SCIENTIFIC THEORIES

A PHILOSOPHICAL ANALYSIS

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PREFACE.

In this essay I have considered some of the philosophical problems involved in attempting to settle the question, What are scientific theories about? And in order to expose these problems, I have dealt with two influential responses to this question of the referents of scientific theories - namely, logical empiricism and realism.

Now according to the logical empiricists, meaningful scientific theories are only about observable entities. Thus any entity that satisfies sentences of the observation language, e. g. (Ex)Ox, must also be an element of the set of entities that satisfy sentences of the theoretical language, e. g. (Ex)Tx. Stated more forcefully, the logical empiricists claim that observational and theoretical terms are coextensive. Having discussed the nature of this claim - in particular, the notion of an observable is spelled out in some detail, I analyse this view. An implication of this claim is that theoretical terms can be reduced to observational terms, or in some sense 'linked' to observational terms. Thus a major question has to be discussed: what is the relationship between these two types of terms, assuming that they are coextensive? In chapter three I try to show that we have no acceptable account of the relationship between observational and theoretical terms. Thus it appears that theoretical terms can not be reduced to observational terms. This conclusion then suggests that the two types of terms are not coextensive, after all. Perhaps the entities that satisfy the sentences of the observation language are not the same entities as those that satisfy the sentences of the theoretical language. This possibility has to be discussed - a discussion that deals with the realist's view of science. Chapter four deals with realism.

Two important points emerge from my discussion in that chapter:

a) The realist is committed to an ontology of observable and theoretical entities: both observable and unobservable objects are therefore thought to satisfy the sentences of scientific theories.

b) Imagination and the creative abilities of scientists feature prominently in the realist's view of science. For if science's primary aim is to understand the unobservable causal structure of the world, as the realist claims it is, how else can the scientist describe and identify these unobservable, or theoretical, entities?

So the realist is committed to an ontology of observable and theoretical entities. Now what role, if any, does the scientific community play in science? And in particular, must an account of the referents of scientific theories (e. g. logical empiricism or realism) acknowledge the influence of the scientific community? In chapter five I deal with these questions. Critically evaluating the views of one philosopher, representative of a number of philosophers, I try to demonstrate that he has overlooked an issue of prime importance - the influence of a background necessary to engage in the theoretical procedures of science: a scientist's descriptions and observations of his experiments are determined by his knowledge prior to any experiment he conducts. Having thus established the dependence of description and observation on a prior established conceptual scheme, I consider in some detail the role of the scientific community in science. This community, I argue, provides its members, the scientists, with a background which they rely on in their theoretical activities. And if it is primarily through education and training that scientists do acquire this necessary conceptual scheme, as I try to demonstrate, any investigation into the theoretical activities of scientists must acknowledge this influence of the scientific community. An account of the referents of scientific theories would therefore be incomplete if it overlooked this role of the scientific community in providing scientists with their necessary backgrounds.

But if the referents of scientific theories are determined by their respective communities, there might be no universal or uniform manner in which the question of reference of these theories can be resolved. In chapter six I deal with this implication of the previous chapter's discussion. More specifically, I discuss the problems of referential indeterminacy and ontological relativity. Having outlined some of the complexities in learning a language, and stressing the role induction plays in this learning process, I argue that we must acknowledge that language is referentially indeterminate: one can never be sure that the sentences of that language have been correctly satisfied. Thereafter, I argue for the necessity of a background coordinating system, or frame of reference, in determining the referents of a language. We must speak of the referents of a language only relative to this frame of reference or network. In other words, one is mistaken in seeking the referents of the terms of a language absolutely - reference, I argue, can make sense only relative to some network, or frame of reference. And if this frame of reference is itself referentially indeterminate, as I suggest it is, reference must be relative to a referentially indeterminate frame of reference.

Finally, in chapter seven I outline some of the implications of my analysis for logical empiricism and realism.

Throughout I have tried to maintain contact with at least one science, and at the same time attempted to say something that is not restricted to that science. The developments in electromagnetism in the first half of the nineteenth century serve as this contact. By the use of examples, drawn from this period, I have tried to support and illustrate my arguments in this thesis. The first chapter has therefore been written with these thoughts in mind.

I am grateful to my supervisor, Professor Ian Bunting, for his guidance in the development of this essay. I must also thank Dr. James Moulder for his helpful comments, and the Human Sciences Research Council for their financial assistance. (The opinions expressed or conclusions reached are not to be regarded as those of the Human Sciences Research Council.) Above all, I want to thank my wife not only for typing the script, but also for her constant encouragement and support.

CONTENTS

115. A picture held us captive. And we could not get outside it, for it lay in our language and language seemed to repeat it to us inexorably.

(L. Wittgenstein: Philosophical Investigations)

CHAPTER ONE.

SOME QUESTIONS ABOUT ELECTROMAGNETIC THEORY.

James Clerk Maxwell published his article, "On a Dynamical Theory of the Electromagnetic Field" in 1864. This article marked the highlight of an important phase in the development of scientific theories in electromagnetism. In this chapter I shall look at some of the factors that contributed to the development of Maxwell's theory. In addition to this I shall demonstrate the significance of this period in science for philosophy.

The history of science submits, no more easily than the history of other subjects, to a number of arbitrary time divisions. The appearance of certain seminal scientific ideas however, does make it possible for us to establish time boundaries which are sufficient for the division of our narrative. In this chapter I shall consider the appearance of some of these scientific ideas in the field of electromagnetism during the first half of the nineteenth century. This period in the history of science provides us with a wealth of material on many of the issues involved in science. The developments in electromagnetism at the turn of the nineteenth century illustrate many of the philosophical issues associated with the so-called 'scientific process'. This illustration shall then serve as the background to my analysis into scientific theories.

In order to place perspective on the developments in the field of electromagnetism I shall begin with an outline of some of the ideas contained in two rival theories of light. As we shall see, these ideas had a strong influence on the theorists in their formulations of electromagnetic theories.

1. An episode in the history of electromagnetic theory.

Towards the end of the eighteenth century there were two leading conceptions of the nature of light: the emission theory and the undulatory theory. In 1675 Newton had suggested that light consisted of corpuscles i. e. particles. These corpuscles thus behaved largely in accordance with the laws of mechanics. According to his so-called 'corpuscular theory', corpuscles of light were shot out at great speed in straight lines by luminous bodies. These corpuscles of light had properties that made them for instance, perfectly elastic when meeting a reflecting surface, yet highly penetrating when directed at transparent media.

Newton's ideas were in part confirmed by the work of Romer. The Danish astronomer Romer made the first successful estimate of the velocity of light in about 1676. From observations of small irregularities in the orbital periods of the moons of Jupiter, he had determined a velocity of light that appeared to confirm Newton's ideas about the speed of light. Newton's own work on gravitation led to an attractive explanation of another observation made by scientists - refraction of light. The deviation that occurred when light entered glass could be explained, thought Newton, by assuming that as each corpuscle of light approached the denser medium (the glass), the particle was attracted towards the glass. In other words, Newton thought that light behaved according to the laws of mechanics.

An alternative theory of light considered that a source of light acts as a generator of waves. According to this rival theory, all space and translucent materials were permeated with a medium, aether, that transmitted these waves of light. In the same way that a stone dropped into a pond causes circular ripples to move radially outwards over the water surface, so a light source caused spherical waves to move out through space. Huygens had been the first to expound this alternative theory in 1678. After his postulation of the so-called 'luminiferous aether', he applied the same principle that can be used to explain the behaviour of ripples and sound waves to the analysis of the propagation, reflection and refraction of light.

In contrast to Newton's prediction that light should accelerate when entering glass, Huygens explained refraction as a consequence of the slowing-down of waves as they passed from air into a denser medium, like glass. In the early part of the nineteenth century it was found that light exhibited the properties of diffraction and interference. These phenomena were difficult to explain on the corpuscular theory. Huygens' views began to gain increased acceptance. Huygens' wave theory of light was later practically universally adopted by the scientific community when Foucalt demonstrated (in 1850) that the speed of light in water was considerably less than its speed in air. The prediction made by Newton had been discredited. The corpuscular theory of Newton had finally been abandoned and replaced by the wave theory of Huygens.

Although the so-called 'corpuscular theory of light' had fallen from favour in the field of optics (Singer, 1943: 316-323), most scientists continued to rely on this particular theory in their research into electromagnetism. Electrical and magnetic charges were regarded as minute particles of electricity and magnetism - an electric current being a collection of these infinitesimal particles in motion between two poles. The movements of these particles were accounted for by Newton's laws - for example, the effects of attraction and repulsion that these charges displayed were due to the interaction of bodies (particles) at a distance. The Newtonian law of gravity and his laws on the inverse-square relationship between bodies had been extended to the field of electromagnetism. That is, Newtonian physics had been extended to electromagnetisn.

However, in the face of experimentation and explanations that relied on the corpuscular-theory, tests carried out by Michael Faraday in England were conducted on lines independently of those carried out on the continent. Rather than follow the example set by Coulomb and Ampere (two leading continental scientists working on electromagnetism), and other continental analysts, Faraday pursued ideas independent to those entertained by the continental analysts. Despite the fact that the scientific community in general subscribed to the corpuscular-theory to express electromagnetic forces, a method Maxwell admitted was a powerful method that had been "warranted by the universal consent of men of science" (Gillispie, 1960: 459), Faraday continued to experiment and state the laws he had discovered in ways which were not accounted for by the corpuscular-theory and Newtonian physics. It is history now that this refusal to comply with the prevalent theories led to significant in-roads into the fields of electromagnetism, and eventually, theories of light.

In his paper, "On Faraday's Lines of Force", Maxwell referred to this 'defiance' of Faraday to be committed to any particular theory (like the corpuscular-theory). In physics, Maxwell wrote, we must discover some method of investigation which allows the mind "at every step to lay hold of a clear physical conception, without being committed to any theory founded on the physical science from which that conception is borrowed, so that it is neither drawn aside from the subject in pursuit of analytical subtleties, nor carried beyond the truth by a favourite hypothesis". (Maxwell, 1856: 27) Physics, according to Maxwell, needs to steer between the Scylla of the abstract and the Charybodis of the concrete. (Gillispie, 1960: 460-461) This is what Faraday attempted to do.

Faraday had learnt of the 'mishap' at one of the lecture demonstrations given by Hans Christian Oersted in 1820. Scientists had long suspected that there is a connection between magnetism and electricity. It was Oersted who discovered this connection. When Oersted passed a compass needle at right angles to a wire carrying electric current, nothing happened. However, when the magnetic needle of the compass was placed parallel to the wire, it deflected to a position at right angles to the wire. This result was unexpected and an explanation was sought that would account for the 'irregular' behaviour of the compass needle.

Ampere took the investigation up immediately and proceeded to a mathematical analysis of these phenomena. Working with the Newtonian model of particles in motion, he formulated the theory that magnetism is the result of molecular electric currents - particles in motion. Ampere had applied Newtonian physics, by means of the 'corpuscular-theory', to electromagnetism. Faraday, on the other hand, worked in another direction.

Puzzled by the connection between electricity and magnetism, Faraday concerned himself with his so-called 'line of force'; a primitive conception that referred to the line passing through any point of space so that the line represents the direction of the force of the magnetic or electric effect. Any wire conducting electricity and any magnetised object would be surrounded by these 'lines of force' - hence Oersted's problem could then be explained in terms of the interaction of these lines of force surrounding the electrified wire and the compass needle. Maxwell became impressed with Faraday's ideas.

In his first paper on electromagnetism, Maxwell made a careful distinction between the approach to the subject he had chosen and the approach taken by the continental analysts - "that method which I have called the German one", as Maxwell was later to put it in his 'Treatise'. In "On Faraday's Lines of Force", Maxwell wrote that the then state of electrical science "seems peculiarly unfavourable to speculation". A large number of experimental results as well as "a considerable body of most intricate mathematics" had to be mastered before one could make any progress in electromagnetism. Rather than embroiling himself in this "considerable body of most inticate mathematics" that was bound to cause him "to lose sight of the phenomena to be explained", Maxwell proposed to make use of analogies, based on Faraday's ideas, to "search after the true interpretation of the phenomena". (Maxwell, 1856: 27)

Maxwell's paper relied on an analogy between the flow of an incompressible fluid and Faraday's lines of magnetic force. Extending Faraday's concept of 'line of force', Maxwell considered these lines of force "as fine tubes of variable section carrying an incompressible fluid". (Gillispie, 1960: 462) Not only could Maxwell now give an account of the irregular behaviour of Oersted's compass needle, in terms of Faraday's 'lines of force', but he was also able to account for the intensity of the movement of the magnetized needle. These 'tubes' that were "carrying an incompressible fluid" would then be "mere surfaces, directing the motion of a fluid filling up the space" around the electrified or magnetized object. The intensity of the movement of the magnetized needle would then be due to the velocity of the 'fluid filling the space' around the electrified or magnetized object. However, Maxwell emphasized that no matter how suggestive this analogy might be, it was no substitute for "a mature theory, in which physical facts will be physically explained". If the results of "mere speculation which I have collected are found to be of any use to experimental philosophers, in arranging and interpreting their results, they will have served their purpose, and a mature theory . . . will (eventually) be formed". (Gillispie, 1960: 462, my insertion) Maxwell himself was to provide this 'mature theory' nearly ten years later.

Maxwell was aware that there was supposed to be a difference between Faraday's way of conceiving phenomena and that of the other investigators in the field of electromagnetism. It has already been pointed out that he knew that there was "a considerable body of most intricate mathematics" that had to be mastered before one could make any progress in electromagnetism. He had resolved to read no mathematical literature on electromagnetism (literature that was influenced by the continental analysts), until he had first read through Faraday's Experimental Researches on Electricity. He knew that Faraday and the continental analysts were not "satisfied with each other's language". (Maxwell, 1881: ix) Once familiar with Faraday's ideas and investigations into the nature of electricity and magnetism, Maxwell intended to formulate these observations into a theory possessing mathematical form. His article, "On a Dynamical Theory of the Electromagnetic Field" constituted the expression of this formulated theory.

In this paper Maxwell showed that electromagnetic action travelled through space at a definite rate. Breaking away from the views of the continental analysts and their reliance on the corpuscular-theory, he formulated a theory that drew on the ideas of Faraday. Faraday's lines of force transversed the space between magnetised or electrified objects in waves, and these waves, like the waves of light, are "transverse to the direction in which the waves are propagated". (Singer, 1943: 316) Faraday's concepts of 'lines of force' and 'field' were analysed mathematically by Maxwell, and as David Steel (1970) points out, his mathematical analysis was later to yield an equation that described both the electric and magnetic fields. But, the unexpected and yet fascinating outcome of Maxwell's mathametical analysis of Faraday's research was the derivation of an equation of the "same form as that describing the wave theory of light". (Steele, 1970: 163) This unexpected outcome of Maxwell's mathematical analysis was to have far-reaching consequences both for electromagnetism as well as for theories of light.

