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THE CERTIFICATION OF FACTS AND THEORIES IN SCIENTIFIC DISCOURSE: THE CASE OF SOCIOLOGY

by

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The undersigned certify that they have read, and recommend to the Faculty of Graduate Studies and Research, for acceptance, a thesis entitled THE CERTIFICATION OF FACTS AND THEORIES IN SCIENTIFIC DISCOURSE: THE CASE OF SOCIOLOGY submitted by Anthony M. Simmons in partial fulfilment of the requirements for the degree of Master of Arts.

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ABSTRACT

The purpose of this thesis was to examine the relations between the languages of science and the status of knowledge claims in Sociology. In particular, we have investigated the degree of variation between alternative sets of criteria for the certification of propositional statements as either factual (empirical) or theoretical. Towards this end, we have constructed a typology of the alternative sets of certification criteria and have demonstrated how they function as rules of evidence within alternative models of scientific discourse. Thus, from an idealised model of language usage, the binary language model of scientific discourse, we have derived two basic logical variations: an observational reductionist model of scientific discourse and a theoretical reductionist model of scientific discourse.

It has been further shown that these two reductionist variations of the binary language model of scientific discourse underlie competing programmes for the growth of scientific knowledge. Thus the original logical empiricist programme for a unified science, as well as the Kuhnian programme for paradigm closure, together presuppose an observational reductionist model of scientific discourse. Alternatively, the phenomenological programme for the relativisation of scientific languages, as well as the neo-critical ra-

tionalist programme for theoretical pluralism, together presuppose a theoretical reductionist model of scientific discourse.

It has been concluded that neither of the reductionist variations of the binary language model presents a satisfactory model of scientific discourse. Instead, a dialectical model of scientific discourse has been formulated with reference to examples drawn from critical hermaneutics and a related programme for the complementarity of binary language has also been suggested.

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CHAPTER I

INTRODUCTION

Problem

The relationship of language to knowledge claims has received insufficient attention in the literature of theoretical Sociology. Such attention as has been paid to this problem has invariably coincided with the broader development of these issues in the neighbouring disciplines of philosophy of science and epistemology. Thus, for example, the early programme of the logical empiricists for the logical reconstruction of the foundations of scientific discourse was developed into a corresponding legacy of logical empiricism in Sociology by such writers as Lundberg (1939) and Zetterberg (1954). Similarly, the later writings of Wittgenstein (1953), which laid a basis for the subsequent development of the analytic philosophy of language were introduced into Sociology by such writers as Winch (1958) and later by the Ethnomethodologists. Today, the more recent arguments over the incommensurability of alternative theoretical vocabularies from such writers as Kuhn (1970a) and Feyerabend (1970a) have exercised such sociologists as Klima (1972), Martins (1972), Spinner (1973), Philips (1974), Lammers (1974), to go no further.

The intention of this thesis, therefore, is to reopen the problem of language in Sociology as one of the key pro-

blems in trying to evaluate the present state of knowledge in Sociology. For the investigative languages of the social sciences provide the special theories of sociological discourse with the categories that make the social world available to research and experimentation, and thus, an examination of the structure of the investigative languages is an important preparatory step towards evaluating their overall contribution to particular programmes for the growth of scientific knowledge.

This type of investigation, in a number of senses, coincides with a renewed interest among many sociologists for the greater pursuit of foundational studies into the cognitive structure of their discipline. For although, until now, the tradition of critical literature in Sociology has tended to concentrate upon the more imminent theoretical debates within the discipline as in the case of such writers as Lynd (1939), Mills (1959), Gouldner (1970), Andreski (1972), and Friedrichs (1970), to name but a few, the renewed interest in the epistemological foundations of sociological theory may now be seen as a response to wider intellectual currents originating beyond the boundaries of the present discipline.

Not that there is anything wrong with this new direction; on the contrary, such investigations may indeed be the pre-condition for new possibilities to find their way onto

the agenda of sociological theory and research. As one writer closer to the subject than many remarked:

> Periods of re-evaluation of belief systems and their methods involve an inherently philosophical manoevre of 'going back to basics'. The present is such a period of re-evaluation for the belief systems and practices comprising Sociology. And as such it requires, beside political critiques of Sociology as a practice, philosophical critiques of Sociology as a belief system. Its beliefs concern social action and social structures, their relations and the methods most appropriate for their study.
>
> Roche, (1973:ix).

Max Scheler (1960:64), perhaps, closely formulated the starting point of our investigations when he observed that "each type of Knowledge develops its own language and its own special style in formulating its knowledge". (Translated in Stehr and Simmons, 1975.) For the purpose of this thesis is to examine the different models of discourse in Sociology which provide the alternative sets of criteria for the certification and legitimisation of knowledge claims. Thus the certification of specific classes of terms and propositions as <u>factual</u> (empirical), and other classes as <u>theoretical</u>, always follows from the criteria provided by a particular model of scientific discourse. We have, therefore, attempted to construct a logical typology of the variations upon an idealised model of scientific discourse, and have shown the relevance of an application of this typology to the case of sociology.

Models of scientific discourse, however, are also related to identifiable theories of scientific development. Thus it also becomes important to reveal the fundamental presuppositions of alternative models of discourse, for these presuppositions are implicitly related to existing theories of scientific development.

Having identified the logical variations of an idealised model of scientific discourse, and specified the relations of these variations to alternative theories of scientific development, it is also possible to construct a related typology of programmes for the growth of scientific knowledge. For these programmes presuppose both a particular model of scientific discourse and an identifiable theory of scientific development. These typologies, although constructed on the basis of investigations into the natural sciences, may also be usefully applied to the case of Sociology.

Objectives

The objectives of the thesis may be stated as follows:

(i) To discover the significant variations in the certification criteria of facts and theories, and to find a simplified literary form for their conceptualisation.

(ii) To investigate the relationship of the main certification criteria to identifiable theories of science.

(iii) To investigate the further relationship of certifica-

tion criteria, and theories of science to identifiable programmes for the growth of scientific knowledge.

(iv) To examine the implications of the above set of conclusions for a critique of Sociology.

Rationale

The procedures used to explicate and investigate the problem of this study will proceed along the following lines:

(i) To present the problem of alternative models of language, with particular attention to the problem of reductionism.

(ii) To present the problem of alternative programmes for the growth of knowledge, with particular attention to the problem of paradigms.

(iii) To present as current problems of sociology: theoretical pluralism and methodological dissensus.

(iv) To investigate the alternative solutions of meta-languages or complementary languages.

Limitations

In brief, the massive limitations of this study are partly determined by the pioneering state of these investigations. It cannot be regarded as a well defined area with easily identifiable and isolatable problems. Quite the contrary. Also, the qualifications already imposed on the literature survey will greatly limit the scope of this investi-

gation. For we are not concerned here for example, with any coverage of the growing literature on the sociology of language. This is an area of competence that straddles a number of sub-disciplines, including the sociology of knowledge, social stratification, and socio-linguistics. From the reports of a recent literature review, (Grimshaw, 1974), this is a rapidly expanding area which has benefited from a cross-fertilisation with related disciplines. However, although in many ways integrally related to the subject of the present study, the contributions of the sociology of language have not been cited or evaluated, and their significance for the main arguments of this thesis have been neglected. Similarly, another area that must be ignored for present purposes, although, if anything, its relevance to the topic at hand is more pressing than the sociology of language, is the enormously fertile area of linguistic anthropology. The revolutionary writings of Sapir and Whorf must be felt by anyone who undertakes any kind of a study of language, but their impact on this investigation will remain for the most part unacknowledged and implicit.

And finally, the contributions of the ethnomethodologists to the problem of languages in sociology are also avoided. This, perhaps, is the most difficult omission to defend, for their interest in these problems has been unremitting and

thorough. But, there has also been no systematic effort to evaluate their contributions on this occasion. Perhaps the future will provide time and resources to rectify these omissions.

Explanation of Theoretical Terms

We shall introduce a number of terms into this paper which refer to some of the central theoretical concepts of our analysis. For this reason, therefore, it is necessary from the outset to provide a clear definition of these terms as well as some indication of the way in which they are used throughout the study.

The first concept to be introduced into the study is that of the model of discourse. The model of discourse, in this study, refers to a set of the various analytic distinctions which have been used to conceptualise the general status of language in the practice of science. Foremost among these distinctions is the traditional distinction separating the languages of observation (L_o's), from the languages of theory (L_t's). (See, for example, Spector, 1966-67; Wisdom, 1970, Hesse, 1970.)

In brief, however, what is generally indicated by this distinction is, on the one hand, the existence of a stable language of observation: a set of propositions which more or less faithfully record the empirical phenomena of the subject

domain and certify them as 'facts'. The language of theory, on the other hand, refers to the existence of a set of interrelated propositions (propositional calculus) which are connected to the sentences of the observational language by means of 'correspondence rules' which may partially interpret, but may never fully translate, the propositions of the L_t into propositions of the L_o. In many sciences the language of theory has been formally reconstructed from the terms of ordinary language discourse to the re-axiomatised terms of the theoretical science through the application of logico-mathematical operations to the semantic content of the theoretical terms.

To some extent, all the theoretical and observational terms of the sciences depart from the conventions of ordinary language discourse. This is largely a result of the fact that in the exact sciences, terms such as 'mass', 'velocity', 'acceleration', etc., refer to a fully specified class of object predicates, whereas in ordinary language discourse the casual use of these terms is frequently in violation of the classical rules of logical inference (see Lachenmeyer, 1971).

In addition to the above ideas included in the general concept of model of discourse we should also explain the use of some other terms. The term proposition as used throughout this paper refers to those propositions of the L_t or L_o of a

science which make assertions about specified events or classes of events within the context of a set of certification criteria. The form of certification of these claims refers to the rules of evidence that are invoked to test the cognitive significance, or meaningful content, of propositions formulated in a particular model or discourse. The set of rules which are invoked to certify the legitimacy of the class of empirical evidence may vary with the particular programme for the growth of knowledge of which they are a part.

Thus the classical programme of logical-empiricism presumed a set of correspondence rules which would provide for the possibility of a full empirical verification of all theoretical terms. In this case, the rules of evidence were determined by a correspondence theory of meaning or truth (see Hempel, 1965). For the analytic philosophers of language, however, the cognitive significance of propositions is tested by rules of evidence which have as their object the elucidation of propositions or classes of propositions in relation to the structure of the total relevant context of discourse, or the [redacted]age game (Wittgenstein, 1953).

Finally, the concept of programme for the growth of knowledge as used throughout this paper refers to identifiable strategies for the cognitive organisation of scientific specialities. Such distinctive programmes as the logical empiri-

cist programme for a unified science have already been suggested in the earlier literature. At present, the competition of alternative programmes for the growth of knowledge is increasingly centred on the debate over the desirability of theoretical pluralism in the natural and social sciences.

CHAPTER II

THE BINARY LANGUAGE MODEL OF SCIENTIFIC DISCOURSE

Introduction

The desire to distinguish between the various classes of logical operations which may be performed through the use of language dates back to the turn of the century. For it was during the early part of the twentieth century that Ludwig Wittgenstein succeeded in placing the problem of language right at the top of the philosophers' agenda, and in most respects it has stayed there ever since. In addition to Wittgenstein's attempts to logically reconstruct the foundations of ordinary language, however, other philosophers of this period began similar attempts to logically reconstruct the foundations of the more specialised and technical languages of mathematics and physics. Thus the revolutionary impetus initiated by Wittgenstein was maintained by the philosophers of the Vienna Circle, and has achieved a permanent expression in the philosophy of logical empiricism. For an interesting account of the development of logical empiricism and the origins of the Vienna Circle see Joergensen (1951).

One of the earlier distinctions drawn by some analysts of the languages of scientific discourse was that between the observational and the theoretical functions of language usage. Although originally this was a relatively uncontroversial dis-

tinction, the passage of time has left as one of the most disputed distinctions in the contemporary philosophy of science. The original basis for distinguishing the observational language (L_o) from the theoretical language, (L_t) was established in the earlier writings of Wittgenstein. For in the pages of the Tractatus, Wittgenstein (1923) set out to demonstrate that the complex structure of language is derivable from a series of elementary propositions. These propositions can be further broken down into their constituent primitive terms, and it is these terms which stand as axioms within the propositional system, as they cannot be defined by reference to any more fundamental terms. It is these primitive terms and their grammatical arrangement into elementary propositions that constitutes the empirical basis of ordinary language. For it is only the elementary propositions within a language system that are directly connected to the world. They exist as logical 'pictures' of the elementary facts of the world: facts which cannot be further broken down into their internal parts. Elementary propositions, therefore, correspond to 'atomic' facts, (Tractatus 4:21).

From the elementary propositions it is possible to derive (complex) propositions which are dependent for their cognitive significance on the elementary class of propositions. Unlike the elementary propositions, the (complex) pro-

positions do not stand in a direct relationship to the world. Their primitive terms are abstract and cannot function as logical pictures of atomic facts. For this reason they can only exist as 'truth-functions' of the elementary propositions, which means that the truth or falsity of their assertions can only be determined by translating them into elementary propositions. Only the elementary propositions are capable of verification, for only they are directly connected to the world. It was with this distinction in mind that Wittgenstein insisted that all propositions which cannot be translated into an elementary form where their truth conditions may be judged, cease in any logical sense to be meaningful. For meaningful propositions must be capable of being tested for their truth content and such a test can only be made of an elementary proposition.

It was, therefore, on the basis of this programme for the logical reconstruction of the foundations of ordinary language, that Wittgenstein set out to revolutionise the traditional methods of philosophical analysis.

> Most of the propositions and questions to be found in philosophical works are not false but nonsensical. Consequently, we cannot give any answer to questions of this kind, but can only establish that they are nonsensical. Most of the propositions and questions arise from our failure to understand the logic of our language. Tractatus 4:003).

The important historical task of Wittgenstein's early philosophical method was to illustrate that many (if not most) of the central problems that had perplexed the earlier philosophers, such as the nature of the mind-body dualism, the existence of God, et cetera, were without meaning. That is, that the classical way of framing philosophical questions frequently transcended the logical limits of language. Such metaphysical or religious questions could only be provided with metaphysical or religious answers, but under no circumstances could they be answered logically, as either true or false. Thus, for Wittgenstein, the verifiability of propositions is inseparably bound up with the meaningfulness of propositions. It was, therefore, with this programme that Wittgenstein believed that many of the fundamental propositions of classical thought could be "dissolved", when the problems themselves could be shown to be instances of the incorrect use of language. For sentences which included as their predicates such terms as "soul" or "mind", or whatever, were incapable of being defined as primitive terms of an elementary proposition. And it was on the basis of this distinction between the complex classes and the elementary classes of propositions that the later distinction between the theoretical languages and observational languages of scientific discourse was originally founded. For Wittgenstein's

earlier programme for the logical reconstruction of ordinary language became, in the hands of the philosophers of the Vienna Circle, a programme for the logical reconstruction of the foundations of scientific languages.

Theory and Observation Languages

The analytical distinctions that Wittgenstein had made between the elementary and complex propositions of ordinary discourse were later converted into similar distinctions for the logical analysis of scientific discourse. In place of the elementary proposition, which provided an empirical foundation for the language system of ordinary discourse, a series of other terms were introduced to describe what later came to be called the "observational language", (L_o). For it was the L_o that was to provide the empirical foundations of scientific discourse. Thus, for example, the philosopher of science, F.P. Ramsey, distinguished between the primary system of discourse (in science), which contains all the terms and propositions of the universe of discourse in question, and the secondary system. The secondary system, or theoretical system, is related to the primary system by means of a dictionary which defines "the functions of the primary system in terms of those of the secondary system", (Ramsey, 1931:215). Similarly, another philosopher of science, A.J. Ayer, has spoken of "observational statements" which are alleged to re-

cord an "actual or possible observation". He regards only those statements as directly verifiable which are either observational statements, or statements "such that in conjunction with one or more observation statements it entails at least one observation statement which is not deducible from these other premises alone", (Ayer, 1946:13). He further distinguishes directly verifiable statements from those which are indirectly verifiable but which appear in scientific theories without designating anything observable. Another writer who has made similar distinctions is R.B. Braithewaite. He distinguishes between "propositions about observable entities", such as "flashes of light or pointer-readings of a measuring instrument", and those containing theoretical concepts, examples of which he cites as "fields of force, wave-functions, electrons, (Braithewaite, 1953:51). Direct meaning is given to the first class of propositions, and indirect meaning to the second. (I am indebted to Mary Hesse, 1958, for these examples.)

These few examples must suffice, for the present, to show the ubiquity of the dichotomisation of scientific discourse into the two classes of terms, observational and theoretical. It is a distinction that is common to writers who have analysed the languages of science through the criteria of logical empiricism. We shall refer throughout to this

particular conceptualisation of the scientific enterprise as the Binary language model of scientific discourse.

The content of the L_o has been the subject of considerable debate and revision among the logical empiricist writers. Some followed the early positivists in seeking an introspective experiential base for the L_o, while others sought a base in the sensory data of human perception. More recently Carnap, (1936, 1937, 1938) has argued for the adoption of a language to describe the concrete physical reality of the world. These changes clearly record the shifting interpretations that have surrounded the career of the binary language model from its earliest inception.

> The history of logical empiricism could well be told in terms of the problems and changes involved in characterising the 'observation language' of science on the basis of the content of the observational vocabulary - what the terms refer to: eg. subjective impressions, inter-subjective sense data, and finally, gross physical objects and their properties.
>
> Spector, (1966:7).

Origins and Early Evolution of the Binary Language Model

One of the earlier advocates of the distinction between the L_o and the L_t was the philosopher, Rudolph Carnap. And it has been at his hands that the distinction acquired a modified and more defensible form. For although many of the earlier writers influenced by Wittgenstein tended to regard the distinction between the L_o and the L_t as an absolute se-

paration of two languages, Carnap was less dogmatic.

> There is no sharp line between observable and non-observable predicates because a person will be more or less able to decide a certain sentence quickly, i.e. he will be inclined after a certain period of observation to accept the sentence. For the sake of brevity we will have to draw a sharp distinction between observable and non-observable predicates. By this drawing an arbitrary line between observable and non-observable predicates in a field of continuous degrees of observability we partly determine in advance the possible answers to questions such as whether or not a certain predicate is observable by a given person.
>
> Carnap, (1936).

