefore, not only systematically mislead scientists, but also can urge them to take a dogmatic stand, of the kind Popper has rightly criticised, towards their theories\textsuperscript{10}.

From the above discussion it should be clear that the proposed metaphysical picture provides scientists with a better chance of progress, in comparison to not only the metaphysical picture tacitly embraced by modern anti-realists like van Fraassen, but also to the one an entity-realist like Cartwright wants to uphold. Cartwright, as we have seen, suggests a new metaphysical model in which different, unrelated, autonomous, parochial laws are at work. Comparing this model with the model presented above, it is clear that the chances of improving knowledge in her proposed model of universe are slimmer than the chances of improving knowledge in a universe in which it is assumed that the disparate phenomena are related by underlying simple causes\textsuperscript{11}. In fact, even if the universe were actually exactly as Cartwright’s metaphysical picture depicts, scientists will still have more chances of advancing scientific knowledge if they assume maximal comprehensibility (in the sense explained above) for the universe.

The two requirements of maximum lawfulness and maximum knowability can be translated into the following two general assumptions:

= It is assumed that physical reality (in such maximally comprehensible universe) consists of many layers or strata with different degrees of complexity, diversity and variation, of which the level of observable phenomena, or appearances has the highest degree of complexity.

= It is assumed that at the most basic level of physical reality, there lies a simple, unified pattern or structure which consists of a few (possibly only one) simple entities (entity),
with a small number (again possibly one) necessitating, invariant properties (property\textsuperscript{12}). This unified pattern gives rise to all diversities and changes at the upper levels\textsuperscript{13}. The fundamental properties of this pattern are (assumed to be) \textit{invariant} through all changes, and \textit{simple} (in a non-anthropomorphic sense i.e., not dependent upon \textit{our} choice of languages or conceptual schemes). These properties determine (probabilistically or deterministically) the precise way in which that which changes does change. The fewer the different kinds of fundamental entities and their essential properties, the more unified the ‘pattern’ of the ultimate physical reality.

These two characteristics combine the ideals of Heraclitus (perpetual change) and Parmenides (oneness of physical reality). History of science provides us with ample examples of such unified underlying patterns. Boscovich for example, had tried to combine the Newtonian point-particle universe with the Leibnizian conviction that force (field) is an essential property of matter. Invoking the Newtonian notion of action at a distance, Boscovich grafted it with Leibniz’s view that there must be repulsive forces associated with matter to explain impenetrability. The result was a new metaphysical pattern in which the physical universe was made of point atoms surrounded by force fields which were repulsive at very small distances, attractive at very large distances, and might be alternatively attractive and repulsive at intermediate distances. For Boscovich, "force" denoted the propensity of masses to approach and recede. This picture, as we know, bears partial resemblance to the celebrated picture of interatomic forces upheld by physicists today\textsuperscript{14}.

### III. Viable Aim and Structure for Science

#### III.A. Metaphysics as an Essential Part of Physics

One of the major limitations imposed by standard empiricism is the claim that for
science to be rational, one should not make any substantial metaphysical assumptions, about the world, or about the phenomena one is investigating. In other words, according to the conventional wisdom among those philosophers who subscribe to standard empiricism, metaphysics and science should be sharply separated. All non-empirical presuppositions, made out of necessity by scientists, must be regarded as personal preferences of individuals which have nothing to do with the main body of science proper.\(^{15}\)

Some realists, (e.g. Popper, Agassi) who are not unsympathetic towards metaphysics, have tried to square the circle by calling such principles "methodological rules". Popper for example, has emphasised that: "Consistently with my attitude towards other metaphysical questions, I abstain from arguing for or against faith in the existence of regularities in our world. But I shall try to show that non-verifiability of theories is methodologically important. ... This principle ['principle of the uniformity of nature '], seems to me, expresses in a very superficial way an important methodological rule, ...").\(^{16}\)

Still other standard empiricists such as Kuhn and Lakatos, who are more sympathetic towards metaphysics and have acknowledged the rôle of metaphysical ideas (e.g., paradigms or hard cores) as guiding principles in developing scientific theories, nevertheless, have adhered to a model of science which allows only for a two-level structure for science, namely, level of observation (empirical evidence) and level of theory. In view of these philosophers, who do not subscribe to correspondence theory of truth, scientists' advocacy of certain metaphysical ideas, do not mean any bias in favour of certain picture of the nature of world,\(^{17}\), but only a temporary acceptance of views which scientists hope to lead to further problem solving-effectiveness or more empirical adequacy. These philosophers, despite their advocacy of non-empirical assumptions about
the world, do not entertain the possibility of rational theory choice on the basis of these *a priori* assumptions about the nature of reality. In line with their standard empiricism conviction they maintain that the fate of theories should be decided solely on empirical grounds.

The refusal of standard empiricists of allowing *a priori* metaphysical assumptions in deciding the fate of theories have only served to render the *rational* theory choice according to these methodologies impossible⁰⁸.

However, as the discussion of the previous section showed, and as will become clearer below, the metaphysical conjectures concerning the underlying unified pattern of reality are far more significant than purely methodological or pragmatic principles. The adoption of these conjectures will set the course for all (basic) scientific inquiries. If physical reality consists of a unified, simple pattern, then the most rational way forward for the scientists to acquire knowledge about fundamental physical reality is to try to represent this unified pattern by means of a unified grand theory T, which in turn can explain all diverse, ever-changing physical phenomena in terms of few necessitating properties⁰⁹.

In pursuing scientific research the ultimate aim of the scientists, then, should be to convert a more or less vague metaphysical theory — which states that the universe is ultimately simple, unified, coherent in the above sense — into a precise, fully articulated, empirically testable, unified scientific theory. This ultimate goal is to be pursued by progressively putting forward more and more accurate guesses concerning the nature of underlying reality and by trying to convert them into testable scientific theories (via appropriate mathematical models) with ever greater explanatory and predictive power.

Progress in theoretical science, therefore, should be understood in terms of success
that is achieved in realizing the above aim. This in turn means that abandoning the standard empiricists’ view concerning the relation of physics (science) and metaphysics, gives scientists a better chance for scientific progress. Far from being two separate, unrelated disciplines, metaphysical doctrines (or blueprints a la Maxwell) should be regarded as integral part of grand scientific theories. Metaphysical doctrines, in this sense, are nascent scientific theories. Science therefore, should be regarded as a three-layered structure, namely, level of observation, level of scientific theories, and level of general metaphysical blueprints.

