

SCIENTIFIC CHANGE AND THE MEANINGS OF TERMS:
AN EXAMINATION OF P. K. FEYERABEND'S
INCOMMENSURABILITY THESIS

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Thesis presented in fulfilment of the requirements
for the degree of Master of Arts of
the University of Cape Town

October 1976

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ACKNOWLEDGEMENTS

I am indebted to the University of Cape Town and the Human Sciences Research Council for financial assistance towards meeting the costs of this research. Opinions expressed or conclusions reached are my own, and are not to be regarded as a reflection of the opinions and conclusions of either body. I would like to thank my supervisor, Dr B. P. Keaney, for his valuable criticism and encouragement. I am also indebted to Mr P. Collins for his helpful advice. In particular I would like to express my gratitude to Professor J. Howes for his careful reading of the draft and his incisive and critical comments. Thanks go also to Ms S. Jones and Mrs T. Case for their assistance in typing the manuscript; and to Mr A. Vital, both for his suggestions and encouragement, and for his invaluable assistance in the production of this work.

ABSTRACT

Feyerabend's incommensurability thesis concerning scientific change engenders a number of logical problems. While it is possible to examine Feyerabend's theory in relation to his historical arguments, the defects implicit in his arguments for the theory render more appropriate an analytical approach. These defects arise from the conjunction of presuppositions and theses that form the background to Feyerabend's claims for an incommensurability thesis. This background contains Feyerabend's criticisms of the traditional empiricism of the twentieth century and its reductionist account of scientific development, his objections to any attempt to rationalize science, his claim that there are fundamental conceptual and ontological changes in science, and his adoption of a meaning variance thesis which envisages wholesale changes in the meanings of all descriptive terms when one theory is replaced by another. While the criticism against traditional empiricism can be upheld, it does not necessitate the conclusion that alternative theories are incommensurable. Feyerabend's attack on Lakatos' rational reconstructionism is not conclusive: he overlooks the possibility that there do exist standards of criticism, which can be termed "rational", operating within the sciences. The suggestion, supported by Hanson and Kuhn, that there are fundamental conceptual changes in science is open to criticism. The case against radical meaning variance is more complex as it requires

the support of a theory of meaning. It is not clear that Feyerabend can, using Whorf's controversial ideas about language, provide a suitable theory of meaning to support his claims. A more satisfactory theory of meaning, based on views of Frege and Wittgenstein, while not denying some changes in the meanings of scientific terms, does not entail the consequence that there are necessarily radical changes in meaning from theory to theory. Although the objections to traditional empiricism are sound and a moderate thesis of meaning variance is acceptable, these do not give rise to the view that competing theories are incommensurable. Historical evidence shows the need to take into consideration the gradual, rather than revolutionary, nature of scientific development. This is compatible both with a moderate thesis of meaning variance and with a modification of the network model developed by Duhem, Quine and Hesse.

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CORRIGENDA

- p. 32, line 26: for but queries read but one which queries
- p. 34, line 23: for to read too
- p. 42, line 10: for be read by
- p. 84, footnote 8: for Russel read Russell
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CHAPTER I

One of the central tenets of traditional empiricist philosophy of science¹ is the thesis of theoretical reductionism. Briefly, this is the view that a successful theory in one domain of enquiry can be explained in terms of a theory often, though not necessarily, in another domain, by means of a set of correspondence rules. These rules serve to relate relatively autonomous theories to common experience and knowledge on the one hand and more general theories on the other, according to some hierarchical pattern. This explains the existence of the empiricist ideal of a comprehensive theory which will serve as the foundation for all less general theories in the natural - and possibly social - sciences. Whether such an ideal will ever be realized remains a problem for these philosophers; nevertheless it is still subscribed to, for according to Nagel "the phenomenon of a relatively autonomous theory becoming absorbed by, or reduced to, some more inclusive theory is an undeniable and recurrent feature of the history of modern science. There is every reason to suppose that such reduction will continue to take place in the future" (Nagel, 1961, p. 337).

This picture of science implies and is connected with

¹ I use the term "traditional empiricism" to refer to the empiricism of the logical positivists, especially Carnap, Nagel, Scheffler and Ayer. I will deal more fully with the tenets of this empiricism later in the chapter.

several further positivist doctrines. Firstly it presupposes that different theories are commensurable with one another by way of either the meanings or the extensions of their terms. Furthermore it is connected with the view that there is an independent and stable observation language containing only observable entities. This is to be distinguished from the theoretical language of any particular theory which is to be characterized partly by the theoretical terms peculiar to it which purportedly refer to unobservable phenomena and entities. While a theoretical language is dependent on a particular theory and may thus change on the basis of a change in the theory, the meanings of the terms in the observation language remain invariant throughout scientific progress as the referents of these terms are fixed. It is the observation language which provides the final court of appeal for all scientific theories, for by means of the above-mentioned correspondence rules it provides theoretical terms with their cognitive content and permits adjudication between conflicting theories.

Traditional empiricism has been the focus of attack from developments in the second half of the twentieth century. The basis of these attacks is the refusal to accept the empiricist reliance on common-sense knowledge as providing the ultimate justification for proposed theories. It is claimed firstly that justification does not always proceed in that direction, and secondly that common-sense knowledge is itself constitutive of a theory no different

in kind from those we call "scientific" and therefore no less immune, in principle, to revision or replacement.

The alternative proposals to traditional empiricism differ in the extent of their attack, yet they all stem from this common disagreement. It is held that the meanings of at least some terms occurring in a scientific theory are dependent upon the principles of the theory; furthermore, the meanings of these terms change if the theory is replaced by a different one containing the same terms. Although this is a common attack on traditional empiricism, it is a doctrine that is upheld to differing degrees. Some of the more radical proposals, notably those of Feyerabend, intend the doctrine of meaning change to apply to all terms occurring in a theory, even the so-called observation ones, thus recognizing a wholesale change of meaning during the transition from one theory to another; the existence of correspondence rules is denied altogether. The claims that a theory rests ultimately on foundations external to it, that it is rooted entirely in observational experience, are questioned. Thus the relationship between an old and new theory is not one of deducibility of the old from the new, as empiricists had held to be the case. Rather, different theories are seen as internally justifiable systems, pertaining to different domains and no part of an older theory can, except by distortion, be shown to be derivable from a new one.

In this way the view that conflicting scientific

theories are incommensurable arises. The chief defendant of this view is P. K. Feyerabend, who has supported a thesis that scientific theories are incommensurable in several of his recent publications. It is his particular thesis that I am concerned to examine, specifically in the form of its most recent statement in Against Method (Feyerabend, 1975, ch. 17).

The incommensurability thesis presented by Feyerabend seems to make the following claims: alternative scientific theories of the more general and advanced kinds² - such as Aristotelian physics, Newtonian mechanics, Relativity Theory, or quantum mechanics - are incomparable with respect to their individual content, basic principles, and truth claims. This is because there is no set of standards which accord with some "rationality theory" by means of which the "content classes" or extensions of the theories can be compared in the "usual manner".³ Thus the methodological demands proposed by philosophers who believe in the rational progress of science must be rejected since they are based on misconceptions about the relationship between theories; this includes a rejection of the Popperian demand for increased content (Popper, 1959, ch. VI) as well as the modifications to this view developed by Lakatos.⁴

² These are what Kuhn (1962) has termed "paradigms".

³ See Feyerabend (1975), chs. 16 and 17.

⁴ Lakatos' views will be discussed in detail in ch. IV below.

Feyerabend's claim is not only that there is no overlap between old and new theory, or rather that whatever limited overlap there is, is generally achieved by distorting the old theory (Feyerabend, 1975, p. 178); but also that it makes no sense to speak of the usual logical relations of inclusion or exclusion of content holding between the theories.

By way of an explicit rendering of the incommensurability thesis this is all that can be said, for according to Feyerabend it "depends on covert classifications and involves major conceptual changes [and] it is hardly ever possible to give an explicit definition of it" (Feyerabend, 1975, p. 225). Nevertheless, from his writings certain general points can be extracted.

We may note, firstly, that Feyerabend's arguments hinge, in part, on his claim that the more general scientific theories are "sufficiently general, sufficiently 'deep', and have been developed in sufficiently complex ways to be considered along the same lines as natural languages" (Feyerabend, 1975, pp. 224-225). To embark on analysis of the incommensurability thesis along the lines of this claim, from the point of view of theories of language and of meaning, appears to be fruitful in several ways.

In the first place we can avoid making our analysis totally dependent on particular historical arguments. Feyerabend leans heavily on a particular interpretation of historical instances to lend support to his conclusions. This recourse to historical case studies is gaining

increasing attention in recent philosophy of science.⁵ As Hesse (1974) points out, these studies, in many instances, reject not only traditional empiricist presuppositions, but also the logical and analytical style of this school. Thus we cannot expect to find in Feyerabend the same treatment of his material as we would find in earlier philosophy of science.

Historical case study is important for adequate understanding in many areas in the philosophy of science, including the question of scientific change. Many valuable contributions to the philosophy of science have been made by philosophers who have presented careful interpretive studies of actual periods in the history of science. There are two reasons, however, why I shall not examine Feyerabend's argument by studying his historical examples. First, his historical interpretations have come under attack from various philosophers and scientists⁶ and I am not equipped to deal adequately with the issues at hand. Thus I leave as an open question whether or not Feyerabend's historical interpretations and the conclusions he bases on these are correct. Second, Feyerabend explicitly states a connection between scientific theories and language, and many of his claims about the meanings of terms can be questioned without the need for historical examples. A theoretical argument against Feyerabend, as I plan to offer,

⁵ For example, see Hanson (1958), Toulmin (1961), Kuhn (1962), and Feyerabend (1970, 1975).

⁶ An important attack has come from Zahar (1973).

will therefore not be open to the objection of not having met him on his own grounds. I shall thus choose to approach Feyerabend's incommensurability thesis by way of questions about language and meaning.

According to the view of language adopted by Feyerabend - namely B. L. Whorf's - there exist in any particular language certain "covert classifications"⁷: for example, in English, names such as "John", "Alice", or "Mary" do not have the distinguishing overt gender mark such as can be found in Latin names, yet each of these names has a covert "invariable linkage bond", "connecting it with absolute precision either to the word 'he' or to the word 'she' " (Whorf, 1956, p. 68). Such "covert classifications", Feyerabend maintains, appear also in particular scientific theories.

Discussions leading towards a changeover to a new theory "often reveal hidden ideas, replace them by ideas of a different kind, and change overt as well as covert classifications" (Feyerabend, 1975, p. 225). For example, Feyerabend maintains that Einstein's theories about simultaneity revealed some implicit, though up to that time unknown, features of Newtonian physics which bore directly on the latter's ideas of space and time. In the same way as covert classifications in language create "patterned resistances to widely divergent points of view" (Whorf, 1956, p. 247), Feyerabend argues, "scientific

⁷ This is Feyerabend's expression. Whorf (1956) uses the term "cryptotypes".

arguments may indeed be subjected to 'patterned resistances' and we expect that incommensurability will also occur among theories" (Feyerabend, 1975, p. 225). Hence Feyerabend's refusal to give an explicit account of his thesis and his decision to concentrate rather on "confronting" the reader with particular instances of it.⁸

Before turning to an analysis of Feyerabend's thesis I shall attempt to place his claims more fully in the context of the particular movement from which they arose. I shall therefore outline in a little more detail some of the more important aspects of the "new" empiricist movement from which Feyerabend takes his starting point.

One of the striking features of science is that it contains such terms as "electron" or "light wave" which purport to refer to unobservable entities. The traditional empiricist account of these types of terms is based on a distinction at two levels between theory and observation. The one is an ontological distinction between observable and unobservable entities; the other is a corresponding terminological distinction between an observation language, comprised of observational terms and statements, which is

⁸ In his most recent publications (Feyerabend, 1970 and 1975) Feyerabend's incommensurability thesis is intended to describe a relationship that exists generally between different cosmologies, and in particular between different "general" scientific theories. Thus the thesis is put forward as an epistemological doctrine. While it may provide the most adequate description of the relationship between different cultural mythologies (in the broad sense) - a claim which I shall not examine - I do not believe that it provides the best analysis of the relationship between different scientific theories.

stable, and a changing theoretical language. However obvious the distinctions may seem, there are nevertheless some difficulties with them.

The task of characterizing what it is to be observable, which for the traditional empiricists was unproblematic, presents numerous difficulties when we consider those entities that are theoretical posits but which can be detected, or at least their effects can, if not with the unaided eye, with the help of refined instruments. To deny atoms the status of "observable entities" may be said to be tantamount, at one level, to denying distant stars the same status just because they are too far to be seen with the naked eye. Nevertheless there is rightly some ground for unease about regarding atoms as observable once one acknowledges the changes in atomic theory over recent years. After all, observable entities are supposed to admit of a more or less stable description. However further problems arise if one notes that most of our common "observables" have undergone changes in taxonomic description at some stage. The difficulty with the distinction is that it does not appear to be a natural one but seems to rest on a prior decision about where to draw the boundary line.

I shall concern myself here chiefly with the terminological distinction. A parallel problem arises in the characterization of an observation language. At first sight there would appear to be no problem. Ayer, for example, uses "the phrase 'observation-statement'... to

designate a statement 'which records an actual or possible observation' " (Ayer, 1936, p. 11). Nagel speaks of designating "as 'observation expressions' those expressions in [the class of descriptive expressions of a given science] that refer to things, properties, relations, and processes capable of being observed" (Nagel, 1961, p. 350). Clearly such definitions are circular unless one can adequately explicate "observable" or "observation". However it is not the circularity of such attempted definitions that has come under attack, for circularities abound in many systems and are not always vicious. Rather it is the supposed necessity for the distinction which has played such a fundamental role in traditional empiricism that has been the focus of numerous objections. I believe that two theses, a weaker and a stronger, can be extracted from the various attacks. The weaker one, which I think many positivists would now accept, is that there is no absolute and decisive line to be drawn demarcating theoretical from observation terms. That empiricists have accepted this difficulty is quite clear from the literature on the issue⁹, and there has been considerable debate, given the importance still attached to the distinction, on what might be the best way to characterize it. However, attempts along this line tend to enforce the distinction rather than to undermine it.

There is a stronger thesis that emerges from these

⁹ For example consult the introduction and articles in Grandy (1973); see also Hempel (1965), Scheffler (1963) and Nagel (1961).

objections. This is that the distinction does not in fact arise at all - at least in the way it has been formulated - not because there is no clear boundary line, but because the original problem of the need to draw the distinction, namely the problem of how theoretical terms can be given a meaning, arose from the mistaken assumption that terms can be considered and compared with the world in isolation from one another, and in isolation from a particular theory. The old problem, it is claimed, arose from a mistaken theory of language.¹⁰

This objection is based on the claim that we do not make neutral observation reports, but that each part of our experience is in some way clouded by our present, alterable conceptual scheme, by an already formulated problem, and by certain expectations about what will in fact be seen. This objection attacks the very roots of positivism, namely its verificationist criterion, and the success of the opposition to traditional empiricism depends on the successful destruction of verificationism. The various holistic doctrines¹¹ opposing verificationism have

¹⁰ Feyerabend's objections, which echo this claim, will be discussed below in ch. III.

¹¹ By "holism" I refer to the empiricism that characterizes the epistemology of Duhem, Quine and Hesse (See Duhem (1906), Quine (1951) and Hesse (1974)). An important feature of this empiricism is its theory of coherence: statements of fact do not stand in isolation, but are bound together in a network of laws which constitutes an explanation of experience.

all emphasized the importance of the interconnections and relations between the network of sentences making up a body of knowledge. Within the network one may distinguish, on different grounds, between more or less observational statements, but it must be remembered that these statements cannot be verified or falsified independently of any theory.

The above view of the nature of knowledge and theories has led to a new concept of "theory-ladenness" of terms, and, to differing degrees, it has been argued that theory-ladenness permeates even the terms of the so-called "observation language". Whether or not one embraces this view depends on the extent to which one admits that knowledge is expressible only in a holistic manner as a set of changing but interconnected sentences.

Holism attempts to give a more reasonable account of the recalcitrant elements of our experience than either verificationism or falsificationism have done, and this description might serve to give a clearer understanding of what is meant by "theory-laden". Rather than viewing a theory as a static whole to which those elements would be considered as counter-evidence, one can take the holistic approach of viewing them as a set of sentences with a more or less permanently, though not necessarily, fixed core. The implications of this view are abbreviated by Hesse:

The point [that recalcitrant experience calls for modification of our laws and definitions] should lead to a far-reaching appraisal of orthodoxy regarding the theory-observation distinction. To summarize, it entails that no feature in the total landscape of functioning of a descriptive predicate is exempt from modification under pressure from its surroundings. That any empirical law may be abandoned in the face

of counterexamples is trite, but it becomes less trite when the functioning of every predicate is found to depend on some law or other and when it is also the case that any 'correct' situation of application - even that in terms of which the term was originally introduced - may become incorrect in order to preserve a system of laws and other applications. It is in this sense that I shall understand the 'theory dependence' or 'theory-ladenness' of all descriptive predicates.

(Hesse, 1974, p. 16)

A further implication of this view questions the so-called "objectivity" of science. What it suggests is that the idea of antecedently existing neutral facts waiting to be collected in order that one might arrive at a true account of the world is mistaken. Perhaps all that can be said of what is given to us in experience is that it is an amorphous mass that stimulates our sensory organs. Yet to say this is to say nothing that can imaginably be denied or is of any interest. However, it is claimed that talk of sensations, sense data, material objects, or particles of theoretical physics at once invokes a particular theory, psychological or scientific.¹² Each, in its own way, is fundamental and thus the positivistic demand to give priority to one of these categories as the unchanging foundation of our knowledge is challenged. Data, it is claimed, are not detachable from a particular theory (Hesse, 1969, p. 281); what is counted as data is as a result of some prior theoretical interpretation.

Hence it is argued that the objectivity of science is at best relative - relative to the accepted theory of the

¹² For example Quine (1948), pp. 16-18, for a statement of this claim.

time - and the phenomenon of theory-ladenness extends even into the roots of a system of knowledge. It is an unwarranted claim that there is, as Strawson maintains, an in principle "massive central core of human thinking which has no history ..., [that] there are categories and concepts which, in their most fundamental character, change not at all" (Strawson, 1959, p. 10). In short, a holistic doctrine believes that nothing in the way we interpret the world is immune from revision.

On the one hand, this type of relativistic holism solves the original problem of providing a criterion of demarcation between theoretical and observation language: there is no difference, in principle, between the ways in which different terms in a scientific language acquire their meanings. On the other hand, however, it creates what appears to be a new problem. If the above view is sound, then it would appear that there is no way in which two theories could be seen as rivals. For if understanding the meaning of any term involves understanding its role in the theory and understanding the principles of that theory, then it might be argued that a term occurring in more than one theory plays a different role, and therefore has a different meaning, in each of the theories in which it occurs. In the same way as disagreement between theories would be impossible, so would agreement between them be impossible. From this it would appear that theories establish a rather peculiar relationship to one another. Apart from exhibiting tokens of the same word, which marks

only a trivial similarity between different theories, theories appear to have no common element.

Yet this seemingly problematic outcome is welcomed by Feyerabend as the very phenomenon which he sees as fundamental to science: radical meaning changes of terms and incommensurability between different theories. Despite arguments for the thesis there is something intuitively unacceptable about it, for it depicts a strange picture of science, destroying at once its progressive evolutionary nature and its rationality.

To evaluate the merits of this picture we must look more closely at the question of scientific change. It is in this area that Feyerabend's views of language play a part. Several attempts have been made to account for the nature of scientific change. We have already mentioned the traditional empiricist view that, in the course of the history of science, older theories have become absorbed by, or are reduced to, more inclusive, more accurate, newer ones.¹³ On this view a theory is a relatively autonomous body of knowledge which attempts to provide a true explanation of diverse natural phenomena.¹⁴ The need for a change in theory would arise directly from an anomalous experimental situation. Such an experiment could point easily to the fault in the existing theory. The task of

¹³ Nagel (1960, 1961, ch. 11) has been the major exponent of this view.