Rather than distinguishing between predicates which refer to observables and predicates which refer to unobservables as though this were an absolute logical distinction, (as it was for the early Wittgenstein), Carnap recognized that the category of observability could itself be broken down into relative degrees of observability. What may be observable to the trained observer may not be readily observable to the naive observer. To accommodate these fuzzy elements in the concept of observability, therefore, Carnap introduced the criterion of "quick decidability" as the practical condition of observability. It was only those sentences whose truth or falsity could be ascertained by a quick decision which should be regarded as observation sentences. Other sentences which required a lengthier (and more inferential though not necessarily so) judgement process should be regard-

ed as theoretical sentences.

> A predicate 'p' of a language L is called observable for an organism (e.g. a person) N, if, for suitable arguments N is able to come to a decision with the help of a few observations about a full sentence, say 'p(b)', i.e. to a confirmation of either 'p(b)' or '-p(b)', of such high degree that he will either accept or reject 'p(b)'. ... There is no sharp line between observable and non-observable predicates because a person will be more or less able to decide a certain sentence quickly...For the sake of simplicity we draw a sharp distinction.
>
> Carnap, (1936).

Thus Carnap's earlier criterion for distinguishing between the L_o and the L_t, as has been remarked by other writers (e.g. Spector, 1966, Feyerabend, 1962), was based very much on a pragmatic definition of "observability". This fact is of interest for two reasons, for it shows the real differences which existed between Carnap and some of his contemporaries of the Vienna Circle who remained much closer to the "logical atomism" of the Tractatus, and it also reveals an early tendency in Carnap's thought that remained largely undeveloped in his later work. In fact, the pragmatic criterion of observability seems to be forgotten and finally lost to the later writings of Carnap: it is only retrieved by writers as diverse as Grover Maxwell and Paul Feyerabend who use it to propose reforms or refutations of the classical L_o, L_t distinction.

However, there were other more fundamental departures

from the earlier logical atomism of the Tractatus that Carnap introduced into the doctrines of logical empiricism. The most basic of these departures was the denial of verificationism as a reasonable criterion for the cognitive significance of propositions. For in its earlier form, (Russell, 1921, Schlick, 1949), the condition of verifiability had specified that a sentence could be accepted as cognitively significant only if it was not analytic and was "capable at least in principle, of complete verification by observational evidence", (Hempel, 1956:103). This condition assumed, therefore, the logical possibility of obtaining the relevant observational data to fulfil the conditions for a complete verification. However, these conditions were later shown to be far too restrictive for use in scientific discourse. For when these criteria of cognitive significance were strictly applied, they succeeded in defining as meaningless all propositions expressing universal regularities or general laws. They precluded from meaningful discourse, therefore, the very propositions that are basic to the formulation of scientific theories, (Hempel, 1956:105). Recognising these limitations on the criteria of verification, Carnap resolved to modify the criteria.

> The historical merit of this thesis (i.e. verificationism) was that it called attention to the close connection between the meaning of a sentence and the way it is confirmed...This formulation thereby

> helped, on the one hand, to analyse the factual content of scientific sentences, and, on the other hand, to show that the sentences of trans-empirical metaphysics have no cognitive meaning. But from our present point of view, this formulation, although acceptable as a first approximation, is not quite correct. By its over-simplification, it led to a too narrow restriction of scientific language, excluding not only metaphysical sentences but also certain scientific sentences having factual meaning. Our present task could therefore be formulated as that of a modification of the requirement of verifiability.
>
> Carnap (1936).

In the place of the former criteria for the cognitive significance of propositions, Carnap evolved a less restrictive set of conditions which he called the criteria of confirmation. The significance of propositions was no longer determined by the logical possibility of retrieving relevant observational data in order to make a verification. For the new criteria of confirmation simply demanded that the vocabulary of significant propositions be made up either of logical terms, or of terms with empirical significance. The terms of empirical significance are those which are capable of being defined in observational terms, or can be logically related to them in other ways, (e.g. through explication). Thus, for Carnap, the criterion of verifying propositions through an examination of their correspondence with observational evidence become subordinated as a criterion for cognitive significance, to the requirements of formulating pro-

positions in a language made up only of logical and empirical terms. He later gave the name "thing-language' to describe this mode of discourse.

> Let us give the name 'thing-language' to that language which we use in everyday life in speaking about the perceptible things surrounding us. A sentence of the thing-language describes things by stating their observable relations subsisting between them. What we have called observable predicates are predicates of the thing-language. (They have to be clearly distinguished from what we have called perception terms...). Those predicates of the thing-language which are not observable, e.g. disposition terms, are reducible to observation predicates and hence confirmable.
>
> Carnap, (1936).

In defining more fully the content of the thing-language, Carnap also made it clear that he was only including the concrete physical terms of the natural language, and not the introspective terms relating to sense-date that many earlier empiricists had made the foundations of their observational language. He had replaced the language of phenomenlism by the language of physicalism And in addition to these revisions Carnap also introduced some modified rules governing the logical connections between propositions of the thing-language and propositions of a higher order construction.

Whereas previously the old requirement had insisted that all theoretical terms should be capable of complete definition in the primitive terms of the L_o, Carnap now rejected this condition as too restrictive. Part of the problem of the require-

ment for definability lay in the fact that certain classes of terms, especially disposition terms, cannot be defined without taking into account the context of their usage.

> The property 'magnetic' is an example of a disposition term: it designates not a directly observable characteristic, but rather a disposition, on the part of some physical objects, to display specific reactions (such as attracting small iron objects) under specifiable circumstances...Since an object may be magnetic at one time and non-magnetic at another...a contextual definition...has to be sought.
>
> Hempel, (1970:676).

It is impossible, therefore, to provide a definition which can cover the finite range of contexts in which these terms may be used. To overcome this difficulty Carnap proposed that instead of definitions, "reduction sentences" should be used to connect theoretical terms with observational terms. Reduction sentences, unlike definitions, specify the meaning of a term only conditionally or partially and do not provide a way of eliminating the term from all contexts in which it may occur.

It was these revisions in the older conceptualisation of the connectibility of observational and theoretical terms that brought about a redefinition of the relationship between propositions of the L_o and propositions of the L_t. For Carnap had shown that theoretical sentences could no longer be considered as parts of a fully translatable language system, but

only as parts of a partially interpretable language system. His criteria for the cognitive significance of propositions had already travelled some considerable distance from the Tractatus.

In a later contribution Carnap, (1956) has made it clear that even these revisions may impose too great a restriction on any criterion of significance for theoretical sentences. The relationship between T_o's and T_t's he therefore concedes, is even weaker and more indirect than acknowledged in his earlier formulation.

However, as Child (1971) has shown, the assumption that the theory and observation languages form two independent languages in scientific discourse, remains central to Carnap's system. The problems within this system are related to the difficulties of specifying the connections between these two levels of language. For whereas at one time, Carnap expressed these connections in the form of correspondence rules, in his later work (1956) he rejects this form as too restrictive, and replaces it with the more flexible form of correspondence postulates. At the same time, however, it is evident that Carnap's commitment to the binary language model remains unaltered, although his discrimination between the L_o and L_t seems no longer to be based on the former pragmatic criterion of "quick decidability".

> In discussions on the methodology of science, it is customary and useful to divide the language of science into two parts, the observational language and the theoretical language. The observational language uses terms designating observable properties and relations for the description of observable things or events. The theoretical language, on the other hand, contains terms which may refer to unobservable events, unobservable aspects or features of events...
>
> Carnap, (1956:38).

In summary, therefore, Carnap revised a number of assumptions contained in the earlier formulations of the relationships between L_o and L_t, which can be expressed accordingly:

Stage 1

Verification $T_o \Longleftarrow T_t$ (definition)

$S_o \Longleftarrow S_t$ (translation)

Stage 2

Confirmation $T_o \Longleftarrow T_t$ (reduction)

$S_o \Longleftarrow S_t$ (partial interpretation)

S = Sentence

T = Term

O = Observation

t = Theory

$\Longleftarrow$ = derivable through appropriate correspondence rules

This summarised picture of the evolution of Carnap's thinking about the problems of verifiability and translation of theoretical terms is necessarily oversimplified and incomplete.

The Present Distinction between Observation and Theory

Anyone with more than a passing interest in these problems will be obliged to return to Carnap's own work on these subjects for a fuller statement of his position. However, the intention here has simply been to introduce the binary language model of scientific discourse through the early ideas of one of its more powerful advocates.

The present controversy around the binary language model has produced a literature as voluminous as it is scattered. (For one of the best up-to-date bibliographies on this subject see Mary Hesse's article, 1970a.) However, for the sake of brevity and simplicity it will be argued that all discussions about the L_o, L_t distinction may be placed into one of three categories:

(i) arguments for the erasure of the distinction.

(ii) arguments for retaining the distinction in a modified form.

(iii) arguments for turning the established distinction on its head.

As is inevitable with any generalised typology, it is

always possible to encounter specific examples which may be appropriately fitted into more than one of these categories. Therefore, it is worthwhile adding the proverbial caution that such typologies are only analytic aids for breaking up reality in thought, before putting it all back together again in practice. For our purposes, however, these differentiations should clarify the problem more than they obscure it.

(i) Arguments for the Erasure of the Distinction

One writer who has clarified some of the problems surrounding the observational-theoretical distinction is Peter Achinstein. Achinstein (1968) has argued that general distinction between T_o and T_t is not a logically defensible one, because, in his view, when the general distinction is logically analysed it may be seen to dissolve into a series of related and overlapping distinctions. He therefore concludes that if a distinction between T_o's and T_t's is to be retained, then the criteria for this distinction must necessarily be fixed by every particular context of its use.

This is, as he acknowledges, quite contrary to the intentions of those who seek to demonstrate the existence of an observational language which is independent of theoretical frameworks.

> The thesis that a list of 'observational' terms can be compiled is defended by those envisaging the possibility of constructing an 'empiricist

> language'. An underlying assumption of this program is that there exists a unique (or at least a most suitable) way to describe what is or can be observed - a special 'physical object' or 'sensedatum' vocabulary eminently fit for this task.
>
> Achinstein, (1968:178).

Achinstein's main argument is that the observational-theoretical distinction cannot be logically justified as a general distinction. For an insistence on the importance of such a distinction, he claims, conceals the problematic and ambiguous use of terms like "observation" and "theory". The problem is that these terms are not always defined consistently, and consequently different and inconsistent criteria are frequently used to justify the L_o, L_t distinction.

If lists were to be made up containing both theoretical and non-theoretical terms it is evident, as Achinstein suggests, that the non-theoretical list would probably contain terms such as "red", "warm", "water", "moves", et cetera, while the theoretical list would probably include terms such as "electric field", gene", "mass", "electron" and so on. This separation of observational terms from theoretical terms is frequently justified for (one of) two reasons: either that T_o's refer to observables whereas T_t's refer to unobservables, or that T_o's are independent of theoretical frameworks whereas T_t's can only be understood within their contexts. There are then, already two possible sets of criteria

for making the distinction between L_o and L_t: those dealing with the problem of observability and those dealing with the problem of facticity. However, as Achinstein has shown, there are a number of difficulties which result from any attempt to apply these criteria on behalf of a general distinction between observation and theory.

The assertion that T_o's refer to observables while T_t's refer to unobservables provides no guidance about the use of these terms in any given situation. For as Achinstein suggests there are a number of different ways in which the word "observe" may be used. There is, for example, the meaning given to the term "observe" which includes the everyday sense of looking at ordinary objects such as tables and chairs. On the other hand, scientists also use the term when they read off, or interpret, certain types of basic measurements. Thus temperature changes are "observed" as well as the movement of electrons inside cloud chambers. In the last two cases, the meaning of the term "observe" has changed, for it is not the temperature or the electron, themselves, that are preceived as either objects or processes, but rather such physical evidence as is necessary to infer the presence of the phenomenon. Moreover, as Achinstein goes to show, the presence of a particular phenomenon may be observed and reported in any number of different ways. The physicist may say that he observes

electrons passing through a cloud chamber, or alternatively, the tracks produced by the electrons; or again, he may simply report an observation of strings of tiny water droplets that have condensed on gas ions. His final "observation", in other words, is not simply predicated on what he sees, but also on the situation in which he is making a report: e.g. whether to professional colleagues or to a lay audience. There is therefore, no unique way of describing or reporting phenomena in the terms of an absolutely standardised observational vocabulary, because what is observed and described is always relative to the context of interpretation. It is for these reasons that Achinstein disowns the traditional distinction between observation and theory as inconsistent and unworkable. At the same time, he makes it clear that he is prepared to discriminate between observable and non-observable referents, but only within the specific context of any given case, and never as an absolute distinction.

Another writer who has made essentially the same criticisms against the ambiguity of the established L_o, L_t distinction is Marshall Spector, (1966). He presents an argument similar to Achinstein to show that the observability-unobservability distinction is conventially used to cover a series of different distinctions. Spector argues that because the distinctions between L_o and L_t are not absolute

ones, but are rather definable in each separate context of their usage, it is, therefore, inadmissable to attach absolute logical criteria to these distinctions. As a consequence of this position he denies the validity of criteria which permit semantical rules to be given to the primitive terms of the object (i.e. observation) language, but not to the terms of the theory language.

This criterion was introduced by the logical empiricists as a consequence of accepting the binary language model of scientific discourse, and is also criticised by Achinstein. The procedure of giving semantical rules to terms in the scientific object language is used for the introduction of new terms. It is the method whereby terms in the object language can be fully defined in terms of something observable. The semantical r[illegible]fore, is not expressed in the object language its[illegible]n the metalanguage, which contains the term as ant[illegible]y understood.

> An example of a sema[illegible]al rule would be "R" means (or designates[illegible]e property red, where "R" is a term in the [illegible]ientific object language and "red" a term [illegible] the metalanguage. To terms in the second c[illegible]ss, the theoretical terms, meanings are [illegible] assigned by semantical rules.
>
> Achinstein, (1968:69).

Theoretical terms are not given semantical rules because they do not refer to observables and, therefore, cannot

be fully defined within an axiomatised language system. Instead, T_t's are given correspondence rules, or 'what Carnap calls mixed sentences': They contain at least one theoretical term and at least one non-theoretical one and will be part of the theory itself, (Achinstein 1968:69). It is for this reason that T_t's are subject to "partial interpretation" because the connection between theoretical and non-theoretical terms is indirect and mediated by the mixed sentences.

However, Spector's argument is that such absolute logical criteria for the separate uses of L_o and L_t are not justified. For any attempt to show the rational basis of L_o and L_t distinction inevitably ends up in a series of related and over-lapping distinctions, none of which is strong enough to support the fundamental distinction of the binary language model. Yet another writer who has devoted considerable space to an ongoing investigation into the binary language model, of scientific discourse is Mary Hesse, (1958, 1970a, 1970b). She has argued that although most philosophers have conventionally employed the vaguest of criteria to define what they choose to call the "observation language", most of their critical energies have been spent in problematising the status of theoretical terms. Hesse departs from this custom by investigating the observational terms of scientific discourse as a primary problem, and reflecting on the consequences of

her analysis for the established L_o, L_t distinction.

She begins by placing the isolatable term at the centre of her inquiry, rather than the sentence, although the latter form has traditionally been the standard unit of analysis. The rules of sentence formation and grammatical construction, she argues, may be assumed the same for scientific discourse as for natural discourse, from which the former is derived. Therefore, the central task of her criticism is to show that terms which are accepted as standard observational predicates, rely in part for their proper "functioning", on conformity to general laws. (The "Function" of a word is Hesse's term to describe the rules which are given for the introduction, learning, understanding and using of a word). No matter how well "entrenched" an observational predicate may appear, it is always possible to reveal some of the conditions of its use, which are provided by general laws. This is true, Hesse maintains, even for those terms that appear most independent of laws and theories for their significance. Thus, although a descriptive predicate like "red" may seem quite autonomous from any theoretical determination of its meaning when used in an observational sentence, there are cases when it may be shown to be dependent upon a theoretical context. Consider the case cited by Hesse, (1970a:44).

> It can't have been red because it was a sodium flame, and sodium flames are not red.

This example or any similar one, indicates that indeed there are certain laws which govern the use of the term "red" in any given situation, and that disagreements over the proper use of the term may be resolved by a public appeal to these laws. Maxwell provides a similar example:

> Even a term such as 'red' has part of its meaning provided by, for example, the lawlike sentence 'No surface can be both red and green all over at the same time'.
>
> Maxwell, (1962:23).

It is possible therefore, to distinguish between predicates on the basis of their relative degree of entrenchment, the degree to which a meaning of a term has remained stable over the several different contexts of its use, and consequently, the degree to which the inductive character of the term has thereby been forgotten. The fact that any predicate is well entrenched should not delude us, says Hesse, into believing that its function will remain unchanged in all future contexts of its use. For the more entrenched a term becomes, the more shocking appears its later change of function.

Therefore, Hesse suggests that the traditional distinction between L_o and L_t should be abandoned in favour of some other model of scientific (and ordinary) discourse.

> ...the present account amounts to a denial that there is a fundamental distinction between theoretical and observational predicates and statements and implies that the distinction commonly made is both obscure and misleading.
>
> Hesse (1970a:61).

In the place of the binary language model, Hesse has proposed acceptance of a "network" model of discourse. The main difference between the two models is that whereas the binary model implies the existence of levels of language: i.e. that theoretical terms may be reduced to a more empirical level, the network model does not. Instead of accepting the design of different language levels, and the ontological commitments that frequently accompany it, the network model stresses the importance of centrally and peripherality of terms in the structure of scientific theories. Thus as Hesse insists, irrespective of how well entrenched a particular predicate, or how central a given law may be, neither are exempt from modification under pressure from their surroundings. It is precisely because the binary language model has a tendency to reify the highly confirmed and publicly accepted statements of scientific discourse, that it should be abandoned.

This verdict is shared by J.O. Wisdom, (1971) and stated in somewhat similar terms. For Wisdom sees the binary language model as underlying the three great traditions within the philosophy of science: instrumentalism, conventionalism and induction. And the foundation of this model, he argues, lies in the assumption of the "ultimacy of observation". However, observational reductionism may take one of two pos-

sible forms within the above traditions: either a functional or a structural mode of interpretation. (It appears as though Wisdom is not using the term "interpretation" in the technical sense intended by say, Carnap, to specify the procedures used to form connective sentences between the L_o and the L_t. Some of his uses of the term approximate this meaning, whereas others do not.)

A functional mode of interpretation is used both by the instrumentalist and conventionalist traditions in performing logical and mathematical operations on the observation language of scientific discourse. For to the instrumentalist, scientific theories are regarded exclusively as instruments, neither true nor false, but only more or less efficient for specific computational purposes. Similarly, conventionalism disavows any intentions of using theories to uncover the true state of the world, which is not considered as a logical possibility, but is, rather, interested in the design of appropriate theories to cover the limited range of the given observable phenomena. Both traditions emphasise the importance of correctly handling observations, which leads Wisdom to describe them as methods based on <u>observation-manipulation</u>. The method of induction, on the other hand, is based on <u>observation-construction</u> which is a quite distinct mode of interpretation.