History of science can be viewed in terms of the succession of ever more unified and simpler metaphysical blueprints which have acquired the status of proper scientific theories. Kepler’s favourite blueprint – that heavenly bodies move in conic sections – can be regarded as a successor to the Platonic blueprint – heavenly bodies move uniformly in circles around the earth. Keplerian theory that evolved out of his blueprint, was far more precise and more successful in calculating and predicting the position and orbits of the heavenly bodies. The Newtonian blueprint offered a yet more unified, more comprehensive and simpler framework than its predecessor and when it was translated into an exact mathematical model it surpassed the Keplerian model in terms of accuracy of prediction, exactness of calculation, and comprehensiveness of scope. The same pattern was once again repeated, this time at a higher level of unification and simplicity, when Einstein developed his blueprint and produced his special and general theories of relativity out of that metaphysical framework. Einstein’s blueprint and theory also unified another rival sequence of blueprints and theories, namely, the field theories, due to Faraday, Maxwell, and Hertz.
III.B. Conjectural Essentialism as a Viable Framework for Scientific Theories

Science cannot rely on empiricists’ or phenomenologists’ aim, namely empirical adequacy. This is too impoverished an aim for science to pursue. Science must aim to capture and represent the natures or essences of the building blocks of the universe. It is only in this way that scientists can hope to acquire genuine knowledge about the physical reality. Conjectural essentialism\(^\text{22}\), as a general framework, will enable scientists to shape their conjectures in ways which serves the above purpose. At the heart of the conjectural essentialism lies the notion of *dispositional or necessitating properties*, a notion which is quite common in science\(^\text{23}\). These are the properties in virtue of which the entities, postulated by a theory, must, of necessity, obey the laws of the theory. It is not difficult to see that almost all — perhaps all — physical properties, both common-sensical and theoretical ones, are dispositional or necessitating. In fact if we take ‘disposition ’ to refer to a category complementary to that of ‘occurrence ’\(^\text{24}\), such that it includes *tendencies* (courageous), *capacities* (good at playing chess), *liabilities* (fragile), *habits* (smoker), *powers* (waterfall’s power to run a turbine), and the like (e.g. capability, potentiality, nature,...) then it is hard to see what sort of property is not expressible in terms of dispositions.

If an object (entity) has dispositional property(ies) (e.g. solidity, stickiness, electromagnetic intensity, spin,...) then, in such and such circumstances, of necessity, the object participates in change (or resistance to change) in such and such a way. To say an object is breakable (to use an example used by Popper himself when defending the notion of dispositional properties\(^\text{25}\)) is to say that the object is such that if it is hit by, say an iron bar, then, of necessity, it breaks. In other words, to assert that something is breakable, is to assert that that which exists can only be adequately specified by a term *breakable* (or
its equivalent) whose meaning is such that from 'X is breakable', and 'X is hit by an iron bar'; it follows analytically necessarily, that X breaks. 'X is breakable and X is hit by an iron bar' analytically implies 'X breaks'. In other words, the property 'breakable' is such that it can only be adequately referred to by a word, such as "breakable" if the meaning of the word is such that "if a breakable object is hit it breaks" is analytically true.

Our conjectures concerning the dispositional properties of the theoretical entities of course, may turn out to be wide of the mark. But this is beside the point. What is being argued here is that, these conjectures if true, are necessarily true.

An essentialistic interpretation is not only available to most fundamental theories, but also can be applied to the less fundamental theories in different fields of research (e.g. physics, chemistry, biology, economics, ...)\(^\text{27}\). Here, a certain entity with certain dispositional properties is being conjectured to be responsible for a certain repeatable phenomenon. The conjectured properties, if correct, define the nature of the entity in question and describe the ways it acts (i.e., exerts its power(s)) in statements of causal laws.

Essentialistic construal of physical theories are applicable to both deterministic and probabilistic universes. If universal, invariant, deterministic, essentialistic properties do exist, then any precise true specification of the physical state of an isolated system at one instant in terms of these properties does analytically, necessarily, imply subsequent true state description. If however, as discussed in the last chapter, we assume that the basic constituents of matter are propensitons which are governed by probabilistic rather than deterministic laws, then descriptions of the systems in terms of the their propensities give us probabilistic information about a range of possible outcomes. A specific value of a
propensity \( P \) specifies \( n \) probabilities \( p_1 \ldots p_n \) and attributes a definite probability \( p_r \) to each possible outcome \( O_r \), with

\[
\sum_{r=1}^{n} p_r = 1.
\]

Here, propensities determine how things change probabilistically in certain circumstances and parallel with the case of deterministic properties; here, there can be \textit{probabilistic} necessary causal connections between successive states of affairs given that propensities exist\(^9\).

Conjectural essentialism is not reducible to conditional or counter-factual statements. It is the latter which should be explained in terms of the former. For example, the counterfactual statement 'If \( X \) were hit by an iron rod, \( X \) would break', is true iff \( X \) possesses the dispositional property of breakability. Likewise the distinction between 'nomic' and 'accidental' universal statements should be understood in terms of the dispositional, essentialistic properties and \textit{not} vice versa.

\section*{IV. A Method For Scientific Discovery}

\subsection*{IV.A. Metaphysics Once More}

Standard empiricism's denial of any knowledge other than that based on empirical success imposes yet another undesired limitation on the process of knowledge-garnering; it would disparage rational investigation with regards to developing a rational logic for discovery\(^{30}\). Anti-realist empiricists like Hume, Mach, logical positivists, modern anti-realists, and realist empiricists have all rejected the idea of the possibility of a logic for scientific discovery. Popper for example, has observed that:\(^{31}\)

\begin{quote}
I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically. As to the task of logic of knowledge — in contradistinction to the
\end{quote}
psychology of knowledge — I shall proceed on the assumption that it consists solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained.

Some might object that it would be more to the purpose to regard it as the business of epistemology to produce what has been called a 'rational reconstruction' of the steps that have led the scientists to a discovery — to the finding of some new truth. But the question is: what, precisely, do we want to reconstruct? If it is the processes involved in the stimulation and release of an inspiration which are to be reconstructed, then I should refuse to take it as the task of the logic of knowledge. Such processes are the concern of empirical psychology but hardly of logic.

While the opposition to the possibility of developing a logic for scientific discovery may do less harm to anti-realists who, by and large, seek empirically adequate, e.g., technological knowledge, for realists who regard the central problem of epistemology to be the problem of the growth of (scientific) knowledge, it can severely jeopardise their programme.

To improve our chances of acquiring more knowledge, it is vital to seek rational means to assist the extremely important process of developing or producing new ideas and fresh conjectures concerning the nature of physical reality. In fact, if the growth of knowledge consists of constantly hitting upon new ideas (conjectures) which explain the facts in increasingly more comprehensive, unified, and simpler ways, then it seems that without making rational sense of the process of discovery, it will be impossible to provide a satisfactory explanation of the process of growth of knowledge, and therefore making rational sense of what seems to be the paradigm of human knowledge. Dogmatic rejection of the possibility of a logic for scientific discovery will be harmful to both science and methodology.

It was observed earlier that a viable methodology of science should be able to assist scientists with their scientific investigations. From the above discussions it can be seen that part of the process of discovery involves informed guesses as to the natures or essences of basic or fundamental entities found in nature and hypothetically postulated in science. This notion of a rational, non-mechanical method of discovery, is not tantamount
to the notion of a magical, infallible, all-powerful method for solving all scientific problems. It is not an alternative algorithm to replace ingenuity, flashes of insight, novelty, systematic thinking and hard empirical research. It is rather a rational method for rendering rational (as much as possible), some of seemingly non-rational processes involved in the act of discovery, and to help to bring about (in a systematic way) as much new (fallible, though corrigible) knowledge of the external world, as humanly possible.