¹⁴ This view is supported by Hempel and Oppenheim (1948). See also Hempel (1962).

discovering an erroneous hypothesis within a theory is thought to be unproblematic. The successful portions of the older theory are seen as subsumed under the newer one.

There is an implicit emphasis here on interpreting scientific change as more or less gradual. There is seldom any radical change as one would be limited by the partial success in existing theories which would have to be explained by any new theory. This provides a striking contrast to the new empiricist school of thought: here we discover Kuhn (1962) speaking of "scientific revolutions", and Feyerabend (1970, 1975) proposing anarchy as a desirable feature of scientific methodology. Although partly metaphorical, these phrases are indicative of their new attitude towards scientific change. Change is viewed as a radical alteration of a current theory; consistent with this is Feyerabend's claim that alternative theories are incommensurable.

Neither Nagel's reductionism nor Feyerabend's opposing radical incommensurability seems to me an adequate way of characterizing scientific change and the relationship between different theories. A holistic thesis need not, however, entail such a radical view of scientific change, and its formulation in the writings of Duhem, Quine and Hesse does not emphasize this feature of scientific change. I turn now to a detailed study and criticism of the incommensurability thesis after which I hope to show that this is not the best way of accounting

for theory-ladenness and that the problem with Feyerabend's views lies in the theory of language that he adopts.

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CHAPTER II

I believe that Feyerabend's argument for an incommensurability thesis can be reconstructed so as to be shown to be dependent on the following twelve premises, each of which he supports:

1. Different scientific theories vary in a manner similar to the way in which ideologies of different cultures vary (Feyerabend, 1975, p. 274).

2. Because of this, a logico-mathematical approach is inappropriate to the study of scientific theories. An anthropological procedure is the most profitable way of studying scientific change, since physical and historical conditions influence this change (Feyerabend, 1975, pp. 249-260, 295).

3. The distinction between theory and observation in science is unfounded (Feyerabend, 1962, 1970, pp. 71-72; 1975, pp. 168-169).

4. Science in progress violates, and shows the unreasonableness of, two conditions of traditional empiricism, namely the "consistency condition" and the "condition of meaning invariance" (Feyerabend, 1963, p. 18).

5. Facts are not autonomous; there are no crucial experiments in science (Feyerabend, 1963, p. 27; 1965, pp. 174-175, 214).

6. Comprehensive theories cannot share any statements, since the facts which they purport to describe are not neutral (Feyerabend, 1975, p. 276).

7. Scientific change cannot be rationalized; that is, we cannot provide a set of methodological rules with which to reconstruct the development of science (Feyerabend, 1970, pp. 72-73; 1975, p. 284).

8. The development of a conceptual scheme and perception in an individual and in a community undergoes stages of complete change (Feyerabend, 1975, pp. 273-274).

9. Scientific change involves perceptual and conceptual changes of a very fundamental nature (Feyerabend, 1975, p. 229).

10. Those terms which are common to different comprehensive theories have undergone a change of meaning in their occurrence in alternative theories (Feyerabend, 1965b, pp. 268-271).

11. Hence scientific change entails a replacement of statements from theory to theory, even of those which we might wish to call "observational" (Feyerabend, 1975, p. 276).

12. Whorf's theory of language can adequately account for such instability of meaning (Feyerabend, 1975, p. 223).

13. On the basis of the above, we can conclude that alternative scientific theories are incommensurable.

The twelve premises constitute central claims made by Feyerabend in support of his view of scientific change. Most of the theses outlined can be grouped together in sections revealing certain patterns of thought. Together they form a contentious network of ideas on which I hope to throw both light and doubt. In what follows the theses will not be given equal attention, and I shall outline the

manner in which I shall deal with them.

(1) is a historical claim and (2) a methodological claim, both of which are closely linked with the view upheld by Feyerabend, and those who oppose the traditional empiricist style of viewing scientific enquiry, that the history of science provides an important insight into the nature of science. As I propose largely to ignore the historical aspect of Feyerabend's approach I shall merely summarize his main points in favour of the use of an anthropological method and then raise some questions with respect to these and the claims in (1).

Theses (3) to (6) constitute the skeleton of Feyerabend's attack on twentieth century traditional empiricism. These are put forward as historically adequate descriptions of science as well as methodological demands. These will be given fairly brief treatment except for the attack on the condition of meaning invariance. This latter is best understood in connection with Feyerabend's claims about language and meaning in general, and will thus be dealt with more fully in conjunction with theses (10) to (12).

Thesis (7) is a statement of Feyerabend's position against Popper and Lakatos - major representatives of a recent espousal of the rationality of scientific knowledge. Feyerabend's point of view will be attacked and this will throw doubt on his incommensurability thesis which he sees as "closely connected with the question of the rationality of science" (Feyerabend, 1970, p. 72).

Theses (8) and (9) deal with the questions of conceptual

and ontological change, and with the view that different theories espouse different conceptual schemes. The majority of issues in this area will be restated in terms of questions about meaning.

Theses (10) to (12) are representative of Feyerabend's radical meaning variance claims. It is from this area that most of the difficulties with Feyerabend's philosophy of science spring, and part of the reason for this is his adoption of an inadequate theory of language and meaning. I propose to criticize his arguments in detail and put forward some ideas that might lead to a more acceptable view of meaning. In the light of this and the preceding arguments I will show that the incommensurability thesis is unfounded.

In the remainder of this chapter I will raise some brief questions with regard to theses (1) and (2).

Feyerabend's view of science as a form of thought contained within a given ideology (Feyerabend, 1975, chs. 17 and 18) gives rise to his claim that "the views of scientists, and especially their views on basic matters, are often as different from each other as are the ideologies that underlie different cultures" (Feyerabend, 1975, p. 274).

This approach to scientists and their theories is useful insofar as it emphasizes the creative aspect that Feyerabend, like Popper, perceives to be fundamental in the formation of scientific theories. In addition, it places the work of scientists within a socio-temporal framework, rather than the atemporal one given by positivistic

interpretations of science. It thus accords well with Feyerabend's emphasis on the history of science.

However, if we study the term "ideology", we see that the view Feyerabend has of scientific theories is questionable. If we interpret "ideology" very broadly as pertaining to a body of beliefs that are in some sense fundamental to a given culture, then it is not at all clear that the views of different scientists are as different from each other as different ideologies. It is true to say, for example, that Copernicus and Ptolemy held very different sets of beliefs about the earth and its status with respect to other heavenly bodies. But the term "ideology" has a ring of relativism about it, and this is no doubt intended to be understood by Feyerabend's reader. Sociologists of knowledge view different cultures and their ideologies from the point of view of a doctrine of relativism, and Feyerabend appears to echo this attitude in his comparisons of science with myth (Feyerabend, 1975, p. 295) which leave one with the impression that the status of science is to be seen as no greater than that of different "primitive" myths. Feyerabend is thus offering us an extreme form of relativism if he compares the different beliefs of scientists with different ideologies. This is reiterated in his claim that "there are frameworks of thought (action, perception) which are incommensurable" (Feyerabend, 1975, p. 271). If scientific theories are incommensurable, then one must deny the existence of any common standard of

truth against which their individual claims can be measured. Even if one does foresee difficulties in trying to identify such a standard of truth, an extreme relativism is not the only alternative to traditional empiricism, and there are effective ways of constructing a standard of comparison. Without examining this claim any further, I would like to suggest that this way of viewing the beliefs of different scientists is not the most acceptable or satisfactory one.

The advocacy of an anthropological method in thesis (2) as the best approach to studying scientific change is consistent with both the above thesis and Feyerabend's concern with the importance of the history of science. It is not clear whether Feyerabend's defence of this method amounts to anything more than the plausible demand that certain fields of enquiry in the philosophy of science require a study of some relevant historical factors that might be thought to add to one's understanding of a particular theory. There is no justification of a methodology but only some scattered hints about the types of principles Feyerabend feels should be borne in mind when examining scientific theories. He provides the reader with examples from social anthropology intended to parallel cases in the philosophy of science.

One needs to query Feyerabend's assumptions - and their implications - first, that a "logical reconstruction" would affect one's understanding and interpretation of a particular theory, and second, that understanding the

background of a theory - and this presumably includes understanding socio-historical and cosmological details concerning the scientist and environment of the theory - by methods hinted at in the text of Against Method is necessary and desirable for the study of certain philosophical issues about science.

In defence of the first assumption Feyerabend points out that different theories might rest on different logical systems, and that an analysis of a theory and the meanings of its terms from just one point of view - that of a particular logical system - will distort the theory. His task now is to substantiate his claim that different scientific theories of the kind to which he is referring do rest on very different logical systems, to show what is meant by the expression "different logical system", and also to show that an anthropological method will serve the purpose of detecting these differences. The unsettled question of a logic for quantum mechanics - which is thought by some to defy the law of non-contradiction - is perhaps one issue from which Feyerabend might derive some support for his thesis¹. However, this is an atypical example in that it is perhaps the only case of a scientific theory which has stimulated questions about alternative logics. It is by no means clear that Feyerabend can find further support within the realm of scientific theories.

¹ Feyerabend has in fact chosen this particular example (see Feyerabend, 1975, pp. 253-254). The question of a logic for quantum mechanics, though, deserves fuller treatment than he has given it, because it is a contentious issue.

Some difficulties arise with the second assumption. Feyerabend speaks of "the method" (1975, p. 249) of the anthropologist, without appearing to acknowledge that the question of method is a contention amongst anthropologists. However, this is not a serious difficulty. More problematic is the fact that Feyerabend has not really given us a justification for his argument that an anthropological method is desirable for the study of science.

As a proposed method for the study of science, anthropology, itself a branch of the social sciences, is in need of scrutiny, and its methods have not been shown to provide any certainty for the pursuits of philosophy of science. It is not at all clear that the anthropological method is, therefore, a desirable one for philosophy of science. The problems that arise in connection with Feyerabend's assumption have been summed up by Karl Popper:

. . . the idea of turning for enlightenment concerning the aims of science, and its possible progress, to sociology or psychology (or, as Pearce Williams recommends, to the history of science) is surprising and disappointing.

In fact, compared with physics, sociology and psychology are riddled with fashions, and with uncontrolled dogmas. The suggestion that we can find anything here like 'objective, pure description' is clearly mistaken. Besides, how can the regress to these often spurious sciences help us in this particular difficulty? Is it not sociological . . . science to which you want to appeal in order to [answer these questions?]

(Popper, 1970, pp. 57-58)

A further difficulty is that if scientific theories are in fact incommensurably different from one another, and only

an anthropological method will be appropriate to study different theories, then the anthropologist must be able to free himself from any of his own cultural beliefs and expectations in order not to impose these on the underlying cosmology of the theory he is studying. Only in this way can a historically more primitive theory be studied without bias. Yet it is by no means clear that all this is possible for the historian or philosopher of science, for it entails that the philosopher must ignore the meanings of terms he already knows and learn the new theory, as it were, from scratch.

In conclusion I shall note that one must carefully examine the claims that scientific theories are so very different from one another, and that an anthropological method is necessary and desirable for studying scientific change. The latter issue is closely connected with the argument for the importance of history of science, and will not be discussed in any more detail. The former - the question as to how two theories really do differ - will be discussed in a related form throughout the latter half of this thesis.

CHAPTER III

In this chapter I discuss, again fairly briefly, theses (3) to (6). Examination of these will conclude the discussion on traditional empiricism and its defects. However, it will emerge that Feyerabend's position is not necessarily the only or best alternative.

We have already considered (ch. I above) some objections to the traditional distinction between theory and observation. The objection on which Feyerabend's approach is based is that the traditional distinction rests on the mistaken verificationist theory of language. Implicit in this verificationist thesis is the view that there is a stable and independent observation language, the meanings of whose descriptive terms are determined by that to which they each refer. So-called theoretical terms, it is held, are given meaning on the basis of a connection established between these terms and observational terms which thus resulted in a partial interpretation for theoretical terms. The difficulties encountered in attempting to explicate the connections between the two types of terms led to the initial disillusionment with the distinction and point of view on which it was based.

In his earlier writings, Feyerabend (1962, 1965, 1965a) characterizes the traditional empiricist view of observational terms as a "semantic theory of observation" (Feyerabend, 1965a, p. 243). According to this position, observation statements are distinguished from other state-

ments by virtue of the meanings of their terms - that is, there is assumed to exist a "logical connection . . . between the meaning of a term . . . and the fact that it is, or is not, an observational term" (Feyerabend, 1965a, p. 242). Feyerabend's chief criticism of this view is that it guarantees the "perennial truth" of certain synthetic statements which are principles governing the application of observational terms. There, he claims, will be synthetic a priori statements, synthetic because they are about the world, a priori because they are necessarily true. His objection, framed in this way is not particularly cogent, for it is not clear that the statements to which he is referring would be regarded as synthetic.

However, Feyerabend does seem to be correct in placing the present difficulty with the view that observational terms are thought to have an independent meaning. An insight that has emerged fairly recently¹ is that attention should not be focussed on individual terms, but rather on the network of terms that occur within a theory in order that one might understand questions about their cognitive significance. The distinction between theoretical and observational terms may still be upheld if found useful, but it is to be characterized in a different way.

In this same period of his writing Feyerabend makes useful reference to a theory he claims to have been

¹ Duhem (1906), Quine (1948). For an enumeration of the difficulties involved in the old distinction the reader may consult Hempel (1965a).

initiated, but subsequently dropped, by Carnap (1936), a theory which Feyerabend calls the "pragmatic theory of observation" (Feyerabend, 1962, p. 36; 1965, p. 212). Observational statements are still given special status within a theory but they are to be distinguished from other statements psychologically, "not by their meaning, but by the circumstances of their production" (Feyerabend, 1965, p. 212). Observation statements are thus distinguished pragmatically, by virtue of the fact that they are the ones that produce most easily and reliably the assent of the scientific community.²

This means of drawing the distinction is useful in several respects: it does not demand a rigidly defined and unchanging set of observation sentences; it avoids difficulties associated with an extreme verificationism; nevertheless it is able to maintain a distinction that is useful in that it marks out those sentences which are considered to be preferred ones, ones which are relatively more stable and less liable to be removed in situations of scientific change than the less observational ones.

When criticizing the distinction between theory and observation Feyerabend's position is more radical than might appear from the above points. He holds that the principles of each theory permeate the observation

² Quine (1960, ch. 2) has a similar characterization of observation sentences as those whose stimulus meaning is fairly constant from speaker to speaker, and the response of speakers depends on little information other than what is present in the near surroundings.

statements, or rather dictate a set of observation statements, to the extent that each theory has its own observation language. This highly contentious claim is intended to refute the idea that our theories can be criticized by an independent observation language expressing our common-sense knowledge. Feyerabend is correct in doubting the irrefutability of common-sense claims, for such claims are often shown to be fallacious, as, for example, statements about the moving sun or flat earth were at some stage proved false. He takes this view to its extreme when he suggests that statements like "The table is hard" must be regarded as false because they conflict with current atomic theory.

Although we can agree upon the usefulness of the pragmatic method of distinguishing between those scientific statements which are preferred and the rest, and furthermore acknowledge the fact that what we decided to call observation statements by no means constitute a rigid and unalterable set, it does not follow, as Feyerabend claims, that the observational language of each scientific theory is different from and incompatible, or incommensurable, with that of a different theory.

In his more recent writings Feyerabend's view seems to undermine even the more insightful aspects of his earlier position, and emphasizes instead the radical side of his claims. He explicitly rejects the relevance of the theory-observation distinction (Feyerabend, 1970, pp. 71-72; 1975, p. 168) on the grounds that experience

does not play any essential role in science!³ The pragmatic theory of observation is no longer referred to, and as a result there is now no room for the notion of privileged statements in science which are protected more closely than others in times of crisis. The chief difficulty with Feyerabend's criticism of the theory-observation distinction - and this will emerge more clearly in the last three chapters of my thesis - is its extremism, the fact that it does not allow for any overlap of observation statements from theory to theory. Feyerabend's reasons for this become apparent upon consideration of the next three theses. I turn now to an examination of the fourth thesis.

Traditional empiricism, in accordance with the principles of theoretical reductionism, is bound, according to Feyerabend, by two conditions:

(a) Theories are admissible in a given domain only if they contain theories already in that domain, or are consistent with them, and

(b) the meanings of terms and phrases used in theories will have to remain invariant with respect to scientific progress: more advanced theories must guarantee such invariance (Feyerabend, 1963, pp. 17-18).

Feyerabend's contention is first that these conditions are methodologically undesirable as they prove to be

³Feyerabend (1970), in various sections, undermines the importance attributed to the role of experience in science, a thesis for which I have no room for discussion here.

restrictive, and second "that the development of science very often violates them, and that it violates them in exactly those places where one would be inclined to perceive a tremendous progress of knowledge" (Feyerabend, 1963, p. 18).

The dangers of the first condition are easy to pick out. If rigorously adhered to, it leads to an untenable dogmatism from an empiricist point of view, for one would be forced to abandon any new theory that conflicted with (yielded contradictory predictions to) an existing theory. Feyerabend clinches this neatly: "it eliminates a theory not because it is in disagreement with the facts; it eliminates it because it is in disagreement with another theory" (Feyerabend, 1963, p. 25). An existing theory can only be removed by showing it to be inconsistent with known facts. But presumably there are ways of retaining a theory in the face of disconfirming evidence by means of ad hoc hypotheses or similar devices, and this is where dogmatism is liable to set in.

Traditional empiricists are not unaware of such difficulties⁴ and might reply that Feyerabend has distorted their position by overstressing the importance of a condition which is only one heuristic device in the logic of scientific discovery. However the attack is not one questioning how much emphasis should be placed on this condition, but queries the need for and desirability of

⁴ For example, see Hempel (1966), ch. 4.

this condition at all, thereby questioning the whole thesis of reductionism. Other conditions, such as simplicity, elegance, universality, may well be introduced into the traditional empiricist thesis, but even with such modifications the sole arbiter is in the end a collection of facts. Such an attitude relies on the existence of a crucial experiment which will enable us to decide which of two conflicting theories "agrees with the facts". It is these implications of the consistency condition that Feyerabend is questioning.

The consequences of adopting the consistency condition would be methodologically undesirable, Feyerabend maintains, for it would result in the view that "discussion of incompatible facts" leads to progress while "discussion of incompatible alternatives will not" (Feyerabend, 1963, p. 27). Facts are thus regarded as sufficiently autonomous to admit agreement with them as the sole criterion for the acceptance of a theory. Methodology would thus advocate the collection of facts in order to bring about scientific progress. However, this presupposes that facts exist and can be discovered independently of theories, and it is this claim which Feyerabend wants to contend:

Not only is the description of every single fact dependent on some theory (which may, of course, be very different from the theory to be tested), but there also exist facts which cannot be unearthed except with the help of alternatives to the theory to be tested This suggests that the methodological unit to which we must refer when discussing questions of test and empirical content is constituted by a whole set of partly overlapping, factually adequate, but mutually inconsistent theories; in short, it suggests a theoretic pluralism as the basis of every test procedure.

(Feyerabend, 1965, p. 175)

Before evaluating these claims against the autonomy of facts, which constitute the fifth thesis, I shall complete my analysis of the objections to the consistency condition of meaning invariance.

We have seen two related objections to the consistency condition; first, it is claimed that it lends itself to an untenable dogmatism in that it allows for the rejection of theories on account of the fact that they conflict with ones already accepted in that domain; second, facts, rather than theories, are seen to constitute the methodological unit of testing and acceptance of a theory. Because of this, it is felt that the condition is undesirable if interpreted as a primary methodological condition.