Maxwell went on to calculate the expression for the velocity of his electromagnetic waves and discovered that it agreed closely with the velocity of light that had been experimentally determined by Fizeau. The discovery of oscillatory currents by Hertz in experiments he conducted between 1885 and 1887 was to provide the first 'direct' evidence in support of Maxwell's synthesis of light and electromagnetism. Maxwell had provided a theory that gave Faraday's research mathematical expression. He had formulated an elegant and coherent explanation of Faraday's observations. "On a Dynamical Theory of the Electromagnetic Field" represented that 'mature theory' that Maxwell had earlier said would be formed, "in which physical facts will be physically explained". The electromagnetic theory that had been formulated was the theory that the non-continental analysts required - a theory that would prove useful "in arranging and interpreting their results". (Maxwell, 1856: 30)

So much for my account of this period in science. Here I must add, however, that Maxwell was well aware of some of the philosophical problems involved in the scientist's attempts to formulate a theory. The following quotations appear in papers he wrote in 1853 and 1856, respectively:

> Design! The very word . . . disturbs our quiet discussions about how things happen with restless questionings about the why of them all. We seem to have recklessly abandoned the railroad of phenomenology, and the black rocks of Ontology stiffen their serried brows and frown inevitable destruction. (Campbell and Garnett, 1884: 339)

And again:

> If, on the other hand, we (decide to) adopt a physical hypothesis, we see the phenomena only through a medium, and are liable to that blindness to facts and rashness in assumption which a partial explanation encourages. We must therefore discover some method of investigation which allows the mind at every step to lay hold of a clear physical conception, without being committed to any theory founded on the physical science from which that conception is borrowed, so that it is neither drawn aside from the subject in pursuit of analytical subtleties, nor carried beyond the truth by a favourite

hypothesis. (Maxwell, 1856: 27)

These fairly extensive quotations appear because I think that they provide us with strong evidence that Maxwell was aware of some of the philosophical pitfalls theorists encounter in their attempts to formulate scientific theories. In the following section I shall outline a few of these issues - issues that still bother philosophers of science.

2. The significance of this episode.

There are many issues of philosophical interest encompassed by this illustration. I shall comment briefly on some of these problems in this section, and try to demonstrate that to a greater or lesser extent they all presuppose that we have a satisfactory answer to the central question of my thesis: what are scientific theories about?

2.1 Interpretation

It has been claimed that Maxwell's mathematical analysis of Faraday's research yielded equations that described the electromagnetic field. But what is it that was being described? What is the electromagnetic field? According to Maxwell, the electromagnetic field consists of a medium. On the other hand, the continental analysts assumed the existence of particles behaving according to Newton's laws. Now, Maxwell had known that the continental analysts and Faraday were not "satisfied" with each other's language":

> I was aware that there were supposed to be a difference between Faraday's way of conceiving phenomena and that of the (continental) mathematicians, so that neither he nor they were satisfied with each other's language. (Maxwell, 1881: ix)

But how would one know that their 'languages' were about the same things? And perhaps more importantly: how does one know what a particular 'language', or scientific theory, is about, let alone decide whether two or more scientific theories are about the 'same things'?

In his introduction to the paper, "A Dynamical Theory of the Electromagnetic Field", Maxwell admitted that the rival continental theory "may yet be useful in leading to the coordination of phenomena". (Magie, 1935: 529) Yet, having said this, he went on to state that due to difficulties with the continental theory, he had "preferred to seek an explanation of the facts in another direction . . . ".

(Magie, 1935: 530) Here again, the underlying assumption is that both Maxwell and the rival continental theorists were referring to the same facts. But were the so-called 'facts' Maxwell refers to the same facts that the continental theorists were speaking about? On the assumption that one already knew what the referents were of any one of these scientific theories, could we settle the question: were these scientists talking about the same things?

2.2 Theoretical terms

Maxwell and Huygens both spoke of mediums surrounding objects: the dielectric medium and the luminiferous aether, respectively. But do these mediums exist? Although the effects of the interaction of magnetic and electric phenomena are observable through a deflecting compass needle, perhaps, the medium itself that has been posited by Maxwell is unobservable. Clearly then, the term 'dielectric medium' is a theoretical term. Do theoretical terms, like 'dielectric medium' and 'magnetic field' refer to entities in the world? Some philosophers claim that non-theoretical terms refer to objects that are observable. Well, do theoretical terms also refer to entities - although non-observable entities?

Hempel has remarked that it "is a remarkable fact, that the greatest advances in scientific systematization have not been accomplished by means of laws referring explicitly to observables, i. e. to things and events which are ascertainable by direct observation, but rather by means of laws that speak of various hypothetical, or theoretical, entities, i. e. presumptive objects, events, and attributes which cannot be perceived or otherwise directly observed by us". (Keat and Urry, 1975: 17) Now why is this such "a remarkable fact"? On one account, the occurrence of theoretical terms in scientific theories is surprising, and a problem. Yet for the realist it is not surprising, nor problematic, that theoretical terms occur in scientific theories. So the question remains: do theoretical terms refer to existing, yet unobservable, items in the world?

Answers to the questions raised above again presuppose that we have settled the prior issue of what scientific theories are about. For unless this more general consideration has been adequately dealt with, one cannot tackle the specific problem of the referents of theoretical terms - terms that form part of a scientific theory.

2.3 Confirmation

Foucalt demonstrated that the speed of light in water was considerably less than its speed in air. This demonstration had apparently discredited Newton's prediction that light would accelerate on entering a denser medium. The discovery of oscillatory currents by Hertz is said to have provided the first 'direct' evidence in support of Maxwell's synthesis of light and electromagnetism. So, apparently Foucalt and Hertz were confirming the predictions made by Huygens and Maxwell, respectively. Well, were their experiments confirmation instances of the respective theories? To what degree can a theory, or hypothesis be confirmed? And if a scientist fails to falsify, has the theory or hypothesis then been confirmed? For that matter, need we distinguish between the failure to falsify and the confirmation of any theory or hypothesis? Harrè, for one, says yes, and criticises Popper for failing to maintain the distinction. (Harrè, 1972: 157-177) Would the confirmation one theory receives then entail that the rival theory ought to be rejected?

Here we have another example of an issue of philosophical interest that rests on the assumption that we have satisfactorily answered the question of the referents of scientific theories. Hertz could not confirm the predictions made by Maxwell unless he already knew what Maxwell's theory was about. Equipped with the knowledge that Maxwell's theory was about the electromagnetic field, and knowing what this electromagnetic field was, Hertz could proceed with his experiments to confirm that scientific theory. In short, the confirmation of any scientific theory presupposes an answer to the question, what is that theory about?

2.4 Commensurability

We have witnessed the rise and fall of a few theories. By 1850 the wave theory of light had been universally accepted by the scientific community. Newton's corpuscular theory had been abandoned. Similarly, Maxwell's theory ultimately 'triumphed' over the continental theories. Thus at certain stages in the history of science one or another theory dominated. Why? Was the dominant theory somehow 'a better theory' than the rival theory? Answers to these questions suggest affirmative replies to the following more fundamental queries:

a) Can scientific theories be compared?

b) Do we know what each of these scientific theories is about?

We cannot begin to adjudicate between Maxwell's electromagnetic theory and the continental theory without settling the prior question: what are these scientific theories about? If the one theory was about X and the other about Y, remarks about the 'superiority' of the one over the other are misguided in an important respect. Admittedly, on one level we can comment on the presentation or formulation of any two scientific theories, e. g. Maxwell's theory is perhaps more elegant and economical than the continental theory. But on another level, could we for instance declare that Maxwell's electromagnetic theory was better than Keynes's theory on the marginal propensity to consume? No. We could not adjudicate between these two theories because these theories are not about the same things to begin with. Thus before attempting to compare two, or more, scientific theories one needs to decide that these theories are about the same things.

2.5 Scientific change

As we have seen, the corpuscular view on the behaviour of electromagnetic phenomena was ultimately replaced by its rival - the wave theory. And for a while the wave theory was subscribed to by the scientific community, until it too was replaced by some other view. Why did these changes occur? Oersted's demonstration, that had encouraged Faraday to carry out research in electromagnetism, had posed a problem for scientists. This problem, or 'anomaly' as Kuhn calls it, brought about this shift - a change in view from corpuscles to waves. Oersted had prompted Faraday's researches, while Faraday in turn had provided those necessary "flashes of intuition through which a new paradigm is born". (Kuhn, 1970: 123)

So it was Faraday, with the impetus gained from Oersted's demonstration, that ushered in a new paradigm into the scientific community engaged in electromagnetic research.

But unless we knew what the earlier scientific theories (relying on the previous paradigm) were about, this change in perspective brought about by the new paradigm would escape our attention. Unless one already knew what the original scientific theories were about, one would not realize that a new paradigm had been born. It therefore becomes apparent that there is an intimate connection between questions on the changes in paradigms, and questions on the referents of scientific theories that rely on these paradigms: having determined the referents of a scientific theory one can appreciate the significance of a change in the paradigm previously associated with that theory.

3. Summary and conclusion.

As is clear by now, this episode contains many issues of philosophical interest. Having sketched in some of the main ingredients of this episode in the history of electromagnetism, I have isolated a few of the philosophically interesting issues relating to this illustration. I certainly cannot hope to deal with all of these problems - what would a complete treatment be like anyway? Instead I shall confine myself to an issue central to the episode and the problems raised here: what are scientific theories about? This issue has been selected because, as I have tried to demonstrate, any attempt to deal with many of the other issues raised by this illustration presuppose that we already know what a theory or theories are about. In my analysis I shall consider some of the philosophical issues involved in attempting to settle this crucial question.

But what is a scientific theory? This is certainly a question that cannot be tackled lightly. As Suppe points out, "if any problem in the philosophy of science can justifiably be claimed the most central or important, it is that of the nature or structure of scientific theories". (Suppe, 1974: 1) In my analysis I have accepted the view that scientific theories are formal systems. I have done so in order to gain a clearer understanding of the question central to my thesis: namely, what are scientific theories about? In the following chapter I shall say something, in broad terms, about the nature of scientific theories.

CHAPTER TWO.

FORMALIZATION AND SYSTEMATIZATION.

I said in chapter one that my main task in this thesis will be to analyse two influential responses to the question "What are scientific theories about?" But before discussing these replies to this question of the referents of scientific theories we need to say something in very general terms about scientific theories themselves.

Einstein wrote that science may be described "as an attempt to make the chaotic of our sense impressions correspond to a logically uniform system of thought".(Hutten, 1958: 29) The theory the scientist works with can be regarded as this 'logically uniform system of thought'. However, the term 'theory' is ambiguous:

a) the term may refer to a loose set of propositions formed by the scientist in an ad hoc manner,

b) 'theory' may refer to the organized coherent system of propositions that have been formed by the scientist.

I shall concentrate on the second interpretation of 'theory', preferring to confine the use of the term 'hypothesis' or a collection of 'hypotheses', to the first interpretation. As Hanson points out, in answer to the question, what is it to supply a theory: "It is to offer an intelligible, systematic, conceptual pattern for the observed data. The value of this pattern lies in its capacity to unite phenomena which, without the theory, are either surprising, anomalous, or wholly unnoticed". (Hanson, 1958: 121)(For a comparison with Hanson's views, see Achinstein, 1971: 121-156)

Both Einstein and Hanson have emphasized a common point: it is only when the propositions gained by the research and the investigations of experimenters, like Faraday, have been organized in a systematic manner that we begin to speak of a theory, or even a science at all. No mere listing or catalogue of truths is ever said to constitute a system of knowledge or a theory. We have a theory or system of knowledge only when those propositions setting out the results of the scientist's researches are organized in a systematic manner, to display their interrelation. If a system of knowledge or a theory is to be formed, those propositions must be arranged in a systematic fashion.

Experimentation and observation are therefore not sufficient conditions for the formation of a theory. An additional stage is essential - a stage concerned with the systematization of the propositions obtained by experimentation and observation. It is only when the propositions setting out what the scientist knows, due to his research, have been organized in a systematic manner that we can claim that we are dealing with a theory. Systematization is thus a fundamental feature of any theory. We will now consider in a bit more detail, what this systematization amounts to.

In any science, some propositions can be proved or deduced on the basis of other propositions. For instance, Kepler's laws of planetary motion and Galileo's laws of falling bodies are all derivable from Newton's more general laws of gravitation and motion. The discovery of these deductive interrelationships certainly was an exciting phase in the development of the science of physics. Likewise, the synthesis Maxwell made between his electromagnetic theory and the wave theory of light is another example of the surprisingly close relationship between two theories. These examples illustrate an important point - one important relationship among the propositions of a science or a theory is deducibility. Those propositions that contain knowledge of a subject (like electromagnetism) constitute a theory of that subject only when they have been arranged or ordered in such a way that some of these propositions are displayed as conclusions that have been deduced from other propositions. As Braithwaite points out, when writing about the structure of a scientific system: "A scientific system consists of a set of hypotheses which form a deductive system; that is, (a system) which is arranged in such a way that from some of the hypotheses as premisses all the other hypotheses logically follow". (Braithwaite, 1953: 12, my insertion)

However, besides the role of deduction in systematizing a theory, we can also identify a second feature possessed by a theory - definition. In any theory, some of the terms of the propositions of this theory are defined by other terms. For example, the term 'acceleration' is defined as 'the time rate of change in velocity' and a term like 'density' is defined as 'mass per unit volume'. And in defining some of the terms on the bases of other terms, the interrelations among the propositions of a theory are also revealed. This definition of some terms by means of other terms shows their concern with a common subject matter and also integrates the concepts of the theory - just as deduction integrates the laws or statements of this theory.

Ideally, a theory consists of propositions, all of which are provable or deducible, and these propositions contain terms, all of which are defined. This ideal is impossible to achieve, however. Deductions can establish their conclusions only on the basis of a given set of premisses - our starting points, the axioms. As it is impossible to prove all propositions or define all terms or justify all inference rules within any system or theory, except either circularly or by an infinite (and useless) series of definitions and proofs, we must accept that the system or theory must contain some undefined terms, unproved propositions or axioms, and inference rules that are unjustified. As Howard Kahane points out, it would be useless to define 'automobile' as 'car' and then to define 'car' as 'automobile'. "And it would be useless to attempt to present an infinite series of definitions, because to understand it one would have to run through the entire series, something it is impossible to do". (Kahane, 1973: 327) Primitives are unavoidable by-products of our attempts to systematize our knowledge. So, although a primary motive for constructing a theory is rigour, and the inclusion of undefined terms and unproved propositions may appear at first sight self-defeating, we must accept, albeit reluctantly, the fact that we are always stuck with some primitive terms and primitive propositions in our system or theory.

A number of philosophers of science have emphasized the role of deduction and definition as the two operations essential to systematization. Carnap serves as a prime example of these philosophers.

Carnap firmly believed that we can gain a clearer and greater insight into the claims made by the scientist if we analysed the so-called 'theoretical procedure' of science in terms of calculi and their interpretation: where 'theoretical procedure' refers to the activities of a scientist when he engages in attempts to formulate "the results of his observations in sentences, compares the results with those of other observers, tries to explain them by a theory, endeavours to confirm a theory proposed by himself or somebody else, makes predictions with the help of a theory, etc". (Carnap, 1953: 1) In his Foundations of Logic and Mathematics Carnap explicitly states that "it is one of the chief tasks of this essay to make clear the role of logic and mathematics as applied to empirical science". (Carnap, 1953: 2) The material on which the scientist works in his theoretical activities consists of reports of observations, theories, scientific laws and predictions. In other words, "formulations in language which describe certain features". (Carnap, 1953: 2-3) Accordingly, Carnap writes, "an analysis of theoretical procedures in science must concern itself with language and its applications". (Carnap, 1953: 3) An analysis of the theoretical procedures of science is then best carried out from the point of view of calculi, their interpretation and their application in empirical science. This analysis of the theoretical procedures in science in the light of calculi can clearly display the interrelations between the propositions of a theory, and can also outline the relations between various theories themselves. An analysis from the point of view of calculi would thus capture the fundamental feature of scientific theories - systematization.

Carnap stressed the importance that deduction plays in science. Consider, for example, some of the theoretical activities of an astronomer. (Carnap's example) Having made a number of observations of a certain planet, the astronomer describes his observations in a report, O1. In describing his observations the astronomer takes into consideration a theory T about the movements of planets. (T cannot be used in isolation, as Carnap rightly points out: "Strictly speaking, T would have to include, for the application to be discussed, laws of some other branches of physics, e. g. concerning the astronomical instruments used, refraction of light in the atmosphere, etc". (Carnap, 1953: 1) The astronomer would then deduce a prediction P, from O1 and T.