But can such a notion be rationally justified, i.e., be shown that it is an achievable ideal? In recent years a number of philosophers of science, by invoking historical cases, have argued in favour of the possibility of a logic of scientific discovery. E.Zahar [1983] for example, having quoted Lakatos as claiming that heuristics belongs to some sort of limbo which is rational and non-psychologistic, goes on to add:

I intend to show that the process of discovery is much more rational than it appears at first sight; that it is neither inductive nor largely intuitive; that it does not belong to any kind of limbo, but rests largely on deductive arguments from principles which underlie not only science and deductive metaphysics but also everyday decisions. The choices of consistent sets of such principles constitutes the heuristics of research programmes.

To understand the mechanism of a given discovery, analysis should, instead of reconstructing the psychological preconditions (as is the case with N.R.Hanson’s Patterns of Discovery), or the sociological (in the broader sense of the term) factors (as is the case with Kuhn and sociologists of knowledge) that presumably accompany or facilitate the flashes of insight in a particular mind, be committed to reconstructing the objective situation in a science at the relevant historical moment. Such a situation in science is always characterised by a set of accepted propositions (i.e. metaphysical blue-prints) which constitute the ontology of science and set constraints as well as guidelines for its development, by logical relationships and mutual dependencies between the set’s elements, by accepted epistemological values, by normative (methodological) ideals for research,
and so forth.

Following Madej\textsuperscript{36}, we may call such situations which are responsible for the emergence of new ideas, \textit{objective discovery-generating situations}. The prime rôle in these situations are played by basic metaphysical conjectures which act as the premises from which the construction of new theories starts, and which also give rise to new sets of methodological guidelines (e.g. new sets of invariance or conservation principles).

It must be emphasised however that such a logic, despite its plausibility, is quite under developed at present. Even among those writers who \textit{have} paid attention to the rôle of metaphysical theories, or have sought to produce a logic of discovery, many have not bothered to introduce any \textit{preference} criterion for picking up the best available metaphysical theory at a certain time, and others have at most made a hand wave at the issue, and have not produced a full treatment of it. As a result the literature on this crucial issue is quite meagre.\textsuperscript{37}

Bearing the difficulty of the task in mind, we shall try to offer a number of criteria, which while not representing a complete and exhaustive set, are, it is hoped, on the right track and can be regarded as first steps in the direction of achieving a more comprehensible set of criteria.

In the first place, since virtually anything can act as an stimulus or motivating force for encouraging scientists to pay attention to certain phenomena rather than others, it is necessary to make a distinction between metaphysical elements which guide research and facilitate discoveries, and other non-empirical, non-metaphysical motivating forces. Because our concern in this essay is with the \textit{logic} of discovery and not \textit{psychology} of research, we only take into consideration those non-empirical elements which have both \textit{heuristic} and \textit{constitutive} values. In other words, we are only interested in the synthetic
a priori assertions about the world and its constituents.

The scope of these assertions (which are to be regarded as credible candidates for scientific-theory generating blueprints) should be wide. That is to say, they should cover a large range of facts of experience. The larger the scope, the more attractive the proposal. Moreover, the coverage in question must be translatable or developable into empirically testable theories. In other words, the metaphysical assertions at stake must be amenable to mathematical modelling. This provides us with a way of testing the viability of the metaphysical proposals:

Two different metaphysical views offer two different interpretations of a body of known facts. Each of these interpretations is developed into a scientific theory, and one of the two scientific theories is defeated in a crucial experiment. The metaphysics behind the defeated theory loses its interpretive power and is then abandoned.38

Each metaphysical doctrine carves the reality in its own favoured way and introduces a number of new categories and basic entities. This fact provides us with some opportunities for rational assessment of plausibility of the suggested metaphysics. Chief among these considerations is that of whether the newly introduced ontology causes difficulties for the established theories in different disciplines which are relying on the old ontology? Or does this new ontology promotes research in other fields in a smooth way consistent with the old ontology? In the former case, unless the proposed ontology is developed into a mathematically manipulatable model, it will remain of dubious value. However in the latter case, its rôle in further unification can be taken as a sign for its being on the right track.

The basic conservation, invariance, and symmetry principles are also of significance in deciding between rival metaphysics. If the new metaphysics, as is usually the case, is offering a new set of conservation, invariance and symmetry principles which violates the old and well established methodological rules, then it must provide
satisfactory explanation as to how the old principles can be regarded as the approximate cases of the new ones.

It follows form here, that if we want to understand the mechanism (i.e. logic) of scientific discovery, we have to reconstruct various discovery-generating situations. To accomplish this task, one, of course, must recourse to historical materials. But revealing the most general features of such situations, and the logical reconstruction of the passage from old to new scientific knowledge, is the job of the philosophers of science.

IV.B. Changing Aims and Methods

As science progresses, it is to be expected that the aim of fundamental theoretical science will change for the better as well. Better aims are introduced for fundamental research, by progressively putting forward better and better conjectures concerning the actual unified pattern which, we conjecture, is inherent in all natural phenomena. The discovery of these fundamental patterns constitutes our aim at each stage of scientific progress. However, if the basic aims of fundamental scientific enquiry are ever changing, and if each aim requires certain methods (ways and means) for its realization / appraisal, then rationality demands that the methods and methodological rules employed in science (e.g. the rules concerning invariance, conservation, correspondence, and symmetry principles) change and develop in a fashion corresponding to the changing aims.

As our knowledge and understanding improves, our ideas about the domain of our ignorance improves, our aim improves, and so too our methods. With improving knowledge, our knowledge about how to improve our knowledge improves as well. This in turn increases our chances of error elimination and acquiring more reliable knowledge. In other words there is a continuous trade-off between metaphysical blueprints, methodological rules and our scientific knowledge. Such a promising prospect, however,
is lacking in any philosophy of science which is limited to the confines of standard empiricism and adheres to fixed methods and fixed aims.

The picture which results from this way of looking at the scientific enterprise is somewhat like Laudan’s reticulational model, with the fundamental difference that this reticulational structure, contrary to that of Laudan’s, is being governed by one fixed aim (or if you like meta-aim), i.e. the ultimate truth about the most basic physical reality. The postulation of this fundamental aim which governs the whole enterprise of science would mean that the changes in basic aims of scientific enterprise (i.e., temporary conjectures concerning the basic underlying pattern) do not result in relativism.

It should also be noted that the change in the basic aims of fundamental science is not incompatible with the stability of a number, even a large number, of lower level aims. The change in the basic aims is, somewhat, like a Kuhnian revolution whereas the stability of the lower level aims and methodological rules resembles the situation during the periods of normal science activities. However, contrary to Kuhn’s conviction successive phases of scientific development (i.e. different paradigms in Kuhn’s parlance) are not incommensurable. A considerable degree of continuity and correspondence, is always present between successive paradigms even in cases of the most radical paradigm shift.