In addition Feyerabend shows that the condition has been violated in actual scientific progress. He gives several illustrations from the history of science: "Newton's theory is inconsistent with Galileo's law of the free fall and with Kepler's laws; . . . wave optics is inconsistent with geometrical optics; and so on" (Feyerabend, 1963, p. 20). It now becomes clearer what such inconsistency amounts to. It is not just the fact that predictions differ, for in most cases - those involving relatively short distances and periods of time - the differences are generally too small to be detectable. The type of inconsistency referred to is "logical inconsistency"; in other words, the assertions of the two theories in the same domain are contradictory. As an example we can consider the type of inconsistency that is met in assertions about velocity in Newtonian and

Einsteinian physics: the former sees no upper bound on the velocity any body can attain, while the latter defines the speed of light as the greatest velocity.

The point about such examples is that they are not isolated cases but can generally be seen in major turning points in the history of science. If this is the case, and there are several instances to support this claim, then the consistency condition is methodologically undesirable as Feyerabend claims. However, Feyerabend's alternative proposal - methodological pluralism - does not in fact offer the intended support for the incommensurability thesis, and after a consideration of his other theses which support incommensurability, it will become clearer why this is the case. I will return to this point towards the end of chapter VII.

We turn now to the second condition of traditional empiricism to which Feyerabend objects, the condition of meaning invariance. The case against this condition is not quite so straightforward and for various reasons I think it best not to discuss it in full at this stage. For one thing it is intimately tied up with the tenth and eleventh theses of Feyerabend's argument; for another, a discussion of sameness and difference of meaning is more appropriate in a section on language and meaning. We may note at this point that there is an obvious sense in which the meanings of terms used in contradictory theories may be said to be different. If meaning is determined in part by the way in which a term is used in a particular theory,

a term will differ in meaning if used in two incompatible theories merely because there are slight differences in the ways in which it might be used.⁵ However, there are two kinds of questions one must ask: one is whether all instances of meaning change are equally important and non-trivial, the other is whether the different meanings will be incommensurably different as Feyerabend would like to maintain. I shall thus leave discussion of this thesis until chapter VII below.

We now return to the fifth thesis which claims that facts are not autonomous. Two sides of Feyerabend's argument in connection with this claim do not quite hold together. On the one hand, he uses his claim to support the notion of a theoretical pluralism in order that "questions of test and empirical content" (Feyerabend, 1965, p. 175, emphasis mine) be facilitated.

New theories are introduced to establish "the relevance and the refuting character of many decisive facts" (Feyerabend, 1965, p. 176) that tell against the view to be tested. From this one can interpret Feyerabend to be supporting a view that, in the end, crucial experiments, which are only possible with the existence of alternative theories, do exist in order to refute a given point of view (Feyerabend, 1965, p. 176).

⁵ For example consider the possible differences in the meaning of "velocity" in the case discussed above, pp. 34-35.

However, this conflicts with the claims a little further on in the same paper⁶:

As has been pointed out . . . criticism must use alternative theories. Alternatives will be the more efficient the more radically they differ from the point of view to be investigated. It is bound to happen, then, at some stage, that the alternatives do not share a single statement with the theory they criticize. The idea of observation that we are defending here implies that they will not share a single observation statement either. To express it more radically, each theory will possess its own experience, and there will be no overlap between these experiences. Clearly, a crucial experiment is now impossible. It is impossible not because the experimental device would be too complex or expensive, but because there is no universally accepted statement capable of expressing whatever emerges from observation.

(Feyerabend, 1965, p. 214)

For Feyerabend, thus, there can be no crucial experiments because there is an important sense in which there are no neutral facts common to different theories.⁷ This is one sense in which different theories are claimed to be incommensurable. Furthermore, this passage directly entails the sixth thesis, which is that statements are replaced from theory to theory.

Both thesis (5) and (6) can be queried. It may well be the case that data, or interpretations of experience, are not detachable from a theory, but from this it does not follow that there can be no overlap between the experiences

⁶ I take this aspect of Feyerabend's argument to be more representative of his general philosophical position.

⁷ It is interesting to compare Feyerabend's reasons for rejecting the existence of crucial experiments with those of Duhem (1906). For Duhem there can be no crucial experiment between theories because theories can develop protective devices in the form of ad hoc hypotheses in order to prevent their refutation by any conceivable counter-instance.

of two theories. Nor does it follow that theories cannot share statements. Newtonian and Einsteinian mechanics are regarded by Feyerabend as "strong alternatives" since they satisfy the required conditions; however, it is a dubious claim that they share no statements.

I have been necessarily brief when dealing with these four points, partly because they will re-emerge in different form to be dealt with in later chapters. The question of the status of facts will appear in the chapter on ontological change. The issue of statements differing from theory to theory links up with questions on the meanings of terms constituting these statements.

In conclusion I shall remark that I am basically in agreement with the general tenor of Feyerabend's objections to the various tenets of traditional empiricism, and would agree that they are not descriptively adequate principles of science. Nevertheless, his radical views, which in the end support an incommensurability thesis, are not the only alternatives to traditional empiricism, and it is with these extreme points of view that my criticism arises.

CHAPTER IV

The belief that the history of science illustrates a path of development and progress which can be shown to be dominated by rational procedures has been the guiding factor in attempts to provide a methodology for natural science. This belief stems, in part, from the attitude that considers science to be the dominant field of enquiry in the realm of knowledge and truth. Consequently there have arisen various theories of rational appraisal of science, most notably those associated with verificationist or falsificationist schools of thought.¹

One striking feature of such theories is that the term "logic" is invariably associated with their approach to scientific method.² Philosophy of science is generally thought of as being akin to a logical system in view of the extent to which science, its explanations and proofs, can be thought of as a formal structure. As a result, the philosopher sees his task to be one of providing either an inductive logic of confirmation as, for example, Carnap (1950) and Hempel (1945) have attempted to do, or a deductive logic of falsification in the way Popper (1959)

¹ Of the former there is the work of Carnap, Ayer and the logical positivists; of the latter there is the work of Popper, Lakatos and Braithwaite. However, there is by no means a clear dividing line between the two schools - Hempel's work, for example, being characteristic of both.

² For example Ayer (1936), Carnap (1950), Hempel (1945) and Popper (1959).

has tried to do. Rather than deal with actual scientific practice, the tendency of these philosophers has, on the whole, been to provide a reconstruction which demonstrates the rationality of the scientific method.

Actual examples from the history of science are not central to the main arguments of these various theses, and the formal approach taken is not connected with any sociological or psychological theories that might be prominent in a historical study. A sharp contrast is thus provided by an examination of the approach taken by Koyré, Kuhn, Polanyi, Feyerabend and others who oppose the methods of both the verificationist and falsificationist schools of thought. Let us for the moment consider the differences in approach between those who accept and those who reject the belief in an underlying rational procedure for science as a preliminary to further discussion.

The group represented by Feyerabend and Kuhn emphasize the importance of the history of science to any thesis that purports to be dealing with scientific methodology. Verificationism and falsificationism are criticized for distorting actual science in their ahistorical reconstructions of its method. There is a parallel emphasis on the importance of the sociological and psychological influences on the history of science. Once we abandon the idea of any rationality underlying scientific method however, we abandon criteria for growth and progress since standards associated with these ideals would rely on a notion of rationality. Science thus loses the privileged position,

accorded to it by rationalist theorists, as the most sophisticated and correct body of knowledge. In contrast, science is seen on a level with myths and other attempts to explain and understand experience. Since "progress" would signify some kind of measuring stick by which we could judge developments as being progressive or retrogressive, the best we can speak of, then, is "scientific change". The philosopher's task becomes one of explaining things, rather than one of characterizing progress, and this task, it is felt, requires not so much a logical approach as one which analyzes change in terms of social psychology. Historical and sociological data are seen as a background to such changes, and the emphasis on socio-historical conditions is thought to destroy the claim that there can be only a formal or rational assessment of scientific method.³

There is much to be said as regards the problems facing the various verificationist and falsificationist schools of thought in order that we might evaluate their claims for the rationality of scientific method and the progress of science. But this would lead me too far astray from my main concern. I will regard these problems merely as grounds for an examination of the debate between Lakatos and Feyerabend for and against rationality in science respectively. Lakatos' claim is that one can modify the conventionalist-based falsificationism of Popper "by a

³ The points mentioned above are characteristic of many claims made by Feyerabend. (See for example Feyerabend, 1975, ch. 16).

sophisticated version which would give a new rationale of falsification and thereby rescue methodology and the idea of scientific progress" (Lakatos, 1970, p. 116). His proposed "sophisticated falsificationism" is, of course, intended to be free of any major difficulties. Feyerabend's rejoinder to this is that Lakatos' philosophy is merely an "anarchism in disguise" (Feyerabend, 1975, p. 181) which is consistent with his "anything goes" principle and which cannot be increased in rigidity "by adding further standards, i.e. be making reason tougher" (Feyerabend, 1975, p. 199). In other words, Feyerabend seems to be claiming that Lakatos cannot consistently develop a liberal methodology of scientific research programmes and claim to have defended the belief in the rationality of science.

Before we can satisfactorily evaluate the debate, we must first settle some questions about the notion of rationality. First, what does it mean to say that scientific method employs standards of "rationality"? Second, why are such standards to be prized so highly?

To maintain that science is "rational" is not to say that the history of science reveals again and again a series of logical or rational developments. One point which both verificationist and falsificationist philosophers stressed is that there is a distinction, originally drawn by Reichenbach (1938) between a "context of discovery" and a "context of justification". Feigl, for example, writes that "it is one thing to trace the historical

origins . . . of scientific theories; and it is quite another thing to provide a logical reconstruction of the conceptual structure and of the testing of scientific theories" (Feigl, 1970, p. 4). Popper admits that "every discovery contains an 'irrational element', or a 'creative intuition' . . ." (Popper, 1959, p. 82).

The crucial task for philosophy of science, which is concerned not with the discovery of theories, but with their epistemology and empirical claims, would be to concern itself with the "context of justification", that part dealing with the testing of theories, the explanation of laws, and any task that involves critical appraisal of scientific claims. What happens on the level of discovery is of no concern since it is not the task of philosophy to investigate the history of an individual's intuitions. It is claimed that the context of justification reveals rational elements in that the methods of testing theories, and the critical evaluation of these theories, always proceed according to fixed standards or rules (although, of course, the set of standards may vary among different schools of thought). These standards can be spelled out in such a way that they will enable us to tell when a theory is scientifically acceptable by virtue of its confirming instances or success in testing. Moreover, these standards will tell us how best to test competing theories, and what will count as confirming instances for them.

In other words, given a set of rules, we should be able to show, firstly, how science ought to proceed, and

secondly, that it has in fact in many instances conformed to such rules once we have reconstructed the basic principles involved in the various theories. The set of rules will most likely contain certain definite heuristic devices in addition to certain requirements to be fulfilled, all of which will indicate the formal and systematic nature of the scientific enterprise. To say, then, that scientific method is rational is to say that one can establish, at least in retrospect, that science employs certain more or less fixed standards by means of which it tests and judges theories for their acceptability as highly confirmed or corroborated theories. The fact that science does not deviate from these standards means that its procedure can be termed "rational".

It may, then, be asked why these standards of rationality are considered so important. In reply to this one might consider the status of science over the past century: it is generally believed amongst those influenced by modern science that its theories about the universe most nearly approximate the truth about nature and provide us with knowledge, useful tools, and, most important, ways of predicting future events and being able to have some controlling influence over them. This is very much the attitude that prevailed towards Newtonian physics in the late eighteenth century. The so-called "objectivity" and "rationality" of science is felt to be its distinguishing characteristic amongst so many other "theories" about the world which are held to be mythical or fictitious. It is

this ideal of rationality that is felt to enable us to distinguish between science and "non-science": for logical positivists it is important to distinguish between science (sense) and metaphysical theories (nonsense); for Popperians it is crucial to distinguish between science (genuinely falsifiable theories) and "pseudoscience" (unfalsifiable theories).

It is clear that the ideal of rationality is significant for those who believe in the superiority of science as a means of leading us by a progressive movement toward truth. It is this issue in particular that gives rise to a major difference between the proponents and opponents of the view of the inherent rationality of science. One of Feyerabend's chief contentions is that the status of science is no greater than that which he classifies as "myth" (Feyerabend, 1975, ch. 18). Moreover, he maintains that science does not proceed in accordance with any set of rationality standards.

One final point we must consider before examining the debate between Feyerabend and Lakatos is the relationship between irrationality and incommensurability. After rejecting the idea that there can be a rationality theory providing scientific activity with certain procedural standards, Feyerabend goes on to say that ". . . these standards, which involve a comparison of content classes are not always applicable. The content classes of certain theories are incomparable . . ." (Feyerabend, 1975, p. 223). He maintains that one of the most common objections to an

incommensurability thesis comes from those who prize rationality so highly: the mere idea that there are incommensurable theories in the history of science is rejected on grounds of the "fear that they would severely restrict the efficacy of traditional, non-dialectical argument" (Feyerabend, 1975, p. 271). If philosophers in general abandoned the importance attached to notions of rationality it is Feyerabend's belief that they will more readily accept an incommensurability thesis.

Feyerabend and Lakatos begin from the same starting point: a disillusionment with existing methodologies as grounds for the belief in the rationality of science. Both regard falsificationism as paradigmatic of theories of rationality and focus their attention initially on its defects. Lakatos in particular has provided an extensive survey of the development of falsificationist methodology (Lakatos, 1970, pp. 93-132). After an examination of what he calls "dogmatic" (or "naturalistic") falsificationism and "methodological" (or "naive") falsificationism in which the defects of both are presented, Lakatos offers the following comment:

If we look at the historical details of the most celebrated crucial experiments, we have to come to the conclusion that either they were accepted as crucial for no rational reason or that their acceptance rested on rationality principles radically different from the ones we just discussed.

(Lakatos, 1970, p. 114)

This is the dilemma that not only Lakatos, but also Kuhn and Feyerabend, are facing. Lakatos, however, in contrast to Feyerabend, opts for the more optimistic horn and

consequently sets himself the task of specifying acceptable rationality standards.

Lakatos' distinction between various strands of thinking in falsificationist methodologies is, I think, an illuminating and useful one. It serves to forestall a number of premature criticisms of Popper's theory by showing where it is more satisfactory than earlier less sophisticated forms of falsificationism were.

What emerges from Lakatos' discussion is that it is a mistake to object to falsificationism on the grounds that it appears to rest on the notion of independent and infallible basic statements which enable one to reject a theory that is in conflict with any one of these. The notion of a "basic statement" is, of course, central to a theory of falsification for it is basic statements that provide the final grounds for the rejection of any theory. The problem of what to count as one's basic statements is thus a problem for any theory of falsification. Popper himself attempts to justify a particular use of "basic statement" (Popper, 1959, sections 28-29) which does not lay itself open to the objections facing the so-called "protocol-statements" of the Vienna Circle.⁴ The important point to note is that a theory of falsification need not assume a natural division between theoretical and observational statements in order that a theory be considered refutable. One may still accept that no statement can be

⁴ For a discussion of these, see Popper (1959), sections 26-27.

shown absolutely to be true or proven from "facts" and yet uphold a falsificationist methodology; one's basic statements then merely rest on some further specifiable decisions for their acceptance. It is with this consideration that Lakatos feels he can defend a theory of scientific rationality. A brand of falsificationism which makes claims about science as progressing by means of successive refutations of theories "with the help of hard facts" (Lakatos, 1970, p. 97), Lakatos terms "dogmatic falsificationism" because of its reliance on certain dogmatic assumptions about its observational basis. He indicates how such a view can be dismissed and then presents and discusses a view which is much closer to Popper's view and is a naive variant of the view he ultimately wishes to defend.

The naive variant - which both Kuhn and Feyerabend attack - can be spelled out briefly as follows⁵: its most prominent feature is the element of conventionalism to which it adheres. Popper acknowledges that in answering the question "how do we select a theory?" he adopts a position similar to that of a conventionalist⁶ insofar as the fate of a theory is seen to rest ultimately upon

⁵ This is chiefly Lakatos' account and I will indicate where it might seem to deviate from Popper's account.

⁶ By "conventionalism" here, we mean the view whereby systems of knowledge, in particular scientific theories, rest on a set of postulates, in this case "basic statements" which are accepted not because they can be determined to be true in terms of certain observable facts, but because we have made some decision to accept them as the foundations of our theory.

our decisions with regard to the basic statements we accept. However, he claims, there is an important difference between himself and what he calls "conservative conventionalists": according to his theory, the statements decided by convention are not universal, but singular, that is they are "basic statements". Thus he is able to combine an element of conventionalism with falsificationism that is no longer dogmatic, for theories are now not rejected on grounds of conflict with a set of "true" or "absolutely justified" basic statements. It is important that he be able to retain this element of falsification in order that he might succeed in attributing a notion of rationality to science: ". . . we must find a way to eliminate some theories. If we do not succeed, the growth of science will be nothing but growing chaos" (Lakatos, 1970, p. 108).

The intrusion of a conventionalist element nevertheless requires some justification. Without acknowledging such an element we would be faced with some difficulty in attempting to justify acceptance of our basic statements. It was precisely at this point that verificationism fell down: examination reveals an uncritical acceptance of the absolute truth of certain disjoint statements on the grounds of empirical evidence - sense data. Extreme conventionalism is not a satisfactory alternative: first, it can lead to a form of arbitrary dogmatism; second, it can lead to a complete denial of the role of experience in science. Popper desires to retain an empiricist element in his

theory of falsificationism. Thus he accepts that perceptual experiences do influence our choice of basic statements (Popper, 1959, section 29), but he stresses that they play no stronger role since the basic statements we decide to accept can in turn be queried and subjected to test at any time we might feel the need. "We do not attempt to justify basic statements by these experiences. Experience can motivate a decision, and hence an acceptance or rejection of a statement, but a basic statement cannot be justified by them" (Popper, 1959, p. 105).

There is thus a trace of relativism in Popper's adamant denial that experience justifies our acceptance or rejection of a basic statement. For the ultimate justification of our choice of basic statements rests, as Lakatos shows (1970, p. 106), on two types of decisions involving the demarcation of "observational" from "non-observational" singular statements and of acceptable from unacceptable ones. It is implicitly clear that such decisions could have been different from what they are. The question we must ask is how far this affects Popper's intention of providing a rationalist appraisal of scientific method. It is this kind of question which concerns Feyerabend in his attempt to uncover the problems he believes both Popper and Lakatos are facing. However, before considering Feyerabend's criticisms it is necessary to spell out Lakatos' development of Popper's theory of falsificationism.

An obvious criticism of Popper's theory as it stands

is that it does not characterize actual science, or that science, when viewed from his standpoint, in fact appears to be irrational. Scientific change is not just a matter of conclusive falsification. Experimental conclusions are frequently challenged in order to retain a theory. In many instances experimental evidence is ignored rather than considered as an instance of falsification. For a number of years the perihelion of Mercury was seen as an anomaly; only when a possible alternative to Newton's theory emerged was the anomaly actually regarded as a falsification of Newton's theory. In the face of such counterexamples to a falsificationist methodology one could, as Feyerabend and Kuhn have done, abandon the search for rationality in scientific theory and claim science to be irrational. Alternatively one could adopt Lakatos' attitude and attempt to salvage parts of Popper's theory and so defend rationality standards by "reduc[ing] the conventional element in falsificationism. . . and [replac- ing] the naive versions . . . by a sophisticated version which would give a new rationale of falsification and thereby rescue methodology and the idea of scientific progress" (Lakatos, 1970, pp. 115-116).

The central feature of Lakatos' version of falsifica- tionism is its abandonment of two characteristics which he believed had contributed to the naivety of Popper's theory. These are, firstly, that a test is seen as a two-cornered fight between a theory and an experiment, and secondly, that the only scientifically useful outcome of a test is

falsification of that theory. Where both Feyerabend and Lakatos seem to be in agreement is in their attitude towards these two features of science. Both understand that in many cases rival theories compete simultaneously for acceptance⁷, and that the acceptance or confirmation of one theory is frequently of scientific importance. It is in their attempts to explain these features that they part company.