> Inductionists do not try merely to manipulate observation: they want observations to grow into something, to grow into a truth wider than themselves, in other words they want to make observations produce theories.
>
> Wisdom, (1971:215).

Both these methods are, therefore, premised on the assumption of the "ultimacy of observation", with the structural mode of interpreting observations resembling a calculating machine, and the functional mode having more in common with a jigsaw puzzle, (p.216). The problem for Wisdom is, however, to show that there is not ultimate state of uninterpreted observation, and to point out the consequences of this fact for the investigative methods based upon these premises. Unfortunately he gets no further than a simple elaboration of some of these questions leaving aside any detailed reconstruction of an alternative model of scientific discourse.

> Once the ultimacy of observation is given up - without insinuating the ultimacy of theory - we may see the structure of science not as a gap to be bridged from data to theory, but as a theoretical structure with observational layers with the boundary shifting according to the problem confronted.
>
> Wisdom, (1971:220).

(ii) <u>Arguments for Retaining the Distinction in a Modified Form</u>

One writer who has recently responded to criticisms of the L_o, L_t distinction is Ernest Nagel (1969). Although Nagel makes it quite clear from the outset that he is not

prepared to defend any absolute expression of the binary language model, he is of the opinion that, with certain qualifications, the distinction between observation and theory is a useful one. His paper is very much concerned with offering a criticism of the current arguments against the established binary language model, particularly those which he regards as eventuating in a "sceptical relativism".

His argument for preserving the L_o, L_t distinction is a familiar one for most empiricists and consists of showing the necessity for criteria of justification. Even if the distinction between L_o and L_t is not an absolute one, he argues, the final need for being able to distinguish observational predicates form theoretical predicates is given in the process of validating a theory. For theories must be reducible to the level of publicly justifiable discourse in order for their claims to be objectively validated. And although such a language may not be totally free of "theory-loading", Nagel argues that it is based on observational predicates to a much greater degree than the theory language.

Thus the differences between the L_o and L_t are not absolute distinctions of kind, but are rather relative distinctions of degree. From this position, Nagel claims that he thereby avoids the polar extremes of either a rigid dichotomization of scientific discourse into two separate languages

(and ontological domains), or the other extreme of sceptical relativism, which denies the objective basis of any comparison whatever between different (incommensurate) theories.

Nagel, therefore, handles the L_o, L_t distinction with kid gloves. He makes it abundantly clear that his employment of the distinction is primarily heuristic (or, as he says - 'pedagogic'), and is preserved for want of any more useful alternative.

> It would be idle to pretend, however, that there are no difficulties in drawing a distinction between observational and theoretical statements; and I certainly do not know how to make such a distinction precise. Nevertheless, I do not consider that this distinction is therefore otiose any more than I believe that the fact that no sharp line can be drawn to mark off day from night or living organisms from inanimate systems makes these distinctions empty and useless.
>
> Nagel, (1969:19).

In turning to the major criticisms levelled at the L_o, L_t distinction, Nagel opens his case for retention by accusing the critics of basing their argument on two false assumptions which they mistakenly attribute to most users of the L_o, L_t distinction. These two assumptions are given as the alleged opacity (non-observability) of T_t's and the absolute separation of L_o from L_t.

The distinction between T_o's and T_t's cannot be purely and simply based on the observability or non-observability of the respective predicate designata. For as numerous previous

writers have been at pains to point out, what are theoretical terms in one context of use may very well become observational predicates in another context. To accommodate this qualification, therefore, Nagel insists that, although any given term may cross the threshold from theoretical to observational and back again, in different situations, we may still differentiate the functions that different predicates perform in any specific context. So the fact that a term may change its meaning over different contexts of its use, does not prevent us from isolating its stable meaning <u>within</u> each of these contexts. On this basis, therefore, it is meaningful to analytically distinguish between observational and theoretical predicates by examining the <u>role</u> they play in the conduct of scientific inquiry. Thus theoretical predicates "...<u>prescribe</u> how the things identified in gross experience with the help of observation terms are to be analysed", at the same time they also serve as inferential <u>links</u> to connect the given product of observation with the constructed product of generalisation and abstraction. Observational terms, on the other hand, have a separate series of functions to perform, which include the tasks of defining the perceptual field, establishing family resemblances between observables, describing instrumentation, reading measurements and codifying data. The most important qualification for using these

criteria, as Nagel again reminds us, is to remember that given predicates may only be assigned an observational or theoretical status within a specific context of use. When this context changes, the epistemological (and ontological?) status of these terms may also change. This general argument is used by Nagel to show that even if certain criticisms of the binary language are accepted, (e.g. Hesse's concept of "entrenchment") they do not demonstrate any logical necessity for abandoning the L_o, L_t distinction. Indeed, for specific realms of experience, certain T_o's remain stable over different theories: to this extent they may be regarded as "core" predicates. For if we were confronted by a genuine and extreme case of incommensurable theories, suggests Nagel, where the theoretical frameworks uniquely assign total meaning to observational predicates within their jurisdiction, then we would be unable to compare such theories. Because, in order to empirically compare two competing theories it is necessary to find some observational predicates which are common to both, and can be used as a standard test of their respective truth conditions. Therefore, Nagel concludes, the very possibility of continuing empirical science is rejected by those writers who criticise from a standpoint of sceptical relativism.

Another influential writer who has defended the dis-

tinction between observation and theory, but in a modified form to accomodate recent criticisms, is Grover Maxwell, (1961, 1962). He like many other contemporary writers, disowns the older and more rigid dichotomisation of L_o and L_t, especially in the form which assumes that theoretical terms are merely "convenient fictions" and should be translatable without remainder into observational terms. Maxwell has been particularly anxious to show that unreasonable ontological consequences result from the attempt to establish absolute logical distinctions between the various functions of language in scientific discourse.

For any attempt to establish an absolute boundary line between observational and theoretical terms is bound to be an arbitrary distinction, fulfilling some criteria of significance and rejecting others. Maxwell argues that such a rigid dichotemisation of observational and non-observational terms has the effect of granting reality to some entities and denying reality to others under conditions which are neither consistent nor rational.

> Certainly, we will often find it convenient to draw such a to-some-extent-arbitrary line; but its position will vary widely from context to context...But what ontological ice does a mere methodologically convenient observational-theoretical dichotomy out? Does it attain physical thinghood and-or "real existence" in one context only to lose it in another? Or, may we ask, recalling the continuity from observable to unobservable, is what is seen through spect-

> acles a "little bit less "real" or does it "exist to a slightly less extent" than what is observed by unaided vision?"
>
> Maxwell, (1962:8).

He rejects, in particular, what he regards as two traditional criteria for classifying statements as either observational or theoretical, the criterion of direct observability, and the criterion of observability in principle. For to stipulate that all theoretical sentences and their constituent terms must be reducible to elementary observational terms, terms, that is, which have a direct connection with the "real world" leads us into familiar problems. Do terms like "hydrogen molecule" have a different ontological status from terms like "salt molecule", where the first can only be observed with instrumentation, whereas the second is capable of unaided observation? If this is so, then it evidently leads to all sorts of unreasonable consequences, such that two stars X_o and X_u, one of which is observable and one which is not (without instrumentation) have greater and lesser degrees of reality, respectively. It also means that whereas X_o as an observational predicate may be given semantical rules, X_u can only have its meaning established by the total body of theory of which it is a part. Thus the criterion of direct observability breaks down as any reasonable criterion for the cognitive significance of statements, says Maxwell,

on account of the unacceptable ontological consequences that follow from such a criterion.

On the other hand, the criterion of observability in principle is also an unsatisfactory way to classify the different logical functions of scientific discourse. For what may be unobservable in principle at one point in time may become observable at another point, given the disclosure of new associated phenomena and the development of observational instrumentation. Hence many objects, from elementary particles to extra-terrestial bodies, have at one time been assigned the status of "convenient fictions" before an advanced instrumentation made them available to an aided observation, and thus certified their material reality. Maxwell concludes from ~~this~~ backward look into the history of science that there is no logical necessity for electrons (for example) to remain unobservable in principle, notwithstanding the uncertainty principle. The difficulty in certifying their material reality is essentially an empirical problem of measurement, a problem of learning to simultaneously compute momentum and position.

This leads to the requirement for a more lenient criterion for distinguishing T_o's from T_t's. Instead of the more rigid dichotomisation of the binary language model based on direct observation, observation in principle, or any such

similar classification, Maxwell proposes a pragmatic distinction. He argues that observational sentences should be taken as "quickly decidable" sentences, propositions whose truth conditions may be subjected to quick and easy judgement.

> We should take (as the [illegible] observational base of science) not 'o[illegible]nal term' but rather, the quick[illegible]deci[illegible]sentence....a quickly decidable sent[illegible] on[illegible]echnical sense employed here) may [illegible]defi[illegible]as a singular, non-analytic sentence [illegible]h t[illegible] a reliable, reasonable sophisticated [illegible]ngu[illegible] user can very quickly decide whether to a[illegible]r deny it when he is reporting on an occu[illegible] situation. 'Observation term' may now b[illegible]ined as a 'descriptive (non-logical) term wh[illegible] may occur in any quickly decidable sentence'.
>
> [illegible]axwell, (1962:13).

Thus Maxwell chooses a [illegible]agmatic definition of the realm of the observable (whic[illegible]e attributes to Feyerabend) and which, of course, was also used by Carnap in his earlier days. His purpose is to retain[illegible]e usefulness of the concept of an observational base, "fo[illegible] is absolutely necessary as a confirmation base for statements which do refer to entities which are unobservable at a given time".

At the same time, however, he seeks to avoid the unreasonable consequences of the more rigid and classical dichotomy. For the consequence which appears most unreasonable is that of granting or denying predicates the status of "reality" on the condition of their observability. To Maxwell, therefore, theoretical entities do not exhibit a different onto-

logical status which makes them "less than real" or "convenient fictions" or whatever. They must be assigned the same ontological status as observational predicates, regardless of the violence done to traditional metaphysical criteria of reality.

(iii) Arguments for Turning the Established Distinction on its Head

Without a doubt, the most radical criticisms of the established binary language model of scientific discourse comes from the historical tradition of the philosophy of science. This tradition, is, perhaps, well represented in the writings of Kuhn, Feyerabend, Hanson and Toulmin. The point of this chapter, however, has been to show that (i) criticisms of the observation-theory distinction go back to the origins of logical empiricism, and are not limited to the more recent debates. (As Giedymin, 1970, has observed, earlier discussions in favour of the L_o, L_t distinction tended to be strong on logic, whereas later criticisms of the distinction tended to be stronger on history.) And (ii), that there are two types of criticism of the observation-theory dichotomy: an argument for the erasure of the distinction, and an argument for the transformation of the distinction.

Too often, it seems that the heterogenous nature of the criticism of the binary language model is forgotten, and the

recent arguments of Kuhn, Feyerabend et al are introduced as the only alternative positions to that of total acceptance or the observation-theory distinction. We have tried to show, however, that things are never quite so clear-cut, and that there (at least) two classes of consequences that derive from the respective critical positions.

There are some significant disagreements within, what we have chosen to call the historical tradition in the philosophy of science, over a number of related topics. Toulmin, (1967a, 1967b), for example, has suggested that an evolutionary model stressing differentiation and selection more closely approximates the historical growth of scientific knowledge than does Kuhn's model of paradigm revolutions. More irreconcilable, perhaps, has been the disagreement between Kuhn and Feyerabend over the type of methodological research programme recommended to the special softness: whether or not the pattern of past scientific development signals a corresponding pattern for the future. Some of these disagreements will be more fully discussed in the next chapter, where their possible ramifications for the social sciences may also be considered. For the moment, however, we shall conclude this chapter by attempting to summarise the common points of departure that these historical writers exhibit in their treatment of the binary language model of scientific discourse.

It will become apparent that although most of these writers reject the basic cannons of the logical empiricists, they share with them a preoccupation with the importance of language for coming to a closer understanding of the problems of knowledge.

The attitude of the historical writers towards the established distinction between observation and theory may, perhaps, best be introduced through Thomas Kuhn's notion of incommensurability. For, as is well known, Kuhn has argued that at certain critical points in history the established rules for puzzle-solving in scientific research undergo a distinct revolution and are replaced by an alternative set of rules. These sets of rules, or paradigms, contain the "law, theory, application, and instrumentation which together provide models from which spring particular coherent traditions of scientific research", (Kuhn, 1970a:72). In the intervals between such revolutionary crises, these coherent traditions of research provide the organised framework for the everyday practice of what Kuhn call "normal science".

In this way the growth of knowledge in the scientific disciplines can be described as a series of revolutions in which old paradigms are usurped and new paradigms are enthroned. The novelty of this particular interpretation of scientific development lies in the fact that it is proposed

as an alternative to the conventional description of the growth of science, which has traditionally represented the advance of knowledge as an evolutionary and accumulative process. Therefore, where previous writers have described the replacement of one scientific theory by another in terms which are meant to suggest that the new theory contains the old theory as a limited case of a more general law, Kuhn suggests that the two theories are simply unequivalent, or incommensurable. He makes clear that what he means by this is that the old theory cannot be totally incorporated into the new theory without remainder, and that some of the operations performed by the old theory will be lost in the new.

Instead of viewing the growth of knowledge as an inevitable march of progress, therefore, Kuhn implies that such progress is largely illusory. For each historical epoch only sets for itself such questions as it can answer, and this it does in its own unique way.

Setting aside, for the present, many of the controversies surrounding Kuhn's historiographical method and conclusions, we shall briefly examine the consequences of his analysis for the observation-theory distinction and the binary language model. There are two senses in which Kuhn intentionally uses the term 'paradigm', a sociological and a philosophical sense, (although other critics such as

Masterman, (1970), have managed to distinguish as many as twenty-one different uses of the term).

> On the one hand, it stands for an entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science.
>
> Kuhn (1970a:237).

When paradigms undergo the fundamental transformations that characterise the periods of revolutionary science, therefore, everything within the paradigm changes. This includes not only the material factors of technology and instrumentation but also the symbolic factors of language and conceptual frameworks. Between incommensurable paradigms, Kuhn suggests, there may be little outward change in the vocabularies of the two systems, although the meanings attributed to the new terms will have been greatly altered, (1970a:267). Thus, even in those cases where it is possible to point to a term which was present in T_1, and to show its persistence in T_2, the significance of the term has changed for its meaning has been reaxiomatised.

There is, therefore, no <u>neutral</u> observational language available to scientists working within incommensurable paradigms, for the very observational terms used in making an empirical justification of different theories have had their

meanings established within the context of these theories, (Kuhn 1970a:263). For this reason it follows that the theories represented in the great historical breakthroughs in science cannot be justified on the basis of their correspondence to an independent reality, for the way such a reality, itself, is perceived is dependent upon the methods and theories contained in the historical paradigm. The data of observation, in other words, are dependent upon the investigative languages used in the practice of scientific research.

It may be asked, in response to Kuhn's thesis of incommensurability, (as, for example, in Kordig, 1971), how any comparison between different theories is possible if there is no common observational language available? Kuhn's answer (1970a:264) is that communication between scientists within incommensurable paradigms is possible through a process of translation. By first establishing which theoretical terms are still held in common, they may next proceed to clarify those terms which are used unproblematically within paradigms but are the source of misunderstanding between paradigms. Finally, the residual problem terms may be expressed through the natural languages available to scientific communities, and by this series of translations it is possible for scientific working within different paradigms to understand each other's theoretical vocabularies, (1970a:264). Kuhn, however,

makes it quite clear that in his estimation the only real communication problem involved between the participants in different paradigms is not that of learning the new vocabulary, but in translating it, (1970b:267).

The consequences of Kuhn's analysis for the binary language model are, therefore, more radical in intent than either of the previous sets of criticism. For instead of abandoning altogether the observation-theory distinction, he is prepared to turn it on its head and assert the primacy of theory over observation. He dismisses in a single paragraph, for example, any pretext for retaining the concept of an observational language in the traditional sense of the term.

> That such a language (i.e. an observational language) lies ready to hand has been widely assumed since at least the seventh century when philosophers took the neutrality of pure sensation-reports for granted and sought a 'universal character' which would display all languages for expressing them as one. Ideally the primitive vocabulary of such a language would consist of pure sense-datum terms plus syntactical connectives. Philosophers have now abandoned hope of achieving any such ideal, but many of them continue to assume that theories can be compared by recourse to a basic vocabulary consisting entirely of words which are attached to nature in ways that are unproblematic and, to the extent necessary, independent of theory...I have argued at length that no such vocabulary is available.
>
> Kuhn, (1970b:266).

Kuhn argues, in fact that the observation data of the world are "cut up" by the theoretical languages, and because

of this "we have no access to a neutral sun-linguistic means of reporting", (1970b:268). Therefore, the consequences of adopting a particular theoretical language entails a commitment towards a restricted way of observing the world. The empirical criteria used for the justification of scientific theories are made available through the theories themselves. For, "in the absence of a neutral language, the choice of a new theory is a decision to adopt a different native language and to deploy it in a correspondingly different world", (1970b:277). Much of Kuhn's discussion of the observation-theory distinction, however, has had a tendency to remain subordinated to his more central concern, that of the discontinuous character of the growth of knowledge. And although his opposition to the established binary language model is clearly revealed in both his analysis and criticism of traditional interpretations of the growth of science, it has remained for other writers to further develop the historical critique in this direction. Thus, Paul Feyeraband has emerged as one of the more eloquent critics of the binary language model, (or what he has called the 'layer-cake model') and in the course of his assaults upon this position has elaborated a 'pragmatic theory of observation'. The pedigree of these ideas, as well as those of Kuhn, dates back to the earlier attacks on radical empiricism made by Karl Popper, (see for

example, Popper, 1968:59 footnote). There are, however, great disagreements between these writers on other issues relating to the recommendation of methodological programmes for the special sciences, and these will be more fully discussed in the next chapter.

Both Kuhn and Feyerabend are united in their opposition to the models of scientific discourse which stress the dependency of theories on observational facts. Feyerabend, (1962, 1968, 1970a, 1970b) has argued consistently that all such 'facts' can only properly be thought of (to use Hanson's expression) as "theory-laden". In other words, it is only through the formulation of new theories that new realms of 'facts' can be brought into the new observational categories which then become available. He therefore enjoins empiricists, as those with an interest in examining observational data, to constantly seek to extend the range of such data by freely engaging in the formulation of new theories. For it is only in the course of elaborating new theories that new facts may be generated.