The changes in the symmetry, invariance and conservation principles, which are paradigms of methodological rules, can be best understood in this light: each new conjecture concerning the nature of the underlying pattern/structure of reality requires a new set of methodological principles and guidelines. In other words certain metaphysical propositions have prescriptive counterparts which can in turn be ‘translated’ into meta-statements about scientific hypotheses. An ontological thesis can obviously impose certain
constraints/conditions on the content of scientific theories which are operating within its boundaries, i.e. ontology may be taken to have prescriptive import. It is essential that such prescriptions be translated into propositions, or rather into meta-propositions to provide methodological rules. For example, within the confines of Newtonian metaphysics which states that the physical reality consists of point particles, the corresponding meta-proposition is that all laws of nature contain only concepts of position, time, mass, and density. The methodological guidelines are, Galilean transformations, conservation of energy, angular momentum and mass, and the mirror symmetry. In contrast in the metaphysics of relativity, the corresponding meta-principle is that there is no privileged reference frame, and the methodological rules are; Lorentz transformations, and conservation of mass-energy, as well as angular momentum.

V. Aim-oriented Empiricism

The above strands (as discussed in sections II, III, IV), constitute the backbone of a more viable theory of science known as Aim-oriented Empiricism (or AOE for short) due to N.Maxwell⁴¹. We shall briefly introduce this theory and assess its promises in dealing with the problems discussed in this essay. One such problem is providing a reasonable account of verisimilitude. The other is the all-important problem of induction. A third one is the problem of theory choice. And a fourth problem is the issue of continuity and change and the possibility of progress through revolutions.

The basic tenets of AOE can be put in the following way:

1. In contrast to the majority theories of science, realist and anti-realist alike, which advocate various versions of standard empiricism and would recognize only two legitimate domains for scientific activities, namely, the domain of empirical facts and the
domain of testable laws and theories, AOE maintains a third domain, namely the domain of metaphysical blueprints can be legitimately added to the proper realm of scientific activity.

2. AOE advocates the view that the basic aim for all scientific (theoretical as against technological) activities is striving towards truth, i.e., improving knowledge and understanding of the universe which is presupposed to be comprehensible. However, according to AOE, due to the changes in our metaphysical blueprints, which represent our best guesses as to how the universe is comprehensible, the aims of our fundamental theories will be in a state of change and evolution. Such changes will give rise to changing methods. It is important to appreciate that, within this framework of interconnected and changing aims, methods and theories, as our knowledge about physical reality improves, our knowledge about how to improve our knowledge also improves and thus gives us a better chance of reducing our mistakes and improving our understanding of the physical world.

3. AOE advocates the existence of a rational, non-mechanical though fallible method of scientific discovery. This method, as discussed earlier, rests on the possibility of producing various metaphysical blueprints and developing them into fully-fledged scientific theories. The advocacy of the existence of a rational method of discovery is, of course, based on the thesis that the comprehensibility of the universe is a part of current scientific knowledge.

4. AOE maintains that, to make proper sense of scientific activities, it not enough to interpret scientific theories realistically (where appropriate); it is also necessary to interpret these theories in terms of conjectural essentialism, that is to say, to regard them as attributing necessitating properties to the postulated entities. It is only in this way that
one can explain why the laws of the theory are obeyed.

The following diagram depicts, in a schematic way, the model of science introduced by AOE:

![Diagram of the model of science](image)

V.A. A Solution for the Problem of Verisimilitude

The key point in accounting for the growth of knowledge via a series of false theories, T₀, T₁, T₂, ..., Tₙ, ... and arguing for the progress of science and the increasing verisimilitude of scientific theories is the notion of "approximate derivation". If an explanatory empirically successful theory T₀ is superseded by a theory T₁, with greater explanatory power, empirical content, and empirical success, which explains the partial empirical success of T₀, then it can be said that T₀ is "approximately derivable" from T₁. What entitles us to regard such "derivations" as valid is that it is always possible to reformulate the derivation so that T₁ logically implies some T₀* (some approximate version of T₀).

Progress towards the truth can be viewed as a series of successive theories T₀, T₁,
in which each term is "approximately derivable" from its succeeding term, though not vice versa. The correspondence between entities postulated by these theories can be explained in the following way. Suppose that two successive theories in the above series, $T_i$ and $T_j$ ($i < j$) have postulated unobservable entities $E_i$ and $E_j$ respectively, and $E_i \not= E_j$, i.e. if $T_j$ is true (and $E_j$ exist) then $E_i$ do not exist. We assume that, as we move from $T_i$ to $T_i^*$ (as defined above) so we move from a theory $T_i$ which postulates precise entities with precise properties, $E_i$, to a theory $T_i^*$ which postulates imprecise entities with imprecise properties, $E_i^*$.

As an example of this distinction between precise and imprecise entities, consider the following two versions of Newtonian theory interpreted to be about unobservable point-particles interacting by means of gravitation.

**NT:** point-particles have precise Newtonian gravitational charge in the sense that the particles obey precisely $F = Gm_1m_2/d^2$.

**NT*:** point-particles have imprecise gravitational charges in the sense that the particles obey the imprecise law $F = Gm_1m_2/d^r$ with $r$ is some number between 1.5 and 2.5.

Here the point-particles of NT are precise unobservable entities, whereas those of NT* are imprecise, vague, or approximate.

Granted the truth of $T_i$, and the existence of $E_i$, we have also that $T_i^*$ is true and that $E_i^*$ exist. We can identify $E_i^*$ with entities $E_j$ in some special state. Suppose for example, that $T_i$ is Atomic theory with atoms interpreted to be 'corpuscles' — entities that are indestructible and without internal parts; suppose further that $T_j$ is the Rutherford-Bohr theory of atoms. Here $T_i$ and $T_j$ are incompatible. If $T_j$ is true and atoms $E_j$ exist, then corpuscles $E_i$ do not exist. Given $T_i$, we can however define $T_i^*$, which asserts merely that corpuscles behave as if indestructible and without internal parts. We can 'derive' $T_i^*$
from T_j by restricting the domain of T_j to systems of atoms interacting at sufficiently low energies for the atoms to remain in the ground state. In this domain the atoms E_j of T_j are identical to the imprecise corpuscles E_j* of T_j*.

**V.B. Change and Continuity**

The above account of verisimilitude furnishes us with a coherent way of looking at the problem of continuous scientific progress through changes and revolutions. The problem that has forced many realists to reconsider their realist conviction, namely, the paradox of acquiring knowledge by means of refutable and refuted theories, can be easily solved by AOE. According to AOE the progress of science is diachronic. It is not the case that all scientific conjectures introduce a progress in knowledge. However, in the case of successive theories which have enjoyed reasonable success, it will be possible to apply the notion of "approximate derivation" to show the continuity and smooth progress towards better understanding of physical reality. This point can be strengthened by noticing that in the case of the most radical scientific revolutions, only the top highly theoretical level will be destroyed, but the main bulk of the lower level structure of the superseded theory will be preserved, albeit occasionally under new interpretation. The retention of the explanatory content of the past successful, though refuted, theories within a restricted domain of phenomena ensures that unobservable entities approximately like those postulated by theories in question do exist. In this way, one can hold that scientific theories provide us with knowledge of unobservable world, even though they are, strictly speaking, false.