Having made this prediction, the scientist would then make a new observation and formulate it in a second report O2. The prediction P is then compared with the second report O2, thereby either confirming or refuting the prediction. Confirmation of the prediction P may provide the astronomer with a partial confirmation of theory T. Repeated applications of this procedure either provides the astronomer with an increasing degree of confirmation for T, or else a disconfirmation. On the other hand, the astronomer may be surprised by the second set of observations he makes - certain anomalies arise. Explanations would then be called for - explanations initially in terms of theory Y, but later perhaps in terms of other theories.

Here Carnap outlines some of the principal theoretical procedures in science: prediction, confirmation and explanation. For our purposes at the moment, however, the important point to notice is that for Carnap all these theoretical procedures "involve as an essential component deduction and calculation; in other words, the application of logic and mathematics". (Carnap, 1953: 2) If we then analyse the theoretical procedures of science from the point of view of calculi and their interpretation, we display this essential component, deduction. We thereby display the interrelations between the propositions of a specific theory and can also display the interrelations between various theories.

In my inquiry into the referents of scientific theories I shall pursue Carnap's suggestions made here that analysis be conducted from the point of view of calculi, their interpretation and their application. That is, I shall accept the formal approach proposed by Carnap. However, this is not to say that scientific theories are formal systems in the strict sense. A large element of idealization is involved in the claim that scientific theories can be construed as formal systems. (On the merits, or otherwise, of axiomatization see Suppe, 1974: 110-115; Hempel, 1970; Suppes, 1967: 57-59 and Kyburg, 1968) I have accepted this view of scientific theories insofar as it clarifies the issues I am interested in. This is not to say that the proper form for a theory is that of a strict axiomatic system. Instead, I am suggesting that this view of scientific theories is useful in that it provides us with a narrow framework with which to deal with the question on the referents of scientific theories.

CHAPTER THREE.

INTERPRETATION AND OBSERVATION.

In the previous chapter I accepted, for the sake of my discussion, that an analysis of scientific theories can be conducted from the point of view of formal calculi, their interpretation and their application. This view of scientific theories has the particular advantage that it permits us, when tackling the specific question of reference, to make use of the formal machinery Quine and Tarski have developed. They enable us to specifically rephrase the question of the reference of scientific theories as the question, "What sequences of objects count as the values of (i.e. satisfy) the bound variables of the sentences of scientific theories?" This is now the question to which I will turn.

This formulation now highlights two important issues:

a) What types of sentences are there in scientific theories?

b) Which objects satisfy these sentences?

In this chapter I shall consider an influential response to these questions - logical empiricism. Having explored, in general terms, the logical empiricist's answers to these two questions, I shall critically evaluate one aspect of the logical empiricist's position. More specifically, I shall consider the question of the relationship between observational and theoretical terms.

1. Sentences and objects.

According to the logical empiricist, we can identify three types of sentences employed in a scientific theory:

a) sentences containing only observation terms - terms from the observation vocabulary. (For convenience here, 'Ox' indicates an observational term or predicate. Therefore these sentences could be of the form '(Ex)Ox').

b) sentences composed solely of theoretical terms - terms found in the theoretical vocabulary. (Symbolized as '(Ex)Tx', where 'Tx' is a theoretical predicate.)

c) mixed sentences i.e. sentences composed of terms from both the observation and theoretical vocabularies. e.g. '(Ex)Ox.(Ex)Tx'.

For the logical empiricist, sentences from a) made up a sublanguage called an 'observation language', while sentences from b) constituted the sublanguage called the 'theoretical language'. The second question arises now: namely, what objects satisfy these sentences?

According to the logical empiricists these objects are observable entities. That is, the logical empiricists claim that language, and in particular, scientific theories, must be about observable entities if the language or these scientific theories are to be meaningful. Just as observable entities, and only observable entities, can be the values of the bound variables of sentences of the form (Ex)Ox, so observable entities, and only observable entities can be the values of the bound variables of sentences of the form (Ex)Tx if these expressions are to be meaningful. Embracing a doctrine known as the verification theory of meaning, the logical empiricists held that the only meaningful discourse was that conducted in terms of an observational language, or a language that used terms which were abbreviations for (i.e. could be rephrased equivalently as) expressions in an observational language. (Suppe, 1974: 12-15) On this account therefore, all cognitively significant discourse about the world must be empirically verifiable. And if scientific theories are to be classified as cognitively significant, then they too must be empirically verifiable i. e. be about observable entities. But what is an observable entity? Or more accurately, what do the logical empiricists think observable entities are?

There was an early disagreement between the logical empiricists whether observational terms ought to receive a phenomenalistic interpretation, or whether these terms should receive a physicalistic interpretation. Carnap initially favoured the former approach: the observation language would be a sense-datum language which would therefore provide a phenomenalistic characterization of experience, i. e. on this account, sense-data are observable entities. On the other hand, the observation language could receive a physicalist interpretation, where one now speaks, not of sense-data, but of material entities, and ascribes observable properties to them. The latter interpretation finally won out. As Suppe points out; "the Vienna Circle opted for physicalistic language very early". Thereafter, "remarkably little attention was devoted to further development or specification of the notion of being directly observable". (Suppe, 1974: 46) Carnap however, has provided us with as full a definition of 'observable' as the notion ever received:

> A predicate 'P' of a language L is called observable for an organism (e. g. a person) N, if, for suitable arguments, e.g., 'b', N is able under suitable circumstances to come to a decision with the help of a few observations about a full sentence, say 'P(b)', i.e. to a confirmation of either 'P(b)' or '-P(b)' of such a degree that he will either accept or reject 'P(b)'. (Feigl and Brodbeck, 1953: 63)

As an example, Carnap refers to the predicate 'red'. This predicate is observable for a person possessing normal colour vision: "For . . . say a spot (C) on the table before N, N is able under suitable circumstances - namely, if there is sufficient light at C - to come to a decision about the full sentence "the spot C is red" after few observations - namely by looking at the table". (Feigl and Brodbeck, 1953: 63) But whereas 'observable' applies to such properties as 'blue', 'hard' and 'hot' (where these properties are directly perceived by the senses)(Carnap, 1966: 225-226), the predicate 'an electric field of such and such an amount' is not observable to anybody, "because, although we know how to test a full sentence of this predicate, we cannot do it directly, i.e. by a few observations". (Feigl and Brodbeck, 1953: 64) Thus the intensity of the magnetic field can not be observed - it is inferred from what is observed, namely the position of a pointer on some instrument. (Carnap, 1966: 225-226)

I think we can, for our purposes here, adequately sum up this discussion with the remark that, for the logical empiricist, 'observable' applies to that which is directly perceived by the senses - whether it be sense-data only (as with the phenomenalistic interpretation of 'observable'), or whether it be material objects or entities (the physicalistic interpretation). I shall not pursue this particular matter any further. (See Austin, 1970 on sense-data) However, what certainly does require further attention is the more general claim, made by the logical empiricists, that scientific theories are only about observable entities (in whatever form one likes). This claim must now be subjected to detailed analysis. An implication of this view is that theoretical terms can be reduced to observational terms, or in some sense 'linked' to observational terms. Now is this possible?

The logical empiricists, as I have indicated, claim that scientific theories are only about observable entities: any object that satisfies a sentence of the theoretical language (i.e. a sentence of the form (Ex)Tx) must be an element of the set of objects that satisfy the sentences of the observational language (i.e. sentences of the form (Ex)Ox). Stated more forcefully, the claim is that observable entities satisfy both the sentences of the observation language and the sentences of the theoretical language - observational and theoretical terms are therefore coextensive. What then is the relationship between these two types of terms, assuming that they are coextensive?

In the remaining part of this chapter I shall consider this question on the relationship between observational and theoretical terms. Having explored the views of three philosophers representative of those directly affected by this question, I evaluate their suggestions. I want to argue that these suggestions are inadequate, and that we therefore have no acceptable account of the correlation between observational and theoretical terms.

2. A few suggestions about the relationship.

Hume wrote that all "the perceptions of the human mind resolve themselves into two distinct kinds, which I shall call IMPRESSIONS and IDEAS". (Hume, 1896: 1) Having drawn this distinction between ideas and impressions, which he further divided into 'simple' and 'complex', Hume argued that every simple idea we have has a simple impression. All simple ideas are derived from simple impressions, "which are correspondent to them, and which they exactly represent". (Hume, 1896: 4) Thus all the ideas we have, no matter how creative or original, are based on experience. In support of his claim, Hume offers the following argument: "To give a child an idea of scarlet or orange, of sweet or bitter, I present the objects, or in other words, convey to him these impressions; but proceed not so absurdly, as to endeavour to produce the impressions by exciting the ideas. Our ideas upon their appearance produce not their correspondent impressions, nor do we perceive any colour, or feel any sensation merely upon thinking of them". (Hume, 1896: 5) We always find, continues Hume, that any impression is constantly followed by an idea, which resembles it, "and is only different in the degrees of force and liveliness. The constant conjunction of our resembling perceptions, is a convincing proof, that the one are the causes of the other; and this priority of the impressions is an equal proof, that our impressions are the causes of our ideas, not our ideas of our impressions". (Hume, 1896: 5)

There are two closely related points that can be extracted from Hume's analysis:

a) All our ideas must come ultimately from impressions.

It is because our impressions always precede our ideas, says Hume, that we can claim that "our impressions are the causes of our ideas, not our ideas of our impressions". Clearly, Hume is a proponent of the view committed to the epistemological primacy of the observable. For if 'every simple idea has a simple impression, which resembles it' and 'every simple impression a correspondent idea', while 'our impressions are the causes of our ideas, not our ideas of our impressions', then all the knowledge a person has must, on Hume's account, ultimately be derived from his impressions i. e. experience.

b) Experience is the foundation of meaningfulness.

Take Hume's example: How do we give a child an idea of 'scarlet or orange, sweet or bitter'? In other words, how do we teach a child to understand the terms 'scarlet', 'orange', 'sweet' or 'bitter'? Hume says that one must not be so absurd "as to endeavour to produce the impressions by exciting the ideas. Our ideas upon their appearance produce not their correspondent impressions ..." (Hume, 1896: 5) Instead, one must "present the objects, or in other words, convey to him these impressions". In order to explain the meaning of terms, like 'sweet', 'orange' and so on, we have to present the objects i. e. present the referents of the terms that need to be understood.

Shapere has also drawn attention to these two points that I have raised here. According to him, the distinction between 'impressions' and 'ideas' was introduced by Hume primarily to fulfill two purposes: "For, in maintaining that all ideas are based on impressions, the distinction embodied the views that (1) all meaningful concepts (terms) obtain their meanings from experience and (2) all meaningful propositions (statements) are to be judged true or false, acceptable or unacceptable, by reference to experience". (Achinstein and Barker, 1969: 115-160) Experience therefore occupies a central role in Hume's argument: the referents of terms, if these terms are to be meaningful, must be things which are experienced. That is, for a term to be meaningful, that term must be satisfied by an observable entity. Now Hume may have mixed irrelevant psychological considerations into his argument, as some critics have pointed out. But what is of importance is the claim that all meaningful terms must refer to simple elements of experience. This view has been taken up by other philosophers.

Braithwaite (1953) has accepted Hume's suggestion that terms are meaningful only if they are definable exhaustively by a set of terms that refer to elements of experience. He writes that we use a calculus (i. e. a formal system) in an abstract science - a calculus "which we interpret as a deductive system". (Braithwaite, 1953: 51) We give "direct meanings to those formulae of the calculus which we take to represent propositions about observable entities; we (then) give indirect meanings to the other formulae . . . " (Braithwaite, 1953: 51, my insertion) He continues by saying that a "calculus designed to represent electrical theory will be constructed so that its final formulae express propositions about observable flashes of light or pointer-readings of a measuring instrument". (Braithwaite, 1953: 51) In other words, when a scientist interprets a calculus, he begins by providing some of the terms with a 'direct' interpretation - an interpretation about observable entities. The nonobservational terms, or theoretical terms will be given meaning (i. e. an interpretation) by virtue of their occurrence in the formulae of the calculus. (Braithwaite, 1953: 51-52) He writes that the formulae of the calculus about "fields of force, wave-functions, electrons and other theoretical concepts (as we shall call them) will be fitted to the deductive system derivatively in virtue of their place in the calculus". (Braithwaite, 1953: 51-52) Theoretical terms are thus given an interpretation indirectly. But the question arises: How are these theoretical terms interpreted indirectly? Or in other words, how are these theoretical terms related to observational terms?

Braithwaite rephrases this question: "The question is in what way a theoretical concept like an electron is an empirical concept; it cannot be answered by denying that an electron is an empirical concept at all". (Braithwaite, 1953: 52) He claims that Mach and Pearson provided answers to this question implicitly. Russell's doctrine of 'logical constructions' contains the explicit answer, says Braithwaite.

According to this view, electrons are regarded as logical constructions out of the observed events and objects by which their presence can be detected. As Russell said, the supreme maxim in "scientific philosophizing is this: Wherever possible, logical constructions are to be substituted for inferred entities". (Russell, 1953: 148) "This is equivalent to saying that the word "electron" can be explicitly defined in terms of such observations. Every sentence containing the word "electron" can, on this view, be translated without loss of meaning into a sentence in which there occur only words which denote entities (events, objects, properties) which are directly observable". (Braithwaite, 1953: 52-53)

Suppose an astronomer stated, "The sun is composed of hydrogen". For Russell, the astronomer's 'sun' must somehow relate to the ordinary man-in-the-street's definition of 'sun'. That is, theoretical terms, like the astronomer's 'sun', must "lead ultimately to terms having only ostensive definitions, and in the case of an empirical science the empirical terms must depend upon terms of which the ostensive definition is given in perception". (Russell, 1965: 32) And as Braithwaite realizes, the translations or correlations, between these theoretical terms and observational terms must be made - a task he assigns to the philosopher of science. In carrying out the translation, the philosopher of science will "show how the theoretical terms of a science can be explicitly defined by means of observable entities". (Braithwaite, 1953: 53)

I think that we are in the position to provide a summary of the most important points raised by the discussion so far. I think that the following statement is a general, yet precise enough statement of this view. This statement shall then serve as the basis of our subsequent discussion. On the assumption that the terms of the lexicon of any scientific theory can be divided into two distinct classes, i. e. observational terms and theoretical terms, the view we have considered can be summarized as follows:

> Statement A. An expression of a scientific theory that contains theoretical terms is meaningful if, and only if, these terms are exhaustively definable by observational terms.

This formulation highlights an important feature of the suggested relationship between theoretical and observational terms: there is an assumption that observational terms are themselves already meaningful. That is, this view presupposes that, as far as observational terms are concerned, there can be no doubt about the referents of these terms. As Quine would put it, this view clearly assumes that the observational terms are 'referentially determinate' to begin with - otherwise why regard observational terms as the termini of analysis when it comes to questions of meaningfulness?

I shall question the validity of this assumption that observational terms are referentially determinate later, in chapter six. In the next section, however, I shall evaluate the view I have outlined here: namely, the view as summarized in Statement A.