Lakatos' modification involves certain semantic changes in the theory of falsification. Its chief characteristic is its concern with a series of theories as distinct from a single theory, and this is linked with a principle stating that the appraisal of theories need not follow immediately upon their inception, but can be judged in the "long run".⁸ Important for science are what Lakatos terms "theoretically progressive problemshifts", that is, a series of theories, each of which "predicts some novel, hitherto unexpected fact" (Lakatos, 1970, p. 118). A series also constitutes an "empirically progressive problemshift" if some of this increased content in the form of new facts is corroborated. Scientific progress is thus judged in terms of progressive as opposed to degenerating problemshifts.

It is of interest to note that "scientific" now applies only to a series of theories, namely those "which

⁷ See Lakatos (1970), pp. 115, 121; Feyerabend (1963, 1965).

⁸ There may be a problem in deciding how long a theory might be given to prove itself, but this does not appear to constitute a crucial difficulty.

have corroborated excess empirical content over [their] predecessor[s]" (Lakatos, 1970, p. 116). In addition, "falsification" can now only apply in the context of rival theories (Lakatos, 1970, p. 119). The notion of "excess empirical content" plays a crucial role for it is scientifically important that problemshifts produce more new facts than their predecessors. The prominent notion in our new theory of falsification, then, is not the anomaly (falsifying instance), nor the verifying instance, but instead the (rare) instance of corroborated excess information.

Thus Lakatos' rational reconstructionism is intended to be more representative of the history of science by taking into account the fact of the delay in the final appraisal of a theory. For example, the overthrow of Newton's theory took place long after the accepted anomaly was recognized, and then only when there was a more satisfactory rival. Furthermore, Lakatos' theory broadens the scope of scientific activity by showing that the concern of a scientist is not with a single theory and a test, but with a series of theories and their empirical confirmation and infirmation. By stressing the need for a proliferation of theories as a prerequisite for scientific progress, Lakatos shifts the standards of intellectual honesty, which, for him, are standards of rationality: "one should try to look at things from different points of view, to put forward new theories which anticipate novel facts, and to reject theories which have been

superceded by more powerful ones" (Lakatos, 1970, p. 122).

We have noted above certain apparent similarities between Lakatos and Feyerabend, but then seen that Lakatos attempts to lay down fairly rigorous procedural methods for science, which are intended to accord with the history of science while showing its inherent rationality.

Feyerabend's response to this, as I have mentioned above, is as follows:

Even the ingenious attempts of Lakatos to construct a methodology that (a) does not issue orders and yet (b) puts restrictions upon our knowledge-increasing activities does not escape [the] conclusion [that there is not a single rule that remains permanently valid]. For Lakatos' philosophy appears liberal only because it is an anarchism in disguise. And his standards which are abstracted from modern science cannot be regarded as neutral arbiters in the issue between modern science and Aristotelian science, myth, magic, religion, etc.

(Feyerabend, 1975, p. 181)

Feyerabend's argument turns on a discussion of four related issues: "the standards [Lakatos] recommends, his evaluation of modern science . . ., his contention that he has proceeded 'rationally', as well as the particular historical data he uses in discussion of methodologies" (Feyerabend, 1975, p. 184). I shall not discuss here the second and fourth issues, but will focus my attention, instead, on the first and third.

The central feature of Lakatos' standards, according to Feyerabend, is that although they are descriptively adequate for the appraisal of research programmes, they are not methodologically adequate in that "they do not yet advise [the scientist] how to proceed" (Feyerabend, 1975,

p. 185; cf. Lakatos, 1971, p. 104). "The standards, taken by themselves, have no heuristic force. Reason as defined by Lakatos does not directly guide the actions of the scientist" (Feyerabend, 1975, p. 186).

This criticism misrepresents Lakatos' intentions. We should ask, first of all, what we require of an acceptable methodology. Lakatos himself notes that if we study contemporary methodologies we will notice that

they are all very different from what used to be understood by 'methodology' in the seventeenth or even eighteenth century. Then it was hoped that methodology would provide the scientist with a mechanical book of rules for solving problems . . . Modern methodologies . . . consist merely of a set of . . . rules for the appraisal of ready, articulated theories.

(Lakatos, 1971, p. 92)

In a footnote he stipulates the need to distinguish between methodology, as providing means for appraisal of already existing scientific theories, and heuristics, as containing stronger prescriptive rules for means of arriving at theories. In other words, he is suggesting that normative philosophy of science today employs a weaker sense of "normative", and provides the scientist not with a fixed set of procedural rules, but rather with means for the appraisal of a theory already devised.

Feyerabend appears to ignore this semantic issue in his critique, or at best he implicitly suggests that a rationality theory must contain fixed standards by which it can offer heuristic advice.⁹ If one accepts the

⁹ See Feyerabend (1975), p. 196, where he stipulates that such standards must never be overruled by others and that they must have heuristic force.

distinction which Lakatos is trying to enforce, one can agree that he does provide the foundations for a relatively sound methodology: a means for distinguishing between progressive and degenerating problemshifts, linked with the value judgement that the former are scientifically important.

This does not imply that science cannot take the opposite path to the one suggested by methodology at any given moment. Lakatos offers as an example Lorentz's ether theory which can be strengthened sufficiently to become mathematically equivalent to Einstein's etherless theory (Lakatos, 1970, p. 164). Feyerabend employs an excessively rigid notion of "rule" in his counter to Lakatos' thesis that it is the progressive research programmes that are valuable. Certainly it is "legitimate to do the opposite" (Feyerabend, 1975, p. 185), as Lakatos himself acknowledges; methodology is not so much concerned with the delimitation of "legitimate" moves for the scientist as with an indication of where appraisal lies. It is only with a very weak sense of "rule" that Lakatos' methodology is concerned. By ignoring this sense of "rule" and demanding that either a rationality theory be able to provide rules that "directly guide the actions of the scientist" (Feyerabend, 1975, p. 186), or else it cannot claim to have the status of a methodology, Feyerabend argues that, on the basis of Lakatos' principles, "anything goes".

Feyerabend's chief contention is that Lakatos wants

to have things both ways: adopt a methodology by which he can argue for the rationality of science, and yet make it sufficiently liberal to avoid the kinds of objections that have been raised against previous methodologies. Intent on safeguarding any liberalism for the side of epistemological anarchism, Feyerabend refuses to concede Lakatos' claim for a methodology. Yet in doing so he overlooks the possibility of a midpath between rigid prescriptivism and "anything goes", namely that governed by the weaker sense of "normative" which Lakatos defends. The fact that his standards are not absolutely rigid does not mean that they are unacceptable as standards of rationality, as Feyerabend would like to suggest.

An earlier article (Feyerabend, 1970, p. 78) offers a more fruitful analysis of Lakatos' position. Here Feyerabend associates Lakatos with Popper, rather than attempting to show him as an unwitting ally of himself. The critique of Lakatos points to those principles Feyerabend believes to be irrational. Lakatos' view is irrational

because there no longer exists a single set of rules that will guide us through all the twists and turns of the history of thought (science) . . . as historians who want to reconstruct its course; . . . [furthermore,] local reasons which change from age to age are never sufficient to explain all the important features of a particular episode.

(Feyerabend, 1970, p. 78)

This passage reveals more clearly any infelicities we might detect in Lakatos' rational reconstructionism.

Yet are we still not asking too much from a rationalist theory? Again we should decide whether or not we accept Lakatos' idea of "rationality" in accordance with which he admits that "one may rationally stick to a degenerating programme until it is taken over by a rival and even after. What one must not do is to deny its poor public record . . . It is perfectly rational to play a risky game: what is irrational is to deceive oneself about the risk" (Lakatos, 1971, note at bottom p. 104). It is clear that "rationality" means something different to Feyerabend from what it means to Lakatos. The former identifies it with heuristic advice, the latter with what he calls "intellectual honesty". Lakatos seems to be justified in this for one must acknowledge that ideas of rationality change, as he has pointed out, in different scientific communities, and that it is therefore pointless to look for a rigidly held set of rules as constitutive of "scientific methodology".

If we accept, with Lakatos, that a methodology should be distinguished from a rigidly prescriptive set of rules, then there is a certain appeal in the notion of "intellectual honesty". For while not guaranteeing a universal set of rules (and being irrational in Feyerabend's eyes), it does not thereby grant licence to the appraiser of scientific theories, but rather a respectable amount of freedom. Hence it does not follow that "anything goes", since certain (intellectually dishonest) activities are excluded.

If we permit the building of a definite value judgement

into our notion of science, we can defend a liberal methodology, along the lines suggested by Lakatos, and thereby offer grounds for the belief in the rationality of science. Feyerabend has not shown, except in a highly restricted and personal sense, that scientific progress - or change - is not rational. His arguments against Lakatos, therefore, are not conclusive.¹⁰

¹⁰ In a concluding footnote I would like to comment briefly on Feyerabend's reply (1975, Appendix 4) to Lakatos' question "where is the epistemological anarchist who out of sheer contrariness walks out of the window of a 50-story building instead of using the lift?" (Feyerabend, 1975, p. 221). Feyerabend's "decisive" answer employs the following claims: firstly, that the fear of heights is very likely innate (and hence not rational, as animals are often guided by innate mechanisms); secondly, that people stay away from windows as a result of training and because of their belief in "rumours": "reports on the deadly effects of high falls" (Feyerabend, 1975, p. 221) - the epistemological anarchist may legitimately be a coward; thirdly, "what the epistemological anarchist does deny is that he can give reasons for his fear which agree with the standards of some rationality theory" (Feyerabend, 1975, p. 222). That he does not jump is irrelevant: these, for Feyerabend, are the points at issue.

As regards the first, "innate fears" are often cured. For example, the child's fear of water may be cured by swimming lessons. What are often incurable are "acquired fears", reasonable or unreasonable. As regards the second, again Feyerabend is confusing the issue by calling such reports "rumours". Reports which are continually corroborated by evidence, even though unwitnessed by the major portion of the community, can hardly be called "rumours". A strange form of empiricism! Finally, Feyerabend is again demanding an excessively rigid sense of "reason". It is little wonder that he cannot support a rationalist theory of science since such "reasons" hardly ever arise in the actual history of science.

CHAPTER V

We now turn to a closer analysis of the issue of theory change in science to determine what kind of support can be provided for the incommensurability thesis.

The objections to the reductionist model of scientific change have generated deeper interest in the question of what occurs during a period of scientific change. From the extreme and controversial position adopted by Feyerabend, Hanson and Kuhn, one can extract the view that two fundamental changes take place during scientific transition: conceptual change and linguistic change. These two types are not entirely distinct, and I hope to show that much of what is contained within the notion of conceptual change can be completely analyzed in terms of linguistic change as Feyerabend conceives of it.

In order to understand what can be meant by the claim that theory change in science entails conceptual change, and to evaluate this claim, it is worthwhile to study what the above-mentioned authors have said in this regard.

Feyerabend (1965) indicates support for what he calls a "theory of dependence of perception upon belief" (p. 220), a theory which he illustrates with examples of perception and representation within the visual arts. He extends his argument for this theory to the realm of science, claiming that radically divergent theories "will possess [their] own experience[s], and there will be no overlap

between these experiences" (p. 214). Elsewhere he states that

introducing a new theory involves changes of outlook both with respect to the observable and with respect to the unobservable features of the world . . .

Scientific theories are ways of looking at the world; and their adoption affects our general beliefs and expectations, and thereby also our experiences and conceptions of reality. We may even say that what is regarded as "nature" at a particular time is our own product in the sense that all features ascribed to it have first been invented by us and then used for bringing order into our surroundings.

(Feyerabend, 1962, p. 29)

Feyerabend's more recent ideas about conceptual and perceptual change¹ appear to have taken on a more normative character. He offers as a foundation for his own views Piaget's account of the perceptual changes that take place during the development of perception in the child. Feyerabend argues that these kinds of change need not be limited to childhood: "is it not more realistic to assume that fundamental changes, entailing incommensurability, are still possible [in the adult] and they should be encouraged lest we remain forever excluded from what might be a higher stage of knowledge and consciousness?" (Feyerabend, 1975, p. 229). This passage contains many arguable assumptions. The suggestion that we encourage perceptual changes in our own experience is linked with the pluralistic methodology

¹ Feyerabend uses "perceptual change" interchangeably with "conceptual change". A similar usage is evident in the writings of Hanson and Kuhn.

Feyerabend continuously propounds, and, in order to justify the normative demands he needs at least to be able to justify his version of pluralism. The suggestion also assumes that conceptual changes can take place within human beings whose perceptual development is thought to be complete. This latter assumption is one which has frequently been rejected by numerous philosophers, significantly by Kant. Feyerabend's claims are not obviously correct, for even if it is the case that our perceptions are the product of our beliefs, it does not necessarily follow that we can alter these beliefs so radically that we can be said to have experienced conceptual changes.

Clearly we need to clarify what is meant by "conceptual change", and to extricate some criteria for conceptual change. But it is constructive first to continue our examination of the claims made by our three authors. Feyerabend's use of "conceptual change" cannot be understood to mean merely "change in beliefs", for our beliefs can change without there being a corresponding change in our conceptual scheme. To understand the notion of conceptual change one must first take cognizance of the fact that the scientific enterprise involves the formulation of "rules according to which objects or events are collected into classes. We may say that such rules determine concepts or kinds of objects" (Feyerabend, 1965b, p. 268). Changes can be of two sorts, according to Feyerabend: "changes brought

about by a new point of view [that] occur within the extension of these classes and, therefore, leave the concepts unchanged" (Feyerabend, 1965b, p. 268). Such changes are not conceptual changes. Conceptual change occurs "either if a new theory entails that all concepts of the preceding theory have extension zero or if it introduces rules which cannot be interpreted as attributing specific properties to objects within already existing classes, but which change the system of classes itself" (Feyerabend, 1965b, p. 268).²

Two questions immediately arise: can (and does) change of the second sort, namely conceptual change, occur within the field of human enquiry? Second, even if the answer to this is affirmative, does the description of conceptual change best characterize scientific change? To the first question the answer given by Feyerabend, Hanson and Kuhn is clearly affirmative. Feyerabend adopts a theory of perception elaborated by Gombrich in his Art and Illusion. This theory rejects the assumption that there is only one way of representing events and situations in an artistic medium, namely representative realism which employs the rules of perspective developed in the Renaissance. Instead, it draws on the very different art

² Feyerabend uses this description as a characterization of meaning change, but it suits our present purposes as an account representative of his views on conceptual change, since he provides no other. It will receive fuller discussion in the chapter on meaning change (ch. VII).

provided.

A different bias for arriving at the same answer to the first question is evident in Kuhn and Hanson. Both rely rather heavily on elements of Gestalt psychology, and draw analogies between various perceptual experiments conducted in psychology, and certain situations in the history of science.

Hanson's theory of perception begins with the question "Do Kepler and Tycho Brahe see the same thing in the east at dawn?" (Hanson, 1958, p. 5). The question, he feels, is not one that can receive an automatic response, but rather motivates an enquiry into the concepts of seeing and observation. There is a sense in which "something about their visual experiences at dawn is the same for both: a brilliant yellow-white disc, centred between green and blue colour patches The same view, or scene, is presented to them both" (Hanson, 1958, p. 8). However, he maintains that this notion of seeing does not exhaust the concept because it deals merely with the physical state the two men are in. A "visual experience", he would claim, is something additional to just being in a particular physical state. The scientist qua scientist, because of his theoretical assumptions, sees an object in a certain way. Thus Hanson introduces the concept of "seeing-as". It is in this sense that Tycho can be said to see the sun (as) rising, while Kepler, who regarded the sun as fixed, would see the horizon move below his eye-sun line. Numerous examples from Gestalt psychology instantiate

this sense of "seeing": the same configuration of lines on a printed page may be seen either as a duck or as a rabbit. The analogy between these examples and instances in the history of science is intended to hold because Hanson believes that scientific theories are interpretations of what is seen in the neutral sense of "seeing", so that within a theory objects can only be "seen-as", or seen in a certain way. Nevertheless there are difficulties with this analogy.

Kuhn seems to have perceived some of these difficulties and exhibits more caution here. He notes that although

psychological experiments are suggestive, they cannot, in the nature of the case, be more than that. They do display characteristics of perception that could be central to scientific development, but do not demonstrate that the careful and controlled observation exercised by the research scientist at all partakes of those characteristics."

(Kuhn, 1962, p. 113)

These are some of the difficulties associated with the analogy: first, the Gestalt experiments are controlled by some external standard which acts as an explanation of the switch in vision. In the case of scientific change there is no such standard, and in general the scientist cannot make the same perceptual switches that the subject of a Gestalt experiment can. Second, if the analogy is taken seriously, it commits the scientist to a form of relativism: since each interpretation of the pattern of lines - as being either a duck or a rabbit - is equally valid, it should follow that each "interpretation" (that is, each scientific theory) of what is presented to the

scientist is equally correct.

Although Kuhn does make similar criticisms of the Gestalt analogy and Hanson's acceptance of the analogy (Kuhn, 1962, pp. 113-115), he nevertheless argues that there are perceptual transformations during scientific revolutions for which evidence can be given. One of his examples is Herschel's discovery of the planet Uranus. Kuhn writes that

On at least seventeen different occasions between 1690 and 1781, a number of astronomers . . . had seen a star in positions that we now suppose must have been occupied at the time by Uranus.

(Kuhn, 1962, p. 115)

Herschel's careful investigations, with an improved telescope, of what appeared to him to be unusual for a star

disclosed Uranus' motion among the stars, and Herschel therefore announced that he had seen a new comet! Only several months later . . . did Lexell suggest that its orbit was probably planetary. When that suggestion was accepted there were several fewer stars and one more planet in the world of the professional astronomer. A celestial body that had been observed off and on for almost a century was seen differently after 1781 because . . . it could no longer be fitted to the perceptual categories (star or comet) by the paradigm that had previously prevailed.

(Kuhn, 1962, pp. 115-116)

Similar interpretations of other scientific examples follow (Kuhn, 1962, pp. 116-119), each of which is intended to provide evidence for the claim that a "shift in vision" occurred in these instances in the history of science. It is clear that Kuhn is using the word "see" in the above quotations in a sense similar to that of Hanson's "seeing-as". It will be instructive to analyze this notion of

"see" as it should provide some insight into what is involved in these so-called instances of shifts of vision or conceptual change. Feyerabend's view of observation, although not explicitly similar, would, I think, be sympathetic towards the general views expressed by Hanson and Kuhn.

Kordig has attacked Hanson's argument that there is a scientifically relevant sense in which Tycho and Kepler see different things (suns).³ Hanson (and Kuhn and Feyerabend) obviously feel the need to claim the relevance of this sense of "see" in order to capture the difference between, for example, the geocentric and heliocentric conceptions of the universe within their model of explanation. Before examining Kordig's objections we need to analyze in more detail this particular notion of seeing.

We have noted Hanson's use of the expression "seeing-as" which is intended to bring out an important feature of seeing, namely "the way in which visual experience is had" (Hanson, 1958, p. 15). Another equally significant aspect of seeing which is captured by this expression is, according to Hanson, its cognitive aspect. As soon as we move beyond talk of observing certain shapes and patches

³ Lest we feel inclined to satirize Hanson's view in the way Hooker (1973) has done, allowing the "Aristotelian patrician" to see something different from the "Newtonian gentleman", we can restrict the application of this particular discussion to scientists. Of course, this has the consequence of separating the layman "culturally" from the scientific community, wherever one decides to draw the line.

of colour, described by "seeing" in its neutral sense, to talk about seeing birds, voltmeters, copper sulphate crystals, and so on, some claim to possession of a certain sort of knowledge enters. This feature of seeing can be brought out by the expression "seeing that" (Hanson, 1958, p. 20).