**V.C. The Problem of Theory Choice**

AOE’s model allows scientists to break away from the straitjacket of empiricism and to invoke extra-empirical values in judging the merit of theories without rendering
their own position inconsistent. From the AOE’s point of view, values like simplicity, unity, and explanatory power should be taken alongside empirical adequacy, predictive power, internal consistency, and coherence in the valuation of theories. These extra-empirical and non-logical values are not pragmatic criteria, which represent the personal preferences of scientists. They are, in contrast, real (albeit conjectured) features of the physical reality which our theories try to represent. Those theories which import these features in their structures, i.e. those theories which provide a more unified, simpler view of the universe in line with their respective metaphysical blue-print and combine it with empirical success are more likely to be on the right track than those theories which only offer empirical adequacy in an ad-hoc and cumbersome way.

V.D. The Problem of Induction

The age-old problem of induction also finds a reasonable solution within the framework of AOE, though it should be emphasised that this way of handling the problem of induction has had precedent in the history of philosophy which goes back to Aristotle. If the universe is lawful and nature acts uniformly, in the way described by our metaphysical (conjectural) blueprints and our scientific theories which are developed out of these conjectures, then the problem of induction ceases to be a problem any more. Of course, as was discussed above, we may have been wrong about the lawfulness of nature. In that case "the strangeness of reality" will refute our (as yet naïve) conjectures and will force us to revise our views of the sort of underlying pattern that we have assumed and the type of lawfulness we have ascribed to nature.

V.E. Objections to AOE’s Programme

It is now time to consider a couple of criticisms against AOE’s programme for science. One of the main objections to AOE is that by introducing metaphysical
presuppositions into scientific theories it endangers the objectivity of science. Thus in a recent book A.O'Hear, while criticizing empiricism, has observed that:

Maxwell's attempt to conflate the assessment of aims in science with the pursuit of knowledge is likely to be damaging to both activities... it must be perfectly obvious that the backing of a given set of political or metaphysical values is neither necessary nor sufficient for scientific truth... We cannot, as Maxwell appear to want us to do, assume in our scientific work one version of a specific value, and then expect that nature is obligingly going to fit it. In so far as nature impinges on the realization of our values, we may have to modify either the values themselves or our ideas about how they are to be applied. Huxley's point here is correct: people respect the findings of science precisely because they can be separated from sets of values, political or metaphysical, and provide a realistic framework for the realization of our ethical and political projects.^^

A number of points can be raised in reply to O'Hear's objection. Firstly, a preliminary clarification. O'Hear seems to be of the opinion that all non-empirical cognitive elements, are, from an epistemic point of view, on a par. This position does not seem to be correct. Metaphysical doctrines, as we have seen, are conjectures about the nature of physical reality, and as such have both heuristic as well as constitutive values. However political, moral, ideological, cultural and socio-economic elements or factors, are not necessarily about the factual reality. To put the factual statements on a par with the normative assertions, results in reducing metaphysics to methodology (as was the case with Popper). Conversely, to put normative assertion in the same boat with metaphysical statements, amounts to, as was the case with Laudan, the naturalization of methodology. However, neither of these two positions, as we have already noticed, are tenable.

Secondly, if as O'Hear has emphasised, "the backing of a given set of metaphysical values is neither necessary nor sufficient for scientific truth", then, (even forgetting about the fact that sufficiency thesis is something which no one has ever claimed) it will be of great interest to see how, a self-declared non-empiricist like O'Hear can solve the perennial questions of theory of knowledge, without an appeal to metaphysical assumptions concerning the nature of reality.

In defending a Bayesian approach for solving the problem of 'induction' O'Hear,
having mentioned the shortcomings of such an approach goes on to stress that:

Despite this, Bayes's theorem remain a neat way of suggesting that severe testability is not all we want in a scientific theory. We do want that, but on the assumption that we are conducting our scientific investigations in a relatively stable world (or relatively stable corner of the world), we also want theories which have some reasonable initial probability relative to what we already know. Our presuppositions are always with us, never more so than when we think we are doing without them.

O'Hear's difficulty which has led him to contradict and undermine his own position is that he wants to defeat empiricism on its own terms. He intends to uphold scientific realism, but is at pains to reconcile this conviction with the fact that our presuppositions are always with us. On the one hand, he has conceded that sharp demarcation between science and non-science is not possible, and yet, on the other hand he has emphasised that, "It is always possible to avoid falsification in these and other ways, but it is not scientific." It is as though he has already instructed the reader as to what is scientific and what is not.

The objectivity of science cannot be preserved by solely resorting to empiricist principles. The cases of Russell, Logical Positivists, and many other well known empiricists who all started their philosophical carriers with high hopes of defending realism and the objectivity of science but who ended in despair of degenerate forms of solipsism, is a strong *reductio ad absurdum* argument against this strategy. To preserve the much venerated objectivity of science one, far from avoiding metaphysical presuppositions about the nature of reality, should formulate them in explicit manner and subject them to rational assessments and criticisms. Here, one of the pieces of methodological advice offered by Popper and upheld Feyerabend, namely, *proliferation* is indeed quite commendable for our case: The more metaphysical blueprints at any given time, the better chances of developing more verisimilar scientific theories.

The second objections against AOE is directed towards the notion of necessity
invoked in conjectural essentialism which constitutes a central part of AOE. Maxwell, contrary to some other writers like Harré and Madden, and Bhaskar, has not appealed to the notion of natural necessity. Instead, he has tried to invoke the notion of analytic necessity in his model.

Maxwell's strategy to introduce analytic necessity in the discourse of science can be summarised in the following way: He has first argued against Hume's injunction on the possibility of the existence of necessary connections in nature. Secondly, he has argued that concepts used in science are by and large dispositional. Lastly, he has argued that fundamental theories of science can be interpreted essentialistically in the sense that they attribute necessitating properties to postulated entities which require, as a matter of logic, that the entities obey the laws of the theory.

One of the examples of a fundamental theory being interpreted essentialistically provided by Maxwell is the case of Newtonian theory. It consists of the following axioms: 

"(1) Everything is made up of point-particles with invariant material mass m and gravitational charge g, with m=g. Here (2) F = ma is true analytically, in virtue of meaning of 'inertial mass' and 'force': and likewise (3) f = Gg^2/d^2 is true analytically, in virtue of the meaning of 'gravitational charge'. This does not make Newtonian theory itself analytic, for (1) is a massively contingent assertion (which is in fact false given general relativity or quantum theory)."

Despite the apparent plausibility of this account, it seems there is some difficulty, yet to be resolved, concerning the meaning of the terms used by the fundamental theories. The terms used by T must obtain their meanings entirely from the rôle they play in T. This is because T, ex-hypothesi is regarded as the most fundamental theory and as such its terms cannot rely for their meaning and content on any other commonsensical or
scientific theory. However, in real science there are many more shades of meaning and extra-theoretic connotations and vagueness to scientific terms such as "force" or "mass" than what is allowed by the above postulates. In fact, as M.Scriven has argued long ago, one of the main features of scientific laws is their inaccuracy and lack of exact meaning. It is such inaccuracy and vagueness which makes room for further development of the theorise and laws.