3. An evaluation of the suggested relationship.

Scientists in the field of electromagnetism often use the term 'magnetic'. For instance, someone might point at an object on the table and say, "The iron is magnetic", or "The electrified wire is magnetic", or "The magnetic effect is due to the lines of force surrounding the object" and so on. Now, according to the view I have discussed in the previous section, if these statements are meaningful (i. e. if they have referents), it must be possible to translate them without gain or loss in meaning by other statements that contain no theoretical terms. But how do we carry out this translation? A few suggestions have been submitted by philosophers on how to do this translation. In this section I shall evaluate two of these suggestions to "show how the theoretical terms of a science can be explicitly defined by means of observable entities". (Braithwaite, 1953: 53)

3.1 Operational definitions.

Take the statement, "The iron is magnetic". How do we translate this statement, without gain or loss of meaning by another statement that contains no theoretical terms? One method is by explicit definition. Using this method all theoretical terms are supposedly unnecessary because they can always be replaced by an observational term, its definiens. As Hempel points out, if "all the primitives of a theory T are thus defined, then clearly T can be stated entirely in observational terms, and all its general principles will indeed be laws that directly connect observables with observables". (Hempel, 1958: 50) One of the influential methods suggested to replace theoretical terms with observational terms is an operational definition:

a method Bridgeman (1932) introduced. (Also see P. Frank, 1961: 45-92)

This form of definition may be regarded as a rule for the replacement of terms. As Suppe has pointed out, operational definitions have appeared in a variety of guises e. g. correspondence rules, coordinating definitions, epistemic correlations etc. (Suppe, 1974: 17-61) Take the following statement:

a) "The object is magnetic".

This statement contains a theoretical term - 'magnetic'. Can we apply an operational definition and thereby replace this statement, without loss of truth, by a statement containing only primitive observational terms, as the proponents of operational definitions think we can? In other words, can we successfully translate this statement into a statement containing no theoretical terms? To do so, we need an operational definition of the form that an object has (by definition) a particular theoretical property sought if, and only if, it is such that if it is under test conditions of a certain kind, then the object responds in a certain way e. g.

> Operational Definition: "An object is magnetic if and only if, if any iron filings are placed near the object, these iron filings will move to the object".

(This can be symbolized as:

$$O.\ D.\ (x)(Mx \equiv (Fx \supset Ax))$$

This operational definition is supposedly the device we need to translate the previous statement containing the theoretical term i. e. statement a). Following this suggestion, we can translate this statement into a second statement containing no theoretical terms:

b) "The object is such that if iron filings are placed near it, the iron filings will move towards the object".

There are two important features of this statement that can be identified, after the operational definition has been applied:

i) The resultant translation is expressed as a conditional

i. e. if certain test conditions are fulfilled then the object displays certain responses. As I hope to demonstrate, this feature constitutes a serious problem for the adherents of operational definitions.

ii) Reference is only made to so-called 'observables'.

As Hempel has pointed out, "an operational definition gives experiential meaning to the term it introduces because it enables us to decide on the applicability of that term to a given case by observing the response the case shows under specifiable test conditions". (P. Frank, 1961: 59) Each theoretical term is supposedly replacable by observational terms, expressed as conditionals. Application of the operational definition thus provides theoretical terms with 'experiential meaning', and what matters in securing this 'experiential meaning' to a term "is simply that the relevant test conditions and the requisite response be of such a kind that different investigators can ascertain, by direct observation . . . whether . . . the test conditions are realized and whether the characteristic response does occur". (P. Frank, 1961: 59)

It appears then that the application of the operational definition has been successful - the theoretical terms have been exhaustively defined away and replaced by observational terms. This result was much sought after by the proponents of the view discussed in the previous section. Unfortunately, there are problems with this enterprise.

3.2 An evaluation of the operational definition.

How are we to translate the conditional in statement b)? Clearly, the conditional can be read as '$\supset$'. We can then symbolize this second statement as:

c) $Fa \supset Aa$ (Fx: the iron filings are placed near x; Ax: the iron filings will move to x; a: an object.)

The important question now is this: Does '$Fa \supset Aa$' have the same meaning as the statement containing the theoretical term - "The object is magnetic"? (Remember, the proponents of operational definitions thought that these definitions were the devices needed to translate without gain or loss of meaning.) In order to demonstrate that '$Fa \supset Aa$' does not have the same meaning as the statement containing the theoretical term, we shall look at a statement that would normally be regarded as false, yet when translated with the '$\supset$', turns out true. Or even more pointedly: I shall hope to demonstrate that something has been 'gained or lost' in the translation: the truth-value of the original statement.

Take the statement,

d) "Any piece of rope is magnetic".

According to the advocates of operational definitions, this statement can be translated as,

e) "If iron filings are placed near any piece of rope, these iron filings will move to the rope".

This becomes,

f) "$(x)(Kx \supset Nx)$". (Kx: the iron filings are placed near the piece of rope; Nx: the filings will move to the rope)

Now we know that any conditional statement that has a false antecedent is true. If we then do not place the iron filings near the piece of rope, i. e. if 'Kx' is not satisfied, our statement "$(x)(Kx \supset Nx)$" is still true, because as already mentioned, any conditional statement with a false antecedent is true. But if '$(x)(Kx \supset Nx)$' is true, how can it be the correct translation of the statement "The piece of rope is magnetic" which is false? How, in other words, can a statement (like 'd)') and its supposed translation (that of 'e)') have different truth-values? - an unfortunate outcome we are faced with here, if we subscribe to the view that theoretical terms are exhaustively definable by observational terms.

This unfortunate outcome is due to the conditional that appears in the suggested translation. I looked at a statement that was obviously false, but a similar criticism could be directed at a statement that may be true. Advocates of operational definitions stress the need for particular 'test conditions' or 'antecedent conditions': once the iron filings are placed near the object, then . . . But the use of the conditional in the suggested translation introduces a serious problem: whenever these test conditions are not fulfilled (e. g. '-Fx' of '(x)(Fx $\supset$ Ax)' is the case), the translation turns out true, irrespective of the truth or falsity of the original statement containing the theoretical term. There are two possible sources of this difficulty:

a) the conditional in the suggested translation has not been correctly interpreted by the '$\supset$'.

b) perhaps the '$\supset$' is correct, but the stress on providing definitions that are exhaustive is to blame.

The first suggestion that we replace the '$\supset$' has not met with much success. Attempts to read the conditional as some form of 'necessary connection' and thereby resort to modality have not been fruitful. The second suggestion has however, received much attention, especially by Carnap.

3.3 Carnap's suggestion about the relationship.

According to Carnap, there is a relation between theoretical and observatonal terms, but this relation has been distorted by the insistence on the exhaustive definability of theoretical terms. Theoretical terms cannot be explicitly defined away by observational terms, he argued, but this is not to say that no definition could be applied to theoretical terms. Carnap's so-called 'reduction sentences' would preserve this relationship between the two types of terms, but do so in a more flexible manner - the reduction sentences would give only a partial interpretation of the theoretical terms of a scientific theory.

In the simplest case, the operational definition that we used previously is replaced by a reduction sentence of the form that if an object is under certain test conditions, then it has the theoretical property sought if and only if the object responds in a certain manner. So, in the case of the theoretical term 'magnetic', the application of Carnap's reduction sentences provides us with the following formulation, symbolically expressed as

g) "$(x)(Fx \supset (Mx \equiv Ax)$"

Carnap's formulation escapes the criticism levelled at the operational definition. If the antecedent test conditions are not fulfilled, although the entire formula is then still true, "this implies nothing as to whether the object does, or does not, have the (theoretical) property . . ." (Hempel, 1958: 52, my insertion) The reduction sentence specifies the meaning of the theoretical term only partly, namely, for just those objects that meet the prior test conditions - for those objects that do not meet these prior test conditions the meaning of the theoretical term is left unspecified. As Hempel points out, "reduction sentences offer an excellent way of formulating precisely the intent of operational definitions. By construing the latter as merely partial specifications of meaning (interpretation), this approach treats the theoretical concepts as "open"; and the provision for a set of different, and mutually supplementary, reduction sentences for a given term reflects the availability, for most theoretical terms, of different operational criteria of application, pertaining to different contexts". (Hempel, 1958: 52) Thus according to Hempel and Carnap, reduction sentences are more flexible than operational definitions, in that they allow for different operational criteria in different contexts.

Both Hempel's and Carnap's reliance on reduction sentences and their belief in the 'superiority' of this form of definition over operational definitions provides us with a revision of Statement A that was formulated earlier. Instead of the emphasis on exhaustive definability, as was found in the work of the writers discussed earlier, the relationship between theoretical and observational terms is now to be viewed as follows:

> Statement B. There are at least some expressions of a scientific theory, namely, those expressions containing theoretical terms, that can be interpreted only partially by observation.

This statement of Carnap's and Hempel's view highlights the point that, according to this view, the theoretical terms in a scientific theory are not dispensable. Experience, or observation, cannot provide a complete interpretation of all the terms in a scientific theory.

3.4 An evaluation of Carnap's suggestion.

Is this position tenable? Are Hempel and Carnap correct in stressing the need for partial interpretation in science? This notion of 'partial interpretation' is certainly unclear, as both Achinstein (Achinstein, 1963: 89-105; and 1971) and Putnam (1962) have pointed out. This unclarity has encouraged at least the following six interpretations of the assertion that partial interpretation has a role to play in the interpretation of scientific theories:

a) To say that a term 't' has been partially interpreted means that the term has a meaning, but only part of that meaning has been given.

b) A term 't' is partially interpreted if there are no observational conditions all of which are logically necessary for 't' and whose conjunction is logically sufficient, but there are other sorts of analytic statements relating the term to observation terms.

c) A term 't' can be said to be partially interpreted if, among the sentences in which the term appears in the theory, there are none of the form 'ta≡øa', where øa is a sentence containing observational terms and the sentence is not analytic. (Achinstein, 1971: 85-91)

d) To partially interpret theoretical terms is to specify the nonempty class of intended models having more than one member. (By 'model' is meant a semantical interpretation of a theory's calculus or formal system such that all the axioms are true. A model for a calculus is thus an interpretation such that all the axioms and hence the theorems of the calculus turn out true.)

e) A term 't' is partially interpreted if a verification-refutation procedure is specified which does not apply to all individuals within the extension of 't'.

f) To partially interpret a language is to interpret part of the language (for example, leaving some terms mere dummy symbols, while providing translations into common language for the other terms).

Here are six possible, yet different interpretations of 'partial interpretation'. Which of these formulations is the correct one? Neither Hempel nor Carnap supply answers to this question. Suppe (1974), Achinstein (1963: 89-105 and 1971) and Putnam (1962) have assessed each of these versions of 'partial interpretation' and conclude that there are problems with each of these interpretations, although Suppe does suggest that a) captures part of what is involved. (Suppe, 1974: 86-95)

Now the fact that 'partial interpretation' is open to at least these six different interpretations implies that the suggested correlation between observational and theoretical terms is inadequate - an inadequacy due primarily, in this context, to the vagueness of a crucial notion, 'partial interpretation'. So, although Carnap's reduction sentences overcome the problems encountered by the operational definition, the vague notion 'partial interpretation' ushered in by the reduction sentences compels us to conclude that this account, in turn, is unacceptable. However, a further factor complicates the evaluation of Carnap's revision of Statement A.

4. Craig's suggestion about replacement programs.

Besides the problem of deciding on a precise meaning for the notion 'partial interpretation', an additional factor complicates the evaluation both of Carnap's revision of Statement A, and the previous suggestions. This complication is due to suggestions made by W. Craig. According to Craig, "the outlook for a program which includes a replacement program is dim". (Craig, 1956: 52) His reasons for this conclusion are twofold:

a) In the first place, "for any such program there remain the difficulties of providing an objective formulation and an effective dichotomy". (Craig, 1956: 52)

b) Secondly, this program is beset with the problems we have just been discussing, namely "the difficulties of replacing individual expressions". (Craig, 1956: 52)

By an 'objective formulation' Craig meant a formulation that "should leave no room for guesswork or individual interpretation": thereby guaranteeing an exact specification of what is meant by a correct solution in the program - a solution "carrying universal conviction". By an 'effective dichotomy' is meant "a manner which allows any one, given any expression, to determine in a finite number of steps whether or not the expression belongs to the class". If the auxiliary and nonauxiliary expressions of a theory have been effectively distinguished, "we shall say that an effective dichotomy has been given". (Craig, 1952: 39-40)

According to 'Craig's theorem' any theory that uses both theoretical and observational terms is functionally equivalent to a second axiomatized theory that has a lexicon containing only observational terms. The second theory is thought to be functionally equivalent "in the sense of effecting, among the sentences expressible in the non-theoretical vocabulary, exactly the same deductive connections as (the original theory)". (Hempel, 1958: 76-77, my insertion)

This certainly adds an interesting facet to the discussion. So far, as we have seen, the philosophers concerned with the problems of interpreting scientific theories have all attempted to determine the specific nature of the relationship between two apparently exclusive sets of terms in the theory's lexicon i. e. the relationship between the theoretical and the observational terms. On the one hand, we found authors using operational definitions to relate these two classes exhaustively. On the other hand, attempts were made to determine a more flexible relationship, using partial interpretations. Craig provides yet another suggestion: eliminate the replacement program altogether. Rather than attempting to replace theoretical by observational terms, using whatever method one pleases, Craig is suggesting that the philosopher of science ought to "formulate . . . programs in a manner which no longer includes replacement programs" (Craig, 1956: 52) at all. This suggestion is clearly counter to that made by

Braithwaite, who wrote that it was "the business of a philosopher of science to exhibit how these translations are to be made . . . "(Braithwaite, 1953: 53)

Can we accept Craig's suggestion? I think we cannot, and my reasons for saying so are briefly as follows: the main reasons for rejecting Craig's suggestion that science should avail itself of his method of formulating theories couched exclusively in terms which have direct observational reference and thereby ignore any replacement programs, is that

a) the set of axioms yielded by Craig's method is always infinite. This result leads to an undesirable loss of economy in attempts at systematization.

b) the resulting set of axioms do not lend themselves to inductive prediction and explanation.

c) the resultant theory has the pragmatic defect of being less fruitful heuristically than the system using theoretical terms. (See Hempel, 1958: 77-87 and Suppe, 1974: 32)

In addition to these reasons for rejecting Craig's suggestion, there remains a more fundamental issue, the consideration of which strongly suggests that his suggestion is unacceptable. For however 'radical' his suggestion may appear, it is still made on the assumption that the privileged status afforded observational terms is unproblematic. More specifically, Craig presupposes that observational terms are already meaningful i. e. referentially determinate. It is this presupposition that prompts Craig to claim that, even without a replacement program, the aims of an empiricist program may be less spectacular, but "perhaps more valuable. These aims (continues Craig) are to stand guard against frameworks of beliefs whose relation to experience is unreliable, and also patiently to improve and cement the relation to experience of those beliefs which do seem worth while". (Craig, 1956: 52-53, my insertion) It therefore remains to be seen whether this assumption that observational terms are referentially determinate is acceptable.

5. Summary and conclusion.

According to the logical empiricist, scientific theories must be about observational entities, if they are to be meaningful. Therefore, the claim is that observable entities satisfy the sentences of both the observational and theoretical languages of a scientific theory. Having outlined, in the first section of this chapter, what these claims amount to, in the remaining part I critically evaluated one important aspect of this view. More specifically, I examined the relationship between the apparently coextensive observational and theoretical terms.

Having looked at some of the work of three philosophers who stressed the priority of the observable, and who therefore had to find a method of dealing with non-observational (i. e. theoretical) terms, I summarized their view as follows:

> Statement A. An expression of a scientific theory that contains theoretical terms is meaningful if, and only if, these terms are exhaustively definable by observational terms.

The philosopher of science had therefore, on this view, to show how theoretical terms were related to the supposedly meaningful observational terms. (As I was to demonstrate later in the chapter, the stress on 'exhaustive definability' constitutes a serious problem for the advocates of this view.)

Now what is the relationship between the theoretical and observational terms of a scientific theory? In the second section of the chapter I took a look at two important suggestions: operational definitions and reduction sentences. Having considered how the operational definition worked, I put it to the test. On the surface it appeared that the operational definition had done the job - theoretical terms had been eliminated and replaced by observational terms. But, as I argued, the resultant formulation now contained a conditional. As I suggested, the problem was how to read this conditional.