Feyerabend's method of argument consists in showing how many of the significant breakthroughs in the history of science violated the presently restrictive cannons of empirical research. Thus in his analysis of the shifts from geocentric to heliocentric cosmologies, represented in the re-

placement of Ptolemaic by Copernican and Galilean astronomies, he shows that the empirical evidence offered in support of the new ideas was secondary and anterior to the elaboration of the theories themselves. In many important senses, therefore, we may see that the Galilean arguments for the thesis that the earth relates upon its own axis could not be empirically justified in terms of the range of observational facts available to the contemporary astronomers of that period, such as Kepler or Tycho Branche. It was only through an application of the new theories that new categories of observation were imposed upon the traditional domains of experiential data, (see Feyerabend 1970a, 1970b). (This same example is also used by Reichenbach, but to very different effect; see Reichenbach 1968:96-98).

Thus Feyerabend's claim is that what has been traditionally sought by empiricists as a secure epistemological foundation for scientific knowledge, namely, a neutral observational language, is in principle, unavailable. For all observational languages contain within them a series of unanalysed assumptions which derive from older theories, metaphysical systems, and ordinary discourse. Because we cannot eliminate those contextual influences from scientific (or any other) language, Feyerabend suggests that we make them explicit in the unrestricted formulation of new theories.

This much agrees with the conclusions arrived at by Kuhn and other writers who have championed the Historical method as an indispensable means of clarifying the relation between the philosophy of science and the recorded practice of scientific research. Feyerabend, however, proceeds to outline an alternative theory to the binary language model which he calls the "pragmatic theory of observation". And it is these ideas which have the most direct relevance to the controversies involved in the observation-theory distinction.

For Feyerabend has argued that whereas the observational sentences in an observational language do not reflect the empirical content of any independent reality, they do represent the established practice of a given historical and behavioural system, (a system composed of sociological, philosophical, psychological, physical aspects, et cetera). In this sense, therefore, observational sentences may be distinguished from other sentences in a theory by the causes of their assertion, rather than by the content.

> Now it is most important to realise that the characterization of observation statements implicit in the above quotations is a causal characterization, or if one wants to use more recent terminology, a pragmatic characterisation: an observation sentence is distinguished from other sentences of a theory, not as was the case in earlier positivism, by its content; but by the cause of its production, or by the fact that its production conforms to certain behavioural patterns. This being the case, the fact that a

> certain sentence belongs to the observation language does not allow us to infer anything about its content; more especially, it does not allow us to make any inference concerning the kind of entities described in it.
>
> Feyerabend, (1962:36).

What is made available through the expression of the observational language, according to Feyerabend, cannot be described as a connection of observational terms to some uninterpreted reality in the world, but rather as a routinised way of looking at the world which has become codified into a semi-permanent, (but far from immutable) language. In an important sense, it seems, that Feyerabend has adopted a behaviouristic principle where the L_o has become the response which each observer, or class of observers, makes in the shifting contexts of practical stimuli. Thus theories are of paramount importance, and the L_t's of scientific discourse predetermine the categories through which observation may take place, and fix the boundaries within which the realm of 'facts' is open to empirical inspection. However, Feyerabend's method is not without internal contradiction, as several writers have sought to demonstrate, (Leplin, 1969; Townsend, 1970; Kordig, 1971; Koertge, 1972). But further investigation of these problems must be left for later discussion.

In terminating this brief review of the more recent criticisms of the binary language model to have emerged from the Historical tradition, we can do little more than identify

the remaining representatives of this tradition. For to undertake any serious exposition or comparative analysis would necessarily require more time and space than present circumstances permit. It remains only to indicate the interest that other writers have shown in the controversial distinction between observation and theory.

Toulmin, for example, states the difference between the empiricist philosophy of science and his own, in characteristically succinct terms.

> For the great divide in twentieth century philosophy of science is that between the Platonisers who seek to find in science (or to impose on it) some permanent 'structure' or 'system', and those of us who see the crucial problem, instead, as being to discover the rationale of scientific change.
>
> Toulmin, (1969:199).

The thrust of any criticism of the empiricists' criteria for dichotomising observational terms and theoretical terms must come, suggests Toulmin, from revealing the internal inconsistencies in the empiricist programme. Chief among these is the attempt to make "the sensory observation on which we rely for evidential purposes serve, at the same time, as uninterpreted descriptions of the contents of our sensory fields". (1969:204).

This raises essentially the same class of objections that Feyerabend has made, which is hardly surprising as both

writers continue to acknowledge each other with predictable regularity. However, what is noticeable in the presentation of these various positions is that whereas Feyerabend insists that the manifold of observable experience is organised for human cognition through the formulation of different theories, Toulmin, and also Hanson, place a greater emphasis on the importance of language. For although languages universally contain tacit and informal theories about the world, their pervasiveness is more extensive and more fundamental than the explicit categories laid down by more formalised theories. Similarly, for Hanson, the influences that are embodied in language function as "spectacles that we wear behind the eyes". And it is in this sense of everyday language, as much as of specialised theoretical languages, that leads Hanson to describe observation as a "theory-laden" operation.

> Facts, true statements, and causal connections are all the way they are because the world is what it is. Were the world different, our ideas about facts, truth and causality would be profoundly different. But this is not to say that facts, truth and causality are somehow built into the world like great pieces of terrestial furniture. They are, as I have urged, to a large degree built into the structure of our language. They are not for this reason, however, subjective, chimerical notions.
>
> Hanson, (1969:312).

These then are the major representatives of what we have called the Historical tradition. Their ideas have been

powerful influences in challenging the established view of the binary language model, which was so highly lauded during the earlier part of the twentieth century.

The debate in which they have participated is not yet over, and a backward look at the history of other longstanding problems in the philosophy of science suggests that it may never be decisively concluded.

Summary

The central problem of this chapter has been to identify the various ways in which facts and theories may be said to be connected in scientific discourse. That there is no unanimity among philosophers on these matters is evident from the foregoing account. To simplify matters, therefore, we have proposed that the connections between facts and theories can best be elucidated by examining the relations between terms of the observational languages and terms of the theoretical languages of scientific discourse. We have called the model which expresses the relations between these two sets of vocabularies, the binary language model.

There are, it seems, at least three possible variations on the binary language model. Firstly, there is what may be called the original binary language model, which dates back to the start of logical empiricism. But whereas the older version of logical empiricism demanded that the L_O of scien-

tific theories be used as a basis for the potentially complete elimination of theoretical terms, the more recent versions are considerably weaker. Nowadays, the L_o is demanded mainly as a means of justification, as a relatively stable language which can serve as a basis to compare different theories for their empirical contents and respective truth conditions.

Secondly, there is the possibility of abandoning the binary language model altogether. Arguments commonly used to support this proposal do so through disclosing the logical inconsistencies involved in trying to distinguish observational from theoretical terms.

And thirdly, as we have also seen, there is the possibility of replacing one form of assymetry in the binary language model - the ultimacy of observation - by another, the ultimacy of theory. These two polar variations of the binary language model we shall refer to as observational reductionism and theoretical reductionism, respectively.

CHAPTER III

THE RELATIONSHIPS BETWEEN MODELS OF SCIENTIFIC DISCOURSE AND PROGRAMMES FOR THE GROWTH OF SCIENTIFIC KNOWLEDGE

Introduction

Having separated the three main logical variations on the binary language model of scientific discourse, we turn now to an examination of how two of the polar variations on this model, observational and theoretical reductionism, are related to some different current programmes for the growth of scientific knowledge. For our present purposes, therefore we have selected three contemporary writers each of whose work indicates a preoccupation with a particular programme for the growth of scientific knowledge.

The character of each of these programmes has been greatly influenced by the underlying theory of science which each writer has developed during the course of his work. Thus the task in this section of the thesis is to identify the theory of science to which each particular writer is committed, and to examine the implications of these theories for a model of scientific discourse and a programme for the growth of knowledge. It will be argued throughout, that an adequate programme for the growth of scientific knowledge cannot be satisfactorily grounded on a reductionist model of scientific language, whether observational or theoretical, but

must somehow provide the wherewithal to resolve this long-standing contradiction in traditional theories of scientific knowledge. The three writers whose programmes for the growth of scientific knowledge form the material for our comparison are Thomas Kuhn, Paul Feyerabend, and Jurgen Habermas. The three programmes associated with these three writers have been identified respectively as The Programme for Paradigm Closure, The Programme for Paradigm Proliferation, and The Programme for Paradigm Complementarity.

Although, in a sense already alluded to in the previous chapter, these three writers have frequently been indiscriminately labelled either as Historical philosophers of science or as Radical philosophers of science, there is another justification for their inclusion here. For the very different implications that arise from these separate programmes for the growth of knowledge illustrate not only the considerable differences that exist between these writers, although these are often concealed under the more common stereotypes, but also a convenient polarisation of views which is most instructive for our main purpose. This purpose remains throughout the single one of showing the relationships between programmes for the growth of scientific knowledge on the one hand, and models of scientific discourse on the other.

The Programme for Paradigm Closure

The account of the growth of scientific knowledge set out in the writings of Thomas Kuhn is at once radical and conservative in its implications and, therefore, appears to satisfy concurrently two incompatible ideologies of our time. In one sense we may say that it is radical in the use of the historical method of investigation, but conservative in terms of the theory of science which underlies Kuhn's programme for the growth of scientific knowledge. What exactly do we mean by this distinction?

It has already been shown that what has been called the 'Historical School' represents one of the main challenges to the more traditional orthodoxies in the philosophy of science. Writers such as Kuhn, Feyerabend, Hanson and Toulmin have succeeded in challenging the normative method by which the earlier established philosophy of science had traditionally been practised. For whereas the previous generation of philosophers such as Russell, Carnap, Hempel, Feigl, and so forth, sought to investigate the problems of scientific knowledge by means of a logical or mathematical method alone, the new generation re-established the historical method as a further, and at times an alternative, means of investigating these and other problems. This distinction has been remarked by a number of commentators on the historical development of the

philosophy of science.

> ...there is one important sense in which the Carnap of the Philosophical Foundations of Physics is a kindred spirit with the Aristotle of the Posterior Analytics. Each reduces the philosophy of science to a logic of science, to the study of science, considered as a formal system. The only fruitful methodological issues, therefore concern the way in which the different propositions in the system are related to one another, the types of inference used to validate one proposition on the basis of others.
>
> McMullin, (1970:13).

Giedymin (1970) has also drawn attention to the differences of method which separate many of the previous generation of philosophers of science from many of the present generation.

> A confrontation of the discussion of the problem of comparability of theories thirty-five years ago with its treatment in recent contributions reveals at least the following difference: the older approach was logically far more refined and sophisticated, whereas recent contributions are much richer in historical detail.
>
> Giedymin, (1970:257).

Thus the use of the historical case-study has again come into prominence. For it has been through an historical re-examination of the scientific practices of some of the great and eminent scientists of the past, that many of the traditional beliefs governing the theory and practice of science have been brought into question. In this sense, therefore, Kuhn may be seen as an opponent of the methodology of logical empiricism which has attempted to reduce all signifi-

cant problems of scientific knowledge, and indeed all problems of knowledge in general, to the level where they may uniformly be analysed as problems connected with the logical structure of propositions. By restoring the historical criticism of knowledge and its foundations as a further means to be employed in the current practice of the philosophy of science Kuhn cannot, therefore, escape partial identification with a philosophical tradition that includes among others Hegel, Dilthey, Croce, Collingwood, Mannheim, Koyre, and Bachelard. It is a tradition which stands in profound opposition to that of logical positivism.

At the same time, however, it can be shown that although the method of investigation used by Kuhn stands in clear contrast to that of the previous generation of philosophers of science, the theory of science that he uses is much less hostile to logical positivism than others who have adopted the historical method. In fact, many of the distinctions established by the logical positivists for classifying knowledge are retained by Kuhn, although the earlier justifications for using them are replaced by new arguments from Kuhn's own theory of science.

The importance of Kuhn's theory of science is that it underlies his programme for the growth of scientific knowledge, and implicitly defines what he takes to be an adequate

model of scientific discourse. It is these two aspects of Kuhn's work that we shall now undertake to critically examine in greater detail. For our present purposes, we shall distinguish between the two main parts to Kuhn's theory of science which are, (i) the theory of the transformation of normal science to revolutionary science, and (ii) the theory of the growth of non-science, or proto-science, to mature science. It will be argued here that there are serious inconsistencies between these two parts of Kuhn's general theory of science, and that these have led to certain undesirable (and unreasonable) consequences in his overall conclusions about the necessary conditions for the growth of scientific knowledge as well as his implicit definition as to what may or should serve as an adequate model of scientific discourse.

(i) The Transformation from Normal Science to Revolutionary Science

The special part of Kuhn's general theory of science which deals with the transformation from normal to revolutionary science has probably caused more controversy than any other part. In order to illustrate its significance, therefore, for Kuhn's programme for the growth of scientific knowledge, it is necessary to briefly recapitulate his account of this transformation.

Kuhn argues, it will be recalled, that much of the past and present activity of scientists has always consisted of the routinised research and experimentation that is carried on within a particular cognitive tradition. It is in the context of such a tradition that the major theoretical problems of the scientific community are recognised by its members, and through which the methods and instrumentation are provided for the research and resolution of these problems. Scientific practice, for the most part, suggests Kuhn, is carried out as a co-operative and social enterprise, and the problems and puzzles to which scientists find themselves, drawn are the product of an elaborate division of labour which is set up within the context of the current research tradition. It is to the total set of theories and methods which make up this tradition that Kuhn gives the title of 'paradigm"! Normal science, therefore, always proceeds within the context of a given historical paradigm.

The routine and normal practice of science, in Kuhn's eyes, is practised within an institutionalised cognitive and social tradition, or paradigm, which supplies the problems and puzzles for normal scientific research. The set of principles and beliefs which govern research activity within the paradigm is generally accepted without debate by all scientists of the research community, for they have been similarly

socialised into its traditions and share, therefore, a commonly held view of the past history as well as of the present priorities of their discipline. Normal research within a paradigm does not, therefore, critically reflect upon the entrenched theories and methods which constitute the foundations of knowledge within the scientific speciality. These are, for the most part, accepted as non-problematic.

For this reason, Kuhn goes on to add, there is an absence of any real testing (falsification) within the normal research tradition, for the relevance of puzzle-solving in terms of the paradigm is much greater than that of testing for normal scientific practice.

The difference between the process of testing and that of puzzle-solving, as Kuhn makes clear on a number of occasions, (1970b:5) is that whereas the former indicates a criticism and debate over the fundamental theoretical and methodological premises of research, the latter is simply a measure of the success of an individual scientist working within these premises. Only when these premises are brought under serious review can the routinised pattern of normal science be said to undergo an interruption.

Such interruptions in the normal pattern of scientific practise, according to Kuhn's account, have occurred periodically throughout the history of science. They are brought on

by the failure of normal science, at certain times, to successfully resolve such problems as have remained refractory to the range of solutions supplied by the extant paradigm. Over a developing period of time the crisis of the paradigm becomes steadily more evident in the visible multiplication of those anomolies which cannot be fitted into the pattern of puzzle-solution that has traditionally succeeded in the science. It is only at this time that the paradigm, itself, comes under a new and critical examination from the scientific community, and the theories and methods which were once uncritically accepted become again the centre of debate. This is a period that Kuhn has called "Extraordinary Science".

> All crises begin with the blurring of a paradigm and the consequent loosening of the rules from normal research.
>
> Kuhn, (1970a:84).

The result of such a breakdown in the capacity of scientific paradigms to generate puzzle-solutions is their eventual replacement by another paradigm. This paradigm will have the advantage of being able to resolve the anomolous problems that lend to the damage of its predecessor, although some of the earlier solutions to research problems may again appear as re-problemated. Not all of the successful research tradition of the past paradigm can be conveniently fitted into the structure of the new, there may be a large but never a

complete overlap of successive paradigms. (Kuhn 1970a:85).

There may also be a considerable discontinuity between scientific paradigms and, in fact, Kuhn goes so far as to suggest that on some occasions, and in some ways, the relationship between an old paradigm and the new may be one of incomparability: the two paradigms may be incommensurable to each other. The times of scientific revolution are, therefore, times when a total change in world-view converts the community of scientists to a new way of perceiving the world which may be greatly at odds with what went before. Such an acknowledgement has significant implications both for the reconstructed history of science as well as for a model of scientific discourse.

The notion of paradigm incommensurability has some important consequences for a model of scientific discourse. To begin with, it serves as a criticism of any model of language that is based on an observational reductionism. For at a time of paradigm revolution it is the observational language as well as the theoretical language of a research tradition that is replaced. The replacement of one observational language by another, Kuhn carefully adds, has not necessarily meant the replacement of all the old scientific terms by new ones. What has frequently happened is that the meanings formerly attached to principal terms such as 'mass', 'velocity',

'acceleration' and so forth, have been replaced by new meanings, although the terms, themselves, remain linguistically unchanged. Thus those terms which were used in an earlier paradigm tradition may become re-axiomatised which as to say that although the established terms, themselves, remain unchanged the way in which they are now defined and the objects of their prediction may be quite at odds with their earlier usage.

> Two men who perceive the same situation differently but nevertheless employ the same vocabulary in its discussion must be using words differently. They speak, that is, from what I have called incommensurable viewpoints...They cannot, that is, resort to a neutral language which both use in the same way and which is adequate to the statement of both their theories or even of both those theories' empirical consequences.
>
> Kuhn, (1970a:200-201).

Thus, where previous accounts of the growth of science have assumed a semantic uniformity in the historical use of conventional scientific terms, Kuhn argues for the recognition of an actual semantic relativity of terms between different and incommensurable historical paradigms.

Kuhn, therefore, explicitly rejects the idea that there exists anything like a neutral observation language, which is constantly available to scientists at all times and in all places, and to which all reports about natural phenomena may ultimately be reduced. For, as we have seen in the previous

chapter, Kuhn argues that the re-axiomatised terms of a new observational language are supplied in the context of the new paradigm, and not by reference to some unchanging correspondence to an independent reality. To this extent, Kuhn's model of scientific discourse is grounded upon the ultimacy of theoretical languages over observational languages, and is a model based on a theoretical reductionism. With a transformation from the research tradition of one paradigm to that of another, there is a radical transformation in the use of scientific terms. It is a sufficiently radical change, says Kuhn, to warrant description as a new language, or what may perhaps call, a new paradigm language.

> In the absence of a neutral language, the choice of a new theory is a decision to adopt a different native language and to deploy it in a correspondingly different world.
>
> Kuhn, (1970b:277).