Maxwell has tried to rebut this objection by allowing for a range of interpretations of the fundamental theories in question. These interpretations vary from the maximally essentialistic at one extreme, to the more and more anti-essentialistic at the other extreme. Maxwell has observed that in the case of Newtonian theory:

The meaning of 'force', 'inertial mass' and 'gravitational charge' becoming progressively more and more vague, presupposing the truth of laws that are more and more vague, as we move from the essentialistic to the anti-essentialistic. Thus the meaning of 'gravitational charge' is such that \( F = G \frac{g_1 g_2}{d^2} \) is true analytically, the meaning of 'gravitational charge' might be such that no more than 'objects with gravitational charge tend to move towards each other in some way that is proportional to their mass' is true analytically.

The move towards the less essentialistic interpretations, admittedly provides us with a wider base for giving content to the theories and meaning to its terms. However, it seems that the price to be paid is the loosening of the notion of analytic necessity central to conjectural essentialism.

Maxwell's one possible reply might be an appeal to the notion of "approximate derivation" discussed before. Loosening up the meaning of the theoretical terms in a way such that each new theory, while preserving its essentialistic nature, can be derived in an approximate way from the original model, would allow Maxwell to utilise the best of the two worlds, namely, to keep his analytic necessity and allow further contact with the real world of experience.

All in all, it seems that Aim-oriented Empiricism, in comparison to other existing
theories of science, provides scientists with a better methodological research programme and gives them a better chance of progress in their scientific investigations.
NOTES (Chapter Seven)

1. See Popper [1959/68], [1983].

2. Popper himself has referred to a "whiff of inductivism" in his approach and has noted that: "it enters with the vague realist assumption that reality, though unknown, is in some respects similar to what science tells us or, in other words, with the assumption that science can progress towards greater verisimilitude." (Popper [1974], p.1193). For a discussion of Popper's view on induction see O'Hear [1982].

3. R.Bhaskar [1979, p.23] has put this question in the following form, "What the world must be like for science to be possible?" (italic mine). The mere possibility however, as T.Benton [1981] has observed, is much too weak a notion. Because many other pseudo-disciplines (e.g. astrology, palm reading and sorcery) are also possible.

4. Bhaskar op.cit. p.23, has formulated this idea in the following way, "What must science be like to give us knowledge of intransitive objects ...?" Van Fraassen has put the essentially same question in this way, " ... how could the world possibly be the way this theory says it is? ..." ([1991], p.4. italics in original)

5. The conjecture of comprehensibility of nature, of course, has been known to generations of philosophers. Kant [1933/1970] for example, used this principle in his so called transcendental deduction. For extended discussion of this issue and references to works of the previous generations of philosophers see Stuart Brown [1977], pp.21-78.

6. "The most incomprehensible thing about the universe is that it is comprehensible" Einstein, quoted in Hoffmann [1972], p.18.

7. cf. N.Maxwell [1984], ch.9.

8. Occasionalism or parallelism is an old doctrine, introduced by some Muslim thinkers, like A.Asharee and M.Ghazzali of the eleventh century. The idea was later on revived in the West by some of Descartes’ followers like A.Geulincx. Occasionalists maintained a strict dualism and denied any interaction between mind and body, which they regarded as two separate substances. They held that when a person decides or wills to move his arm, it actually moves. But, his will does not cause his arm to move. Rather, there are two parallel series of acts going on simultaneously, one physical and the other mental. When somebody wills to move his arm, on that occasion, God moves it and thereby creates an action parallel to the person’s thought. Occasionalists believed that God has decreed this and other particular parallelism from the beginning of time. See S.Stumpf [1977]

9. For a rather thorough treatment of this issue see Maxwell, op.cit. [1984].

10. Popper [1971], in Lakatos & Musgrave (eds.).

11. Cartwright model, as we remember, is tailor-made to produce technological, engineering knowledge, which is less cumulative and less retrievable than the theoretical knowledge. Technological knowledge, however, when it comes to advanced technologies, is highly dependent on fundamental theoretical knowledge. In this sense, Cartwright’s model cannot dispense with the full-fledged realist universe.

12. Necessitating properties and essentialistic interpretations are discussed in section III.B below.
13. It needs to be emphasised that the number of layers and strata of which reality consists may well be infinite. However, scientists can, on methodological and pragmatic grounds, assume that at each stage of the development of science there exists a (hypothetical) bottom layer with the characteristics described in the text.

14. For Boscovich’s model see L.L. Whyte [1961]. The following diagram depicts Boscovich’s view of the force function.

![Force Function Diagram](image)

The force-function of Boscovich

15. Standard empiricism itself, as has already been discussed, is a metaphysical view. As such, rejection of metaphysics undermines it.


17. Lakatos ([1970], p. 15) for example, has explicitly rejected the idea that the aim of science is to progress towards the "Blueprint of the Universe". cf. N.Maxwell [1974].

18. N.Maxwell [1974] and D.C.Stove [1982] have shown how well-known standard empiricists like Popper, Lakatos and Kuhn have failed in making rational sense of the major problems of philosophy of science including the problem of theory-choice.

19. As pointed out in the first chapter, it must be emphasised that the unified grand theory, is not, as it is commonly said, "a theory of everything". It is only a theory which describes the most elementary constituents of matter and their interactions. The theory cannot, by itself, tell us all that is knowable about the universe. For that purpose other kinds of information are needed as well.

20. "The term 'blueprint' is used here precisely as a technical term ... to unite (or rather stands indifferently for) the two ideas 'most scientifically acceptable metaphysical theory about how the world (or relevant domain of phenomena) is intelligible' and "best aim for science" – two ideas that may of course be distinct for standard empiricism." (Maxwell [1974], p.146)

21. There are many studies concerning the development of different metaphysical blueprints into proper scientific theories. See for example, M.Hesse [1972], W.Berkson [1974], and Agassi [1968].

23. Throughout the text I shall be using the term disposition on a par with the term power, in the following sense:

'X has the disposition (power to) A =_{ad} if X is subject to stimuli or conditions of an appropriate kind, then X will A, in virtue of its intrinsic nature (which may well be — at the sufficiently basic levels — identical with the disposition')

Incidently it has been Popper among the non-positivist philosophers who has advocated the notion of dispositional properties. However, as we shall see in the text, despite his largely valid observations, he has failed to develop the consequences to their logical limits. For Popper's discussion of the dispositional properties see his [1959/68], appendix *X, and his [1963] passim. A.Wright [1990, pp.39,41] reports that modern major writers have by and large neglected the important issue of dispositional properties and have failed to address themselves to this topic. The exceptions are Ryle [1949], Goodman [1955], Carnap [1956], Popper [1959/68] and Quine [1960] who, save Popper, were all against the notion of dispositional properties. Such a negligence on the part of philosophers, as Wright points out undermine any putative attack on empiricism in that it promote the notion of 'event' to the primary epistemological position at the expense of interlinked notions of 'thing-kind' and 'disposition'.

In recent years, probably since 1970s, the idea of dispositional properties have become more fashionable amongst philosopher of science. A.Wright has produced a list of some of such advocates.