Seeing an object as something entails, for Hanson, that one may see that certain things happen to be the case. If I say that I have just seen a voltmeter then, says Hanson, presumably I "could specify some things pertinent to [voltmeters]" (Hanson, 1958, p. 21). This may seem absurd, for I might know nothing about the way current is measured yet I can claim to have seen a voltmeter because someone pointed one out to me. However, we have decided (p. 68, n. 3), to limit our attention to perceptions of scientists, and there is certainly a sense in which meaningful discourse between scientists implies some knowledge or information about the descriptive words they use. Since what follows the "that" in a "seeing that" locution, is a sentential clause, we can represent any sentence containing such a locution most simply as "a sees that Gx", where "a" is the observing scientist, "x" the object observed, and "G" a simple or complex descriptive predicate expressing the scientist's knowledge about the object.

Let us now turn to Kordig's attack on Hanson. On the basis of phrases like "the concept of seeing embraces the concepts of visual sensation and of knowledge"

(Hanson, 1958, p. 25), it would seem most natural, if we wanted to interpret further the notion of "seeing that", to render that locution as "knowing" or "knowing that". This is the first interpretation that Kordig studies and consequently rejects. Let us examine what this interpretation entails.

If "sees that" can be represented by "knows that" then the sentence "a sees that Gx" can be represented by "a knows that Gx". What are the implications of such an interpretation? I will restrict my analysis here to a strict sense of "know", since any weaker sense can be covered by the second interpretation I shall discuss. Under this interpretation, one of the truth conditions of "a knows that Gx" is that "Gx" is true. Let us apply this to our example of Tycho and Kepler. If it is true that they see (in the non-neutral sense) different things, either that the sun moves or that it does not, then we can infer from the truth of "Tycho knows that the sun moves" and "Kepler knows that the horizon turns or moves away from the sun" the truth of both "the sun moves (and the earth is at rest)" and "the horizon shifts (while the sun is at rest)". The two sentences can be shown to be logically incompatible, and by affirming the truth of both we have violated the law of noncontradiction. This consequence is not very serious within the bounds of a relativistic theory of truth. However, Kordig, whose argument differs slightly from my own, shows that Hanson cannot accept a relativistic theory since he takes seriously the dictates

of modern science. Thus, for Hanson at least, this interpretation of "seeing that" is unacceptable.

There is a second way to interpret "seeing that" locutions. If we consider our willingness to grant that, due to lack of sophisticated experimental and observational equipment, earlier scientists have held mistaken beliefs about the world, then we should perhaps talk of a scientist's believing, rather than knowing, that something is the case. It would seem more correct to interpret "a sees that Gx" as "a believes that Gx".⁴ Let us see how this interpretation affects first Hanson's argument and second Kuhn's example in support of his claims for a shift in vision.

It is certainly more plausible to assume that Tycho believed that the sun moved and the earth was at rest: in addition that his belief was shown to be mistaken upon the acceptance of the Copernican view of the universe. But this does not necessitate the conclusion which Feyerabend and Hanson wish to draw, namely that there are different things seen or different observational experiences had by scientists working within different theories. As Kordig argues, "two people can believe that contradictory properties hold essentially of the same object" (Kordig, 1971, p. 9).

⁴ This interpretation may not be so readily acceptable to Feyerabend, Hanson and Kuhn who may - and do in Feyerabend's case - want to grant the validity of perceptions within different perceptual schemes. See Feyerabend (1965), p. 220, where he talks of "genuine observational reports concerning devils and gods".

Furthermore, their beliefs, if incorrect, can be shown to be wrong. One would wish to reply to Hanson that there is an important sense in which two people do see the same object. It is this sense that permits the overthrow of one theory in favour of another on account of mistaken beliefs about what is seen. If "seeing that" were the relevant sense of "seeing" for science, nothing would constitute evidence against a theory: Tycho, who sees the sun moving, cannot be corrected by Kepler because, on Hanson's account, they see different suns. Any statement which contradicted Tycho's could be dismissed as being about a different sun. We discover the dogmatic state of affairs that Tycho's theory cannot be refuted. Our second interpretation does at least preclude such consequences as this.

Let us examine Kuhn's example about Uranus in the light of this interpretation. We find ourselves with a scientific description that accords well with traditional empiricist principles. Kuhn's claim becomes: several astronomers believed there to be a star in positions we now suppose (believe) to have been occupied by Uranus at the time. Herschel, whose investigations were carried out with better instruments, believed this to be not a star but a comet. Only after sufficiently long investigation to understand its motion was it finally claimed to be a planet. Unless one wishes to claim that Herschel and the astronomers were not mistaken originally, this interpretation is certainly more plausible. Shifts in vision are really

shifts - usually fundamental shifts - in belief.

The above contains Kordig's general line of attack against Feyerabend, Hanson and Kuhn, and it provides several insights into the difficulties associated with their views. However, I feel that he has failed to give a suitable account of what these philosophers have intended by their use of such phrases as "shifts in vision", "perceptual change" and "conceptual change". This is partly because their own characterizations are not sufficiently illuminating, and because their arguments, which utilize analogies and case studies, are not really convincing for one who feels the need for further analysis of the notion of conceptual change. I propose to provide some analysis in order that we might judge more easily the acceptability of Feyerabend's point of view.

I begin with some objections to Kordig's critique. The first difficulty is that Kordig does not seem to have been sufficiently aware of the complexity of the notions introduced by Feyerabend and Hanson. He overlooks a point made by Feyerabend (1965b, p. 268) that conceptual changes are not just changes in belief, since the latter do not always necessitate conceptual revision. Furthermore, the second interpretation of "seeing that", namely as "believes that", is too simplistic as it stands, since it requires supplementation in order to account for a distinction between justified and unjustified belief, and furthermore does not seem to make room for an ability to attain knowledge. As a result it oversimplifies the

notion of conceptual revision. A strict sense of "know", and an unanalyzed notion of belief do not seem to exhaust an analysis of the concepts under examination.

Neither Feyerabend nor Hanson nor Kuhn seem to have made clear the implications of conceptual revision, but have concentrated on examples and analogies which may not satisfy the reader that conceptual revisions do occur during scientific change. By isolating some implications of conceptual change we may be in a better position to determine the acceptability of Feyerabend's argument. Most of these implications will be found to pertain more to the notions of meaning and meaning change, and discussion will be deferred to chapter VII.

Although conceptual change implies, as Hanson, Kuhn and Feyerabend point out, different ways of perceiving the world, unless we understand more fundamental issues about the precise nature of such changes the idea of perceptual change will remain vague and unsupported. There is no way of determining whether or not two people perceive the world differently apart from what they tell us and how they talk about their theories. Thus we must consider the kinds of differences that theories of different "world views" will exhibit.

The first implication of conceptual change that I would like to suggest is ontological change; this change should be detectable from an examination of a theory that is conceptually different from another. This implication receives support from Feyerabend's characterization of

concepts in terms of classes or kinds of objects, and his claim that a change in concepts entails either a change in the extension of these classes or a change in the system of classes itself (Feyerabend, 1965b, p. 268).

It cannot be denied that ontological changes accompany scientific development: the discovery of oxygen and subsequent rejection of phlogiston entailed an ontological change; the discovery of subatomic particles led to an increase in ontological commitments. However, Feyerabend's claims suggest that he is talking about a radical shift in ontology: ". . . all concepts of the preceding theory have extension zero" (Feyerabend, 1965b, p. 268). In examining cases of purported radical ontological changes, one must determine whether there are nevertheless sufficient similarities between a rejected theory and its replacement to warrant doubts for the conclusions Feyerabend wishes to draw in support of his incommensurability thesis.

An instance of such radical change discussed by Feyerabend (1975) is the replacement of classical physics (and its "faulty ontology") by the theory of relativity. "In this case, every description inside the domain [of the old theory] must be changed and must be replaced by a different statement (or by no statement at all)" (p. 275). According to Feyerabend's interpretation - which does not seem incorrect - the Newtonian framework presupposed the idea that certain "properties [mass, shape, etc.] inhere in objects and change only as the result of a direct physical interference" (p. 275). Relativity theory denies

the existence of such inherent properties: mass and shapes are now conceived of as "relations between physical objects and co-ordinate systems which may change, without any physical interference, when we replace one co-ordinate system by another" (p. 275).

This example does indicate the occurrence of significant conceptual and corresponding ontological changes. Moreover, these are changes which demand a re-examination of the reductionist conception of scientific development, for it no longer seems possible to construe Newtonian mechanics as a special case of relativity theory. Thus Feyerabend is correct in discovering that at least some important cases of scientific change demand a revised interpretation. But two questions must be asked: do all the examples of conceptual change given by Feyerabend, in addition to those provided by Kuhn and Hanson, fit his model of scientific change more accurately than any other? Second, does it follow, in the case of the Newton-Einstein transition, that "the new conceptual system . . . does not just deny the existence of the classical states of affairs, it does not even permit us to formulate statements expressing such states of affairs" (Feyerabend, 1975, pp. 275-276)? Is he correct in saying that "it does not, and cannot, share a single statement with its predecessor" (Feyerabend, 1975, p. 276, emphasis mine)?

It is important to note that Feyerabend's claim extends even to the observational level of a particular theory. The new theory does not share any observational

statements with its predecessor. It is this aspect of his claim that, even in the light of his rejection of the traditional theory-observation distinction, is rather dubious.

In an attempt to show that alternative theories may have overlapping ontologies, Scheffler has offered a rejoinder to the radical view.⁵ He suggests the need to distinguish between what he terms "categories" and "hypotheses": the former are said to classify phenomena, define concepts, and could be called analytic; the latter are empirical statements, dependent on a prior classificatory scheme and deal with questions about the distribution of phenomena within each category. Although hypotheses or observational statements are thus not independent of some more general category system, it does not thereby follow that alternative hypotheses cannot be formulated using one set of categories. In this way hypotheses can be compared with each other and it will also be possible to formulate statements concerning one hypothesis in an alternative one. Even though they conflict they are still referring to at least some common entities.

The difficulty with Scheffler's account concerns the distinction on which it rests. It may be possible within a theory to distinguish between what we would call conceptual statements and empirical statements, but is it possible

⁵ Scheffler (1967) attempts to modify and retain some of the older empiricist positions regarding the theory-observation distinction and corresponding principles.

to draw this distinction intertheoretically? Quine (1948) has argued that the sort of distinction Scheffler wishes to make cannot be made at the intertheoretical level as he desires. Feyerabend (1962, 1965) has developed a theory of observation similar to Quine's where he states that in the context of rejecting a theory or considering an alternative, one cannot distinguish between its observational and conceptual parts.

Although there is some merit to this point of view the debate does not end there. Kordig, for example, believes that Scheffler's basic ideas can nevertheless be preserved independently of the category-hypothesis issue. He writes that

the fact that what is observed is influenced by belief does not imply that what is observed cannot be shared by holders of different beliefs; not every influence need be a one-to-one influence. Further, there may exist much in common between the two sets of beliefs and this may make common observations possible. The gap in scientific transitions is much exaggerated . . . ; if . . . a list were made of the agreement of the two sides, the list of agreements would be enormously large In a conflict between competing scientific theories there are normally many rather basic principles held in common by advocates of each of the competing views.

(Kordig, 1971, pp. 15-16)

These remarks stand in direct conflict with later arguments by Feyerabend (1975, pp. 269, 277). Kordig's remarks reiterate the need to examine the two kinds of theories in order to evaluate claims for the

incommensurability thesis.⁶

The discussion has drifted inevitably into a realm closely connected with conceptual change, namely that of meaning change. It is clear that conceptual change does imply meaning change: referential variance of the kind discussed so far entails that terms which are used in different theories do not have exactly the same reference. According to one important theory of meaning, this implies that the meanings of these terms has changed.

Within the context of a clearly defined theory of meaning many of the issues between Feyerabend and his opponents can be settled. An important task is to clear away or to reveal some of the differing presuppositions about meanings that underlie their various disagreements. In the next chapter I shall try to formulate and clarify some central issues and theses about meaning and meaning change.

⁶ Similar objections are raised by Koertge (1973) and Shapere (1966). Note that Feyerabend does not deny the existence of any similarities between competing theories (Feyerabend, 1965b, pp. 268-271): his point is that these are irrelevant to questions of scientific change.

CHAPTER VI

This chapter is concerned specifically with the attempt to clarify issues in connection with the meaning and reference of scientific terms. The terms under discussion are primarily referring expressions, expressions purporting to refer to objects, properties of objects, relations and so on. We can begin our analysis by turning to the Fregean distinction between sense and reference, a distinction fundamental for any study of referring expressions.¹ Using this distinction we must ask what constitutes necessary and/or sufficient conditions for the sort of meaning change subscribed to by Feyerabend.

It is clear that the sense of a word may change without there being corresponding change in its reference or extension; this is consistent with Frege's own viewpoint. For example, the sense of "water" as "a colourless, transparent liquid without taste or smell" has been replaced in chemical theory by its sense as "a compound of two parts of hydrogen to one part of oxygen". The reference of the term has not changed. Thus change in sense is not a sufficient condition for change in the reference or extension

¹ I shall not attempt to justify this distinction here. Suffice it to say that one can distinguish between the object(s) to which a word refers, and the sense, or meaning, or intention, expressed linguistically, which the word has. The distinction is particularly appropriate in the realm of scientific terms, where the object, or class of objects, referred to is clearly distinct from their meaning; this is applicable to both the traditional empiricist view of correspondence rules and a view like that upheld by Feyerabend that meanings are theory-dependent.

of a term.² On the other hand, it is entailed by Frege's account³ that a change in the referent or extension of a term is a sufficient condition for a change in its sense. There are obvious cases in which this holds: when the extension of the term "fish" was changed to exclude whales, there was an accompanying change in the meaning of the term to which the definition "breathes through gills" was added. Although this point has been challenged,⁴ the objection is not relevant to my present purposes, because there are important cases to which it is applicable.

One point derivable from the above is that a shift in reference should imply a change in the truth-value of at least some sentences containing the relevant term. Under a realist interpretation of science, such as Feyerabend accepts, sentences obtain a truth-value on the basis that their referring terms refer in a certain way. A sentence is true if and only if the objects referred to satisfy what is being said about them. In the case of

² However, in the case of general or class terms, which have an extension, a refinement of their sense generally results in an alteration of their extension.

³ Frege (1892) speaks of the sense of a word as determining its reference. The reference belongs to the sense rather than to the sign, and is thus dependent on it. Referential change thus results in change in sense.

⁴ For example, by Putnam (1973).

a referential shift, the objects newly referred to need not satisfy the original demand. Thus the truth-value of sentences may, and in some cases will, change.

It would seem then, that an account of meaning variance must take into consideration both a shift in the sense of a word and a shift in its reference. Such shifts would constitute change in meaning, but the important issue is whether such change is trivial, or at least does not support the kinds of conclusions Feyerabend draws from the assumption of meaning variance. The present chapter is intended to offer grounds for settling this issue.

One problem which beset traditional empiricism was to account for the way in which theoretical terms acquired meaning. Various attempts to solve this in terms of the notion of correspondence rules met with little success. Their opponents chose to dissolve this problem by rejecting the distinction between theory and observation. However, they still face the problem of accounting for the way in which a descriptive term in general acquires meaning. Since Feyerabend does not provide any account of this we must provide our own; however, it is likely that any plausible account will weaken some of his claims about the meanings of scientific terms.⁵

We may distinguish three types of referring expressions:

⁵ For instance, Feyerabend relies on the unexplained and rather vague notion of "covert classifications" to substantiate some of his claims (Feyerabend, 1975, pp. 223-225).

those that refer, or purport to refer, uniquely to one object, namely singular terms; those that are true of more than one object, namely general, or class terms; and so-called "mass terms" like "water" and "red" which refer to continuous rather than discrete matter. These three sorts behave differently in various ways.⁶ Their grammatical function is different and our mode of learning them differs. To learn the correct use of a singular term it is sufficient to know the object to which it refers; to apply a general term correctly one needs to know, in addition to its extension, a criterion of identity or individuation of the objects to which it refers in order to be able to use it in different contexts. Mass terms, because their extension is not divided, function similarly to singular terms in this respect.

Terms in the scientist's vocabulary are of all three sorts. However it is not necessary to analyze each of the three separately in order to answer questions about the meanings of these terms. Many of these problems can be dealt with on the basis of a study of singular terms and their meanings. Singular terms include all substantives and substantival phrases preceded by the definite article, and also a class of expressions describable as "proper names".⁷

⁶ For further detail, consult Quine (1960), ch. 3.

⁷ The distinction between proper names and singular descriptions is not always clear. Without attempting to draw the line rigidly, I shall distinguish proper names as having no (or very little) descriptive content.

Philosophers have focussed their attention on the category of proper names.⁸ We can follow their approach for our own purposes. A crucial question is how a link is established between a proper name and an object, given that its function is to refer to an object (present or not), without actually specifying, in the way that a singular description does, any characteristics of the object. I refer again to Frege's distinction which has initiated several replies to this question. He notes that there is a distinction in cognitive value between identity statements whose terms on either side of the identity sign are the same ("a = a") and those where the terms differ ("a = b"). The latter are usually informative in the way that the former can never be. To explain this he says:

A difference can arise only if the difference between the signs corresponds to a difference in the mode of presentation of that which is designated . . . It is natural, now, to think of there being connected with a sign (name, combination of words, letter), besides that to which the sign refers . . . also what I should like to call the sense of the sign, wherein the mode of presentation is contained.

(Geach and Black, 1970, p. 57)

Frege was thus led to associate with every sign both a sense and a reference,⁹ and this enabled him to give an answer to the above question, namely how a link is

⁸ In particular Frege (1892), Russel (1905), Searle (1969), and Kripke (1972).

⁹ There may be thought to be difficulties, not particularly relevant here, in granting that proper names have a sense.

established between a particular object and a proper name. What indicates whether or not we have learnt to use a name (or any descriptive word) is our ability to use the word correctly in situations other than that in which it was initially learnt. This requires of us that we be able to identify what is being referred to in future situations. When the object is present, this is done by ostension; when not, by description. In order to use the name we must be able to associate it with a criterion for recognizing a particular object as its referent.

Frege's view is representative of a thesis commonly held by major philosophers of language this century.¹⁰ Very broadly this thesis stresses that the meaning of a name or descriptive term is tied up with a set of rules for its use. This view appears in modified form: for Frege, the criterion of usage may be specified by a uniquely identifying description; Russell accepts this for most of the expressions we would call proper names. Wittgenstein and Searle have avoided the rigidity of this demand for a specific criterion of identification, by suggesting that the sense of a proper name is given by a cluster of criteria, not all of which need to be satisfied in order that a word be used correctly.

This last modification could profitably be adapted for our purposes to examine scientific terms, partly because it does not attempt to specify a rigid criterion.

¹⁰ Especially Russell, Wittgenstein and Searle.

But further issues must first be clarified.

The view just discussed is one of two that have dominated recent philosophy of language, and arguments for or against meaning variance have drawn, implicitly or explicitly, upon these two views. The second is more recent,¹¹ and has been adapted for argument against Feyerabend. For this reason it is important to consider this view in order to judge its pertinence to a theory of meaning variance. I shall thus provide a brief exposition of this view which was first put forward by Kripke in his article "Naming and Necessity".¹²

Kripke intends to refute the view that a single description, or a family of descriptions, does or can provide the meaning of a proper name. He states that the criterion of identification by means of which a proper name is introduced into a language does not serve to give it a meaning, but merely to fix its reference. A term is introduced in the manner suggested by the Fregean view, that is, by ostension. Whenever the term is used subsequently, it refers to that to which it was originally attached. The advantage of this, according to Kripke, is that one no longer needs to search for satisfactory criteria of application of the term, since these are contained in its act of introduction. A possible difficulty

¹¹ It stems from a theory of proper names introduced by Kripke (1972).

¹² This exposition is necessarily brief. For further detail the reader is referred to Kripke (1972).

with the Fregean or Wittgensteinian view which we have not yet considered, is whether criteria for the application of a term can readily be given.

As an alternative thesis, Kripke suggests that it is in principle possible to trace back a chain of uses of a particular name, thus at once specifying its criterion of application and allowing for the possibility of always being able to refer to that which was initially referred to by the introduction of the name. According to Kripke, a name is always used in this way, subsequent speakers using a name with the intention of referring to what was initially designated by the name. This, for Kripke, satisfactorily answers the question how it is that a name is used to refer to an object: there is the act of introduction, by which the term is attached to its referent, and every subsequent use is causally attached to the initial use, thus successfully effecting an act of reference.