My argument then tried to show that the conditional in the resultant translation (i. e. after the application of the operational definition) posed a serious problem: whenever the antecedent conditions are not met, the resultant translation turns out true, irrespective of the truth or falsity of the original statement, that contains the theoretical term. I suggested that this problem was perhaps due to the attempts to provide 'exhaustive definitions' for theoretical terms - how about a slightly 'weaker' form of definition: Carnap's reduction sentences?

After looking at Carnap's view that philosophers of science ought to work with 'partial interpretations' in their translation efforts, I presented the following statement containing his suggested revision of Statement A:

> Statement B. There are at least some expressions of a scientific theory, namely, those expressions containing theoretical terms, that can be interpreted only partially by observation.

As I pointed out, two major criticisms can be levelled at this suggested revision:

a) The notion 'partial interpretation' is vague.

b) Are observational terms already meaningful i. e. referentially determinate?

The first criticism on the vagueness of the notion 'partial interpretation' can be directed specifically at Carnap's suggestions. The second criticism applies to Carnap, but also to the other proponents of the view that interpretation must be based on observation. Having set out a few interpretations of 'partial interpretation', I concluded that due to the vagueness of this crucial notion, Carnap's suggestion is itself unacceptable.

However, as I pointed out, Craig introduces a problem into the discussion. Rather than worry about attempts to relate the two types of terms in a theory, the philosopher of science ought now to provide theories couched exclusively in observational terms, says Craig. I concluded the chapter with a look at his suggestion, briefly outlined three reasons for rejecting his suggestion, and added that a more fundamental issue requires attention. Craig assumes, as do the others in this chapter, that an observational language can function as a solid foundation for science. That is, Craig still assumes that we can specify in a straightforward way what the referents are of our observational terms. Thus one realized, I argued, that however novel his suggestions, we have here yet another philosopher of science who trades on the assumption that an observational language is referentially determinate. I intend to evaluate this fundamental assumption at a later stage.

CHAPTER FOUR.

REALISM.

The fundamental concepts of a theory, some philosophers believe, are the building blocks of its axioms and definitions: no bricks, no building. And according to the logical empiricists, these fundamental or primitive concepts must be observational terms. Observational terms must constitute the bricks of a scientific theory. In the last chapter I looked at an important aspect of this view - the relationship between observational and theoretical terms.

In my analysis I tried to show that the logical empiricists have not provided us with an acceptable account of the relationship between the apparently coextensive dichotomy of observational and theoretical terms. The investigation there now suggests that the two types of terms of scientific theories are not coextensive after all. That is, perhaps the entities that satisfy the sentences of the observation language are not the same entities as those that satisfy the sentences of the theoretical language? In this chapter I shall consider this possibility. More specifically, I shall look at the realist's view of science. Having explored these views in some detail, I want to highlight the point that according to this view of science, the sentences of scientific theories are satisfied both by observable and theoretical entities. I therefore want to show that the realist is committed to an ontology both of observable and theoretical entities.

1. The realist's view of science.

Maxwell and Huygens, as we have seen, both spoke of mediums surrounding objects: the dielectric medium and the luminiferous aether, respectively. But do these mediums exist? For although the effects of the interaction of magnetic and electric phenomena are observable - a compass needle deflects, for instance - the medium itself that has been posited by Maxwell is unobservable. Clearly then, the term 'dielectric medium' is a theoretical term. So the question arises: do theoretical terms, like 'dielectric medium' and 'luminiferous aether' refer to entities - entities that are unobservable?

For the realist, theoretical terms do refer to unobservable entities. Rom Harrè, for instance, is one philosopher of science who believes that theoretical terms have a denotation that is unobservable. (Harrè, 1965, 1972) According to Harrè, science "is actually interested in discovering the structure and inner constitution of natural things and their relations in the cosmos, in virtue of which phenomena display the regularities and irregularities they do. The use of theoretical terms is precisely the best way of achieving sciences's real aim, for they are just what lead to existential hypotheses about the unobserved. "(Harrè, 1972: 21) Thus for Harrè, and for that matter other realists, adequate causal explanations require the discovery, not only of regular relations between phenomena, but also the discovery of some kind of mechanism or inner structure possessed by the phenomena. As Harrè says, to "explain a phenomenon is to identify its antecedents and to identify or imagine the mechanism by which the antecedents produce or generate the phenomenon". (Harrè, 1972: 261) And it is the theoretical term that the scientist uses to refer to these unobservable mechanisms or inner structures. Therefore, for the realist, in explaining any particular phenomenon, not only must the scientist refer to those (observable) events that initiated the process of change, but he must also give a description of that (unobservable) process itself - a description that relies on the scientist's knowledge of the underlying mechanisms and structures, and the application of theoretical terms.

For the realist then, an important purpose, if not the primary function, of scientific theories is to enable scientists to give causal explanations of observable phenomena, and of the regular relations that exist between them. And these explanations must make reference to the underlying structures and mechanisms that are involved in the causal process. The task of scientific theories is therefore, claim the realists, to describe these "ultimate entities (that) must lie 'behind' whatever can be observed or detected". (Harrè, 1972: 260, my insertion) Therefore the central feature of a scientific theory, according to the realist, is its description of these unobservable entities, and of the way in which they operate to produce or generate phenomena.

But how does the scientist describe entities that are unobservable? Remember that Harrè, for instance, claimed that in order to explain a phenomenon, not only must scientists identify its antecedents, but they must also "identify or imagine the mechanism by which the antecedents produce or generate the phenomenon". (Harrè, 1972: 261) So the question remains: how does one 'identify, or imagine' the unobservable underlying structures of phenomena? That is, how does the scientist describe unobservable theoretical entities?

One of the methods used to identify and describe unobservable underlying structures, or theoretical entities, is the model. There are a number of interesting accounts of the application of models (and analogues) in science, but here I shall rely on the contributions of three authors - namely, P. Achinstein (1971); R. Harrè (1972) and F. Suppe (1974).

1.1 Models in science.

At the outset it is important to distinguish between two senses of 'model' - a mathematical sense and a non-mathematical sense. As Carnap has pointed out, while referring to mathematical models, a "model is said to be a model of the (axiomatic system) AS provided it satisfies all the axioms". (Carnap, 1958: 173, my insertion) Thus, suppose we have an axiomatic system AS and a given domain of objects D. Then, by a mathematical model of the primitives of AS, with respect to the domain of entities D, we mean a value assignment VA to those primitives, whereby the axioms of the system are satisfied i. e. turn out true. This domain of entities D may, for example, be a class of numbers of a certain kind, or of ordered k-tuples of such numbers, and so on. Therefore with this notion of 'model', i. e. mathematical model, one refers to the extensions of the primitives of the axiomatized system provided that all the axioms are satisfied. The realist philosopher of science, however, is more concerned with the other sense of 'model' - the non-mathematical sense.

The term 'model' in its non-mathematical sense can be usefully classified under one of the following three headings:

a) representational models
b) theoretical models
c) imaginary models

Representational models

A representational model is usually a three-dimensional physical representation of an object which is such that by examining it the scientist can ascertain facts about the object this model represents. (Harrè calls this type of model a homoemorph; Harrè, 1972: 38-43) This type of model would include tinkertoy models of molecules, models of solar systems found in museums and analogue models, such as that drawn by Maxwell, when he drew an analogy between the electric field and the tubes containing an incompressible fluid. Also, when atoms are thought of as being like billiard-balls, one is speaking about an analogue model, though still a representational model. Thus an analogue model is still a representational model, although no physical object has been constructed. As P. Achinstein points out, analogue models are one of four different types of representational model. (Achinstein, 1971: 209-211) The analogue model then need not be built, as are most other representational models, but only described. (The model in a case like this is still the three-dimensional object described, not the description of it.) Take Maxwell: he 'built' a model of the electrical field simply by describing these tubes of incompressible fluid, not by building them.

Theoretical models

When we refer to a set of idealized assumptions about the inner structure, the composition, or the mechanism of an object, we are referring to the second sense of model - the theoretical model. The Bohr model of the atom and the free-electron model of metals are two examples of theoretical models. According to Bohr, electrons revolve about the nucleus of an atom in such a way that their orbital angular momentum is quantized, as with the energy absorbed or radiated by the atom. (Harrè refers to the Bohr atom as an example of a 'multiply connected paramorph; Harrè, 1972: 49) According to the free-electron model of metals, valence electrons in the atoms of a metal are free to move through the volume of a given piece of metal. In this sense then, when scientists talk about a model of X, they are not referring to some object (as with representational models), but to a set of assumptions about X.

Imaginary models

Another type of model we can identify is the imaginary model. In this, its third use, 'model' refers to a set of assumptions about a system which are meant to show what that system would be like if it were to satisfy certain conditions. But the important point about these particular conditions is that they are not intended to be taken as assumptions any system actually satisfies. Poincarè's model of a Lobachevskian non-Euclidean world is one example of this type of model. This model attempts to show what a physical world could be like if it were to satisfy Lobachevsky's geometry. Poincarè assumed that in his spherical world the temperature is greatest at the center, decreasing toward the circumference, and so on. In using this imaginary model, one does not commit oneself to the truth, or even the plausibility, of the assumptions made. Similarly, Maxwell's mechanical model of the electromagnetic field is meant to show what this field could be like if it were to satisfy certain conditions initially specified. Thus an imaginary model is used to try and demonstrate that if particular initial conditions could be satisfied, then it is at least logically possible to suppose that the object or system is as it has been described in the model.

Now how would a scientist use a model, in any of the three senses outlined here? As already pointed out, for the realist the model provides the scientist with a method of describing unobservable structures and mechanisms. For in order to explain observable phenomena, and the regularities between phenomena, the scientist must attempt to discover and describe unobservable structures and mechanisms. And because these structures and mechanisms are unobservable, the scientist constructs models that are therefore regarded as hypothetical descriptions of these theoretical entities. Subjecting these hypothetical descriptions to empirical tests, the scientist develops as adequate an account of these theoretical entities as he can. And as some realists suggest, if these tests are successful, "this gives good reason to believe in the existence of these structures and mechanisms". (Keat and Urry, 1975: 35)

For the realist then, access to these ultimate, unobservable, structures and mechanisms can therefore only be by means of "that combination of reason and imagination" that constitutes "the main intellectual tool of creative science". (Harrè, 1972: 260) It is because the ultimate structures and mechanisms lie 'behind' whatever can be observed or detected, that the scientist must rely on "models and pictures, under the control of the principles of analogy" (Harrè, 1972: 260) to describe these theoretical entities. Therefore, according to the realist, scientific reasoning must rely on the use of reason and imagination to arrive at knowledge of the unobservable structures and mechanisms of the world. For without this imaginative capacity the scientist is unable to understand the unobservable causal structure of the world - science's primary aim, according to the realist.

2. Summary and conclusion.

For the realist, as we have seen, the scientist's task is to discover and describe the unobservable underlying structures and mechanisms of phenomena. Thus, in addition to identifying its observable antecedents, the scientist ought to describe the unobservable, or theoretical, entities that

that phenomena possess. But how does the scientist describe these theoretical entities?

As I pointed out, the model is one device that the scientist relies on in his attempts to describe those unobservable structures or mechanisms i. e. theoretical entities. According to the realist, through his use of reason and imagination, as manifested in the models he employs, the scientist develops an understanding of those theoretical entities he is studying. Therefore, having spelled out in some detail the views of the realist in science, I outlined three broad categories of 'model' as used by the scientist. Thereafter, I emphasized the point that because the scientist is primarily interested in understanding the theoretical entities possessed by phenomena, he must draw on his reason and imagination to describe these entities that are unobservable.

Now I think that the discussion in this chapter highlights at least the following two features about the realist's views of science:

a) The realist is committed to an ontology both of observable and theoretical entities. For clearly, the claim that the scientist's task is to discover and describe the underlying structures or mechanisms of phenomena operates on the assumption that there are these underlying structures or mechanisms to begin with. Thus when Harrè, for instance, claims that the "ultimate entities (and structures of the world) must lie 'behind' whatever can be observed or detected", (Harrè, 1972: 260, my insertion) he is suggesting that there are both observable and theoretical entities - entities that can be "observed or detected" and entities that "lie 'behind' whatever can be observed or detected". This reference to 'ultimate' here also suggests that the realist assumes that theories will eventually refer to the same domain of objects - those ultimate entities. Thus for the realist, not only will it be possible (eventually) to determine the referents of theories, but these scientific theories would remain neutral with respect to the types of entities referred to.

b) Imagination and the creative abilities of scientists feature prominently in the realist's account of science. This point follows the previous one closely. For if science's primary aim is to understand the unobservable causal structure of the world, how else can the scientist describe and identify these theoretical entities? For the realist, the scientist must, therefore, use his "imagination to arrive at knowledge of the ultimate (unobservable) structure of the world". (Harrè, 1972: 260, my insertion)

In the following chapter I shall take a closer look at these two features. More specifically, I shall attempt to demonstrate that the scientific community plays a vital role in the scientist's theoretical activities - for instance, both in his postulation of theoretical entities and in the formation of his creative abilities. For as is apparent, the realist underplays, if not overlooks, the role of the scientific community in his account of science.

CHAPTER FIVE.

SCIENTIFIC COMMUNITIES AND ONTOLOGY.

As we have seen, the realist is committed to an ontology of observable and theoretical entities: both observable and unobservable objects are therefore thought to satisfy the sentences of scientific theories. But what are these unobservable entities? And perhaps more importantly, are the theoretical terms of different scientific theories satisfied by the same domain of unobservable objects? Unfortunately I cannot answer these questions in full, but what I can do is provide an important part of an answer to these questions. In particular, I shall demonstrate that one must acknowledge the influence of the community when it comes to settling these questions of the referents of scientific theories. So what then is the role of the community in science?

Before dealing directly with this important question, I would like to evaluate a rather naive, yet influential, view of science. As I hope to show, the principal mistake of this view is that it has overlooked an issue of prime importance - the influence of a background necessary to engage in the theoretical procedures of science. Thereafter I want to argue for the more specific claim that any account of the referents of scientific theories must acknowledge the role of the community in providing scientists with this necessary background.

1. Northrop's view of science.

Let us return to the illustration that formed the background to this inquiry into the referents of scientific theories. As I have already pointed out in that chapter, Faraday had a strong influence on Maxwell. When Maxwell's electromagnetic theory appeared in 1864, many of the concepts of that theory could be traced back to Faraday. Maxwell had provided a theory that gave Faraday's research mathematical expression. He had formulated an elegant and coherent explanation of Faraday's observations. And as we saw, this 'Dynamical Theory of the Electromagnetic Field' was the theory that the non-continental scientists required - a theory that, in Maxwell's own words, would "prove useful in arranging and interpreting" (Gillispie, 1960: 462) the results of Faraday's research. And according to some philosophers of science, scientific theories such as Maxwell's 'Dynamical Theory', pass progressively through definite phases in their development. Northrop (1962) provides us with a clear account of this view of science. (Also see K. Popper, 1959; I. Lakatos, 1969: 149-186; P. Feyerabend, 1976; T. Kulka, 1977: 325-344 on this issue.)

I want to take a look at Northrop's view for two reasons. In the first place, although he uses other examples, my discussion of electromagnetism serves as a very good means of illustrating Northrop's view. Secondly, and more importantly, an analysis of his view underlines, so I think, an important part of my argument in this chapter - namely, the role of a background in the theoretical procedures of science.