25. Popper [1959/68]

26. It is important to appreciate that the notion of dispositional properties introduced here amounts to an ontic necessity as opposed to accidental generalization: an object may break in many occasions that it is hit by an iron bar, and yet it may not be breakable in the ontic sense of the word. The ontic necessity, as indicated above, when translated into words, takes the form of analytic necessity: It is true by virtue of the very meaning assigned to it, in the same manner that 'All triangles have three sides ' is true analytically. This however does not mean the re-appearance of the undesired linguistic essentialism from the back door. The necessity at issue stems from the very fact that the structure of the object is such that it is necessarily breakable.

27. Cartwright seems to be in favour of this type of essentialistic theories with limited range of applicability. However, the view which tries to relate these theories to even more general, more fundamental, and more covering theories, as we have argued, gives the scientists a better chance of progress.

28. This point has been discussed by Harré [1973], Harré & Madden [1975], and Bhaskar [1975/78].

29. For details see N.Maxwell [1988].

30. Standard empiricism has encouraged its subscribers to draw a sharp distinction between the so-called context of justification and context of discovery. This distinction almost by definition puts all talks about rational assessment of the process of discovery, including the possible ways of constructing of a logic for scientific discovery, to the realm of psychology of research and thereby inhibits, from the outset, attempts for developing such a logic.

31. K.Popper [1968], p.31. In this respect the very title of Popper's *opus magnum*, 'The Logic of Scientific Discovery ' seems to be a misnomer. It reminds one of Mackie's *the Miracle of Theism* which is an antitheistic essay.

32. N.Maxwell is perhaps among the first writers who not only has advocated the possibility of a logic of scientific discovery but has also put forward credible programmes for achieving this goal. See his two parts paper of 1974.

One of course may cite Hadamard's *The Psychology of Invention in the Mathematical Field* [1945] and Polya's *How To Solve It* [1945] as two of the earliest systematic attempts on the study of the methods and rules of discovery and invention. The only limitation of these otherwise inspiring books may be that they only deal with examples drawn from mathematics.

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In recent years a larger number of writers have tried to show the untenability and undesirability of the
sharp distinction between the so called context of discovery and context of justification. These writers, by
and large and to various degrees of thoroughness have argued that a logic of scientific discovery is both
See also E. Pietruska-Madej [1985, pp. 7-18] for a similar position, though with less argumentation. Further
insights and arguments can be found in the two following anthologies M.D. Grmek et al. (eds) [1977],

33. ibid. p. 245. Zahar, as the above quotation indicates is in favour of a deductive logic for discovery. There
are other writers who would make use of inductive reasoning as well. See for example Bhaskar [1978].

34. N. R. Hanson [1958]

35. See T. Kuhn [1962].

36. op. cit. p. 17.

37. An exception, is N. Maxwell [1993], part II.

   It must be emphasized that despite his great emphasis on metaphysics, Agassi regards metaphysics as
only regulative ideas which are not part of scientific enterprise and play only a heuristic role for the
advancement of science. Thus in the same article he observes that:
   "Metaphysical ideas belong to scientific research as crucially important regulative ideas; and scientific
physics belongs to the rational debate concerning metaphysical ideas. Some of the greatest single
experiments in the history of modern physics are experiments related to metaphysics. I suggest that this
relevance to metaphysics contribute to their uncontested high status. And yet I contend that the metaphysical
theories related to these experiments were not part of science." (p. 193. emphasis added)

39. Kuhn, despite alterations and modifications in his initial version of incommensurability thesis [1962],
still defends his early intuition projected in his Structures. (c.f. Kuhn's Shearman Lectures, 23-25 November
1987, University College London).

40. See H. Post [1971].

41. Maxwell has developed this model in many of his publications. For a list of references see the
Bibliography. See especially his [1993a].

42. The account introduced in the text is based on N. Maxwell's views. Maxwell has discussed this issue in
various publications. See for example his [1993a].

43. There are a number of case studies by various writers who have actually shown, in some detail, how
to derive (in the approximate sense described above) various superseded theories from their successors. For
the derivation of Newtonian theory from Einstein's theory of relativity see Peter Havas [1964], Troels
E. Hansen [1972].

44. On this point see H. Post [1971].


46. The latter set, under certain circumstances may be amenable to conversion into constitutive elements.
   In this case, they will take the form of statements, describing the state of reality.

47. ibid. p. 49. The above quotation is of course not the only place, where O'Hear acknowledges the
'existence' of metaphysical constituents in scientific theories: 'I shall argue in chapter 6 that some of what
Kuhn says about the unfalsifiability of scientific paradigms, particularly at the level of their core or
‘metaphysical ’ assumptions, is correct.’ p.72. emphasis added.


49. *ibid.* pp.51-74.

50. *ibid.* p.64.

51. It should of course, be noted that standard empiricists who were fighting against metaphysical presuppositions, were themselves, acting within the confines of a (rather meagre) metaphysical picture of the world, namely, empiricism.

52. See Harré and Madden [1975], Bhaskar [1975/78].

53. See Maxwell [1966/74], [1993].

54. *ibid.*


57. *ibid.* p.93.

Adorno, T. *et.al.* (ed) [1976] *The Positivist Dispute in German Sociology* Heinemann London


Agassi, J. [1963] *Towards an a historiography of Science* Beiheft 2, to History and Theory


Bacon, F. [1962] *The Advancement of Learning* Everyman Library


Barnes, B. [1977] Interest and the growth of Knowledge Routledge and Kegan Paul
Barrow, J. [1991] Theories of Everything Vintage
Bell, J.S. [1987] Speakable and unspeakable in quantum mechanics Cambridge University Press
Bohm, D. [1951] The Paradox of Einstein, Rosen, and Podolsky reprinted in
Wheeler & Zurek [1983]


Bohm, D. [1963] *Problems in the Basic Concepts of Physics* An Inaugural lecture Delivered at Birkbeck College


Bohr, N. [1935] *Can Quantum Mechanical Description of Physical reality Be Considered Complete?* reprinted in S.Toulmin [1970]


Casti J.C. [1989]  *Paradigm Lost*    Abacus


Cushing, J. & MacKinnon E. [1989]  *Philosophical Consequences of Quantum Theory: Reflections on Bell’s Theorem*  University of Notre Dame Press


d'Espagnat, B. [1981/83]  *In Search of Reality*  Springer-Verlag


Devitt, M. [1984]  *Realism & Truth*  Basil Blackwell


329


Dummett, M. [1978] *Truth and other Enigmas* Duckworth


Einstein, A. [1949] *Reply to Criticisms* in, A.Schilpp Albert Einstein: Philosopher-Scientist


330

Feyerabend, P. [1962]  *Problems of Microphysics*  in R. Colondy (ed), "Frontiers of Science and Philosophy


Feyerabend, P. [1970]  *Consolations for the Specialist*  in Lakatos & Musgrave (eds.) "Criticism and the Growth of Knowledge"

Feyerabend, P. [1975]  *Against method*  Verso

Feyerabend, P. & M.W.Wartofsky (eds.) [1976]  *Essays in Memory of Imre Lakatos*  D.Reidel


Feyerabend, P. [1988]  *Farewell to Reason*  Verso

Feynman, R. [1985]  *Surely You Must Be Joking Mr. Feynman*  Unwin, London


331
Franklin, A. [1990] *Experiment, Right or Wrong* Cambridge University Press


Gillies, D.A. [1993] *Philosophy of Science in the Twentieth Century* Blackwell

332
Oxford & Cambridge


Grayling, A.C. [1982] An Introduction to Philosophical Logic The Harvester press Essex

Griffiths, A. [1967] Knowledge and Belief OUP


Gutting, G. [1980] Paradigms and Revolutions University of Notre Dame Press


Habermas, J. [1978] Knowledge and Human Interest Heinemann


Hanson, N.R. [1958/68] *Patterns of Discovery* Cambridge University Press

Hanson, N.R. [1963] *The Concept of the Positron: A Philosophical Analysis* Cambridge University Press


Harré, R. [1986] *Varieties of Realism* Blackwell


Hesse, M. [1961] *Fields of Force* Thomas Nelson and Sons Ltd.