There is one primary difficulty with Kripke's account, and this is that it cannot account for or explain a shift in reference. When a name is used to refer to an object, it cannot, on any subsequent use, refer to anything other than that object. This holds for possible as well as actual uses of the name. Kripke has coined the phrase "rigid designator" to apply to any expression or designator that designates the same object in all possible worlds.¹³

¹³ "Possible worlds" is intended to cover all possible situations in which a particular proper name could be used, and more specifically to cover all possible empirical situations.

Proper names, since they always refer to the object originally designated by the use of the name, are therefore rigid designators.

A problem arises. Suppose it were a mistaken belief of ours that the man to prove the incompleteness of arithmetic was Kurt Gödel, and that in fact another person, X, proved it. On Kripke's account, every use of the name "Kurt Gödel" would refer, not to the individual who proved the incompleteness theorem, but to the man who was baptized "Kurt Gödel". Surely a more plausible account would be one which assumes that people who use the name today were referring to X, believing him to be named "Gödel". I am suggesting that there are cases where a term may shift its reference; however, on Kripke's account of names as rigid designators, this would not be possible.¹⁴

The problem becomes more serious if Kripke's view is extended, in the way Putnam (1973) has suggested, to apply to certain types of scientific terms.¹⁵ Putnam believes that the problem with the Frege-Wittgenstein view of meanings of terms lies in the idea that linguistic competence is merely a matter of knowledge. He argues that this knowledge is neither a necessary nor a sufficient condition

¹⁴ Kripke does offer an account of such examples, but the lack of an explanation for a shift in reference seems to hinder any extension of his theory.

¹⁵ Putnam deals specifically with physical magnitude terms such as "force", "electricity" and "mass" (1973), and with mass terms such as "water" (1973a). He does, however, suggest extending his theory to deal with what he calls "natural kind words", such as "fish" and "horse".

for the ability to use a term. The important feature of Kripke's account

is that the knowledge an individual user of a language has need not at all fix the reference of the proper names in that speaker's idiolect; the reference is fixed by the fact that that individual is causally linked to other individuals who were in a position to pick out the bearer of the name, or of some names from which the name descended.

(Putnam, 1973, p. 207)

In other words, knowledge (of necessary and sufficient conditions for membership in the extension of a term) is not a necessary condition for a speaker to be able to use the word correctly in some situations.

Here I agree with Putnam, for this demand is obviously excessive. I am able to use the term "protea" correctly in very many situations, but I know very few of the conditions which a flower must satisfy in order to be included as a member of the class of proteas. However, I am not sure that this factor cannot be accommodated within a modified Fregean-Wittgensteinian account. Putnam appears to have misrepresented at least Wittgenstein's view in attributing to it the idea that "part of the meaning" means "necessary condition for membership in the extension" (Putnam, 1973, p. 200). It will be recalled that for Wittgenstein not all criteria of application - which individually constitute part of the meaning of a term - are individually necessary for the correct use of a term.

The claim that knowledge is not a sufficient condition for linguistic competence is not so clear. Putnam maintains

that "one must, in addition [to possession of linguistic knowledge], be in the right sort of relationship to certain distinguished situations" (Putnam, 1973, p. 202). This seems to suggest the need to take into account the causal feature of linguistic competence, namely that each use of a word be causally linked to previous uses and ultimately to the original one. But it is difficult to see what this adds, for in learning a term one is necessarily involved in a causal process, even from the Fregean point of view. The point of Kripke's theory is, I think, not to add any conditions for linguistic competence, but rather to eliminate the idea that there is involved, in any learning process, anything apart from the fixing of the referent of the term.

The more serious objections to Putnam's account arise from his characterization of scientific terms as rigid designators. Feyerabend has not been incorrect in maintaining that there are shifts of reference, that is, the extension of a term may change. Perhaps there is a difficulty with his view in that it appears to admit excessively radical shifts of reference during scientific change.¹⁶ Nevertheless Putnam's account is deficient in a different way since, as we have seen, it cannot accommodate any shift of reference. This theory falls

¹⁶ cf. Feyerabend's radical statements about conceptual change which we have dealt with above (ch. VI).

down on the question of scientific change.¹⁷ Let me indicate why.

Kuhn suggests as an example of a shift in reference a consequence of Dalton's atomic theory. He remarks that

it implied a new view of chemical combination with the result that the line separating the referents of the terms "mixture" and "compound" shifted; alloys were compounds before Dalton, mixtures after.

(Kuhn, 1970, p. 269)

There are two things, it seems, that Putnam could say about the term "compounds": either he could maintain that alloys, being part of the extension of the introductory use of "compounds", will always be part of the extension; or he might somehow account for the possibility of mistakes and say that what was referred to in this introductory use did not include the alloys. The former alternative is absurd on any account. The latter, too, which incorporates a progressive view of science, leads to absurdity: it is possible that our present use of "compounds" is still mistaken and that developments in chemical theory might again remove part of its extension. We must surely allow for the possibility of shifts in reference, for if not, we would be forced into admitting that we can never know all that we are referring to when using any scientific term.

¹⁷ This is an objection raised by Fine (1975). Putnam (1973) does seem to note difficulties with Kripke's view in this regard (p. 207), but nevertheless does not show how to account for some clear cases of shift in reference such as the one dealt with below in the text.

On the contrary it would seem that we sometimes can, if we know enough about the term we are using and to which objects it refers and which not, know all that we are referring to by using it, and furthermore that there can be shifts in the reference of terms. Kuhn's example supports this claim. The crucial question concerns the extent to which such shifts of reference do actually occur in the history of science, and what effect this has both on an understanding of the meanings of terms and the ways in which they might change, and on the issue of the relationship between alternative theories. Assuming that there are such shifts, a Kripkean theory does not seem particularly useful for the study of scientific terms.

I suggested earlier the potential suitability of a modification of Frege's view along Wittgensteinian lines for an examination of scientific terms. Frege's original account is unsatisfactory because of its rigidity. It demands that there must be a single criterion of identification for a proper name (and any singular term) which an object must possess in order to be a referent of the term. In other words, it seems to require the existence of a "defining characteristic" of an object or class of objects. Although there is generally a single characteristic by virtue of which objects are grouped together

under one name, this is by no means universal.¹⁸ We need to accommodate the fact that the meaning of a term can change, in particular that it can be modified, with or without an accompanying change in reference, with the advent of added information about an object or class of objects. The aspect of Frege's theory which must not be lost sight of is that it does forge a link between meaning and reference in a way that is important for questions about scientific terms. Our ability to apply a term correctly reflects some grasp of its meaning.

The idea that there is a cluster of criteria associated with any term, not all of which are necessary, and of which only a subset need be sufficient for its correct application to an object, seems to be a particularly valuable one. If it can be shown to be an accurate account of referring words, this will have several important consequences for questions about scientific change. In the first instance, this would allow for the idea of degrees of meaning change which could be calculated in terms of the number of criteria that happen to change from one usage to another. These changes may be more or less significant for the notion of an incommensurability thesis. Second, it gives rise to the idea that people need not grasp all the criteria associated with a term in order to be able to apply it correctly. This could lead to the

¹⁸ This is more prevalent in the biological sciences. Putnam (1973) describes one such instance: fish are characterized as living under water and breathing through gills; however lungfish, which are classified as fish for scientific purposes, do not possess these properties.

situation whereby two people need not use the same criteria for their application of a particular word and could, in one sense, be said to mean something different by it. However, this type of meaning variance does not necessarily imply that they are talking about different things, or that they cannot compare their respective statements about a particular object. Third, our present idea should enable us to draw some conclusions about degrees of theory-ladenness which in turn might facilitate our evaluation of the incommensurability thesis.

A full study of these issues would be extremely complex, since not all terms are equal in respect of criteria of application, ease of understanding, and so on; a proper analysis would require an examination of very many kinds of terms. Rather than do this, I shall focus my attention on two questions which are of crucial importance to the issue of meaning variance: what constitutes the meaning of a scientific term? Second, what are the criteria of meaning change?

The first task is to isolate the kinds of terms which will facilitate a suitable answer to these questions. Feyerabend's chief examples in support of his meaning variance thesis are spatiotemporal terms featuring in Newtonian and Einsteinian physics, in particular terms like "mass", "length" and "time duration", and also terms from thermodynamics, such as "temperature". It will be noted that these are all physical magnitude terms and do not exhaust the range of scientific terms. Another

important category of terms, of which Kuhn's example of "compounds" is one, is that class of terms used predicatively to describe an entity. This includes terms such as "electron", "atom", "acid"; Feyerabend does not consider this category. An examination of selected terms from both these categories will be sufficient for our purposes, as this will serve to cover most of the important issues.

We can now turn to a consideration of the first question: what constitutes the meaning of a scientific term? The meaning of a scientific term can be seen to bear a close relationship to features and properties possessed by the entity, process, or phenomenon designated by that term, for it is in recognition of relevant properties that we can know when to apply it. Moreover, the meaning of a scientific term, or its definition, is usually given in terms of the properties of its referent or extension. Thus it is important to understand the relationship between a term and the properties of whatever it designates. This relationship will differ for different categories of terms.

Peter Achinstein (1968, ch. 1) has suggested three basic semantical categories to describe the possible relationships between terms and the properties of their referents. Two of these are unproblematic, but the third requires some analysis if we are to be able to answer the second question about criteria of meaning change.

The first two categories in Achinstein's account have

limited application: they are pertinent to terms which I would like to describe as showing a high degree of theory dependence, that is, ones that are completely dependent on the principles of the theory in which they occur. These categories Achinstein terms "logical necessity" and "logical sufficiency".¹⁹ If a property *p* is logically necessary for something's being an *x*, then anything which lacks *p* cannot be an *x*. If a property *q* is logically sufficient for something's being an *x*, then anything which possesses *q* is correctly classifiable as an *x*, no matter what other properties it has. Terms like "Newtonian system", "Bohr atom", and others clearly possess both logically necessary and logically sufficient properties merely because they are defined with specific reference to particular principles of the theory in which they appear.

The first question, then, can be answered quite simply for these terms: the meaning of a term is constituted by a description of those properties which are logically necessary and/or logically sufficient for the referent of the term. The second question, too, can be answered:

¹⁹ Achinstein uses the prefix "logical", it seems, to include amongst these only properties which an object is said to have in virtue of the principles of a theory. Not all predicative terms can be said to possess either of these properties as there are, for many of them, no direct link with the principles of the theory in terms of which they are defined. Satisfactory definitions can often be provided in these cases without any recourse to the principles of the theory: these definitions are not logically necessary or sufficient, since they may admit of alteration without contravening basic principles of the theory.

a change in any logically necessary or sufficient property associated with a term constitutes a definite change in its meaning.

It is not surprising that we have encountered so little problem with these terms. Our analysis of them would be suited to both traditional and newer empiricist means of dealing with them. Moreover, they are not of particular relevance for our present task; since they are strictly theoretical terms, the criteria for change of meaning would be acceptable on traditional empiricist grounds. We can safely ignore these terms for they present no problem.

Our difficulties arise when we turn to examples from the two categories of terms - physical magnitude terms and predicate terms - that were mentioned above (pp. 94-95). It is these terms for which a modification of the Wittgensteinian "cluster of descriptions" approach will be appropriate for here there are generally no clear properties which are thought to be of either the logically necessary or logically sufficient type. In other words we are saying that there are actual or possible situations in which an object not possessing property p , which is a property normally associated with a term t , might still be correctly classifiable under that term. Putnam's example of "fish" (see above p. 93, n. 18) instantiates this phenomenon. The problem is how to characterize the relationship between these terms and the properties possessed by their referents in order to provide answers to the two

questions above.

Achinstein has suggested the term "relevance" to apply to properties of such terms. Although I am not entirely in agreement with his analysis of the meanings of these terms, I nevertheless adopt the term "relevance" because of its potential advantages in securing an adaptation of Wittgenstein's approach to the meanings of terms. A large number of scientific terms possess, in differing degrees, a network of properties associated with each; in some cases there is a necessary property, or a set of properties whose disjunction is necessary, but there is no specifiable set of sufficient properties. In such cases it is appropriate to talk of properties as being relevant for the correct application of a term:

if an item is known to possess certain properties and lack others, the fact that the item possesses (or lacks) the property in question normally will count, at least to some extent, in favour of (or against) concluding that it is [classifiable under a particular concept].

(Achinstein, 1968, p. 6)

The notion of relevance will now enable us to answer the first question without much difficulty. One advantage is that it permits the idea of degrees of relevance amongst the different properties associated with any one particular term. We can make use of a further feature of Achinstein's account which distinguishes those properties which are "semantically relevant" for the application of a term from those that are not. These might be described as primary properties, ones which are not derivable from other properties of the objects

in question.²⁰ The meaning of a term can then be said most simply to be constituted by a cluster of descriptions expressing the more important semantically relevant properties associated with it. This accounts for the phenomenon that many scientific terms do not have an explicit definition but are understood, as Wittgenstein has shown, by listing the various characteristics that its referent is normally thought to possess. It is thus a mistake to think of the meaning of a scientific term in general to be a rigid definition. On the contrary the meaning of a term should be thought of instead as a description of a flexible set of properties attributed to its referent.

In attempting to answer the second question we must take certain phenomena into account. One is that in the transition from one theory to another a large part of the vocabulary is carried across from the one to the other. We must decide how this factor may affect a meaning variance thesis. Another is that the scientific vocabulary may be partially understood by one who has very little grasp of the physical principles utilizing that vocabulary. We need to see how this, too, affects our conclusions

²⁰ For example, solubility is a property of many chemical compounds. The fact that a compound is soluble, however, indicates something about its chemical composition; that is, the property of solubility is now known to be a consequence of a certain chemical structure. Solubility is therefore said to be a less semantically relevant property than composition.

about meaning variance and incommensurability. On the basis of our selected criteria for meaning change we must also decide whether there are degrees of meaning change, and if so what consequences this has for Feyerabend's thesis.

One needs to distinguish between an actual change in the meaning of a term, and an extension in its meaning. Terms are often given new meaning in the form of a refinement to their existing one. For example, chemical compounds were defined by a variety of properties such as taste, colours, and so on, until developments in atomic theory led to an extension of the existing meaning by describing the chemical structure. We would want to exclude such cases as instances of change in meaning since the terms are used in very much the same way from theory to theory.

We can say that a change in meaning has occurred if one or more significant semantically relevant properties associated with the term in question is removed or replaced by another.²¹ An example of change in meaning occurred in the development of the periodic table of the elements. Mendeleev originally ordered elements according to their atomic weight. Although this yielded a number of correct predictions, this ordering contained some anomalies, and

²¹ My account differs in places from Achinstein's which does not appear to account for degrees of meaning change. See Achinstein (1968), chs. 1 and 3. This account requires further explication, but its present purpose is to characterize as simply as possible the area of meaning change.

it was not until further discoveries about atomic structure that weight was discarded as a necessary property for ordering elements. The new property was atomic number, which succeeded in accounting for the "anomaly" created by the existence of isotopes.

Change will be more or less radical according to both the number of altered properties and their degree of relevance. We may say that a radical change in meaning, of the sort Feyerabend seems to be suggesting, occurs when virtually all semantically relevant properties are altered. Obviously not all changes in meaning are radical ones, and how we choose to view so-called cases of meaning variance in science will influence our decision as to whether alternative theories are incommensurable.

We may safely adopt a meaning variance thesis if only to agree that in most cases of scientific change alterations in meaning do occur. However, this need not force us into an acceptance of the incommensurability thesis for there may be ways of characterizing the relationship between alternative theories without being solely dependent on the fact that the meanings of their respective terms have undergone change. It may appear a little awkward to characterize as changed in meaning a term of whose referent very few semantically relevant properties have been altered. There is no means of drawing a sharp

boundary line between (degrees of) sameness in meaning and change in meaning, as this will vary from case to case. However, I am not concerned so much about how we describe individual cases as I am to point out the defects of Feyerabend's claims for radical meaning variance.

CHAPTER VII

We are now in a position to consider Feyerabend's arguments for adopting a position of radical meaning variance, and his inference that alternative theories are incommensurable. This concludes discussion on the latter part of the fourth thesis in our reconstruction of Feyerabend's argument; it also takes care of the tenth and eleventh.

The most useful starting point for such an examination is a set of quotations representative of Feyerabend's position. As his thesis is intended to be supported primarily by historical evidence, some of his supporting examples will be considered.

It will be recalled (ch. III above) that Feyerabend has put forward the thesis of meaning variance in opposition to what he maintains to be a principle of traditional empiricism, namely that

meanings will have to be invariant with respect to scientific progress; that is, all future theories will have to be phrased in such a manner that their use in explanations does not affect what is said¹ by the theories or factual reports to be explained.

(Feyerabend, 1963, p. 18)

That this principle is inadequate can be shown by an

¹ See also Feyerabend (1965), p. 164. In justification of his statement, Feyerabend (1962, p. 33) quotes Nagel (1960, p. 301): "It is of the utmost importance to observe that the expressions peculiar to a science will possess meanings that are fixed by its own procedures and are therefore intelligible in terms of its own rules of usage, whether or not the science has been, or will be, reduced to some other discipline."

examination of major changes in scientific theory of precisely the same nature as those intended to support the faulty empiricist theory of explanation.

What happens here when transition is made from a theory T' to a wider theory T . . . is something much more radical than incorporation of the unchanged theory T' (unchanged, that is, with respect to the meanings of its main descriptive terms as well as to the meanings of the terms of its observation language) into the context of T. What does happen is, rather, a complete replacement of the ontology . . . of T' by the ontology . . . of T and a corresponding change of the meanings of the descriptive elements of the formalism of T' This replacement affects not only the theoretical terms of T' but also at least some of the observational terms which occurred in its test statements In short: introducing a new theory involves change of outlook both with respect to the observable and with respect to the unobservable features of the world, and corresponding changes in the meanings of even the most "fundamental" terms of the language employed.

(Feyerabend, 1962, pp. 28-29)

It must first be made quite clear just what Feyerabend is asserting, and in particular, what terms he is referring to in his claim that empiricism is mistaken. Feyerabend notes that "most empiricists would admit that the meaning of theoretical terms may be changed in the course of scientific progress" (Feyerabend, 1965, p. 170).² We can assume that Feyerabend wishes to attack the principle calling for the stability of meaning of so-called observational terms, for any stability of meaning would provide the kind of common core between theories whose existence

² This claim is correct. For example see Kordig (1971), p. 35, and Nagel (1961), p. 87. However, Feyerabend (1962, p. 34) seems to have an alternative interpretation of the meaning invariance principle which stipulates that all "descriptive terms" remain invariant in meaning with respect to scientific progress.

he wishes to deny.

Evidence for our assumption is provided by the following: "This argument [against the demand for meaning invariance] is quite general and independent of whether the terms whose meaning is under investigation are observational or not" (Feyerabend, 1965, p. 170). Any belief that meanings do not always change with the theory to which they belong "seems to be refuted by the existence of pairs of theories that may be regarded as competitors and yet do not share any element of meaning" (Feyerabend, 1965b, p. 267, emphasis mine). To exemplify this claim Feyerabend (1965b) proposes the competing theories classical celestial mechanics and the general theory of relativity. In cases such as this "the meanings of all descriptive terms of the two theories, primitive as well as defined terms, will be different" (Feyerabend, 1965a, p. 231).

The attack on the meaning invariance of observational terms is closely linked with Feyerabend's rejection of a distinction between theoretical and observational terms, the third of the twelve premises for the incommensurability thesis.³ Nevertheless, there is a wide gap between a verificationist theory of meaning which stipulates a logical connection between the meanings of certain terms and the fact of their observability, and Feyerabend's alternative which envisages wholesale changes in the meanings of all

³ The reader is referred to ch. III above for a discussion of this thesis.

terms in the transition between certain theories. The possibility of alternative accounts of scientific change must be borne in mind when evaluating Feyerabend's claims.