Thus for Kuhn, the relationship between knowledge and reality is subject to abrupt and discontinuous change. And it is language that predominantly mediates this relationship, for within the categories of language lie the codes which have been established by the implicit cosmologies of particular cultures as well as the well formulated theoretical systems of organised science. It is for this reason, therefore, that what the observational language of one paradigm may accept as evidence, or data, or even as 'facts', may not be ac-

cepted as such by an earlier or a later paradigm language. The structure of the 'real world' is thus pre-constituted for us by the structure of the paradigm language of a given research tradition and as history records, these structures are never universal, permanent or final but are always historical because they are man-made.

This then is, perhaps, the most fundamental criticism that Kuhn makes of the previous generation of philosophers of science and the original reductionist model of scientific discourse: the rejection of a belief in the possibility of a neutral observation language. For without such a stable foundation for scientific knowledge, the meanings of scientific terms can no longer be taken as constant over different historical periods. Once, therefore, the possibility arises that scientific terms may vary their meanings at different times throughout the history of science, then the possibility also arises that the development of scientific knowledge may no longer be realistically characterised as progressive and accumulative.

Thus Kuhn's criticism of the observational reductionist model of scientific discourse leads him to a rejection of the previously established history of science. For much of the recorded history of science, in Kuhn's view, has presented a misleading and mistaken picture of the actual growth of

scientific knowledge. This has been the case for the many philosophers and historians of science who have been under the influence of the positivism set down by Comte and Mach and revised by the Vienna Circle. For these writers the growth of scientific knowledge is often described as though it were strictly a logical and empirical and supra-historical enterprise, the systematic collection and progressive accumulation of empirical facts.

The picture of scientific knowledge which emerges from such historical interpretations of its growth, argues Kuhn, is based on a view of progress which sees the activity of science as that which is in the process of gradually uncovering the true state of an independent reality. In other words, as the historical practice of science builds up an ever-increasing supply of empirical facts about the natural world, the time must someday come when the collection of facts is complete, and our experience of nature is fully revealed. Such a picture of science, suggests Kuhn, is teleological in that it supposes that the practice of science has some transcendental purpose above and beyond the immediate need to solve practical and specific problems.

> We are deeply accustomed to seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance. But need there be any such goal?...Does it really help to imagine that there is some one full, objective account of nature and that the proper measure-

> 'ment of scientific achievement is the extent to which it brings us closer to that ultimate goal.
>
> Kuhn, (1970a:171).

It is only by carefully re-examining individual case studies of scientific practice and discovery, says Kuhn, that we may compose a more accurate picture of the real history of scientific development. For it is a history not of the gradual evolution of rational thought towards the ultimate goal of total objective knowledge, but of periodic revolutions in human thought which are frequently incomparable to previous forms of science and are, in turn, liable to become redundant themselves. The real history of science is, therefore, not an unbroken line of intellectual progress, but a series of revolutions each of which succeeds in decisively changing the prevailing standards of scientific theory and practice.

The reputation that Kuhn has enjoyed as a radical opponent of inductivistic theories of science (to use Mary Hesse's term) is, therefore, based on that part of his theory of science which deals with the transformation from normal to revolutionary science.

> An inductivist historian has implicit Baconian philosophical allegiances, concentrates on describing 'hard facts' and experiments, reconstructs past arguments to fit an inductive structure, and judges past theoretical conceptions as true or false, significant or fit for ridicule, depending upon what are now acceptable theories.
>
> Mary Hesse, (1970:141).

For there would seem to be at least three consequences of his account of scientific change and the growth of knowledge which defy the previous accounts given by writers in the established tradition of logical empiricism and neo-positivism. These may be listed as:

(i) His denial of an independent reality, a reality, that is, which exists independently of our theoretical preconceptions as 'the real world' of uninterpreted facts.

(ii) His denial of the possibility of acquiring objective knowledge, knowledge, that is, which reflects an independent reality.

(iii) His denial of the self-image of scientific knowledge as a collection and accumulation of facts which progresses from one period of history to another.

Taken together, these three denials are an important qualification to the more traditional descriptions of scientific knowledge, which have generally emphasised natural science as being the exemplary form of human knowledge. However, as we shall show Kuhn's criticism of natural science is not as fundamental as many commentators have been led to believe. To illustrate this, we shall now turn to the second part of Kuhn's general theory of science.

(ii) The Theory of the Growth of Non-science or Proto-science, to Mature Science

The principal contradiction in Kuhn's general theory of science, it will be argued, is revealed when a comparison is made between his theory of the transformation from normal to revolutionary science, on the one hand, and his theory of growth of mature sciences, on the other. For whereas the theory transformation is essentially a critique of that philosophy of science which has based itself on the positivistic premises of observational reductionism and historical progress, the theory of the growth of scientific maturity stands in quite a different light. If the former part of Kuhn's theory of science may in some sense be regarded as relativistic, then the latter part may more easily be regarded as a linear stage theory of the growth of scientific knowledge.

Kuhn's theory of the growth from what he has called 'proto-science' to mature science may best be illustrated as a stage-theory if a comparison is permitted with another well known stage theory of development. For the theory proposed by Walt, Rostow in his 'Stages of Economic Growth' (1960) is instructively similar in design to Kuhn's theory of the stages of scientific growth. Both writers have claimed that their models of growth are extrapolations from the recorded history of economic and scientific development and are,

therefore, descriptions of these historical forms. At the same time, however, these descriptions constitute the basis for the formulation of future programmes for the growth of economic or scientific institutions. In the case of Rostow, the switch from historical description to contemporary programme is made quite explicit in his programmatic sub-title, "An Anti-Communist Manifesto". In the case of Kuhn, we shall argue that although the switch is more tacit, and is hedged by numerous reservations, it remains the case that his historical account of the growth of scientific maturity is also converted into a programme for the future growth of scientific knowledge.

Like Rostow, Kuhn's account of the history of scientific development is intended to trace the steps whereby traditional systems of knowledge undergo those transformations preparatory to real scientific growth, the take-off points, and then continue to develop through the path of self-sustaining growth of scientific maturity. But whereas Rostow's conception of the historically necessary steps is explicitly laid out as a series of successive stages of growth, Kuhn's stage-theory is not presented as a manifesto and is, therefore, less obviously programmatic.

The take-off into the real growth of scientific knowledge, according to Kuhn, comes only when an academic disci-

pline develops an identifiable paradigm, which is to say, that it develops a commonly shared research tradition. The emergence of such a paradigm in the research tradition of an intellectual discipline introduces, therefore, a new form of cognitive organisation which correspondingly signals the disappearance of the multiple schools which formerly co-existed within the discipline (Kuhn, 1970a:17). The disappearance of the pre-paradigm schools, says Kuhn, is caused by the triumph of one of these schools in elevating itself as a dominant puzzle-solving tradition and hegenomising its research standards for future method and theory within the discipline. This process of paradigm formation, in Kuhn's eyes, is the single most stable indication of the birth of a new science. It should, perhaps, be added that in more recent writings Kuhn has suggested that it is not the presence of a paradigm alone that is necessary for scientific take-off, but also the nature of such a paradigm; in particular its suitability for developing a problem-solving research tradition. (Kuhn, 1970a:179)

In his discussion of the significance of paradigm formations to scientific take-off, Kuhn uses two types of examples to illustrate the condition of pre-science, or proto-science, prior to the emergence of a paradigm. The first underlies his descriptive account of the past growth of

scientific knowledge while the second provides an application of his programme for the future growth of scientific knowledge.

For Kuhn, the take-off point of such exact sciences as chemistry or astronomy is indicated by the emergence of a single research tradition from among the multiple traditions that formerly existed as schools of alchemy or astrology. The significance of the new paradigm resides in the fact that it permits the growth of a puzzle-solving tradition and hence the practice of a normal science. Prior to this point of take-off, as Kuhn makes clear, the ancient disciplines of alchemy and astrology were fragmented into a number of disparate research communities, each having a separate set of theories and methods and with no overall framework for the common selection of research priorities. Under such conditions, although some advances within the specialised schools may take place, any more general progress towards what is now recognised as a scientific research tradition is very hard to find (Kuhn, 1970a:163).

The success of the new form of cognitive organisation which is now regarded as science lies, therefore, in its ability to replace the earlier schools of thought contained in the ancient disciplines by a single unified research tradition. For where these earlier schools proceeded along very

diverse methodological and theoretical lines, the new research community unanimously adopts a common programme which is based on a shared set of theories and methods. The institutionalisation of this new research tradition and the consequent elimination of the earlier traditions of fragmented schools, we shall refer to as the process of paradigm closure.

It is, however, the more contemporary applications of the theory of paradigm closure that evoke the greatest controversy in Kuhn's general theory and programme for the growth of scientific knowledge. For at an early point in his account of the history of science, Kuhn reveals one of the reasons that led to his discovery of the importance of paradigms.

> ...spending the year in a community composed predominantly of social scientists confronted me with unanticipated problems about the differences between such communities and those of the natural scientists...I was struck by the number and extent of the overt disagreements between social scientists about the nature of legitimate scientific problems and methods...the practice of astronomy, physics, chemistry, or biology normally fails to evoke the controversies over fundamentals that today often seems endemic, among, say, psychologists or sociologists. Attempting to discover the source of that difference led me to recognise the role in scientific research of what I have since called "paradigms".
>
> Kuhn, (1970a:viii) Emphasis supplied.

The evident difference of cognitive organisation and progress between the natural sciences and the social sciences

was, thus, one of the points of departure for Kuhn's investigation into the historical structure of the growth of scientific knowledge. The implications of Kuhn's account of the historical development of science for the future growth of scientific knowledge are, therefore, seen most clearly in his evaluation of the present status of the social sciences.

However, it is at this point that the logic of Kuhn's explanation of the past historical progress of science is apparently suspended. For although it is clear enough from Kuhn's comments that, in many senses, the present social sciences are just as much pre-sciences, or proto-sciences, as were the earlier schools of alchemy and astrology, he stops considerably short of recommending the deliberated creation of common puzzle-solving traditions as a pre-condition for scientific take-off in the social sciences. In fact, he explicitly rejects the idea that a simple "methodological prescription" can convert a proto-science into a mature science.

> If...some social scientists take from me the view that they can improve the status of their field by first legislating agreement on fundamentals and then turning to puzzle-solving, they are badly misconstruing my point. A sentence I once used when discussing the special efficacy of mathematical theories applied equally here: "As in individual development, so in the scientific group, maturity comes most surely to those who know how to wait." Fortunately, though no prescription will force it, the transition to maturity does come to many fields, and it is well worth waiting and struggling to attain. Kuhn, (1970b:245).

Kuhn offers a number of similar disclaimers against the idea that the social sciences can be made 'scientific' by adopting a single paradigm of theoretical principles and methodological procedures.

> I claim no therapy to assist the transformation of a proto-science to a science, nor do I suppose that anything of the sort is to be had.
> Kuhn (1970b:245).

From these observations one may conclude that Kuhn interprets the historical progress of paradigm closure as a form of natural and thus inevitable evolution which proceeds according to its own internal and necessary laws of development rather than through the contingent efforts of individuals to reshape their discipline in accordance with a preconceived plan. For although the course of the history of science, according to Kuhn, clearly demonstrates that the path of progress is necessarily dependent upon the formation of a paradigm, it seems that paradigms cannot be formed at will, but are the outgrowth of a particular state of intellectual and societal preparation within a community of scholars.

There is, however, more than just a mere suspicion of circularity in Kuhn's reasoning at this point, for while he disclaims any intention of offering a therapy to the social sciences for their scientific transformation, he insists that

his methodological prescription (of paradigm closure) is only directed towards the mature sciences.

> Everything that has been said so far, though phrased for science and scientists, applies equally to a number of other fields. My methodological prescription is, however, directed exclusively to the sciences and, among them, to those fields which display the special developmental pattern known as progress.
> Kuhn (1970b:243).

But if Kuhn's programme is only directed at the mature sciences, those sciences which exhibit a pattern of progress, we are again taken full circle of the argument when we recall what it is that Kuhn means by 'scientific progress'.

> Scientific progress is not different in kind from progress in other fields, but the absence at most times of competing schools that question each other's aims and standards makes the progress of a normal-scientific community far easier to see.
> Kuhn, (1970a:163).

Thus although Kuhn cautions against the possibility of attempting a premature paradigm closure in those disciplines presently regarded as proto-sciences, it is clear that his work may be read as a necessary programme for the future development of the social sciences <u>as mature sciences</u>.

What, therefore, is the relationship between the programme of paradigm closure and the model of scientific discourse presented in Kuhn's work? It is at this point that we shall begin to see the restrictive nature of Kuhn's theory of science.

It is clear that Kuhn has decisively broken with the earlier positivist tradition of the philosophy of science in his account of the historical structure of the growth of scientific knowledge. Whereas many previous commentators had taken for granted the essential continuity of science over different historical periods, Kuhn rejects these interpretations. He rejects them most severely on the grounds that the terms that make up the propositions of scientific discourse have proven to be unstable in their meanings, and have, indeed, varied their meanings at different times in the history of science.

The meanings of scientific terms are, therefore, supplied in the context of the particular paradigm of which they are a part. They are an integral expression of the total set of theories and methods which define the boundaries of the scientific enterprise at any given historical moment. There is, in this sense, a similarity between Kuhn's use of the concept of 'paradigm' and Wittgenstein's use of the concept of 'language-game', (Kuhn, 1970a:45). The meaning of scientific terms have thus made dependent upon the particular scientific language-game that is currently played. This interpretation of the growth of science necessarily rejects the idea that scientific terms have a natural connection to the immediate world of human experience, and rather sees scienti-

fic discourse as having its foundation in a series of conventional agreements related to the current use of a particular language. Science as a system of discourse, therefore, is no longer separated from other systems of discourse by reason that is, alone, provides a truthful picture of reality. For the pictures of reality drawn by science are as prone to change as other systems of knowledge, and are equally based upon the changing contexts of contemporary language usage.

At the same time, Kuhn makes it quite clear that the dramatic pattern of progress which is recorded in the different historical [illegible]ents of science is the result of a particular form of cognitive and social organisation. This organisation is expressed in the formation of a paradigm and the elimination of previous forms of pluralism within the former research traditions of the scholastic disciplines. The process of paradigm closure is, for Kuhn, a necessary stage in the attainment of scientific status for a discipline. It is a precondition for the later development to scientific maturity.

The conclusion of Kuhn's general theory of science, and the basis of his programme for the future growth of scientific knowledge comes from his attempt to integrate the relativism of his theory of scientific revolutions and the linearity of his theory of scientific maturity. On the one hand his ac-

count of the history of science suggests that scientific discourse does not have a separate or direct correspondence with an independent reality. Scientific knowledge is not, therefore, the exemplary form of objective knowledge, but a series of historically specific language-games which although no doubt priviledged under present social arrangements co-exist beside other such language-games.

On the other hand, the progress exhibited in the growth of scientific knowledge, according to Kuhn, is a result of the form of cognitive organisation caused by the process of paradigm closure. The formation of a paradigm and the elimination of competing schools is a necessary precondition for scientific maturity. The primary distinction of scientific discourse from other systems of discourse is, therefore, the fact of paradigm closure and, therefore, its relative discontinuity with respect to previous or other contemporary systems of discourse. It is implicit in Kuhn's programme that other systems of knowledge and models of discourse may growth into sciences upon the emergence of a unified paradigmatic research tradition. Whereas the earlier philosophers had denied the possibility of scientific status to anything but a narrowly defined field of enquiry, Kuhn's theory of science has opened the way for the uncritical application of scientific methodology to <u>all</u> fields of enquiry.

Kuhn's general theory of science presents, therefore, what may be seen as an 'external' and an 'internal' picture of the growth of scientific knowledge. His external picture is meant to describe the total historical process whereby one form of science is superceded by a new form, which is itself destined for future displacement. The periodic revolution in paradigms implies also that the terms in which scientific propositions are formulated are necessarily part of the total process of change and replacement. Thus the observational terms that at one historical moment provide the medium for a direct report of the human experience of nature are never permanent foundations for an empirical language of sensation or phenomena. For what are the well confirmed facts of one research tradition may again become problematic in another. The meaning of observational terms are supplied within the context of the total paradigm. In his external view of the growth of science, therefore, Kuhn shows that the L_o's of scientific discourse are dependent for their meaning upon the context of their usage, and not upon their correspondence to an independent reality. His external view of science is thus based on a theoretical reductionist model of scientific discourse.

Kuhn's account of the internal development of scientific knowledge within particular historical periods, on the

other hand, is based on the necessity for paradigms. The consequences of this account for a model of scientific discourse and for a programme for the future growth of scientific knowledge, stand in opposition to his earlier account of the general historical process of the development of science. For the process of paradigm closure entails the elimination of alternative languages of theory and observation and the establishment of a paradigm L_o and L_t. It is evident, therefore, that in Kuhn's reinterpretation of the history of scientific knowledge, those sciences which have been based on an observational reductionist model of scientific discourse continue to find a justification in Kuhn's work, although no longer in the traditional terms of logical or empirical necessity, but in Kuhn's terms of historical necessity. Thus although Kuhn has apparently rejected the traditional epistemological criteria for the justification of an observational reductionist model of scientific discourse, he has replaced them by other criteria of justification which are based on pragmatic and historical grounds. It is these criteria for the justification of an observational reductionist model of scientific discourse that underlie his programme for paradigm closure as a programme for the future growth of scientific knowledge.

The Programme for Paradigm Proliferation

The work of Paul Feyerabend stands beside that of Thomas Kuhn in part as corroboration and in part as refutation. For, as has been previously shown, Kuhn and Feyerabend share together a similar commitment to investigate the structure of scientific knowledge by restoring to legitimacy the use of the historical method. Feyerabend has made clear his agreement with Kuhn on the fundamental importance that historical studies have for a clarification of the status of modern scientific methods of inquiry.

> Kuhn has made it clear to me that history cannot be dismissed out of hand as being irrelevant to the methodologist. The methodologist deals with a certain sticky material - theories - and he wants to change it. Knowledge of the shape in which the material is available and of the circumstances under which the change has to be carried out is essential to his task.
>
> Feyerabend (1969:277).

However, Feyerabend departs widely from Kuhn in his different programme for the future growth of scientific knowledge. For whereas Kuhn has suggested that the development of science towards cognitive maturity is dependent upon the emergence of paradigms, Feyerabend is highly critical of the past role of paradigms in the history of science. In his view, the future development of science may best be achieved through the multiplication of theories and the enlargement of their empirical contents. For this reason, therefore,

Feyerabend stands in fundamental opposition to Kuhn's programme for paradigm closure and to the theory of science contained in it. For Kuhn, pluralism is episodic, for Feyerabend it is essential.