1.1 An exposition of Northrop's view.

Now according to Northrop, there are at least three principal phases, or stages, that can be identified in the development of any theory. On this simplistic view, when Maxwell had translated Faraday's ideas into mathematical form and presented what is now his famous theory of electromagnetism, he had entered the final stage in the development of a theory - the stage where a theory had eventually been formed. On Northrop's view, we can identify at least the following three stages in the development of a theory:

a) The stage concerned with an analysis of the particular problem which initiates the scientist's inquiry.

b) The second stage which deals with the observation of the relevant facts to which the analysis of the problem leads the scientist.

c) Finally, a set of hypotheses that have been suggested by the facts are formed - hypotheses that may form part of the theory that has been formulated to explain certain phenomena which initiated the inquiry.

Maxwell's electromagnetic theory would have passed through each of these stages or phases in its development, according to Northrop.

The unexpected results of Frans Oersted's experiment in 1820 sparked off a vast amount of research. Here was a problem that could not be accounted for by the theories then current in the scientific community. New theories had to be formulated to explain the unexpected behaviour of that compass needle. The unexpected outcome of Oersted's lecture demonstration provided the stimulus that eventually was to culminate in Maxwell's electromagnetic theory over forty years later. The irregular behaviour of the compass needle provided the problem that initiated a great deal of inquiry - inquiry that demanded further tests, experiments and observation. Oersted had sparked off the first stage in the development of the electromagnetic theory.

When the editor of the British journal "Annals of Philosophy" asked a young man, Michael Faraday, in 1821 to undertake an historical survey of the work then being carried out in electromagnetism, the second stage in the development of the electromagnetic theory had been reached. When Faraday was asked to summarize that vast amount of activity in the field of electromagnetism that had been inspired by Oersted's discovery the year before, he was being asked to describe, classify and observe the results of a number of experiments then conducted in electromagnetism - the second stage in the development of the electromagnetic theory had materialized.

Faraday introduced a number of new concepts into electromagnetism. The concepts 'line of force' and 'field' are two examples of concepts that feature prominently in the ultimate theory Maxwell formulated. These concepts had been introduced to analyse the problems that he was investigating. Faraday introduced these concepts to facilitate his primary objective - to analyse the problematic situation brought about by Oersted's experiment. These terms were introduced by Faraday to clarify the recordings of his observations, and enabled him to classify his observations with greater precision. Speaking about the concepts 'anode', 'cathode', 'anions' and 'cations' that he had introduced in his Experimental Researches in Electricity, Faraday wrote that these "terms being once well-defined, will I hope, in their use enable me to avoid much periphrasis and ambiguity of expression". (Gillispie, 1960: 462) Equipped with a collection of concepts that had been clearly defined by Faraday, he was able to proceed with his investigations into electromagnetism with far more precision and rigour than would otherwise have been the case had he not introduced these concepts.

This second stage in the development of a theory comes to an end, according to Northrop, when the facts that have been designated by the analysis of the problem in the first stage "are immediately apprehended by observation, expressed in terms of concepts with carefully controlled denotative meanings by description, and systematized by classification". (Northrop, 1962: 35) Whereas the first stage in the development of a theory began with the discovery of a particular problem and ended with attempts to narrow down the inquiry to the relevant facts, the second stage begins with these immediately apprehended facts, and ends when these facts have been as adequately described as is possible. The importance of the introduction of new concepts (like those Faraday introduced) now becomes apparent - these concepts offer the scientist a means of ordering the 'chaos' he is attempting to describe and classify. Description and classification gain in precision.

When Maxwell published his article, "On a Dynamical Theory of the Electromagnetic Field", the third, and final stage in the development of the electromagnetic theory had been reached. The research carried out by the scientists in electromagnetism, especially the work done by Faraday, had received mathematical expression. A 'mature theory' that Maxwell had spoken about in which certain facts would be explained had been formed - a theory that would be useful "in arranging and interpreting" the results of the "experimental philosophers". (Maxwell, 1856: 30)

Over the last few pages I have outlined the development of a theory - Maxwell's electromagnetic theory. Now, on Northrop's account of theory formation, theories pass progressively through some finite number of distinct phases, or stages; in our case, three stages. Theory formation is neat and regular. And implicit throughout this account is the view that these theoretical procedures are conducted independently of any background. For instance, Northrop assumes that it is possible for the scientist to observe, classify and describe scientific research independently of any theory, or prior established conceptual scheme. That is, he assumes that there is a simple and neat (i. e. untainted) way of satisfying the scientist's observation language sentences. Now can we accept this view? More specifically, can we accept the view that observation is theory-neutral?

1.2 An evaluation of Northrop's view.

On what basis did Faraday describe, classify and observe the results of a number of experiments then conducted in electromagnetism? Consider the following example, due to Duhem. (Duhem, 1962: 145) Suppose Faraday went into a laboratory to record his observations. On the table he finds many different instruments - copper wire wrapped in silk, an electric battery, coils, vessels filled with mercury, and so on. Plunging the metallic stem of a rod that is mounted with rubber, into small holes, Faraday notices that a piece of iron oscillates. This oscillating piece of iron has a mirror attached to it, and this causes a spot of light directed at the mirror to vibrate. Here we have an experiment - Faraday can minutely observe the oscillations of the piece of iron by means of the vibrations of the spot of light. But, the important question is this: what is happening? And how must Faraday describe what is happening?

Does Faraday reply that what he is observing is simply the oscillations of the piece of iron carrying the mirror? Not likely. Instead, one is bound to be told that the experiment is meant to establish the electrical resistance of a coil. This reply may be surprising, but as Duhem points out, if "you are astonished and ask him what meaning these words have, and what relation they have to the phenomena he has perceived . . . he will reply that your question would require some very long explanations, and he will recommend that you take a course in electricity". (Duhem, 1962: 45) Not only must one be attentive and alert enough to make an observation, of a scientific experiment perhaps, but one must also attempt to understand what has been observed. "Any man can, if he sees straight, follow the motions of a spot of light on a transparent ruler, and see if it goes to the right or to the left or stops at such and such a point; for that he does not have to be a great cleric. But if he does not know electrodynamics, he will not be able to finish the experiment, he will not be able to measure the resistance of the coil". (Duhem,1962: 145) In short, a description of the experiment as well as the carrying out of the experiment relies on two essential components - observation and interpretation.

Not only must the experimenter observe certain happenings on his laboratory table, but he must also grasp the significance of the vibrating piece of metal. As Duhem says, an experiment in physics "is the precise observation of phenomena accompanied by an interpretation of these phenomena; this interpretation substitutes for the concrete data really gathered by observation abstract and symbolic representations which correspond to them by virtue of the theories admitted by the observer". (Duhem, 1962: 147) In other words, what Faraday observed and thus reported on was greatly influenced by his prior knowledge of electromagnetism. It is impossible to 'merely observe' - one can only grasp the significance of an experiment in the light of some prior knowledge. The scientist's report of his experiments will thus be relatively heavily laden with his background knowledge, or theory.

Here we have the essence of Duhem's example: when scientists report on their 'observations' they formulate descriptions that already presuppose an understanding of certain scientific theories. As Duhem observes; "Open any report at all of an experiment in physics and read its conclusions; in no way are they purely and simply an exposition of certain phenomena; they are abstract propositions to which you can attach no meaning if you do not know the physical theories admitted by the author". (Duhem, 1962: 147-148)

The scientist does not report that he is simply studying the oscillations of a piece of metal carrying a mirror. No. Instead, he claims to be measuring the electrical resistance of a coil - a claim that presupposes some knowledge of electromagnetism. But that is not all.

Not only does the scientist's report of his observations or perceptions presuppose particular scientific knowledge, his observations themselves are influenced by the present state of scientific knowledge. What the scientist observes (i. e. perceives) is determined by his previously established scientific knowledge: his theories determine what it is that the scientist perceives.

As Hanson has pointed out, scientific theories "provide patterns within which data appears intelligible. They constitute a 'conceptual Gestalt'. A theory is not pieced together from observed phenomena; it is rather what makes it possible to observe phenomena as being of a certain sort, and as related to other phenomena". (Hanson, 1958: 90) Where Faraday detects an increase in electrical resistance, the unscientific man-in-the-street only perceives a moving beam of light. Where Freud observes an instance of the Oedipus complex, the unscientific man-in-the-street only witnesses an instance of healthy affection. Where Heisenberg observes orbiting electrons, the unscientific man-in-the-street only sees white puffs in a chamber. In all these cases, the scientist's report of what he is observing as well as the observations themselves differ from that of the unscientific man-in-the-street. If a person's report squares with his observations, we must conclude that the scientist's observations are different from those made by the unscientific man-in-the-street. For if the descriptions of the phenomena differ, and the descriptions do correspond with the observations made, the observations themselves must be different to start with.

Feyerabend for instance, speaking of Galileo's observations of the moon, through his telescope, has also referred to the influence of the then prevalent state of knowledge on those observations: "We must also remember the many conflicting views which were held about the surface of the moon, even at Galileo's time, and which may have influenced what observers saw". (Feyerabend, 1976: 130) Now why is there this difference between the observations, or the reports of these observations, of the scientist and the unscientific man-in-the-street? How do we account for this difference?

Clearly, it is the scientific training and education of the scientist that influences what it is that he observes. Equipped with a prior established conceptual scheme, or theoretical framework that he has learnt, the scientist is able to make a number of observations - observations that are thus determined by his knowledge prior to the experiment. Without his scientific knowledge, gained from his education, a person would not report that what he is 'observing' are orbiting electrons, or instances of the Oedipus complex or electrical resistance. I think that we are now in the position to appreciate Northrop's mistake.

Northrop provided us with a view on science that tacitly assumed that one can conduct any of the theoretical procedures of science independently of any background. And in particular, Northrop assumed that observation is theory-neutral. He thought that it was possible for the scientist, operating especially on the so-called first and second stages in the formation of theories, to encounter a problem and write up a report of his observations independently of any scientific theory, or prior established conceptual scheme. Northrop therefore overlooked the vital role a scientist's background, in the form of scientific knowledge especially, plays in his theoretical activities, for instance in his attempts to formulate theories. He has overlooked the necessity of a background that not only selects which experiences are problematical, but even defines what are to count as phenomena. Oersted's lecture-demonstration did not 'throw out' a problem independently of any context - that event in 1820 that provided scientists with a problem occurred within an already established conceptual framework used by the scientists. The sorts of 'phenomena' that provide scientists with a problem are those that deviate from an expected pattern or normality. Only when phenomena deviate from the norm, as stipulated by the already existing theory or conceptual scheme, can the phenomena be classified as 'problematical'. It is thus only relative to this background or conceptual scheme, that one can speak of a problem at all. This background therefore first determines what a scientist takes to be his problems. But perhaps even more importantly, this background determines what are to count as the scientist's so-called 'observations'.

A scientist's background therefore plays a vital role in his theoretical activities. I have suggested that Northrop has overlooked the importance of this background in science. I need to take a closer look at this influence on the scientist. In my subsequent discussion of the nature of scientific communities, and particularly in my discussion of the role of education in scientific communities and scientific theorising, I will be closely following the views of Thomas Kuhn (1970).

2. Communities and the referents of theories.

Does the community play a role in science? More specifically, must an account of the referents of scientific theories acknowledge the role of the community? Why? In what follows I shall address these questions. In particular, I shall attempt to demonstrate that the satisfaction of scientific theories is strongly influenced by the scientific community. For the most part the discussion will rely on my illustration of the developments in electromagnetism.

In his first paper on electromagnetism Maxwell had made a careful distinction between the approach to the subject he had chosen and the approach taken by the continental analysts. And as we have seen, the scientists working on the continent relied on the corpuscular theory of light in their research into electromagnetism. Magnetic and electrical charges were therefore regarded as minute particles of magnetism and electricity. These continental scientists thought that Newtonian (i. e. corpuscular) physics could account for the behaviour of electromagnetic phenomena. Thus these scientists were not only practitioners of a particular scientific speciality (namely, electromagnetism), but these scientists also shared a certain conceptual scheme - a theory founded on the corpuscular behaviour of matter. In short, these continental scientists constituted a scientic community. But what is a scientific community? Before assessing the influence of such a community on scientists, I would like to consider the distinguishing features of a scientific community.

2.1 Characteristics of a scientific community.

In my illustration we find what I think may be regarded as the two necessary features of a scientific community:

i) The members of the scientific community operate within the same field of research.

ii) The scientists share a paradigm, or as Maxwell put it, a "way of conceiving phenomena". (Maxwell, 1881: ix)

Not only must the members of a scientific community be engaged in research in the same area, in electromagnetism as opposed to cancer research for instance, but these members must also share a paradigm - for example, the view based on the corpuscular behaviour of matter. Geographical differences would therefore not imply that scientists necessarily belonged to different scientific communities. As long as the above two conditions are met, I think it would be correct to claim that a given group of people belonged to the same scientific community.

However, what is a paradigm? Some remarks on this question are needed before continuing with the discussion. The term 'paradigm' has been used extensively by many authors, notably Kuhn (1970), and it is his application that I have in mind here. But, as Kuhn himself admits, the notion 'paradigm' is ambiguous (Kuhn, 1970: 174-210 and Suppe, 1974: 459-482). The following three senses of the term capture, so I think, the principal ambiguities of 'paradigm':

a) Different paradigms rely on concepts that cannot be brought into the usual logical relations of inclusion, exclusion and overlap.

b) Different paradigms make us see things differently. Researchers operating with different paradigms not only use different concepts, but they also have different perceptions. (It is this sense of 'paradigm' that Hanson worked at.)

c) Different paradigms demand different methods for setting up research and evaluating its results.

Clearly, I am primarily interested in the second sense of 'paradigm' as characterized here. For my discussion in the previous section has been directed primarily at the point that the knowledge of a particular theory does greatly influence the investigation carried out by the scientist. As I indicated there, not only does the scientist's report of his observations presuppose particular scientific knowledge, his observation themselves are influenced by the present state of his scientific knowledge.

Here I must add that there are problems with the features apparently peculiar to a community referred to as scientific. These are primarily due to the problems central to the debate about the ability to differentiate between science and pseudo-science, the so-called 'demarcation thesis'. Added to these difficulties is the problem due to the circularity involved in my definition of the scientific community. On the one hand a scientific community consists of people who share a paradigm, while on the other hand a paradigm happens to be that which the members of a scientific community share. As Kuhn, a proponent of the view advocated here, admits himself, this problem "is a source of real difficulties. Scientific communities can and should be isolated without prior recourse to paradigms; the latter can then be discovered by scrutinizing the behaviour of a given community's members". (Kuhn, 1970: 176) So instead of trying to distinguish between scientist and non-scientist by means of paradigms, we ought to be able to identify the scientist by other means. Thereafter, we could (hopefully) determine the specific characteristics of that group's paradigm.

So there are difficulties when we rely on the paradigm to serve as a distinguishing criterion of a scientific community. However, in spite of these difficulties, I still think that the distinction underlying these problems is useful, and ought to be maintained. We need to distinguish between the community in general, and that part of the community that may, on whatever criteria, be classified as the scientific community. Without this distinction, the question "Are scientific theories influenced by the community within which they evolve? " is ambiguous. Which community are we speaking about - the scientific, or the community in general? If we do distinguish between the two types of community, then we can be more specific about the origins of the influence on the activities of the scientist.

Following Kuhn's suggestion then that paradigms ought not to function as a distinguishing criterion of a scientific community, we do, I think, find an ingredient possessed by members of the scientific community, yet generally not possessed by the other members of the community - their particular education and training.

2.2 Education and scientific communities.

The members of a given scientific community would have undergone similar educations and have received the same type of training in their subject. Possessing similar educational backgrounds, the members of a particular scientific community would have studied the same technical literature. As D. de Solla Price observes, in concluding his empirical investigation into citations made in scientific papers; the "present discussion suggests that most papers, through citations, are knit together rather tightly". (D. de Solla Price, 1965: 510-521) Having studied the same literature, these scientists are very likely to draw similar conclusions from it.