The chief difficulty with a theory that allows for such drastic changes in meaning seems to be that it overlooks the possibility of admitting something between absolute synonymy and radical meaning change. This, I believe, is the major fault with Feyerabend's position, and to show this, we must look to some of the consequences of his radical meaning variance thesis in order to determine whether any of these can be avoided.

The following are some important implications of the view Feyerabend adopts:

1. Scientific terms are theory-dependent and cannot be understood in isolation from the theory in which they occur. Thus the understanding of a term requires an understanding of the basic principles of the theory.⁴

2. The meaning of a term occurring in a given theory will change if that theory is changed or replaced by another (thesis 10).

3. Change in theory implies a change in the statements constituting the original theory; alternative theories cannot share any statements (thesis 11).

4. Neither the terms nor the statements within a theory can be compared in any relevant way with those of an alternative theory; the two theories are thus

⁴ Achinstein (1964) and Kordig (1971) present this thesis as an adjunct to a doctrine of meaning variance.

incommensurable (the incommensurability thesis).

Let us look at each of these in turn. As it is phrased, (1) is unclear and requires a thorough examination. We need to clarify three issues: first, what it means for a term to be theory-dependent; second, which particular terms in a scientist's vocabulary are theory-laden; third, what kind of understanding is required in order that a speaker may be said to be able to use the terms correctly. I will not deal with these issues separately as they tend largely to overlap.

It is obviously correct to say that the meaning of a descriptive term depends on the context in which it is used, firstly because each term is governed by a convention determining the situations in which it is applicable; secondly because a word can be used for different purposes in different contexts. But this is clearly not what is meant by the claim that terms are theory-laden. An example illustrates this: Ryle (1954) distinguishes, within the field of games, between a term like "Queen of Hearts" and "trump card". The former could be understood and used by those without any knowledge of particular card games, because its referent can be identified independently of any game. "Trump card", on the other hand, cannot be understood without, according to Ryle, learning at least the rudiments of Bridge. "Trump card" could therefore be described as a "Bridge-laden" term, not so "Queen of Hearts"; or rather, if one

wishes to distinguish between degrees of theory-ladenness, "Queen of Hearts" is a less laden term than "trump card". This affords us some criterion of theory-ladenness: a term occurring in a theory which is not theory-laden (or has a very small degree of theory-ladenness) can be used correctly by people who have no understanding of even the basic principles of that theory; a term that is theory-laden will be mishandled by speakers with no understanding of the theory.

Feyerabend subscribes to a strong version of the thesis that terms are theory-laden, for in his eyes theory-ladenness permeates a large portion of scientific vocabulary. An example he gives is the term "temperature" which we would not normally consider to be theory dependent, partly because it occurs in so many nontheoretical contexts. According to Feyerabend, anyone who uses "temperature" to mean "mean kinetic energy of molecular motion" has understood the theory of the molecular construction of matter.⁵

If so much of the scientist's vocabulary is theory-laden as Feyerabend suspects then learning a new theory would be tantamount to learning a new language, not, as is usually the case with a second language, by correlating sentences and terms from the two languages, but learning

⁵ Feyerabend does accept that the layman can still use the word "temperature" without understanding the molecular theory; however, it must be made clear that he "may possess a concept of temperature that is very different from the one connected with the molecular theory" (Feyerabend, 1962, p. 83). The question is, how different?

it from scratch as a child learns his first language. This is because the only way in which one can understand a term is by understanding the principles of the theory in which it features. This description of theory-learning seems to run counter to evidence we have that scientific theories are taught in terms of something already familiar that is part of our everyday language.

One might interpret this demand differently: the meanings of terms may be dependent on the theory in which they occur, but we may be able to grasp part of the meaning of particular terms without first having to learn the principles of the theory. Learning "temperature" in terms of being able to manipulate a mercury thermometer would enable us to use the word correctly in many situations. Should we then turn to a study of the kinetic theory of gases, we can use our present understanding of "temperature" to enable us to grasp both the kinetic theory and any new meaning "temperature" has acquired in this theory. In this way we avoid the undesirable conclusions of the preceding paragraph while at the same time still acknowledging a sense in which terms are theory-laden.

However, this interpretation is too moderate. Feyerabend (1962) explicitly states that there are two different concepts involved in the above example: the "primitive" (pre-molecular) one, and the kinetic molecular theory concept; "what is denied is that anybody can consistently continue using this more primitive concept and at the same time believe in the molecular theory" (p. 83). Thus he

would reject the argument that we can use our prior meaning of a term when learning a new theory in which the term occurs, for the same term expresses "concepts which belong to different and incommensurable frameworks" (p. 83).⁶

It is clear that the above view supports an incommensurability thesis; however it needs some examination. We need to ask whether in fact two "incommensurable concepts" are involved in this particular example and in order to settle this we need a satisfactory criterion of identity for concepts. The man who is learning molecular theory will no doubt be perplexed when he discovers that individual molecules cannot be said to possess a temperature, even though "temperature" is defined as "mean kinetic energy of molecules", for, according to his prior understanding of the word, it refers to individual bodies. So will the man who learns that his desk is not a solid, but a vibrating mass of molecules, that it consists more of empty space than of matter. But is our explanation of this to be in terms of "different and incommensurable concepts", or is there a way of explaining the perplexity by showing that statements in everyday language and in kinetic molecular theory are not really rival and incommensurable ones, but are rather suited to different purposes? The latter is compatible with a less radical interpretation

⁶ Feyerabend accepts that one person may use concepts belonging to different frameworks, but he denies that he can use both kinds in the same argument.

of theory-ladenness; it does not necessitate that we understand a theory before we can be said to understand any of its descriptive terms, nor that the same concept cannot feature in very different contexts. I shall attempt to provide support for this position.

I will distinguish, at a fundamental level, two general types of scientific terms. The first type consists of terms such as "temperature", "mass", "force", and so on, which have permeated our ordinary language either because of their prominence in accredited scientific theories, or because they are part of a world-view which this language has described for many years. Although these terms are part of our everyday vocabulary their use is normally untechnical compared with their use in specific sciences. In these cases, at least part of the meaning of a term may be learnt without reference to the principles of any theory, although a complete understanding may be said to be available only to those who were well acquainted with the theory in which it occurred.

The second group of terms includes those which do occur normally in everyday discourse although they feature in competing scientific theories. I include such terms as "electron", "alpha particle", "light wave", and so on. Clearly these terms are theory-laden, for one is usually introduced to them via the principles of a theory, and they are used meaningfully most often solely in theoretical discourse. The fact that these terms are generally shared by conflicting theories is significant. Without begging

any issues about the comparability of theories on the basis of their shared terms, I would at this stage like to suggest that this fact raises doubts about Feyerabend's claim that "different and incommensurable concepts" are employed in different theories.

In stating that terms are theory-laden and that understanding a term requires understanding some basic principles of the theory in which it occurs, Feyerabend has glossed over two important issues. I shall use the above distinction first to deal with these issues and then to throw doubt upon Feyerabend's radical claims.

The first task is to provide a criterion of identity for concepts. This is a problem carried over from chapter V where it was indicated that problems involving conceptual change would be translated into problems about meaning change. The connection is fairly clear. Feyerabend presumably wants to say that the meanings of the term "temperature" as it occurs in premolecular discourse and in statements in kinetic molecular theory are very different (incommensurably different), hence there are two concepts of temperature. Since Feyerabend has placed so much emphasis on it, we can dwell on this example. That the meaning of the term "temperature" has changed is undoubted; in terms of our criterion of meaning change one important semantically relevant property has changed: the term no longer refers to individual bodies (molecules) whereas in its premolecular use it did. That the meaning has changed radically is not at all clear for there are many

semantically relevant properties common to the two uses. A degree of meaning change is not sufficient to result in conceptual change and it is dubious that two different concepts of temperature are involved here. Some significant common meaning is retained and this seems sufficient to allow that the concepts expressed by the term are identical. This will be of some relevance in considering the second thesis concerned with meaning change.

The second task is to explain the way in which theoretical concepts and the meanings of theoretical terms come to be understood. This is a fundamental problem for philosophy of science⁷ and as any attempt to provide an answer would involve an unnecessary digression at this point I shall merely spell out the framework of an account.

One incapsulating explanation will be unsatisfactory because of the very diverse range of scientific terms of which only one level of distinction has been suggested. Although we may concede that many descriptive terms are theory-laden in some sense, we still require a notion of degrees of theory-dependence for which Feyerabend has not accounted.⁸ The degree of theory-ladenness which a term exhibits will undoubtedly influence the ways in which principles of a theory might be necessary in order

⁷ For example, we can consider the failure of traditional empiricism to provide any clear account of the role of correspondence rules in providing theoretical terms with a meaning.

⁸ Hanson (1958), chs. 3 and 4, admits such a distinction, and thus offers a more reasonable account of the notion of theory-dependence.

to understand its meaning. A term like "Bohr atom", for example, cannot be defined without reference to one of the postulates of the Bohr theory. On the other hand, there are terms such as "atom", or "light wave", about whose meaning much can appear to be understood without reference to any particular atomic theory or theory of light. For example we can know independently of any theory that atoms are microscopically small particles whose structure determines features of the entity of which it forms a part. This leads us to a crucial issue: how is it that we can appear to understand at least part of the meaning of a scientific term without necessarily having recourse to the theory in which it occurs?

There have been several attempts to account for this, and most of these centre around the notion that we learn something new by means of relating it to something familiar. This applies to both groups of terms mentioned above. Terms that feature in both everyday and scientific discourse are more easily understood in the latter field of discourse if they are compared with their non-technical use. In the second group of terms, namely those used chiefly in scientific discovery, we can distinguish between those intentionally coined like "meson" and "electron", and those which are more suitably described as metaphorical, such as "light wave" and "black hole". While both may be said to be theory-laden, the second can be partially understood without reference to a theory because of their metaphoric links to concepts which we have already grasped.

Notions such as those of a model, a metaphor and an analogy are often employed to explain our understanding of scientific terms.⁹ Clearly metaphors provide an important way of understanding terms like "light wave": we are intended to understand that light travels (again a metaphor) in a continuous wave-like motion; each beam of light is made up of many such waves. With newly-coined terms such as "atom" and "electron" models play a crucial role in explaining features of the entities to which these terms refer as simply as possible. I shall discuss briefly some relevant features of models.

At a primitive level models are physical or spatial representations (diagrammatic) of the structure of a theory or its constituents. For example, the molecules of a certain chemical substance may be given a physical representation; in this way we are able to represent the double helix structure of the D.N.A. molecule by a model. These constructs act as important visual aids: they facilitate our understanding of the constituents of a theory, and our manipulation of certain ideas. The importance of these visual aids can be seen in fields such as that of relativity theory where many of the concepts employed are no longer three-dimensional. While an adequate grasp of the meanings of many of its terms may depend on some acquaintance with the principles of relativity theory,

⁹ For example consult the works of Quine (1960), Nagel (1961), Black (1962), Braithwaite (1962), Hesse (1966) and MacCormac (1971).

notions such as curvature of space may be understood with the aid of a physical model: a sphere represents positive curvature of space, while a saddle-shaped construct represents negative curvature.

Models may be neither necessary nor sufficient for a full understanding of a theory and its terms, but they play a prominent role in science. Along with other types of analogies, generally of the verbal sort, models provide a viable account of the way in which we come to understand the meanings of theoretical and other scientific terms. Their important feature is that of explaining something new in terms of something familiar, in a way that is not entirely dependent on an understanding of the particular theory in which the explained terms appear.

The question now is whether Feyerabend would accept this account which in some sense leads to a destruction of his thesis of radical theory-ladenness. As he does not explicitly discuss the issue of understanding the meaning of a scientific term it is difficult to say whether his view is in direct contrast with the one I have suggested above. We have only his claims that different concepts are employed in different theories (the example of "temperature") and his thesis, to be discussed next, that the meanings of terms change from theory to theory. We still have to show that meaning change is not so radical as Feyerabend supposes it to be; if we cannot, then the above account is thrown into doubt since it depends on some constancy and comparability of meanings. If our objections

to radical meaning variance are sound, on the other hand, then I should like to contend that thesis (1), which we have been discussing, cannot be upheld, except in a more moderate form that terms of a theory are more or less theory-laden but not, generally, completely dependent on the theory for their meaning.

Although terms may be theory-laden this does not preclude them from being shared, with the same meaning, by different theories, even incompatible ones. Thus thesis (2) requires independent support. Feyerabend appears to acknowledge this by allowing for the occurrence of certain types of theory change without there being a corresponding change in the meanings of the main descriptive terms.¹⁰

The manner in which Feyerabend has chosen to support this second thesis proves it to be, in my opinion, untenable. While we have acknowledged that some form of meaning variance does undoubtedly occur in periods of scientific change, radical meaning variance, which renders theories incommensurable with one another, seems to be both an inadequate description of what takes place during scientific change, and a philosophically untenable thesis.

An explicit statement of Feyerabend's views on meaning and meaning change appears only once and I shall quote this in full:

¹⁰ Feyerabend (1965b) provides as an example two theories - classical mechanics and one just like it except for a slight change in the strength of the gravitational potential - which disagree without there being a change in the meanings of their terms.

. . . a diagnosis of stability of meaning involves two elements. First, reference is made to rules according to which objects or events are collected into classes. We may say that such rules determine concepts or kinds of objects. Secondly, it is found that the changes brought about by a new point of view occur within the extension of these classes and, therefore, leave the concepts unchanged. Conversely, we shall diagnose a change of meaning either if a new theory entails that all the concepts of the preceding theory have extension zero or if it introduces rules which cannot be interpreted as attributing specific properties to objects within already existing classes, but which changes the system of classes itself.

(Feyerabend, 1965b, p. 268)

Although Feyerabend does not say as much, it appears that he perceives either stability of meaning or change of meaning, and if the latter, then radical change of meaning leading to incommensurability.

In the light of what has been said in the preceding chapter on meaning and meaning change, this account is unsatisfactorily simplistic. There are, in addition, several difficulties contained in the above quotation. One difficulty, pointed out by Shapere (1966), is that the "rules" for collecting objects into classes could vary extensively within the bounds of a single theory. If our class-descriptions are made sufficiently general so as to include, for example, "physical objects", then any two physical theories will involve the same "system of classes" and their difference would lie merely in the way they attributed properties to objects within those classes. Conversely, by making our descriptions sufficiently specific, no two theories could be said to retain a common system of classes. We seem, therefore, to require an added criterion by means of which it would be possible to

distinguish trivial cases of scientific change from the more interesting ones with which we are really concerned.

Feyerabend has noted the ambiguities in his definition. He suggests that to avoid this it is possible to adopt a certain notion of "interpretation" of theories; this would prevent the type of situation, noted by Shapere, which would preclude the possibility of two theories being incommensurable (Feyerabend, 1965b, p. 268: the decision to reject Platonism). However, there is no justification given for his decision "not to pay attention to any prima facie similarities that might arise at the observational level" (p. 270). There seems little reason not to pay attention to such similarities, for these might afford the very basis of comparison between theories that we are looking for. Since Feyerabend implies, on the other hand, that he can give reason for his decision, let us not judge the issue of the comparability of theories on this point, but turn to more problematic aspects of his criterion of meaning change.

There are two conditions, on Feyerabend's account, for change of meaning. We shall examine them individually. The first is the case where a new theory entails that all the concepts of the preceding theory have extension zero. The first difficulty that arises is how we are to ascertain that a concept of an older theory has no extension in a newer theory without there being some means for us to compare the two theories via terms with common meanings. Surely the reason for offering this notion of meaning

variance is to support the conclusion that two theories are incommensurable; yet they cannot be incommensurable if our sole means of deciding whether or not the meanings of terms have changed is by comparing the theories with respect to a set of common meanings. As it stands, Feyerabend's first criterion cannot be tenable, since it demands that each concept be carried over to a new theory in order to determine whether or not a change of meaning has occurred. A second difficulty is that even if this were a workable criterion, it still would not provide a sufficient condition, as it is intended to, for meaning change. It is empirically possible that a particular concept does not have an extension; this would be true if at a certain time nothing satisfied that concept. Yet this empirical fact does not imply that there is a change of meaning involved.¹¹

The second criterion, which refers to a change in the system of classes, is, I think, more tenable, though for reasons different to Feyerabend's. As this criterion stands, it does not necessitate meaning change of the radical sort envisaged by Feyerabend; it could be interpreted in such a way as to allow for modifications from theory to theory that would render the term "incommensurable" inappropriate as a description of the relationship

¹¹ For example, the phrase "smallest positive number" has a referent in the integral number system - namely 1 - yet it has no extension in the rational number system. We would surely not want to say that the phrase has a radically different meaning in both theories.

between the two theories. In other words, not all the concepts need be changed.

However, Feyerabend would want to interpret his criterion differently. His own example - the transition from classical mechanics (theory T) to the general theory of relativity (theory T') - is claimed to involve a change in meaning of all spatio-temporal notions. This, Feyerabend maintains, "is drastic enough to exclude the possibility of common elements of meaning between T and T' " (Feyerabend, 1965b, p. 270).¹²

This last conclusion seems quite unwarranted, even if one does exclude similarities that arise at "the observational level". In explaining how "distance" differs in T and T', Feyerabend employs phrases such as "gravitational fields" and "motion of the observer". "Distance_T" is defined without recourse to these expressions; "distance_{T'}" is defined as being dependent upon the motion of the observer and the present gravitational field. Similarly, discussions of "mass" in the two theories show the differences in meaning in terms of common terms such as "velocity" and "energy". If these expressions are

¹² For his justification of this conclusion, see Feyerabend (1965b), pp. 269-270. A good discussion of this is provided by Fine (1971), pp. 233-235. (Kordig (1971, pp. 42-47) appears to owe an unacknowledged debt to Fine). Fine's objections are that Feyerabend has dealt with only one concept (space); that he has not used the criterion he gives; and that his argument is inconclusive in that a different interpretation, permitting a common element of meaning, would be consistent with historical evidence.

intended to provide a basis for understanding the differences in meaning between "distance" (or "mass") in T and in T', then the expressions must themselves have a common element of meaning in both theories. Thus the conclusion that there is no common element of meaning between T and T' is unwarranted.

It would appear that Feyerabend cannot support his claim for radical meaning variance on logical grounds. This does not mean, however, that there is no meaning variance at the more observational level, nor that Feyerabend's second criterion for change of meaning is not useful. I propose to consider meaning changes in the manner suggested in the previous chapter, that is, in terms of changes in semantically relevant properties of the referent of a given term. It is important to bear in mind a consideration for which Feyerabend makes no allowance: that a term may acquire a new meaning while retaining much of its old meaning, that meaning change is generally a matter of degree rather than radical change.

Feyerabend (1963) contends that "the relativistic concept and the classical concept of mass are very different" (p. 21); that any attempt to give a relational analysis of the classical concept does not lead "to the relativistic idea with its velocity dependence on the co-ordinate system" (p. 21); "we have to conclude, then, that ['mass' in classical mechanics] and ['mass' in relativity theory] mean very different things" (p. 21).

Clearly there is a change of meaning in "mass"

involved in the transition between the two theories; but this does not mean that there are two distinct and incommensurable concepts: classical mass and relativistic mass. It is true to say that within classical mechanics mass is a property of objects whereas in relativity theory mass is a relation between an object and a co-ordinate system. It is also true that mass can be converted into energy in relativistic mechanics whereas it cannot in classical mechanics. Again it is true that in classical mechanics the mass of an aggregate of parts is equal to the sum of the masses of its individual parts whereas this does not hold in relativity theory. We thus have a number of semantically relevant properties which have changed in the case of "mass" in the transition from classical to relativistic mechanics. Nevertheless, there are some properties, unacknowledged by Feyerabend, which do not change. For example, "mass" can still be characterized in relativity theory as "the ratio of force to acceleration". This characterization is sufficiently significant to be expressed as part of the definition of the term, and we can conclude that although there has been a change in the meaning of "mass" this change has not resulted in the existence of two incommensurable concepts.