> Yet while I thought I recognised Kuhn's problems; and while I tried to account for certain aspects of science to which he had drawn attention (the omnipresence of anomolies is one example); I was quite unable to agree with the theory of science which he himself proposed; and I was even less prepared to accept the general ideology which I thought formed the background of his thinking.
>
> Feyerabend (1970b:197).

There are a number of dimensions to Feyerabend's philosophy of science which are instructive for our present investigations. It may, perhaps, be suggested that Feyerabend has, through his originality and erudition, provided the theoretical foundations for a new discipline: political or social epistemology. For his contributions to the philosophy of science have been sufficiently influenced by theories of politics and history, as well as the more traditional areas of logic and the analysis of language, to render them quite distinct from the conventionally received wisdom of the discipline.

In addition to this, his discussions of the status of language in scientific discourse has been more extensive and detailed than Kuhn's peripheral observations. Hence his explicit commitment to a theoretical reductionist model of

scientific discourse is taken to its logical conclusion, which is we will argue, an unsatisfactory conclusion.

We shall, therefore, examine Feyerabend's programme for science and model of scientific discourse under two headings:

(i) The theory and practice of pluralism, and the epistemological and political justifications for it.

(ii) The limitations of theoretical reductionism.

Although as in most complex subjects, such an artificial separation of content cannot be maintained in reality, it is only offered here to simplify the issues of immediate concern to us.

The Theory and Practice of Pluralism

In the same way as we have seen that Kuhn's programme for paradigm closure can best be understood only after the theory of science upon which it rests is openly identified, Feyerabend's programme for paradigm proliferation must be similarly examined. For these two writers who are frequently classified together on the basis of their common commitment to an historical method in the criticism of knowledge, it is important to understand the theory of science which underlies the work of each and separates it from the other.

The theory of science that underlies Feyerabend's programme for the growth of scientific knowledge and his model of scientific discourse derives from the traditions of social

and political philosophy. For in his outright rejection of the established methods of philosophical criticism as practised by the earlier generation of logical empiricists and neo-positivists, Feyerabend has adopted a critical philosophy of science which is informed by the methods of historical and political analysis. There is throughout his work a more explicit connection between theories and politics and theories of knowledge than in most other of his contemporaries.

> Political analysis is wider and much more realistic than the philosophy of science in that it considers historical conditions including the pecularities of individual human thought. It also recognises that the historical conditions always contain layers of different age and of different sophistication so that a progressive idea may be impeded, not by any intrinsic disadvantage, but by the fact that it arises in backward surroundings.
>
> Feyerabend (1969:276).

It will be argued throughout, however, that Feyerabend's application of the historical method to the philosophy of science is based upon an idealist conception of history and science which has unfortunate consequences both for his analysis and conclusions.

The most significant part of Feyerabend's programme for the growth of knowledge, and one that most clearly reveals its political origins, is the epistemological demand for <u>theoretical pluralism</u>. It is this demand which expressed the basis of his disagreement with Kuhn and his rejection of the programme

for paradigm closure.

For Feyerabend, the history of the past development of science is also the history of the societies from which these sciences came. There is, for him, no inevitability or logical necessity in the pattern of growth which may be exhibited in any branch of learning, scientific or non-scientific. The fact that scientific knowledge has traditionally developed within paradigms and through the replacement of old paradigms by new is no guarantee that the organisation of knowledge by paradigms will continue to be the most desirable or reasonable programme for the future growth of knowledge.

Thus Feyerabend rejects any interpretation of the past development of science which may be based on a presupposition of historical determinism and which therefore commits the future growth of science to the traditional pattern of development. Such a deterministic theory of science, in Feyerabend's view, may be read into the Kuhnian account of the structure of scientific revolutions, although as he also observes, this account is by no means free from ambiguity.

> Whenever I read Kuhn, I am troubled by the following question: are we here presented with methodological prescriptions which tell the scientist how to proceed; or are we given a description, void of any evaluative element, of those activities which are generally called 'scientific'.
>
> Feyerabend (1970b:199).

The reason for Kuhn's defence of paradigms in his his-

toriography of science, suggests Feyerabend, rests on two presuppositions. Firstly, that the role of paradigms in a theory of science may be justified by the actual role they have really played in history, and secondly because paradigms provide an heuristic structure for the future growth of knowledge. This second presupposition, as Feyerabend says, is less evident for it is not openly stated by Kuhn as a reason for defending the role of paradigms in science.

> He defends such a procedure first, because it plays a role in actual science as he sees it. This is the description-recommendation ambiquity already dealt with. But he defends it also for a second reason that is somewhat more recondite as the preferences behind it are not made explicit. He defends it because he believes that its adoption will in the end lead to the overthrow of the very same paradigm to which the scientists have restricted themselves in the first place.
>
> Feyerabend, (1970b: 201-202).

It is this defence of the role of paradigms and the implicit formulation of what we have called the programme for paradigm closure, to which Feyerabend is most strongly opposed. For him, the paradigm form of knowledge that regulates the practice of normal science has had the effect of restricting the empirical content of science by rejecting for consideration those theories which differ from or contradict the theories of the paradigm. Thus within the research tradition of a normal science the only empirical evidence certifiable as a

legitimate test of other theories is that evidence which constitutes the set of empirical consequences of the existent paradigm theory. The demand that only those theories be accepted that are justified by the 'facts' is, as Feyerabend shows, a demand that effectively restricts the available realm of facts to that which is generated by the paradigm theory.

This demand of modern empiricism, that those theories which do not fit the available facts be eliminated, has been elevated into a new form of dogmatism. It is not the case, argues Feyerabend, as is popularly advertised, that the method of modern empiricism is free from philosophical or metaphysical bias. On the contrary, the demand that alternative theories of reality or knowledge be eliminated because they cannot be justified in the observational terms of a paradigm theory is one that is made in defence of a dogmatic modern metaphysical system: contemporary empiricism.

> Far from eliminating dogma and metaphysics and thereby encouraging progress, modern empiricism has found a new way of making dogma and metaphysics respectable, viz., the way of calling them 'well confirmed theories', and of developing a method of confirmation in which experimental inquiry plays a large though well controlled role.
>
> Feyerabend, (1968:13).

In opposition to this tendency of the past history of science and to all programmes for the future growth of science

based on paradigm closure, Feyerabend formulates two principles for generating scientific knowledge which he calls the principle of tenacity, and the principle of proliferation. The principle of tenacity demands that a theory should be retained even if there are data which are inconsistent with it (Feyerabend, 1970b:203). The purpose of this principle is to prevent the premature death or unconditional surrender of theories. While the principle of proliferation demands that new theories should be invented which are inconsistent with the accepted point of view, even if these points of view are highly confirmed and generally accepted, Feyerabend (1965: 223-224). The justification for such a multiplication of theories, according to Feyerabend, is both epistemological and political. It is to these two types of justification that we shall now turn for a closer examination.

The main epistemological justification for adopting a programme of proliferation in the sciences, says Feyerabend, is that alternative theories provide us with a measure of criticism (Feyerabend, 1970a:45). For in order to begin the criticism of scientific concepts supplied by a paradigm theory it is necessary to step "outside the circle" and invent a new theory and a new conceptual system which may stand in contrast with the current paradigm theory. Only when we have such alternative theories in existence can we then proceed to criti-

cise one of them in terms of the other. The criticism of theories is only possible, therefore, on the basis of other theories which enable us to have some external yardstick as a means of evaluation. This process of formulating alternative theories to counter established theories Feyerabend calls the principle of counterinduction, (Feyerabend, 1970a: 45). Without such a principle, he suggests, the tradition of scientific research becomes dogmatic for theories are only ever confronted with the empirical consequences of the paradigm theory, and the demand that alternative theories conform to these consequences effectively prevents any genuine test of the paradigm theory. Such a test can come not by demanding that alternative theories must fit the existent 'facts', but by elaborating alternative theories which can then generate different empirical consequences and thus disclose new facts. These new facts can then be used to evaluate the paradigm theory. Such a general conception for a critical method of practising epistemology, Feyerabend has jokingly labelled as his principle of "anything goes". 'Whether or not this characterisation is intended to be facetious, it remains an appropriate slogan to describe in a shorthand way his programme for theoretical pluralism within modern empirical sciences.

> Some of my friends have chided me for elevating a statement such as "anything goes" into a

> fundamental principle of epistemology. They did not notice that I was joking. Theories of knowledge as I conceive them develop, like anything else. We find new principles, we abandon old ones. Now there are some people who will accept an epistemology only if it has some stability, or "rationality" as they are pleased to express themselves. Well, they can have such an epistemology, and "anything goes" will be its only principle.
>
> Feyerabend, (1970a:105).

In demanding such a programme of proliferation for the sciences, we may see more clearly how Feyerabend's account of theoretical pluralism contrasts with that of Kuhn. For in Kuhn's philosophy of science the existence of pluralism at any one time in a scientific discipline is either a sign of scientific immaturity or a sign of crisis.

According to Kuhn, there are certain periods in the history of science when the paradigm form of cognitive organisation may be absent. This may be in the pre-scientific stage of knowledge before the take-off point into science, or it may be at times of crisis when revolutionary science replaces normal science. During these periods of non-paradigmatic research, therefore, the growth of knowledge takes place in the form of different schools and rival intellectual communities which may compete with each other for recognition within a specialised area of knowledge. The content of this non-paradigmatic form of knowledge is revealed in the proliferation of theories and methods that accompany these periods

as well as the universal criticism that underlie them. These periods of theoretical and methodological pluralism are inevitably replaced, however, by a paradigm form of knowledge which lays the preconditions for the further practice of normal science.

In Feyerabend's view, however, the existence of pluralism is necessary and desirable at every stage of growth of scientific knowledge, for systems of knowledge that are self-contained and paradigmatic become dogmatic and resistant to criticism. In a word, they come to violate the very principles of empiricism that they lay claim to represent and actice.

> ...you can be a good empiricist only if you are prepared to work with many alternative theories rather than with a single point of view and "experience". This plurality of theories must not be regarded as a preliminary state of knowledge which will at some time in the future be replaced by the One True Theory. Theoretical pluralism is assumed to be an essential feature of all knowledge that claims to be objective. Nor can one rest content with a plurality which is merely abstract and which is created by denying now this and now that component of the dominant point of view. Alternatives must rather be developed in such detail that problems already 'solved' by the accepted theory can again be treated in a new and perhaps also more detailed manner.
>
> Feyerabend, (1968:14).

Using his theory of pluralism as a guide, therefore, Feyerabend proceeds from a criticism of paradigm closure as

a programme for the future growth of knowledge to its criticism as a description of the historical growth of past scientific knowledge. For whereas Kuhn has insisted that periods of crisis in science are generally concluded by the emergence of a new paradigm capable of explaining the anomolies of the previous paradigm, Feyerabend throws doubt on the existence of paradigms in the history of science, (Feyerabend, 1970b: 207). For if the growth of science has always been predominantly organised within paradigms, except for periods of crisis, how do we account for the periodic emergence of competing theories and the general state of scientific pluralism? We can only account for the rise of competing theories, says Feyerabend, by recognising that they exist at every stage of scientific development, and that Kuhn's notion of normal science is consequently only an historical fiction. Thus when new theories are introduced into a scientific problem area they can never find an unambiguous confirmation on the evidence which has been supplied by earlier theories. Far from resolving existing anomalies, new theories are handicapped by their initial restriction to a realm of empirical consequences which do not derive from the application of their own concepts.

In documenting the problems involved in the introduction of new theories, Feyerabend draws heavily on a number of

historical case-studies which he uses to illustrate his thesis. Thus, in his discussion of Galileo's defence of the Copernican theory of heliocentricism (1969, 1970a), Feyerabend examines the absence of confirmatory evidence for the new theory, and the abundance of arguments against it. Many of these refutations, for example, the argument that the vertical motion of a falling object refutes the theory of the movement of the earth, were empirical refutations based on the directly reported experience of the physical world. In order to counter these objections, therefore, Galileo had to transform the observation language of the day into a new observational language which introduced new categories into the discourse of seventeenth century astronomy. In this particular case the categories of "'operative" and "non-operative motion" were introduced into the observation language of Galilean astronomy in order to connect the new theory to a range of empirical consequences that had never previously been singled out for special identification. The distinction between "operative" and "non-operative" motion enabled Galilean astronomers to distinguish between the observed motion of a moving object in a stationary environment and the unobserved motion of a moving object in a moving environment. It was only by transforming the categories of the old observation language to fit the predictions of the new theory that

Galilean astronomy provided itself with the empirical evidence to support its revolutionary hypotheses.

For examples such as these, taken from the history of scientific discovery, Feyerabend concludes that theories are primary agents in the growth of scientific knowledge. For it is through the formulation of new theories that new facts may be uncovered and the range of human experience consequently enlarged. Theories provide new classifications and new categories which are assimilated into observation languages where they make possible a new organisation of practical ~~human~~ experience.

At the same time, however, Feyerabend makes it clear his concern over another aspect of the dogmatism which he sees attached to the Kuhnian programme for paradigm closure. As well as arguing for proliferation as a means of developing measures of criticism, he advocates proliferation as a necessary means of preventing the <u>suppression</u> of past knowledge. In other words, the birth rate of theories should be made to increase and the mortality rate should be made to decline.

For one of the events that signals the acceptance of a new theory has been the elimination of the multiple schools which previously co-existed within the discipline. Thus, for example, the entrenchment of the Galilean theory of heliocentricism in the astronomy of the seventeenth century simultan-

eously meant the final elimination of the astrological schools and the research traditions they had supported. In the same way the rise of modern chemistry was also followed by the subsequent demise of the earlier schools of alchemy. For Feyerabend, the elimination of these schools and the suppression of the research traditions associated with them is only a further testimony to the dogmatic cognitive structure of modern science. The eclipse of the ancient disciplines of astrology and alchemy was brought about, in his view, not by any refutation of these theories, but simply by their replacement with a new theory and a new observation language. For the rise of modern chemistry produced a new observation language which excluded the older observational terminologies as unscientific or as superstition. The observational terms that were once used to record the experiences of the practical alchemist are no longer the terms used to record the experiences of the empirical chemist. With this transformation in the observational vocabularies of historically separate disciplines comes the parallel transformation in the received world of empirical facts. For the "facts" of one age reported in a given observational language are not the "facts" of another age which has acquired a different observational language. It is for this reason that Feyerabend calls the replacement of earlier traditions of knowledge by a normal

scientific tradition the suppression of past knowledge, for the new theory does not refute the old knowledge in those terms which the old research tradition itself employed. The old knowledge is suppressed because it is not supported by the facts which are generated by the new theory, and it is invalidated on the basis of this realm of facts, rather than the realm of facts contained in the earlier observational vocabulary.

> The so-called scientific revolution led to astounding discoveries and it considerably extended our knowledge of physics, physiology and astronomy. This was achieved by pushing aside and regarding as irrelevant, and often as non-existent, those facts which had supported the older philosophy. Thus all the evidence for witchcraft, demonic possession, all the empirical phenomena one had been able to cite in favour of the existence of the devil, were pushed aside together with the "superstitions" they once confirmed. The result was that "towards the close of the Middle Ages science was forced away from human psychology, so that even the great endeavour of Erasmus and his friend Vives, as the best representative of humanism, did not suffice to bring about a rapproachment, and psychopathology had to trail centuries behind the development trend of general medicine and surgery...As a matter of fact...the divorcement of medical science from psychopathology was so definite that the latter was always totally relegated to the domain of theology and eccliastic and civil law - two fields which naturally became further and further removed from medicine.
>
> Zilboorg: quoted in Feyerabend, (1970a:124).

Thus for Feyerabend, another result of the dogmatic cognitive structure of modern science has been the periodic sup-

pression of all knowledge which is inconsistent with the established theory. A critical philosophy of science must recognise this problem for what it really is and at the same time seek to generate alternative theories to those currently in acceptance, by the process of counterinduction.

We shall now turn to a brief consideration of the political content of Feyerabend's philosophy of science and the way in which it is related to his critical theory of scientific knowledge as well as to his programme for its future growth.

The link between politics and epistemology is made quite openly by Feyerabend as one of his justifications for the programme of paradigm proliferation. His demand for the multiplication of theories in science and the rejection of monistic paradigmatic cognitive structures is simultaneously translated into an equivalent political demand for the abolition of monolithic social structures.

> Proliferation is introduced as the solution to a problem of life: how can we achieve full consciousness; how can we learn what we are capable of doing; how can we increase our freedom so that we are able to decide, rather than adopt by habit, the manner in which we want to use our talents. Considerations like these were common at a time when the connection between truth and self-expression was still regarded as a problem and when even the arts were supposed not just to please, but to elevate and to instruct...Scientific method is part of a general theory of man. It received its rules from this theory and it is

> built up in accordance with our ideas of a worthwhile human existence.
>
> Feyerabend, (1970a:29).

Feyerabend's criticism of the paradigm form of science is, therefore, most fundamentally a criticism of the societal form of life that supports and institutionalises such paradigms. It is a criticism based on the recognition that science has become an alienated form of human activity. For whereas the practice of science at one time may have been responsive to practical human interests, whether these were political or religious preferences, today the laws of science have taken on the appearance of a transcendental objectivity which cannot be altered for or by any kind of human interest. If there was ever a time when the practice of science was regarded as a vocation it is no longer so; today science appears almost as a monolithic machine independent of the wishes or expectations of its human practitioners. It is this familiar image of science that Feyerabend criticises as an alienated conception of what, at its origins, still remains a practical human activity.

> The sciences, after all, are our own creation, including all the severe standards they seem to impose on us. It is good to be constantly reminded of the fact that science as we know it today is not inescapable, and that we can construct a world in which it plays no role whatever. (Such a world, I venture to suggest would be more pleasant to behold than the world we live in today, both materially and intellectually).
>
> Feyerabend, (1970a:91).

It is for this reason, therefore, that Feyerabend rejects the Kuhnian programme for paradigm closure, for the transposition of the pattern of past history onto the pattern of the future is a clear example of the mistaken conception of science as a transcendental rather than as a practical human activity. Feyerabend exposes the reification of science and particular social structures that is implicit in Kuhn's description and programme.