Admittedly, as Kuhn points out, there are "schools in the sciences, communities, that is, which approach the same subject from incompatible viewpoints. But they are far rarer there than in other fields; they are always in competition; and their competition is usually quickly ended. As a result, the members of a scientific community see themselves and are seen by others as the men uniquely responsible for the pursuit of a set of shared goals, including the training of their successors". (Kuhn, 1970: 177)

Therefore, in the education and the training of future scientists we have a criterion that enables us to distinguish the scientist from the non-scientist. Education and training can function as one important distinguishing criterion of a scientific community. It would appear then that Kuhn's plea has been met. Scientific communities can be isolated without prior recourse to paradigms. To do this, we need to concentrate on the particular education received by the members of the scientific community. Now, what influence has the scientific community over its members, the scientists?

2.3 The influence of the scientific community.

In that part of the community we refer to as 'the scientific community' people are taught to associate sign and object -

theory and phenomena. In a society, or a community in general, members learn a language and are taught to relate objects to that language, i. e. they are taught to satisfy the sentences of that language. Similarly, in a section of that society, the scientific community, members are taught to use scientific language - scientific theories. Through education, the scientific community conditions the individual, i. e. the scientist, to regularly associate sign with object - theory with phenomena (both observable and unobservable). In short, the scientific community, in educating its scientists, not only provides them with conceptual schemes in terms of which these members can refer to objects, but also teaches them to satisfy the sentences of these conceptual schemes, or scientific theories. That is, the scientific community trains its scientists to satisfy the sentences of the scientific theories taught to these members of that community - for instance, sentences of the form (Ex)Ox and (Ex)Tx.

We can now appreciate the remarks made earlier in this chapter on the importance of education and training to distinguish between scientist and non-scientist. It is due to his education and training that a scientist reports that he is observing electrical resistance, and not simply an oscillating piece of metal. It is due to his schooling in electromagnetic theories that he can account for particular phenomena in terms of electromotive force, magnetic charge density, and so on. The scientifically uneducated and untrained man-in-the-street did not report on magnetic charge densities, electromotive forces, and so on, because he was not familiar with the same terminology or scientific theories that the scientist was acquainted with. The scientifically uneducated man-in-the-street has not learnt the same language, a scientific language, that the scientist has been taught. It is this difference in education and training that thus accounts for the divergence in paradigms used by the man-in-the-street and the scientist. And it is this difference in education and training that also accounts for the divergence in paradigms used by scientists in different scientific communities. Two examples to support this conclusion come to mind immediately - Faraday and Galileo.

I suggest that Faraday developed a paradigm so very different to that used by the continental scientists because he had not received an education similar to their education. Faraday had not been schooled in the sciences, as the other scientists had been. He had not undergone the education and professional initiations that those of the continental scientists, like Coulomb and Ampere, had received. In fact, Faraday was very unfamiliar with the views and literature on the phenomena he was investigating. And it was this difference in educational background that, I suggest, allowed Faraday to develop a view that did not need to rely on the corpuscular behaviour of matter. Unfamiliar with the literature propounding the corpuscular theory, Faraday had the freedom to formulate a view not necessarily similar to that of the continental scientists.

P. Feyerabend (1976) arrives at a similar conclusion, though in a different context, when discussing the role of scientific ignorance in Galileo's contributions to theories in astronomy. It was "Galileo's ignorance of the basic principles of telescopic vision", suggests Feyerabend, that made it possible for the scientific community to adopt a new scientific theory. "We see at once that the arrival of a new professional ideology was absolutely essential" to bring about the shift in theory acceptance - a new approach Galileo's ignorance introduced. (P. Feyerabend, 1976: 211. Also see 121-161.)(D. Steele (1970) discusses the role of Faraday's ignorance of mathematics in electromagnetism.)

I think we can now answer that question on the role of the scientific community raised earlier. If we accept my argument, I think we must accept the claim that the community, particularly the scientific community, has a very strong influence on the activities of the scientist. For if the knowledge of a particular theory does influence the scientist's investigations, as I have attempted to establish in the previous section of this chapter, and this knowledge has been acquired primarily by scientific training and education, then the theoretical activites of the scientist must be influenced by the scientific community that trained and educated the scientist. In the light of this I must conclude that the scientific community certainly does play a role in science. Any investigations into the theoretical activities of the scientist, for instance in his attempts to formulate scientific theories and especially in his attempts to interpret (i. e. satisfy) theories already formed, must therefore acknowledge this role of the scientific community in providing scientists with a background. No account of the referents of scientific theories would therefore be complete unless attention had been paid to the vital role of the scientific community in providing scientists with their necessary background.

Now, if it is the case that scientific theories are influenced by their respective communities, as I claim they are, an important implication follows - there may be no universal, i. e. uniform, way in which the question of reference can be resolved, either with respect to sentences of the form (Ex)Tx or sentences of the form (Ex)Ox. That is, when the question of the referents of scientific theories arises, one may have to deal with the problems of referential indeterminacy and ontological relativity - something I shall do in the following chapter.

3. Summary and conclusion.

In this chapter I explored one of the factors that I think influences the theoretical procedures of science. In particular, I attempted to demonstrate that we must acknowledge the influence of the scientific community when questions of the referents of scientific theories arise.

The discussion began with an evaluation of Northrop's view of science. I suggested that if we looked at this view, we would appreciate the vital influence that a scientist's background has on his scientific activities. According to Northrop, scientific theories develop in a particular manner, passing progressively through a number of distinct phases. Using my illustration on the developments in electromagnetism I showed that at first glance this view was plausible - Maxwell's theory did appear to pass from 'problem', to 'description and classification' to 'theory expression'. But a closer look at this view would uncover the error made by Northrop. To expose this flaw I would refer to Duhem.

On what basis do scientists make observations, classify and describe their experiments? Using an example from Duhem I tried to show that what scientists, like Faraday, observe and thus report on are greatly influenced by their prior knowledge, say, their knowledge on electromagnetism. A scientist's report of his experiments is relatively heavily laden with his background knowledge, or theory. I suggested that without this prior established conceptual scheme, a scientist would be unable to report on, or even make, so-called observations of his experiments. The theoretical activities of a scientist would therefore be strongly influenced by his prior established background - a feature Northrop had completely overlooked.

In the second section of the chapter I looked at a more specific problem: must an account of the referents of scientific theories acknowledge the role of the community? In order to answer this question I considered the role of the scientific community in providing scientists with a conceptual scheme. Reviewing my illustration on electromagnetism, I pointed out that here we have a good example of two competing conceptual schemes, or paradigms: strong evidence that the scientists involved were members of different scientific communities. And because I am dealing with only one factor that contributes to the formation of a scientist's conceptual scheme, namely the scientific community, a criterion was sought that would enable me to distinguish between the community in general and that part of the community we call the scientific community. Having considered some of the difficulties in defining a scientific community, I suggested that the specific education and training received by scientists could serve as a distinguishing criterion.

If education can serve as a distinguishing criterion between the scientific community and the community in general, then we can, so I suggested, appreciate the role of the scientific community in science: this community provides its members, the scientists, with a paradigm which they rely on in their theoretical activities - whether it be theory formation, theory satisfaction or whatever. And if it is primarily through education and training that scientists do acquire this necessary conceptual scheme, any investigation into the theoretical activities of the members of a given community must acknowledge this influence of the scientific community. For as I argued, if the knowledge of a particular theory does influence the scientist's investigations and this knowledge has been acquired primarily by training and education, then the theoretical activities of the scientist in general must be influenced by the scientific community that trained and educated the scientist. Hence, an account of the referents of scientific theories is incomplete if it overlooks this role of the scientific community in providing scientists with their necessary backgrounds.

CHAPTER SIX.

REFERENTIAL INDETERMINACY AND ONTOLOGICAL RELATIVITY.

In the previous chapter I demonstrated the importance of a background when questions about the referents of scientific theories arise - a background formed by the scientific community. Now if the referents of scientific theories are determined by their respective communities, perhaps there is no uniform manner in which the question of reference of these theories can be resolved. In other words, when the question of the referents of scientific theories arises, one has to consider the possibilities of referential indeterminacy and ontological relativity. In this chapter I shall deal with these two issues.

I want to argue, in contrast to the realist and the logical empiricist, that the language a person acquires is referentially indeterminate i. e. that one can never be sure that the sentences of that language have been correctly satisfied. Secondly, I shall demonstrate that if we want to talk about a language and its ontology, i. e. its referents, we can do so only relatively - such talk must be relative to some background language, or frame of reference. In other words, I want to argue that the suggestion raised in the previous chapter is correct - scientific theories are ontologically relative.

1. Referential indeterminacy.

Induction plays a prominent part in learning a language. Some terms and sentences containing these terms, are learned in the presence of something that the term describes, or in the situation that the sentence reports on. This ostensive method of learning an expression consists of the procedure of learning to associate the sign (in the form of sounds or written marks, perhaps) with the object simultaneously observed. Russell, for instance, while talking about our knowledge of universals, remarks that when "we see a white patch, we are acquainted, in the first instance, with the particular patch; but by seeing many white patches, we easily learn to abstract the whiteness which they all have in common, and in learning to do this we are learning to be acquainted with whiteness". (Russell, 1959: 58) But if it is the case that a person learns a language initially through ostension, can he ever be certain that he is correctly associating the sign with the object? To use Quine's expression, is there perhaps no 'empirical slack' between sign and object?

Take the term 'red'. How does one learn the term? As Hume himself said, to "give a child an idea of scarlet or orange, of sweet or bitter, I present the objects . . . "(Hume, 1896: 5) In other words, if you want to teach someone to understand a term, like 'red' or 'orange', one has to rely on induction - hopefully, by presenting the child with a sufficient number of objects he will learn the term. As Strawson (1969) points out, human beings would not acquire mastery of a language unless they were exposed, as children, to conditioning or training by the adult members of the community. The language learners respond to a number of different situations in ways which either earn them reward or punishment. (Also see Quine, 1960: 6) We are thus trained by our society to associate sign with object - a schooling that initially relies on induction.

This example brings out an important factor involved in learning a term, and for that matter, a language: socialization is an integral factor in learning to associate sign with object. Due to his schooling in his particular society, an individual learns to use a language correctly. The teacher of the term 'red' assumes the role of society's agent, and as such either approves the individual's utterance of 'red' in the appropriate circumstances, or disapproves of the association of the term (i. e. the sign) and the object. On this basis, eventually all the members of a specific society would have (hopefully) learnt to associate particular signs with particular objects.

Generally, if a term is learnt by induction from observed instances in particular situations, these instances have to resemble one another in two ways at least:

a) from the learner's point of view, these instances must exhibit a necessary degree of similarity so that he, the learner, can generalize. For instance, if learner X is to know how to correctly use term 'y', he must have encountered a number of situations, say Si, Sii,. . . Sn, that are fairly similar. As Wittgenstein would say, learner X must be aware of the 'family resemblance' common to the set of encountered situations, before he can correctly apply the term 'y'. The learner must therefore be aware of the similarities of each of the different situations - similarities forming a complicated network that overlap and criss-cross: at times overall similarities, but sometimes similarities of detail. (L. Wittgenstein, 1953: 31-32)

b) from both the learner's and teacher's simultaneous distinct points of view, these instances must exhibit a high degree of similarity. Thus the learner and the teacher must simultaneously be aware of the family resemblances of the observed instances before language learning can proceed.

These two conditions will have to be met whenever terms are associated with observable objects generally. From this it follows that these observed objects are the focus of one's thought, and point of reference when using the language. The individual is thus taught to concentrate on a particular referent and to associate this referent (the object) with the word (the sign).

And once the language has been learnt, the individual knows two 'operations' - one a semantic, the other a phonetic operation. The phonetic part is achieved by the individual imitating the other person's verbal behaviour, while the semantic operation involves knowing how to use the word or term in specific situations. In other words, the semantic part to knowing a term is knowing how to associate the term (i. e. sign) with the object it is referring to, thereby knowing how to satisfy terms of a language. This semantic part of learning a term, or a language is therefore pretty complex, even in apparently simple cases.

I have spoken of the influence of socialization in learning a term, or a language. This uniformity brought about by socialization unites the members of a society, both in communication and beliefs. But one must remember that this uniformity that is enforced by society overrules the many diverse subjective associations that the individual has between sign and object. As Quine has pointed out, the "uniformity that unites us in communication and belief is a uniformity of resultant patterns overlying a chaotic subjective diversity of connections between words and experience. Uniformity comes where it matters socially . . . "(Quine, 1960: 8) Society has thus conditioned the individual to regularly associate sign with object - thereby training the individual to suppress his own diverse subjective associations between sign and object.

Not only must the learner learn the term phonetically by listening to other speakers, he must also see the object, and also learn to suppress his own diverse subjective associations between sign and object. But the situation may arise where the learner does not grasp the relevance of the word to the object being referred to. Therefore, in addition to the preceding two conditions, an additional requirement has to be met - the learner has to see that the speaker, his teacher, is also looking at the object. Clearly then, when one learns a language, or a term, one has to observe our teacher's behaviour. We are thus students of our neighbour's behaviour, forever conscious of his actions. The language-learner has therefore only the overt behaviour of the other members of his society to work with. The complexity in this, the semantic part of learning a term, is the breeding-ground for referential indeterminacy.

This complexity in learning a language compels us to reject the idea that our language, as a collection of signs, is firmly secured to a set of objects that satisfy these signs. There is no neat or simple way of satisfying the signs of our language. As Quine puts it, we must reject the 'museum myth' according to which meanings parade as exhibits, "and the words are labels" (Quine, 1968: 186) that are simply attached to the exhibits. We must reject this view, as I have tried to show above, because we can never be certain that the exhibits have been labelled correctly. We must give up the idea that our language is referentially determinate i. e. appreciate the complexities in learning to associate sign with object. And once these complexities have been acknowledged, I think that we have to accept the conclusion that our language is referentially indeterminate.

Once we realize that the correct use of language, and thus the use of a term of the language as well, has been "inculcated in the individual by training on the part of society" (Quine, 1960: 5), we realize that the language is referentially indeterminate. To begin with, the language is naturally underdetermined by past evidence - there is always a likelihood in the future that one has to modify the association of sign and object. In addition, the language (or the terms in the language) is underdetermined by past and future evidence together. Some 'rectifying occasion' may go completely unnoticed and thus one may continue to associate sign and object incorrectly. (By 'rectifying occasion' I mean an instance when the person using a term (or even a language) realizes that he has been applying the term (sign) incorrectly - he must now modify the association of sign and object.) The consequences of the social method of learning a language therefore cannot be overemphasized. For as we have seen, induction served as the principal vehicle in this learning process. But if sign and object are associated inductively, there is always some amount of 'empirical slack' present: further experience may teach one modify his association between sign and object. This empirical slack, one of a number of factors contributing to the complexities in learning a language, amounts to nothing less than referential indeterminacy.

At this stage I might be accused of overlooking the counterargument to this empirical, behaviouristic account of language-learning. What about a different account of language-learning: say, a view that runs counter to the empirical one in that it suggests that linguistic competence is not learnt, but already innate? You have uncritically accepted a behaviouristic account of language-learning - but how would your argument be affected by the plausible alternative that linguistic competence is innate?

For my purposes here, all I am trying to do is establish the point that language is referentially indeterminate - that there is that slack between sign and object. Admittedly, in attempting to establish this point I have relied on a behaviouristic account of language-learning. But, as I see it, the alternative view based as it is on some form of innate linguistic competence, also supports my claims that language is referentially indeterminate. The proponents of the view (counter to the empirical one outlined here) that linguistic competence is innate in an individual, also speak of the underdetermination of language. This is brought out clearly in the following quote from Chomsky: "Thus it is clear that the language each person acquires is a rich and complex construction hopelessly underdetermined by the fragmentary evidence available". (Chomsky, 1976: 10) According to both points of view language is referentially indeterminate, which is all I need here.