Further examples could be cited, all of which are used by Feyerabend in his argument for meaning variance and incommensurability, which show that the change in meaning is not as radical as he envisages. This does

not preclude the possibility of there being incommensurable concepts. However, it does prevent Feyerabend from drawing the conclusions he wishes with regard to scientific change. Furthermore, it provides some support for the more moderate form of theory-ladenness that was offered as an alternative to Feyerabend's radical version (see above pp. 116-117).

I turn now to thesis (3) that alternative theories cannot share any statements. This can be dealt with briefly. In the course of discussing the transition from classical mechanics to relativity theory, Feyerabend makes the following comment:

The new conceptual system that arises from relativity theory does not just deny the existence of classical states of affairs, it does not even permit us to formulate statements expressing such states of affairs. It does not, and cannot, share a single statement with its predecessor.

(Feyerabend, 1975, pp. 275-276)

However, we have just seen that this is not true. That mass has a common property in both theories can be expressed by a statement common to both theories: "mass is the ratio of force to acceleration". This statement is true in both theories and has the same meaning in both theories, since its constituent terms have the same meaning in both theories. There will be other such statements containing terms like "velocity", "distance", and so on, and yet others, which Feyerabend has chosen to ignore, that are statements of

everyday observable situations.¹³ Thus thesis (3) does not hold, even if we comply with Feyerabend and exclude statements expressing observable situations.

I turn finally to thesis (4) and the apparent paradox it is said to engender. This is the thesis of incommensurability: "there exist scientific theories which are mutually incommensurable though they apparently deal 'with the same subject matter' " (Feyerabend, 1975, p. 274). It has been pointed out¹⁴ that if, as Feyerabend maintains, radical meaning changes do take place during theoretical transitions, certain undesirable consequences follow: these may be summed up by saying that no two theories could then agree or disagree, be compatible or incompatible with one another, be shown to be consistent or inconsistent with one another; in short there could be no sense in which we could speak of two theories as being rivals or alternatives, for there could be no grounds for calling them "alternatives" if there existed no basis for their comparison and a choice between them.

Feyerabend's answer to this type of criticism is to introduce a notion which he claims to be methodologically desirable as well as providing for an accurate historical interpretation of scientific development: the

¹³ See above (p. 119). Note that these statements are not just typographically similar; what is claimed is that there are statements common to different theories which retain the same meaning.

¹⁴ For example by Giedymin (1970), Achinstein (1964, 1968), Fine (1967), Kordig (1971).

thesis that theories are incommensurable. Incommensurability is explained along the same lines along which meaning change was explained, namely in terms of change of concepts.

We have a point of view . . . whose elements (concepts, 'facts', pictures) are built up in accordance with certain principles of construction. The principles involve something like a 'closure': there are things that cannot be said, or 'discovered', without violating the principles (which does not mean contradicting them). Now take those constructive principles that underlie every element of the cosmos Let us call such principles universal principles of the theory in question. Suspending universal principles means suspending all facts and all concepts. Finally, let us call a discovery, or a statement, or an attitude incommensurable with the cosmos . . . if it suspends some of its universal principles.

(Feyerabend, 1975, p. 269)

The difficulty here parallels the difficulty with the account of meaning change: what are our criteria for determining whether or not a violation of the principles has occurred? If the answer is that the principles have been violated if all concepts are suspended then again we need a criterion for suspension of concepts which in turn would seem to require some common element for comparison of the two theories. How does Feyerabend attempt to explicate the notion of incommensurability? A little further on he writes that

it is no use trying to connect classical statements with relativistic statements by an empirical hypothesis . . . [A] hypothesis of this kind cannot even be formulated. Using classical terms we assume a universal principle that is suspended by relativity which means it is suspended whenever we write down a sentence with the intention to express a relativistic state of affairs. Using classical terms and relativistic terms in the same statement we both use and suspend certain universal principles which

is another way of saying that such statements do not exist: the case of relativity vs. classical mechanics is an example of two incommensurable frameworks.

(Feyerabend, 1975, p. 276)

This is surely too extreme a description of the relationship between classical and relativistic mechanics, one which, moreover, appears to be violated by the conclusions we have already reached in this chapter.

However, we cannot dismiss the incommensurability thesis on the basis of the form it has taken here. Feyerabend's chief point is clearly that previous methodologies have overlooked crucial differences between competing scientific paradigms, and the incommensurability thesis is intended as a statement of the relationship between different theories. We need to look more closely at the notion of incommensurability and to examine the thesis finally in the light of our findings in this thesis. In the following chapter I shall study the notion of incommensurability and evaluate Feyerabend's complex argument for an incommensurability thesis. Finally I will indicate briefly areas in which it might be possible to retain certain established notions which Feyerabend has overthrown in his claims for radical change in science.

CHAPTER VIII

Before we finally evaluate Feyerabend's incommensurability thesis, let us look more closely at the notion of incommensurability itself. We can trace the notion back to the field of ancient geometry. It was noted then that a line whose length was a rational number could not be measured against a line whose length was an irrational number. In a right-angled triangle of measurements one unit, one unit and square root of two units, the hypotenuse has no common measure with the other sides because there is no way of dividing up the lines into equal segments so that a ratio can be set up between them. Now if we think of this idea in relation to scientific theories we expect, if two theories are said to be incommensurable, to be able to find no element common to both by means of which we could compare them. This is unacceptably vague. We must know what we wish to compare two theories with respect to before we can say that they are incomparable. We must also know what it means to lack a common element.

A trivial case we may consider is that of theories which may be said to be incomparable because they deal with different subject matter; for example, a psychological theory may in this sense be incomparable with a theory of light. However, even in this type of case the view that theories are incomparable may be denied. It is on this point that disparities between Feyerabend

and older empiricists emerge: the latter would agree that it is possible to give a reductionist account of even the most heterogeneous theories (Nagel, 1961, pp. 342-345).

Feyerabend, on the other hand, is convinced of the failure of reductionism. The relationship he accords to all comprehensive theories which he describes as undergoing changes of ontology in the transition to another theory is one of incomparability. It is the lack of common "content classes" between these theories that renders them, on Feyerabend's analysis, incommensurable: that is, there is no unit for comparison. We cannot establish any normal relationship between theories by means of which we can compare their "consequence classes". How does Feyerabend defend this way of viewing different scientific theories? An answer to this question involves a summing up of the twelve theses laid down at the start of chapter II above, which together constitute a defence of the incommensurability thesis.

The first two theses were dealt with very summarily, partly because they relate to the historical aspect of Feyerabend's philosophy of science which I have left untouched. They are, respectively, the claims that scientific theories are similar to ideologies in respect of the differences between each theory, and that an anthropological approach is the one most appropriate for studying science. Our conclusions were that the first thesis leads to a relativism which is not clearly

defensible and for several reasons might be undesirable; as regards the second there are a number of problems due partly to lack of sufficient clarity on the part of Feyerabend. Nevertheless should a historical approach be favoured and an anthropological approach adopted, this would not necessarily entail that Feyerabend's claims about the relationships between different theories is correct; theories may well be found to be commensurable with one another.

The third thesis is the rejection of a distinction between theory and observation. Feyerabend (1975) admits that "the question 'are two particular comprehensive theories . . . incommensurable?' is not a complete question" (pp. 278-279). The answer depends on the way in which we choose to interpret theories. Feyerabend chooses a realist interpretation which attempts to "give a unified account, both of observable and of unobservable matters" (p. 279). This explains his unwillingness to retain a distinction between a stable observation language and a changing theoretical language. Instrumentalist interpretations - such as those of traditional empiricism - rely on a stable observational language and regard as commensurable those theories which are related to the same observation language; this includes the majority of scientific theories. Are there grounds for choosing one interpretation over the other apart from personal preferences?

An objection to the existence of incommensurable

theories, according to Feyerabend, is that new theories or languages cannot be introduced directly, but must be connected with a stable observational idiom. As a refutation Feyerabend offers the case of a child learning his first language or that of the anthropologist learning the language of an unknown tribe. This does indeed answer the present objection, but does not refute altogether the possibility of a common observation language. Even if new theories could be introduced in a direct manner, this does not imply that they can only be introduced in that manner. A child can learn a language directly, but that same language can be learnt in terms of an established basis of translation. The evidence seems to suggest that different scientific theories are not introduced directly to the student, but are taught, partially at least, in terms of an observational idiom. In order to provide concrete support for an incommensurability thesis Feyerabend would be required to show that theories can only be learnt in a direct manner. Realism does not necessitate this.

The fourth thesis is the rejection of the consistency condition of traditional empiricism. The defence of this consists in the citation of theories in the history of science which have been shown to be inconsistent with one

another.¹ The condition has been rejected further in the form of a methodological demand to proceed "counter-inductively" and invent alternatives which are inconsistent with the accepted theory. But now the question arises as to how this rejection of the consistency condition can support an incommensurability thesis.

Let us assume that Feyerabend's examples have serious consequences for any attempt at a reductionist model of science, although it is not clear that they do, for they show only that some consequences of the two theories are inconsistent. This could be interpreted as showing that (part of) the one theory must be mistaken. Yet the point of his examples and of the request to "proceed counter-inductively" is to show that theories can be logically inconsistent. All that this says is that if "p" is a statement of some consequences of theory X, and "q" is a statement of some consequences of theory Y, then $\neg(p \cdot q)$.

However this means that we can compare the "consequence classes" of the two theories; they both deal with the same subject matter (the same domain), in Feyerabend's example, with bodies in free fall. The fact that they

¹ Feyerabend's favourite example (1963, p. 20; 1965, p. 168; 1975, pp. 35-36) is the inconsistency pointed out by Duhem (1954) between Newton's theory and Galileo's law of free fall. "What is being asserted is . . . the inconsistency of some consequences of Newton's theory in the domain of validity of Galileo's law, and Galileo's law . . . Galileo's law asserts that the acceleration of free fall is a constant, whereas application of Newton's theory to the surface of the earth gives an acceleration that . . . decreases . . . with the distance from the centre of the earth" (Feyerabend, 1963, p. 20).

can be compared in this way runs counter to one description of incommensurability theories in the usual manner, that is, by an examination of consequence classes" (Feyerabend, 1970a, pp. 219-220). No difficulty seems to arise in the comparison of Galileo's and Newton's laws.

Furthermore, the claim that two theories are inconsistent nevertheless permits a common notion of truth, rather than a relative one; the relationship of being inconsistent even insists upon accounts to the effect that certain statements in each theory cannot all or both be true. Again this conflicts with the same explanation of incommensurability which makes the further point that " theory X cannot be said to be either closer to, or farther from, the truth, than theory Y " (p. 220). It is thus safe to conclude that a rejection of reductionism in the form this thesis has taken, even if true, cannot lend support to an incommensurability thesis; as we pointed out above (p. 125), one of the consequences of an incommensurability thesis is that two theories could be neither consistent nor inconsistent with one another.

The two theses concerned with facts and statements of theories follow as consequences of the fourth thesis. As they are also linked with the main points of the later theses it will perhaps be more appropriate to evaluate them conjointly with these.

The seventh thesis is the claim that scientific change cannot be rationalized. It is clear that the

incommensurability thesis cannot be supported without denying that there is a set of standards for criticizing theories which are tied up with some rational ideal as to what our scientific theories should be. We have discussed the issues of rationality at some length and have concluded that if rationality is understood in the sense in which Lakatos understands it, rather than the sense in which Feyerabend understands it, there are grounds for claiming that standards of rationality do exist within (reconstructed) science, even though these may alter from time to time.

The methodological standards set up by Feyerabend - "anything goes", proliferation of inconsistent theories, counterinductive procedures, and so on - are, from one point of view, anti-rational standards: they do not feature in a Popperian or Lakatosian methodological programme. Yet we have had reason to disagree with Feyerabend's prescriptions, for they are not practically sound. Rational standards - whether we like them or not, and Feyerabend has an irrational dislike for them - do and will feature in any field which attempts to bring about the advancement of knowledge. The claim, therefore, that rational standards of criticism do not exist will not provide independent support for an incommensurability thesis.

We come now to one of two central points relating to the incommensurability thesis. This is the claim that in certain cases of ontological change - namely

change between two theories of a fundamental nature - the two theories must be described as being incommensurable with each other. As this sentence indicates, not all cases of conceptual change will result in incommensurability between two theories. The introduction of a few new concepts in a new theory does not mean that it will now be incommensurable with the older one. The type of change required is one where there is a radical shift in an entire conceptual scheme. We expect to find that two incommensurable theories embody different world views, could both be empirically adequate and yet there would be no way in which we could show one to be either consistent or inconsistent with the other, in other words, no means of deciding between them with the aid of a crucial experiment.

To decide whether there are, or could be, two theories satisfying the above requirements we need first of all to clarify what is meant by "conceptual scheme", and, more importantly, by "different conceptual schemes". We then need to see what kinds of relationships can be said to exist between theories embodying different conceptual schemes in order to answer the question whether there are incommensurable theories.

The first task is a perennial philosophical problem and all I shall try to do here is to sketch a possible example of two different conceptual schemes. Two theories must order at least part of the same experience differently otherwise they will be different merely by virtue of

their dealing with different elements of experience. Two theories might be said to have different conceptual schemes if the concepts in one are not extensionally equivalent to the concepts in the other. However, the relevant properties associated with a given concept in one theory may be such that they correspond, apparently haphazardly, to properties in the other theory which are associated with different concepts in that theory. A necessary condition for two theories to be incommensurable is that none of the concepts in either theory are extensionally equivalent, otherwise there would be some overlap of content.² It is highly dubious that Feyerabend could present us with anything more than hypothetical cases of such radically different conceptual schemes. Even the problematic case - problematic for the traditional empiricist - of the transition from classical mechanics to the special theory of relativity does not prove to be one where radical conceptual change has taken place.

Secondly we need to ask what relationship exists between two different conceptual schemes. A minimal condition, in order to discover whether two different conceptual schemes operate, is that we be able to translate the language expressing one into the language of the other. Feyerabend (1975, pp. 250-251) accepts that a translation is possible between the languages of incommensurable theories. But if the theories are incommensurable, the

² cf. Feyerabend (1975), p. 269.

the languages will be incommensurable too, and the translation will require a "bending" of one language. If the theories are incommensurable, then this must also imply that some (all) statements of one theory expressed in the other cannot have a truth value, else the theories would be consistent or inconsistent. In the case of the transition from classical to relativistic mechanics this condition is not satisfied, since many statements true in the one are true in the other.³

It has not been shown that there are different theories in the history of science which have radically different conceptual schemes, and Feyerabend provides no good reason why the development of incommensurable alternatives would be methodologically desirable. Nevertheless, there may well be incommensurable theories if we extend our universe of discourse beyond the confines of scientific theories.⁴ Feyerabend's argument, though not successful in our area of interest, has at least provided an answer to his opponents who deny the possibility of incommensurable theories.

Similar objections hold against the theses in favour of a position of radical meaning variance. As we have pointed out (ch. VII), the history of science shows that

³ These are the statements (chiefly observational ones) that Feyerabend has chosen to ignore.

⁴ cf. Feyerabend's discussion (1975, pp. 260-269) on the difference between the cosmologies of the archaic Greeks and their immediate successors in the 7th to 5th centuries B.C.

the claim for radical meaning variance in Feyerabend's sense has been unsatisfied. Furthermore, there is no reason to believe that comprehensive theories will, in the future, entail a "replacement of statements" from theory to theory. Presented as a prerequisite for "good empiricism" (Feyerabend, 1963, pp. 38-39), it has provided no reasons for its acceptance.

We have not yet discussed the last thesis - the adequacy of Whorf's theory - on which Feyerabend's account of meaning is based. Whorf's claim about the relations between language and different cultures are regarded as controversial (Black, 1962, p. 244), chiefly because of their assumption of an extreme relativism that is thought to exist between different language groups. The claims implicit in Whorf's thesis have been adopted by Feyerabend, not only in his thesis of meaning variance, but also in his claims for conceptual and ontological differences between different cultures.

My chief objection to a thesis of extreme linguistic relativity is this: the thesis presupposes the existence of a background linguistic system for each language group - revealed, for example in the cryptotypes discussed by Feyerabend (1975, pp. 223-224) - which are supposed to determine the conceptual scheme, world view, or metaphysics of each individual language user. This has the consequence that "no individual is free to describe nature with absolute impartiality" (Whorf, 1956, p. 239). We would expect the same to be true of the author, but Whorf

nevertheless believes that the linguist "familiar with many widely different linguistic systems" (p. 214) may be freed from any biases, and may thus be in a position to evaluate the adequacy of different languages. Yet this is not, as it stands, sufficient as an argument to extricate himself from the paradox facing all relativist theories of truth, namely that his own point of view is a product of his own linguistic background. Even his claim to have a privileged position on the grounds that he has studied many languages does not overcome this objection, for the study itself may have been directed by certain presuppositions of his own world view.

This does not imply that a thesis of linguistic relativity is untenable; rather I wish to throw doubt on Whorf's claims that he is in an unbiased position from which he can appraise the merits of different languages. A parallel objection holds against Feyerabend's polemic against science (1975, ch. 18), and also against his insistence that a pluralist methodology will lead to the best theory, for he has not shown the existence of an objective way of appraising theories.

I have indicated a basic disagreement with the incommensurability thesis that Feyerabend has used to characterize scientific change. The objections rest on the belief that this necessitates too radical a view of scientific change, which seems to be unjustified on the

basis of historical evidence.⁵ Nevertheless, there are several points at which I sympathise with Feyerabend's position namely those expressive of his dissatisfaction with traditional empiricism. We have reason to accept some form of a meaning variance thesis because the evidence points to some major changes in the meanings of crucial terms during scientific change. Nevertheless, the important point which I wish to make in conclusion, is that a meaning variance thesis need not entail incommensurability.

Towards the end of the first chapter, I mentioned the network model of Duhem, Quine and Hesse, which, though similar to much of Feyerabend's position, nevertheless does not entail the same radical consequences. The reason for this is that the network model allows for the possibility of a substantial amount of stability during scientific change, which it perceives to be not revolutionary, but gradual. Nevertheless, this model, too, has arisen from a dissatisfaction with the traditional empiricist idea of a stable, neutral observation language. Hesse writes that

There is no need to make a fundamental epistemological distinction between the theoretical and observational aspects of science The network of relatively observational statements can be imagined to be continuous with a network of theoretical relationships The difference between

⁵ Not all work done in sciences parallels examples cited by Feyerabend. Crucial discoveries of a non-revolutionary nature were the result of work by scientists such as the Curies and Pasteur.

them is pragmatic and dependent on causal conditions of sense perception rather than epistemological.

(Hesse, 1968, p. 207)

Thus far there is complete agreement with Feyerabend. However, denying the existence at an epistemological level between theory and observation does not imply that theories cannot share any common elements.

Empirical applications of observation predicates are not incorrigible, and the empirical laws accepted as holding between them are not infallible. A whole theoretical network may force corrections upon empirical laws in any part of it, but not all, or even most of it, can be corrected at once.

(Hesse, 1968, p. 208)

The undesirable consequence of an incommensurability thesis can be rejected once we are prepared to admit the relative stability of a large portion of our language during periods of scientific transition. It seems more reasonable to assume that theory-change is gradual rather than revolutionary, and that our theories are changed, generally, very little at one time.⁶ There are, undoubtedly, more important changes, such as the transition from classical mechanics to relativity theory, but even these do not necessitate an incommensurability thesis in order to explain the relationship between the theories. Feyerabend has, in his attempt to explain some of the more interesting periods of scientific development, overlooked the need to explain the smaller, though not

⁶ See Graves (1971) for a discussion of the gradual, but substantial, modifications to Einstein's original theory of relativity. This aspect of scientific development is incorporated within Lakatos' notion of a research programme.

unimportant, modifications that are continually made to any theory.

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