In the place of Kuhn's programme for paradigm closure, Feyerabend substitutes his own programme for proliferation. This he has described as an ananchistic theory of knowledge which is designed to counter the restrictive organisation of scientific knowledge in paradigms by generating theoretical pluralism in the sciences. Feyerabend's solution to the problem of monopoly in society and monism in science is similarly translated into a practical solution to the problem of social and political monopoly of power and ideas in society. For as in science where his programme calls for the proliferation of multiple theories, his programme of political practice calls for the multiplication of "democratic bases" which within the existing institutions. In this programme of political pluralism Feyerabend sees a solution to the problem of the restrictive organisation of thought and action in modern society.

It will be argued here, that while Feyerabend may choose to label his political programme as anarchistic, it is in practice a programme of idealism and may be criticised on this basis. For it will be recalled that the classical descriptions of capitalist societies as democratic amalgamations of competing interest groups were provided by the liberal theorists of political economy in the eighteenth century (see Miliband, 1969). It is to them that we owe the general theory of pluralism as an explanation of the foundation of the political, economic, and moral orders of modern society. One of the most important founders of the school of classical liberalism was John Stuart Mill, and it is no accident that Feyerabend makes numerous and lengthy references to him.

> Many people are inclined to call Mill a liberal and to dismiss him because of the weakness of the liberal creed they have perceived. This is somewhat unjust, for Mill is very different indeed from much that is called "liberalism" today. He is a radical in many ways. Even as a radical, however, he excels by his rationality and his humanity.
>
> Feyerabend, (1970a:108).

The main objection by Marxist writers to the classical descriptions of capitalism as a liberal democracy has been that the liberals have failed to acknowledge the reality of ruling class interests which dominate other institutions of the modern nation-state. Far from being a composition of competitive plural interests, they have argued that the state

exists as an apparatus of class rule, and that the structure of the modern capitalist state has increasingly eliminated the earlier pluralism of competitive entrepreneurial capitalism (Miliband, 1969, Hobhouse 1964). The solution to the problem of a monopoly class power, according to this criticism, does not lie in trying to turn the clock backwards to a restoration of the status quo ante. For in order to abolish the rule of social classes, the power of the state must pass into the hands of the last social class left in the history of the modern state: the working class. It is only after such a revolutionary seizure of state power that the apparatus of the state may be transformed into a classless polity which permits the growth of a classless political pluralism.

Feyerabend's idealist conception of political epistemology may be criticised along these lines. For his programme for the growth of scientific knowledge and for the transformation of capitalist society suffers from one of the classical failures of liberalism. His programme for political pluralism underestimates the necessity for an organised political struggle against the monopoly-capitalist state (for he is writing of the U.S.A.). He believes, on the contrary, that all the ills of American imperialism may be removed "by propaganda, enlightment, special bills, personal effort..., and

numerous other legal means". (1970a:111).

This failure in Feyerabend's programme for pluralism in knowledge and society, however, is linked to his further failure to adequately account for the present state of centralism which he so strongly opposes. This is the fundamental weakness of Feyerabend's programme and one that is shared with the classical idealist and utopian theorists. For if the present state of private economic centralisation, for example, in capitalist society has developed from a previously more competitive form, then a return to this original form, under the same system of private property, will not in any way prevent the reformation of future centralisation. In order to ensure the elimination of centralisation it is necessary to eliminate the conditions that perpetuate such centralisation.

In the same way, Feyerabend conceives of a general society made up of plural sub-societies which are free to pursue their own objectives, even if they are anti-social (1970a: 110). But the problem with this image of political or intellectual communities is that it fails to predict the conditions under which particular sub-groups can by virtue of propaganda or beligerence or whatever, come to dominate the total political or scientific system. For throughout the history of politics and science, states of competition have invariably given way to states of organisation, and pluralism

has been followed by centralism. This weakness in Feyerabend's programme is the result of his failure to adequately account for the growth of the present state of centralism in science and society, and to specify the necessary conditions for its elimination.

Attention has also been drawn to the idealist content of Feyerabend's work from other commentators on his history and philosophy of science. Thus McEvoy (1975) has argued that Feyerabend's use of the historical method follows principles that are already inherent in Feyerabend's philosophy of science. Feyerabend's claim that the principles of a philosophy of science should be based on an understanding of the history of scientific change, therefore, remains unrealised in his own philosophical programme. Ironically, this problem is most clearly revealed in Feyerabend's committment to a programme of theoretical pluralism and methodological counterinduction, which he presents in response to the dogmatism of previous philosophies of science which had pursued the search for a fixed rationality. For as McEvoy and Machamer (1973) both have argued, Feyerabend's committment to a radical critical rationalism results in considerable distortions in his historical reconstructions of case studies of scientific discovery and change.

> Though depressing, such historical distortions are not surprising given Feyerabend's view of

> history as a source of brute force to be wielded against his philosophical adversaries...instead of learning from the history of science Feyerabend has merely used it to reflect the shortcomings of his own philosophical outlook. A richer rationality, one embedded in a careful study of the history, as well as the philosophy of science, could avoid such pitfalls by including in the constituents of rationality a wider variety of criteria than are to be found in any contemporary philosophies of science.
>
> McEvoy, (1975: 65-66).

Thus the idealist content of Feyerabend's work is evident from the way in which history and science are subordinated to philosophy both in analysis and conclusions.

This failure of his political programme to investigate the fundamental contradictions of American or Western society and to organise in the direction of their deepening crisis is also reflected in Feyerabend's epistemology. For the fundamental problem of Feyerabend's programme for the proliferation of theories in science is that he offers us no selection procedure by which to choose one theory over another. For it could well be argued that if the number of possible theories that may be generated is infinite, we may well be advised to stick with what we have, unless we have access to some type of rational selection procedure. On the other hand, Feyerabend's programme for the proliferation of theories gives licence and support to the most anti-humanitarian and repressive of ideologies. In the social sciences, for example, where research may be more heavily and directly in-

fluenced by public policy considerations than in some of the physical sciences, Feyerabend's programme for an uncompromising pluralism presents formidable political problems. Should a research tradition which is based firmly on a theory of the fundamental inequality of different racial groups be given the same support as a counter theory? The problem of Feyerabend's programme is thus, in part, the problem of an abstract programme which has no concrete relationship to the historical context of the practice of science.

> ...my aim has been to present an abstract model for the acquisition of knowledge, to develop its consequences, and to compare these consequences with science...Such models tell us how to proceed if a certain aim is to be achieved. In this way they form a basis for the criticism as well as the reform of what exists.
>
> Feyerabend, (1965:223).

Feyerabend's recommendations take on the form of abstract procedures for generating knowledge in the sciences without sufficient reflection upon the historical aims of science at given stages of development in concrete societies. Thus, Feyerabend's criticism of Kuhn for neglecting to discuss the aims of science (Feyerabend, 1970b:201), can be made equally well against his own account of the Growth of Scientific Knowledge.

The Limitations of Theoretical Reductionism

The model of discourse that underlies Feyerabend's pro-

gramme for paradigm proliferation in the sciences is based on an uncompromising theoretical reductionism. He reverses the relationship between theory and observation that was formerly posited by the logical empiricists. For where Carnap argued that theoretical terms, as part of an uninterpreted propositional calculus, only acquire their meaning when connected by correspondence rules to observational terms, Feyerabend argues the reverse. For him, observational terms acquire their meaning as the empirical consequences of a conceptual system which is provided by a particular theory.

> The double language system assumes that theories which are not connected with some observation language do not possess an interpretation. The demand assumes that they do, and asks to choose the observation language most suited to it. It reverses the relation between theory and experience that is characteristic for the double language method of interpretation, which means, it gives up this method.
>
> Feyerabend, (1968:23).

There are, however, a number of fundamental problems and unreasonable consequences that follow on the adoption of Feyerabend's model of theoretical reductionism. For this reason, therefore, we shall argue that the theoretical reductionist, no less than the observational reductionist model, is an unacceptable model of scientific discourse.

The most important and controversial part of Feyerabend's model of scientific language has undoubtedly been his thesis of

radical meaning variance. This conception of the status of language in science is, of course, also shared with Kuhn, but combined as it is with a programme for proliferation, it raises greater problems for the internal consistency of Feyerabend's approach. For Feyerabend, the observational terms of science are always provided with a context of meaning by a theory and its related system of concepts. Without such a theoretical context, observational terms are uninterpretable and, therefore, meaningless. In this regard, Feyerabend has criticised what he has called the autonomy principle, the belief that "the facts which belong to the empirical content of some theory are available whether or not one considers alternatives to this theory" (Feyerabend, 1968: 27). Thus the real task of a theory, in Feyerabend's view, is to provide a "correct" account of the world, an account that is, which discloses the totality of facts as seen through its own concepts (Feyerabend, 1970a:89). Both Kuhn and Feyerabend have argued that some different theories may be incommensurable, that is, they may not be subject to any comparison because the meaning of the terms in which the one theory is formulated may be quite at odds with the meaning of the terms of the other. In such a case, therefore, as in the example which Feyerabend frequently provides: the case of the theories of Classical Mechanics (C.M.), and the Special Theory of Relati-

vity (S.R.), the observational languages which provide the empirical consequences and content of each of these two theories are incommensurable to each other.

> Classical physics is a case in point. It has developed a comprehensive terminology for describing the most fundamental properties of our universe, such as shapes, speeds and masses. The conceptual system, connected with this terminology assumes that the properties inhere in objects, not otherwise. The theory of relativity teaches us, at least in one of its interpretations, that there are no such inherent properties in the world, neither observable, nor unobservable, and it produces an entirely new conceptual system for description inside the domain of mechanics. This 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 (1970a:82).

For Kuhn, as we have seen, this thesis of the moaning variance and incommensurability of different theories gives rise to no imminent problems in his theory of the transformation from normal to revolutionary science. Even though successive theoretical systems may be incommensurable, the paradigm form of the organisation of knowledge ensures that throughout the duration of the normal scientific period only one paradigm language of theory and observation is tolerated. The problem of incommensurability is, therefore, simply a problem of learning a new language, and not the much weightier and more questionable problem of translating one language

into another.

For Feyerabend, however, the thesis of incommensurability and meaning variance between theories does create a different order of problem, because of the associated programme for the proliferation of theories. To what extent, one may ask, is Feyerabend's injunction to generate alternative theories, as a measure of criticism, contradicted by his thesis of radical meaning variance? For if two theoretical systems such as CM and SR are really incommensurable, then they cannot be accorded the status of "rival" or "competitive" or even "alternative" theories. In order for two theories to be competitive, and to fulfill Feyerabend's requirement for mutual criticism, they must in some sense be commensurable with each other. This difficulty in Feyerabend's account has been remarked by a number of writers, and has provided the most practical rod with which to beat him.

> Two theories which share no common observation statements would share no common characterisation of the pragmatics of the observation situation either, and the possibility of a genuine crucial test between them would thus seem to be obviated.
>
> Townsend, (1970:208).

Similarly, Giedymin (1970), Nordig (1971), Koertge (1972), and Leplin (1969) all raise the same objection against this apparent contradiction between Feyerabend's theory and programme for science.

Feyerabend's proposed solution to this problem however, fails to overcome this fundamental difficulty in the logical method of comparing alternative theories. On a number of occasions (1970a:54, 84-85; 1970b:220), he has suggested that incommensurable theories may be interpreted on the basis of different observational languages. Thus while two theories may remain incommensurable when interpreted in a phenomenalist observation language they may become commensurable in an instrumentalist observation language. The problem, in this case, becomes one of picking out an appropriate observational language. This suggestion, however, assumes that the meaning of observational languages is independently fixed of theories, and thus violates Feyerabend's earlier (and more basic) commitment to the abolition of the autonomy principle. We are left, it seems, with a logical paradox which even in the face of Feyerabend's expressed attempts, has so far eluded resolution.

The model of theoretical reductionism, as expressed in the thesis of radical meaning variance, has also given rise to other difficulties. Thus Kordig (1971) has tried to show that the commitment to a theory of meaning variance of observational languages leads to unreasonable consequences when considering the logical methods of comparing, communicating, learning and testing different scientific theories.

For these reasons, therefore, Feyerabend's proposed model of scientific discourse is not a satisfactory solution to the earlier problems raised by the observational reductionist model of scientific discourse. For while his criticisms of modern empiricism are clearly appropriate, the replacement of the asymmetry of observation by the asymmetry of theory does not provide us with a model of discourse that is internally consistent. This problem is also present in his programme of proliferation for the growth of scientific knowledge where it reappears in a number of forms: as a methodological problem of selection procedures between theories, as a political problem in the relationship between extreme theory and extreme practice, and so on. These problems of inconsistency in Feyerabend's theory and programme for science have also been recognised by another writer whom, in other ways, remains highly sympathetic to Feyerabend's philosophy of science.

> The methodological anarchism or dadaism of Feyerabend...is compatible with my pluralism, but there is a major difference of emphasis. For me, the notion of philosophical systems is central, and therefore also a strong, 'puritan' stress on internal consistency, or rather coherence, between a researcher's and a community's logic, methodology, ontology, epistemology, ethics, politics. The hedonistic 'Do as a researcher what you wish and have a pleasant time' is qualified by the addition 'and if a philosopher by inclination or necessity, be clear and coherent in developing your synthesis'. Because of requirements in other parts of the

> system, one's methodology in scientific matters may have to be rather rigid. Anything will not go.
>
> Naess, (1972:100).

No matter how useful a critical tool the programme for paradigm proliferation may prove to be, therefore, we should not overlook its failures or the shortcomings of the model of scientific discourse on which it is based. For what remains clear is that Feyerabend does not come to any conclusions as to the aim of science within different historical periods, and is thus unable to provide any guide for the organisation and direction of science in this present situation.

The Programme for Paradigm Complementarity

The final section of this chapter serves to draw together the several themes that have been pursued to this point: the relationship between models of scientific discourse and programmes for the growth of scientific knowledge. We have attempted to show how two of the logical variations on the ordinary language model of scientific discourse, observational and theoretical reductionism, have underlaid the respective programmes of paradigm closure and paradigm proliferation for the future growth of the natural and social sciences. Although the account thus far may lay no claim to comprehensiveness its main purpose has been to serve as a basis for the elucidation and criticism of the reductionist

models of scientific discourse and the epistemological programmes that include them.

Having argued against the acceptance of any model of scientific discourse founded upon reductionism, we turn now to a consideration of another model which offers a possibility for overcoming the objections already encountered. This third model of discourse can be reconstructed from the works of Jurgen Habermas and Karl-Otto Apel, which we have abbreviated under the title of the programme for paradigm complementarity.

The works of these two writers are only now beginning to be read by an English speaking public, although it is, more often than not, a somewhat different public than that which has attended to the writings of Kuhn and Feyerabend. For whereas the work of the latter has mostly preoccupied the circle around the present controversies with the philosophy of science, the work of the former is better known to social scientists in the schools of critical theory and hemeneutics. It is safe to say, that although the concerns of both the historiographic philosophers of science and the critical and hermeneutic theorists coincide on a number of important topics, there has been little commentary that has compared or evaluated them together. Part of this neglect will hopefully be remedied in a small way by this chapter section.

The importance of the ideas of Habermas and Apel to our

present limited investigations lies in their materialisation of the method of political epistemology. For where Feyerabend's struggle against the positivism of the natural and social sciences is conducted at the level of theory, through the theoretical reconstruction of the objects of experience and perception, Habermas recognises that theoretical criticism becomes effective only when it is linked to an historical social practice. For this reason he argues that a profound criticism of knowledge must also become at the same time a critical theory of society. It is only at the fundamental level of practice that theoretical criticism may become a material force in the historical conditions of social change and realise itself as practical criticism.

The most important aspect of the contributions of Habermas and Apel to an elucidation of the problems that have concerned us so far lies in their attempted materialisation of these problems. For whereas, as we have seen, most philosophical discussion of the relationships between models of scientific discourse and programmes for the growth of scientific knowledge has been largely concerned with the intellectual operations of science, whether as in the case of the logical empiricists this is concern over the logical status of scientific propositions, or as in the case of the new historiographers the theory-ladenness of observation languages, the

primary interest of philosophers has been and remains that of the cognitive structure and intellectual operations of science. There has been remarkably little interest shown in the practice of science as a global societal institution.

This is not to say that the role of material factors in the history of science has gone totally unacknowledged. An increasing number of philosophers of science continue to pay lip service to the importance of the material factors in history, and the necessity for a sociology or historiography of science to record the impact of these factors upon the historical practice of science. Thus McMullin stresses the coincidence of interest between the sociologist and historian of science, and the importance of the work of both these specialties to the philosophy of science.

> ...the philosopher and the sociologist have to begin from the activities of real people; they may not invent their material, they have to find it. This can be seen easily in the case of philosophy, because it is for the most part at such a high level of generality that specific reference to concrete instances, instances requiring the skill of the historian to establish or unravel them, is rarely found.
>
> McMullin, (1970:55).

Kuhn also has been prompt in acknowledging the importance of material factors in the history of science, as well as justifying his own neglect of them.

> I have said nothing about the role of technological advances or of external social, economic, and intellectual conditions in the development of

> the sciences...Explicit consideration of effects like these would not, I think, modify the main theses developed in this essay, but it would surely add an analytic dimension of first-rate importance for the understanding of scientific advance.
>
> Kuhn, (1970a:x).

Similarly, many other philosophers of science, positivist and anti-positivist alike, have lamented the separation of those studies which have examined the immanent cognitive structure of science from those which have had as their object the transcendental examination of science as one among many organised historical systems of knowledge.

For Habermas and Apel, however, science is not satisfactorily examined solely from the inside. It has, at the same time, to be investigated as a practical human activity alongside other social institutions. A critical theory of science is thus necessarily part of a correspondingly critical theory of society.

The work of both Habermas and Apel is, therefore, intended as a transcendental critique of science, which examines the internal cognitive structure and self-image of science in relation to the outer historical totality in which it is ultimately embedded. In following their critique, we shall see how the several reductionist variations on the binary language model of scientific discourse correspond to historically specific and separable <u>forms</u> and <u>aims</u> of scientific practice. At

the same time, we shall also consider the proposed conditions for the eventual resolution of the traditional contradictions between theoretical reductionism and observational reductionism, and the model of complementarity that replaces them.