335


Humphreys, W.C. (ed) [1973]  Constellations and Conjectures  Dordrecht, Holland

Ikenberry, E. [1962],  Quantum Mechanics For Mathematician and Physicists  New York Oxford University Press


Kanitsceider, B [1988]  Realism From a Biological Point of View  paper read at the VIIIth International Colloquium : Realism Today

Kant, I. [1933]  Critique of Pure Reason  tr. by Norman Kemp Smith, Macmillan

Kant, I. [1966]  Prolegomena to any Future Metaphysics that will be able to represent itself as a science  Manchester University Press


Kolakowski, L. [1972]  Positivist Philosophy  Pelican


Kuhn, T. [1962] *The Structure of Scientific Revolutions*  Chicago University Press


Laudan, L. [1976] *Two Dogmas of Methodology*  Philosophy of Science Vol.43 pp.585-597


Laudan, L. [1981b] *The Philosophy of progress*  P.S.A.


Laudan, L. [1984b] *Explaining the Success of Science: Beyond Epistemic Realism and*
Relativism in J.Cushing & G.Gutting (eds.) Science and Reality


Laudan, L. [1989a] If it Ain't Break Don't Fix it B.J.P.S. Vol. 40. No.3.


Lipton, P. [1991] Inference to the Best Explanation Routledge


Losee, J. [1982] A Historical Introduction to the Philosophy of Science Oxford
University Press


Mach, E. [1914]  *The Analysis of Sensations* Translated by C.M.Williams Open Court


339


Maxwell, N. [1984] *From Knowledge to Wisdom* Basil Blackwell

Maxwell, N. [1985] *Are Probabilism and Special relativity Incompatible?*" Philosophy of Science, Vol. 52, No.1

Maxwell, N. [1988] *Quantum Propensiton Theory: A testable resolution of the Wave/Particle Dilemma* The British Journal for Philosophy of Science, Vol.39, No.1


Maxwell, N. [1993c] *A Philosopher Struggles to Understand Quantum Theory: Particle Creation and Wavepacket Reduction*


in the Philosophy of Science, Vol VIII, pp.63-82.


Mehra, J & H. Rechenberg [1982] *The Historical Development of Quantum Mechanics* vol. 2. Springer-Verlag


Miller, A. [1990] *Sixty-Two Years of Uncertainty* NATO ASI Series


University Press


Newton, I. [1962] *Mathematical Principles of Natural Philosophy* Translated by A.Motte, revised by F.Cajori, University of California Press


Niiniluoto, I. [1987] *Truthlikeness* D.Reidel

Nye, Mary Jo [1972] *Molecular Reality* MacDonald London


Oddie, G. [1986] *Likeness to Truth* D.Reidel


342


Pais, A. [1982]  *Subtle is the Lord ....: The Science and the Life of Albert Einstein*  Oxford University Press


Papineau, D. [1984]  *Realism and Epistemology*  Philosophy, 367-388


Planck, M. [1932/77]  *Where Is Science Going?*

Planck, M. [1949]  *Scientific Autobiography*  Philosophical Library


343


Putnum, H. [1991] *Does the Disquotational Theory Really Solve All Philosophical Problems?* paper delivered at U.C.L. Department of Philosophy

Quine, W.V. [1953] *From A Logical Point Of View*  Harvard University Press

Quine, W.V. [1960] *Word and Object*  Cambridge University Press


Redhead, M.L.G. [1988] *Physics for Pedestrian*  Inaugural Lecture,


Rohrlich, F. [1983] *Facing Quantum Mechanical Reality*  Science, Vol. 221, pp.1251-


Rösseberg, U. [1990] *Historical Explanation in Modern Physics? The lesson of quantum Mechanics*


Rozental, S. (ed) [1967/85] *Niels Bohr, his life and work as seen by his friends and colleagues* North-Holland Physics Publishing


Salaman, E. [1976] *Remembering Einstein* Encounter


Schrödinger, I. [1935] *The present Situation in Quantum Mechanics: A Transition Schrödinger’s "Cat Paradox" Paper*


Searl, J. [1984] *Minds, Brains and Science* BBC


Shimony, A. [1989]  *Search for a world view which can accommodate our knowledge of microphysics* in J.Cushing & E.MacKinnon (eds.)


Skyrms, B. [1984]  *Pragmatics And Empiricism*  Yale University Press


Tarski, A. [1949] The Semantic Conception of Truth in Feigl & Sellars (eds.) Readings in Philosophical Analysis


Toulmin, S. [19553] The Philosophy of Science Hutchinson University Library

Toulmin, S. [1961] Foresight and Understanding Hutchinson


Trigg, R. [1985] Understanding Social Science Basil Blackwell


349
<table>
<thead>
<tr>
<th>Author</th>
<th>Year</th>
<th>Title</th>
<th>Publisher</th>
</tr>
</thead>
<tbody>
<tr>
<td>Van Fraassen, B.</td>
<td>1971</td>
<td>Formal Semantics and Logic</td>
<td>Macmillan</td>
</tr>
<tr>
<td>Van Fraassen, B.</td>
<td>1986</td>
<td>Aim and Structure of Scientific Theories</td>
<td>in Barean Marcus et.al. (eds.) Logic, pp.307-318. Methodology and Philosophy of Science VII</td>
</tr>
<tr>
<td>Van Fraassen, B.</td>
<td>1989</td>
<td>Laws and Symmetry</td>
<td>Oxford University Press</td>
</tr>
<tr>
<td>Van Rootselaar, B.</td>
<td>1968</td>
<td>Logic, Methodology and Philosophy of Science, Proceedings of the Third International Congress</td>
<td>Amsterdam</td>
</tr>
<tr>
<td>Vasyliov, M &amp; K. Stonowich</td>
<td>1970</td>
<td>Matter And Man</td>
<td>Mir Publisher, Moscow.</td>
</tr>
<tr>
<td>Vision, G.</td>
<td>1988</td>
<td>Modern Anti-Realism and Manufactured Truth</td>
<td>Routledge</td>
</tr>
<tr>
<td>Von Neumann</td>
<td>1955</td>
<td>Mathematical foundations of Quantum Mechanics</td>
<td>Princeton</td>
</tr>
</tbody>
</table>

350
University Press


Weinberg, S. [1983]  *The Discovery of Subatomic Particles*  Scientific American Library


Wright, C. [1987]  *Realism, Meaning and Truth*  Basil Blackwell