The language a person acquires is therefore referentially indeterminate. That is, one can never be sure that the sentences of that language have been correctly satisfied. And as I have already indicated above, this state-of-affairs applies to both the behaviouristic theory of language that I have relied on, and the alternative view, that regards linguistic competence as innate in an individual. On either account, there can therefore be no guarantee that the values an individual assigns to the bound variables of his sentences (of the language he has learnt) are the correct values. In the next section I shall explore some of the consequences of this conclusion. I suspect that the discussion here has many interesting implications for science, but I intend to deal only with a few of them.

2. Ontological relativity.

Does 'rabbit' really refer to rabbit, and not perhaps to rabbithood? How are we to determine the referents of the terms of our language? That is, how are we to decide on the values of the bound variables of our language? If we accept that our language is referentially indeterminate, as suggested in the previous section, I think that we would be mistaken if we expected hard-and-fast answers to the question about rabbits etc, raised here. We would therefore be mistaken if we asked, for instance, whether, in general, our terms 'rabbit', 'rabbit part', 'number' and so on really do refer to rabbits, rabbit parts, numbers and so on: mistaken, if 'really' is read in some absolute sense. It is because the terms in our language are referentially indeterminate that we can never be certain that when we use a term, like 'rabbit', we are really referring to rabbit and perhaps not to rabbithood, or whatever. There is always that element of doubt - that empirical slack between sign and object. In other words, we cannot ask questions like "Does 'rabbit' refer to rabbit and not to rabbithood"? absolutely - we must ask this type of question on the understanding that one can never be sure that the sign has been correctly associated with the object. This inscrutability of reference of our terms seems to make nonsense of reference. Or does it?

Imagine a child learning a language. Following the process outlined in the first section of this chapter, eventually this language-learner is familiar with a whole collection of signs - predicates and auxiliary devices. His vocabulary would include terms like 'rabbit', 'rabbit part', 'number', 'house', 'mouse' etc, as well as other more sophisticated predicates, like identity for instance. One would also find other logical particles in his language. Using these components, the child learns to talk about rabbits, rabbit part, numbers, houses, mice, etc: this is a rabbit, that a rabbit part, this a number, that a mouse, and so on. The important point to notice now, is this: the child uses this vocabulary to talk about rabbits, rabbit parts, etc. It is by means of this vocabulary that the child is able to refer to rabbits, rabbit parts, etc. The vocabulary functions as a network, a frame of reference, in terms of which the child speaks about particular objects. Relative to this network of signs, or vocabulary, we are able to refer to objects. This network then enables us to "talk meaningfully and distinctly of rabbits and parts, numbers and formulas". (Quine, 1968: 200) To talk of reference, in some authoritative absolute tone of voice is thus foolhardy - reference is nonsense unless relative to this network, or frame of reference.

It would therefore be meaningless to ask whether a term like 'rabbit', really does refer to rabbit and not to something else. Questions about the denotation of a term make sense only if they are asked relative to some background language. And as Carnap himself points out, in his later writings, if "someone wishes to speak in his language about a new kind of entity, he has to introduce a system of new ways of speaking, subject to new rules; we shall call this procedure the construction of a linguistic framework for the new entities in question". (Copi and Gould, 1971: 178-193) Our language, on this view, then operates as the 'linguistic framework', or 'frame of reference' - a system of related terms and expressions. It is then only relative to this frame of reference that we can and do meaningfully talk of various objects - rabbits, numbers, spirits, ghosts and so on.

Suppose an anthropologist queried the member of an African tribe, the Zande, about the ontological commitments of his language. For instance, the anthropologist may ask the Zande the following question: "What are your oracles about? " That is, "What are you referring to when you use the term 'oracle'?" (or for that matter 'spirit-world', 'witchcraft', 'ghost', etc.) For the Azande the terms 'witchcraft', 'spirits' and 'oracle' are not empty, but highly significant, enmeshed as they are with his beliefs in witchcraft and magic. As Evans-Pritchard points out, the Azande "do not see that their oracles tell them nothing! . . . they reason excellently in the idiom of their beliefs, but they cannot reason outside, or against their beliefs because they have no other idiom in which to express their thoughts". (Evans-Pritchard, 1937: 237-8) It makes no sense to speak of the referents of the Azande's language, or terms, absolutely speaking. Rather, we must ask how an object is associated with the signs of his language relative to his background.

Evans-Pritchard writes that the Azande's beliefs "hang together, and were a Zande to give up faith in witchdoctorhood, he would have to surrender equally his faith in witchcraft and oracles . . . " (Evans-Pritchard, 1937: 194) The Azande's beliefs are thus very closely interrelated and in "this web of belief every strand depends upon every other strand, and a Zande cannot get out of its meshes because it is the only world he knows". (Evans-Pritchard, 1937: 194) Therefore to query the reference of any one of the terms of the Azande's language, say 'oracle', would elicit a response that relied on other related terms, say 'spirit-world', 'magic', and 'witches'. It is thus only relative to these related terms, forming a network or frame of reference, that we can and do talk meaningfully and distinctly of the objects, or entities that comprise the Azande's world.

Similarly, when Maxwell writes and speaks about electromagnetic fields, lines of force, electrical charges, and so on, he does so only relative to other terms, forming a background coordinating system. Ask him what it is he is referring to when he uses the term 'magnetic field'. His response must rely on other related terms - perhaps, "A magnetic field consists of charged electrons surrounding an object". Relative to these other related terms Maxwell is able to refer to magnetic fields, lines of force, electrical charges, and so on.

Does the term 'rabbit' really refer to rabbits? If someone asked this question, we now realize that another could respond with the further question: refer to rabbits in what sense of 'rabbits'? If my argument so far is acceptable, one appreciates the relevance of this further question. The reference of any one term cannot be queried in any absolute sense - reference must rather be queried relative to some background, or frame of reference: "The background language gives the query sense, if only relative sense; sense relative to it, this background language. Querying reference in any more absolute way would be like asking absolute position, or absolute velocity, rather than position or velocity relative to a given frame of reference". (Quine, 1968: 201)

We need this background to regress into. It is because it makes no sense to seek the referents of a term or language absolutely that we must attempt to relate one sign (or collection of signs) with another. What is needed therefore is a relational theory of what the objects of a language are. If we want to talk about a language and its ontology, i. e. its referents, we can do so only relatively - such talk must be relative to some background language, or frame of reference.

A while back I said that it is our language that functions as the 'frame of reference' or 'coordinate system'. It is only relative to this frame of reference that we can and do talk meaningfully and distinctly of objects. Quite clearly the point being stressed is the distinction to be drawn between metalanguage and object language. For it is only relative to the metalanguage that the object language becomes meaningful. That is, it is only relative to the metalanguage that one can satisfy the sentences of the object language. The object language must therefore be interpreted from the vantage point of the metalanguage.

Now what happens if we seek the referents of this metalanguage? What happens if we attempt to determine the ontological commitment of this background language, that functions as the frame of reference for another language? Do we not need yet another frame of reference for this metalanguage? That is, do we not need to regress another step? Is the regress into background not now gaining momentum? May the momentum not lead to an infinite regress? A given language requires a metalanguage, and the metalanguage in turn requires a meta-metalanguage, and the meta-metalanguage . . . Can there be no end to the regression?

A plausible solution to this problem is that we end the regress of background languages in our mother tongue. (Quine, 1968: 201) We take our mother tongue at face value, thus stopping the regression at home-base. We strike rock bottom with our home language. But this decision to take our mother tongue at face value ushers in a further complication: how secure is this rock bottom? That is, can we acquiesce in our mother tongue and take its words at face value?

If one accepts the earlier discussion about the referential indeterminacy of language in general, one realizes that there are a few serious consequences for the person who takes his mother tongue at face value: the mother tongue, i. e. the metalanguage, is itself referentially indeterminate. In taking our mother tongue at face value, we must do so always conscious of the fact that this language is not without its own problems. This rock bottom, the mother tongue, is therefore not all that secure.

Now this is not to suggest that our quest for the referents of any language is necessarily a thankless task. However, what it does suggest is that we must guard against claims that one can determine the referents of any language absolutely. For reference, as I have tried to demonstrate, can only make sense relatively - relative to some background language. Therefore, if this background language is itself referentially indeterminate, reference must be relative to a referentially indeterminate background language. Thus to query reference in some absolute sense would be foolhardy. Reference only makes sense relative to some background language, that, as it turns out, is itself referentially indeterminate.

3. Summary and conclusion.

In the previous chapter I suggested that the referents of scientific theories are possibly determined by their respective communities. And as I indicated, an implication of this view was that there can be no uniform manner of resolving the question of reference of scientific theories. In this chapter I dealt with this implication - namely, the problems of referential indeterminacy and ontological relativity. My arguments relied on a discussion of language learning.

How is a language learnt? In the first section of the chapter I looked at this question. In my analysis, I tried to show how important induction is in learning a language: a society relies on inductive means to teach its members to associate sign and object. Resorting to conditioning, society teaches the language-learner on an award/punishment basis: reward a correct response and punish an incorrect response.

Induction brought with it certain problems - complexities that I suggested interfered with the language-learning process. Having looked at a few of these complexities, I argued that once these problems were acknowledged, one would have to accept the conclusion that language is referentially indeterminate. There would always be that empirical slack (to use Quine's phrase) between sign and object.

In the second section I tried to argue for the need for a background in interpretation. Having argued for the referential indeterminacy of language in the first section, I now wanted to establish the role of a coordinating system, the background, in determining the referents of a language. I stressed the relativity of the terms of our home language: we speak of the referents of these terms only relative to some network, or frame of reference. It is only relative to this network, or frame of reference, that we can and do talk about the ontological commitments of a language. In other words, one is mistaken in seeking the referents of the terms of a language absolutely - reference, I suggested, makes sense only relative to this network, frame of reference, or background language.

So one needs to regress into some frame of reference, or background language, when seeking the referents of a given language. But then, on this view, two problems arise:

a) May an infinite regress not set in?

b) Can we rely on the background language to determine the referents of the object language?

I outlined a solution to a): we must acquiesce in our mother tongue and take its words at face value. So rock bottom is reached with our mother tongue. Now the second problem becomes pertinent - what if this rock bottom is itself insecure, due to referential indeterminacy? Have we not returned to the problem that prompted our investigation at the beginning of the chapter? That is, are we not back to the question of the referential determinacy/indeterminacy of language, say the background language? Unfortunately, there is nothing that can be done about this problem - in taking our mother tongue at face value we must do just that - learn to live with the consequences of our decision. We must learn to accept this referential indeterminacy of language, even if it is our own home language, our metalanguage.

CHAPTER SEVEN.

CONCLUSION.

In this essay I have considered some of the philosophical problems involved in attempting to settle the question, What are scientific theories about? And in order to expose these problems, I have dealt with two influential responses to this question of the referents of scientific theories - namely, logical empiricism and realism.

Admittedly, a good deal remains to be said about this question of the referents of sientific theories. However, I think we are now in the position to draw this discussion to a close by outlining some of the implications my analysis has for this question central to my essay. In particular, I shall set out a few implications of my discussion for logical empiricism and realism. Thereafter, I shall conclude with the general conclusion of my analysis.

1. Implications for logical empiricism.

According to the logical empiricist, as we have seen, philosophers of science must "show how the theoretical terms of a science can be explicitly defined by means of observable entities". (Braithwaite, 1953: 53) Thus the emphasis is not on the question how we get to know about theoretical entities, but rather how we can justify the use of a familiar language (i.e. an observation language) to describe entities that have been inferred, and what it is we are referring to when we use this language. And as Mary Hesse has pointed out, the logical empiricists met this question "by retreating to the firm base of the observable, where there was thought to be no doubt as to what we are talking about, what our language means, or how we know our assertions to be true". (Achinstein and Barker, 1975: 89) In short, the logical empiricists assumed that the observation language can function as a secure foundation for science - otherwise why regard this language as the termini of analysis? Now what are the implications of my analysis for this feature of logical empiricism?

As we have seen, there are a number of complexities in learning a language - complexities that compel us, so I suggested, to acknowledge that language is referentially indeterminate. In addition to this, I have argued for the necessity of a background coordinating system, or frame of reference, when determining the referents of a language. I have thus suggested that we must speak of the referents of a language only relative to this frame of reference. In other words, one is mistaken in seeking the referents of the terms of a language absolutely - reference, I argued, can make sense only relative to some network, or frame of reference. But what happens if this frame of reference is itself referentially indeterminate? If this is the case, then clearly reference must be relative to a referentially indeterminate frame of reference.

From this it follows that if, for instance, the observation language functions as a frame of reference, or metalanguage, the referents of the theoretical language can only be specified relative to a referentially indeterminate observation language. It follows then, that the logical empiricists are mistaken in their assumption that an observation language can serve as a secure foundation for science, for the observation language can not be referentially determinate.

2. Implications for realism.

As we have seen, the realist is committed to an ontology of observable and theoretical entities: both observable and unobservable objects are therefore thought to satisfy the sentences of scientific theories. Harrè, for instance, claimed that the scientist's task is to discover and describe those "ultimate entities (that) must lie 'behind' whatever can be observed or detected". (Harrè, 1972: 260, my insertion) But what role, if any, does the scientific community play in the theoretical activities of a scientist - say, in his postulation, and description, of these unobservable structures and mechanisms?

For instance, consider Maxwell's electromagnetic theory, that was published in 1864. What was it that this theory was describing? As we have seen, this theory had been strongly influenced by the ideas of Faraday. Thus when it came to determining the referents of this electromagnetic theory, Maxwell referred to lines of force, electromagnetic fields, and so on. That is, when questions about the ontological commitments of Maxwell's theory arose, his response relied on the concepts Faraday had developed. Once familiar with Faraday's ideas and concepts, Maxwell developed a scientific theory that was intended to describe and explain lines of force, electromagnetic fields, and so on - theoretical entities posited by a scientist initially unacquainted with the theories then current in the scientific community engaged in electromagnetic research. And once Maxwell's theory was adopted by that scientific community, thereby replacing the rival continental or 'corpuscular-theory', the members of that community, in turn, began to refer to fields, lines of force, and so on. In other words, Maxwell may have been one of the first to incorporate Faraday's ideas in his research. But it was merely a matter of time before the other scientists had learnt about Faraday's ideas, through education and training, and were also referring to lines of force, fields, and so on.

Clearly then, an account of the referents of a scientific theory, such as that formulated by Maxwell, would not be complete unless attention had been paid to the influence of the scientist's background, primarily in the form of his particular education and training. The realist must therefore acknowledge the role of the scientific community, that trains and educates the scientist, when questions about the referents of scientific theories arise.

Thus the realist is mistaken in his assumption that scientific theories will eventually refer to the same domain of objects - those ultimate entities that "must lie 'behind' whatever can be observed or detected". (Harrè, 1972: 260) For if the scientific community has a role to play in the satisfaction of scientific theories, these theories must be ontologically relative. Therefore one cannot assume, as the realist does, that different scientific theories will eventually be about the same entities.

3. General conclusion.

So, what are scientific theories about? I think that my discussion has shown that there can be no universal answer to this question of the referents of scientific theories. That is, the ontological commitments of a scientific theory cannot be determined universally. For, as I have argued, the scientist cannot engage in any theoretical activity in science without a background. And, as I have suggested, this necessary background is developed, through education and training, by the scientist's particular scientific community. This community thus not only provides the scientist with his necessary background, in the form of a conceptual scheme or frame of reference, but also teaches him to satisfy the sentences of that scientific theory he has learnt. It therefore follows that if the referents of theories are determined by their respective scientific communities, scientific theories must be ontologically relative. In the light of this, I must conclude that there can be no universal answer to the question, "What are scientific theories about?"

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