In the earlier part of this chapter we have shown already some of the connections which exist between modes of scientific discourse and programmes for the growth of scientific knowledge. We have suggested, for example, that Kuhn's programme for paradigm closure in the sciences rests upon a revised version of observational reductionism in spite of the fact that much of the polemical content of Kuhn's work is directed against the original proponents of this mode of discourse. Feyerabend's programme for paradigm proliferation, on the other hand, has led in our estimation to another series of problems. For although the critical impact of his work has been considerable, as has that of Kuhn, in stimulating a renewed significant discussion of the problems associated with the construction, comparison, and testing of theories, his proposed resolution of these problems has proven less than satisfactory. Feyerabend's programme of proliferation, based as it is on a model of theoretical reductionism, has failed as a theory of science to relate the status of science as a <u>theoretical object</u> to its status as a <u>form of social practice</u>. Nowhere is this omission in Feyerabend's programme more no-

ticeable than in his failure to provide any criteria for formulating a selection procedure whereby some theories may be or should be supported at the expense of others at any given historical moment. In Feyerabend's world we are left, as it were, without any real basis for choosing between multiple interpretations of the world and the multiple rules of evidence contained in these interpretations. Such are the problems posed by a theoretical reductionist model of scientific discourse, and while different in kind from those posed by an observational reductionist model of scientific discourse, they appear no less intractable.

Part of the way out of these difficulties has been provided in the work of Habermas and Apel. For although the work of these wri[illegible]n many ways, no freer from criticism than that of [illegible] Feyerabend, the programme for the growth of scientific k[illegible]edge presented in their work offers some possible resolu[illegible]n of these problems already encountered. It is for this reas[illegible] only, that their theory of science is presented here as a positive contribution to the problem of reductionism in scientific discourse.

The Relationship between Knowledge and Human Interest

The part of Habermas' work most relevant to our investigation of models of scientific discourse is his theory of the relationship between knowledge and human interest. For it is

here that Habermas attempts to analyse the institution of science, both as theoretical object and as social practice.

According to Habermas, therefore, the sciences are best classified on the basis of the "knowledge-constitutive or knowledge-guiding interests that underlie them. The concept of "knowledge-constitutive interest" is introduced in Habermas' work to provide a materialist foundation for his theory of science. Thus, in order to elucidate and compare the internal cognitive structures of the special sciences, it is necessary in Habermas' view, to be able to specify the institutional aims of science as expressed in the transcendental material categories of the knowledge-constitutive interests.

> I term <u>interests</u> the basic orientations rooted in specific fundamental conditions of the possible reproduction and self-constitution of the human species; namely <u>work and interaction</u>... Knowledge-constitutive interests can be defined exclusively as a function of the objectively constituted problems of the preservation of life that have been solved by the cultural form of existence as such.
>
> Habermas, (1972:196).

On this basis, Habermas distinguishes the two categories of <u>work</u> (social labour) and interaction as complementary aspects of the fundamental form of human social practice, and thus as the basic types of knowledge-constitutive interests underlying the primary modes of scientific discourse. It is

in his clarification of the dual character of the concept of "social practice" that Habermas reveals himself as a Marxist, as well as a revisionist of the more classical interpretations of Marxian theories of science. We shall briefly examine, therefore, Habermas' rereading of the classical Marxian theory of knowledge and science.

According to Habermas, Marx's theory of knowledge was too dogmatically grounded upon the model of production. For the only basic knowledge-constitutive interest acknowledged by Marx in his theory of science was that of social labour, or work. The development of knowledge in general and scientific knowledge in particular, therefore, is seen by Marx to correspond to changes in the forces and relations of production that dominate the human action systems of any historical period. For this reason, Habermas contends, Marx came to identify the growth of scientific knowledge in general with the growth of natural scientific knowledge in particular. The natural sciences could be readily credited with expanding the scope of human intervention and control over the natural world, and thereby directly contributed to the development of the material forces of production. Thus for Marx, the acknowledged role of science as a form of social practice was largely instrumental in character, inasmuch as the theory and practice of science made possible the technological applica-

tions of modern industry and the constant possibility of new forms of production.

> The natural sciences have developed an enormous activity and appointed an ever growing body of material...In a much more practical fashion, natural science has intervened in human life and transformed it by means of industry... Industry is the real historical relation of nature, and thus of natural science, to man.
>
> Marx, quoted in Habermas, (1972:45).

However, as Habermas points out, such a justification for the growth of scientific knowledge is hardly consistent with the dialectical method of Marx's system of historical materialism. For the history of social change has generally been analysed in Marxian social theory in terms of the relationship between the dual system of the forces and of the relations of production. It is useful, suggests Habermas, to regard these aspects of the general system of production as two separable action (or behavioural) systems. The instrumental action system of social practice which leads to the development of the material forces of production through the application of science to industry and which Habermas calls the system of work, or social labour. It is this aspect of science that has been historically predominant in the natural sciences and which Marx identified as the universal form of science. The knowledge-constitutive interests derived from the action system of work are, therefore, largely technical

and have as their object the extension of natural scientific discourse to resolve all puzzles of nature and to ensure the human hegemony over the natural environment.

At the same time, however, Habermas recalls that the Marxian theory of social change never degenerated into a simple reductionist theory although it was based on a model of production. The reason for this was that in the critical social theory presented in his work on political economy Marx continued to recognise the dialectical relationship between the forces and relations of production. Thus while on the one hand the material evolution of societies was recognised to correspond to the rise of critical revolutionary practice which eventually transformed the relations of production. (Habermas 1972:53). For this reason, suggests Habermas, men stand in a double relation to their own history: both as instrumental-technical agents in the transformation of the natural order and as communicative-political agents in the transformation of the moral order.

> Alongside the forces of production in which instrumental action is sedimented, Marx's social theory also incorporates into its approach the institutional framework, the relations of production. It does not eliminate from practice the structure of symbolic interaction and the role of cultural tradition, which are the only basis on which power and ideology can be comprehended.
>
> Habermas, (1972:42).

Habermas argues, therefore, that although Marx clearly recognised the dialectical relationship between the two action systems contained within the general system of production, he failed to acknowledge their mutual importance as knowledge-constitutive interests in a theory of science. In other words while his method of practicing the science of political economy always retained a dialectical content, his justification for using the method was based on reductionist presuppositions of scientific discourse.

> Thus in Marx's works a peculiar disproportion arises between the practice of inquiry and the limited philosophical self-understanding of this inquiry. In his empirical analyses Marx comprehends the history of the species under categories of material activity and the critical abolition of ideologies, of instrumental action and revolutionary practice, of labour and reflection at once. But Marx interprets what he does in the more restricted conception of the species' self-reflection through work alone...In fact it even prevented Marx from understanding his own mode of procedure from this point of view.
>
> Habermas, (1972:42).

In Habermas' analysis, therefore, it is possible to distinguish two fundamental kinds of knowledge-constitutive interests. The first is technical in content, and is derived from the instrumental action system of work (or social labour). The form of discourse that corresponds to sciences based on this knowledge-constitutive interest is that which Habermas describes as empirical-analytic. The empirical-

analytic sciences, for Habermas, are those sciences designed for the generation of productive knowledge (Habermas, 1972: 308). Theories are constructed as "hypothetical-deductive connections of propositions, which permit the deduction of law-like hypothesis with empirical content", (Habermas, 1972: 308). But perhaps more significantly, the empirical-analytic sciences are based on the "objectivist illusion" that their elementary descriptive statements express observations which "are supposed to be reliable in providing immediate evidence without the admixture of subjectivity" (1972:308). We may say without too much distortion, therefore, that Habermas' category of the empirical-analytic sciences overlaps to a significant degree with our own conception of an observational-reductionist model of scientific discourse.

The second kind of knowledge-constitutive interest, according to Habermas, is derived from the symbolic-interactional action system, especially the system of language, and has a practical rather than a technical content. The form of discourse that corresponds to sciences based on this knowledge-constitutive interest is that which Habermas call historical-hermeneutial. The historical-hermeneutic sciences are not established within the frame of reference of possible technical control as are the empirical-analytic sciences, but in the framework of the practical understanding of symbolic mean-

ings (Habermas, 1972:309-310). At the level of discourse, the logic of explanation through the hypothetico-deductive' method is replaced by the logic of interpretation through the method of hermaneutic understanding. Thus, whereas the empirical-analytic sciences are designed for the production of information that expands our power of technical control, the historical-hermaneutic sciences are designed for the communication of interpretations that make possible the orientation of actions within common traditions. (Habermas, 1972:313).

However, as Habermas acknowledges, the historical-hermaneutic sciences are also grounded upon a reductionist model of scientific discourse. For although they do not share the positivistic commitment of the empirical-analytic sciences to an observational-reductionist model of scientific discourse, they remain committed to a form of theoretical reductionism.

The nature of this form of theoretical reductionism becomes clearer when we further examine the mode of discourse employed in the historical-hermaneutic sciences. Unlike the empirical-analytic sciences that presuppose the possibility of stable observation languages which establish the procedures whereby experience may be linked to theory, the historical-hermaneutic sciences operate from a different model. Here, the range of experience is necessarily restricted by the particular form of the language in currency. Thus the structure

of the language-game used in any community provides the categories for the identification and interpretation of all experience.

> In the context of communicative action,...the role of transcendental framework is taken instead by the grammar of ordinary language, which simultaneously governs the non-verbal elements of a habitual mode of life conduct or practice. The grammar of language games links symbols, actions, and expression. It establishes schemata of world interpretation and interaction... What is real is that which can be experienced according to the interpretations of a prevailing symbolic system.
>
> Habermas, (1972:192).

The distinction that Habermas draws therefore, between the empirical-analytic sciences and the historical-hermeneutic sciences is in large part justified on the basis of this difference in their respective models of discourse. For the empirical-analytic sciences, observations are the building blocks of scientific theories and are connected to them by means of operations performed through the formal languages of logic or mathematics. (Habermas 1972:85). For the historical-hermeneutic sciences on the other hand, the ordinary language games of all forms of life contain the symbolic codes that reveal the structures of meaning available to their given speech communities. The task for the historical-hermaneutic scientist becomes, therefore, one of elucidating the grammar of particular language games and thereby interpreting the structures of meaning contained in ordinary language.

This method of investigation necessarily disclaims any interest in formulating protocol statements, or other types of stable observation languages, for the content of any given observation language is only ndered meaningful when examined in relation to the con xt of the total language game.

We may say, therefore, that whereas the empirical-analytic sciences are based on an observational reductionist model of scientific discourse, where the rules of evidence are supplied by a correspondence, or picture theory of truth, the historical-hermeneutic sciences are based on a theoretical-reductionist model of scientific discourse, where the rules of evidence are supplied by a pragmatic, or contextual theory of truth. In each case, argues Habermas, the mode of discourse employed by either form of science continues to preserve the "objectivist illusion" that conceals the relationship of knowledge to human interests.

In the empirical-analytical sciences, the model of discourse used corresponds to that of a logical monologue. Although this is suitable for generating propositions within a closed system of observational and theoretical languages, Habermas argues that it is not a model of discourse that can be used as a medium of communication between complementary subjects. The discourse of the empirical-analytic sciences always presupposes a relationship between a single subject

and object, rather than between different subjects.

> Deduction, induction, and abduction establish relations between statements that are in principle monologie. It is possible to think in syllogisms, but not to conduct a dialogue in them. I can use syllogistic reasoning to yield arguments for a discussion, but I cannot argue syllogistically with another. Inasfar as the employment of symbols is constitutive for the behavioural system of instrumental action, the use of language involved is monologic.
>
> Habermas, (1972:137). (Emphasis supplied)

Or again,

> ...from the point of view of possible technical control...language is separated out of its embededness in interactions and attains monologic closure. Action is severed from communication and reduced to the solitary act of the purposive-rational utilization of means.
>
> Habermas (1972:193). (Emphasis supplied)

Habermas is describing, in so many words, some of the attributes that we have included in our description of the programme for paradigm closure, which similarly corresponds to the use of an observational reductionist model of scientific discourse. This character of the monologic closure of language in the empirical-analytical sciences has also been dealt with at some length by Apel (1967, 1972).

In the historical-hermeneutic sciences, on the other hand, the preservation of the "objectivist illusion" is a result of the historicist content of this form of knowledge. For although the historical-hermeneutic sciences reject the positivism of the empirical-analytical sciences, and the ob-

servational reductionism that underlies it, the historicism inherent in the theoretical reductionist model of scientific discourse preserves in a different way the "objectivist illusion".

> Much as the cultural sciences may comprehend their facts through understanding and little though they may be concerned with discovering general laws, they nevertheless share with the empirical-analytic sciences the methodological consciousness of describing a structured reality within the horizon of the theoretical attitude. Historicism has become the positivism of the cultural and social sciences.
>
> Habermas, (1972:303).

What Habermas regards as objectification in the historical-hermeneutic sciences is the belief that the meanings of the text of an historical period may be interpreted in their original context by the modern interpreter. The illusion of the historical-hermeneutical sciences resides in the belief that it is possible for the modern interpreter to re-enter the historical language game and participate again within the original structures of meaning. This illusion, according Habermas, is the historicist counterpart to the positicist illusion of the existence of stable observational languages for the standardised reporting of human experience.

> Historicism has taken the understanding of meaning, in which mental facts are supposed to be given in direct evidence, and grafted onto it the objectivist illusion of pure theory. It appears as though the interpreter transposes himself into the horizon of the world or language from which a text derives its meaning. But here, too, the

> facts are first constituted in relation to the standards that establish them.
>
> Habermas, (1972:309).

The process of examing history, therefore, is no more neutral than the process of examining nature. Both forms of science resuppose basic institutional aims that define their respective programmes for the growth of knowledge. The technical aims of the empirical-analytic sciences are related to the growth of possible control over nature and are pursued through the mediation of work, or social labour. Whereas the practical aims of the historical-hermeneutic sciences are related to the development of a mutual understanding in the conduct of life and are pursued through the medium of language. However, both forms of science may be said to presuppose the "objectivist illusion" of reality inasmuch as they fail to acknowledge the particular human interests that underlie them.

To the extent, therefore, that both forms of science have been part of the material and intellectual heritage of modern man, Habermas regards them as complementary. Thus in an important sense, Habermas' programme for the growth of scientific knowledge differs from that of the logical empiricists or the new historiographers of science both in his separation of the natural from the social sciences, and in his recognition of their complementarity. It is for this reason that we have called Habermas' theory of science: the pro-

gramme for paradigm complementarity.

We have so far only sketched out the broadest outline of Habermas' ideas on the complementarity of alternative modes of scientific discourse. For the interest of Habermas' work lies in the use of the category of paradigm complementarity as a solution to the problem of reductionism in traditional conceptualisations of a model of scientific discourse, and a corresponding programme for the future growth of scientific knowledge. We shall now therefore, proceed to examine in greater detail those aspects of Habermas' theory of science that relate most directly to the problem of reductionism and to the relationship between science as a theoretical object and science as a form of social practice. Some of the illustrations presented here are taken from the work of Habermas' colleague (and teacher), Karl Otto Apel, whose interest in these questions is similar to that of Habermas.

Reductionism, Complementarity and the Binary Language Model of Scientific Discourse

Habermas' critique of the two forms of reductionism found at the basis of the discourse of the empirical-analytic sciences and the historical-hermeneutic sciences leads him to formulate a model of discourse which overcomes the problems of theoretical observational reductionism for the case of the social sciences. This form of science Habermas calls critical social science, and it is based on a dialectical

model of scientific discourse. However, in order to evaluate more precisely the s[illegible] Habermas' solution to the problem of reductionism[illegible] turn now to the work of Karl Otto Apel for a [illegible]rth[illegible]ification of Habermas' theory in relation to thi[illegible]robl[illegible]

In much the [illegible]ame [illegible]ay as Habermas, Apel has criticised the positivistic [illegible]ications of the empirical-analytical sciences and has d[illegible] particular attention to the problem of observational reduc[illegible]nism. Writing on the historical failure of the original [illegible]ical empiricists to live up to the early promises of the [illegible]ienna Circle, Apel distinguishes two particular premises that have remained untenable in the practice of science.

> ...it became cle[illegible] that the following ideal postulates simpl[illegible] could not be realised in the logic of science: first, the idea of one syntactico-semantical framework which could be presupposed as the universal language of science as a whole; second, the idea of observational sentences which could be considered as copies of facts (protocol sentences) independent of theoretical contexts already implied in the formulation of the observational sentences.
>
> Apel, (1972:6).

The failure of the logical empiricists' programme for the growth of social scientific knowledge has resulted, in Apel's view, from the dogmatic attachment to the observational reductionist model of scientific discourse. In his criticisms of the use of this model of discourse in the social sciences,

therefore, Apel has attempted to reveal some of the untenable presuppositions that lie unanalysed at the basis of the positivistic use of language in modern science.

Perhaps the most fundamental presupposition that pervades the empirical-analytical sciences and their programme for the semantical reconstruction of the language of science, argues Apel, is that of methodical solipsism. For in their theoretical interpretations of the correct procedures for generating scientific knowledge, formulating meaningful statements, certifying rules of evidence, they fail to reflect upon the social conventions that underlie scientific discourse. Such a failure, suggests Apel, leads to the illusory interpretation that by following the rules of scientific discourse the isolated individual is able to practice science. This misinterpretation of the nature of scientific discourse has arisen through the philosophy of logical empiricism which has assumed that objective knowledge is possible without presupposing at the same time the intersubjective understanding by communities.

> ...I wish to maintain the thesis that a philosophy which postulates a physicalistic-behaviouristic language for objectifying the phenomena of human intersubjectivity involves methodical solipsism to no lesser extent than a philosophy that starts from the assumption that meaning and truth are matters of introspective evidence of private experiences of consciousness.
>
> Apel, (1972:12).

Thus Apel's theoretical criticism of the positivistic justification of a discourse for the social sciences based on an observational reductionism is taken from Wittgenstein's later critical use of the concept of 'language game'. For when the discourse of the sciences is recognised as another, although more specialised, type of language game, it becomes apparent that the rules for the established use of language must rest upon socially followed conventions, rather than on a direct correspondence to the world.

It is, perhaps, worth noting that the term "methodical solipism" was originally coined by Rudolph Carnap as a methodological injunction for personal verification in matters of scientific reporting, but the term was later abandoned in the face of frequent misunderstanding (Carnap 1936: 423-424).

In the hands of Apel, however, the concept of "methodical solipism" is turned to critical account against the empirical-analytical sciences. For these sciences, based as they are on an observational reductionist model of scientific discourse, are constructed as though a stable observational basis for their protocol statements makes possible the construction of a universal language, which is the language of science. (Apel 1972:12). For Apel, however, as for the later Wittgenstein, such a universal language would only be possible if we can also presuppose the existence of a correspondingly

universal form of life. But in a world where all forms of life are mediated by history and culture, such a material universalisation is nowhere to be found. Thus a presupposition of the methodical solipism of the empirical-analytical sciences reduces itself, as Apel shows, to an argument for the existence of private languages. (Apel, 1972:12). In this case, therefore, the descriptive languages of observation in the empirical analytical sciences must be justified on the basis of the priviledged verification provided by individuals. But as Apel further recalls, the arguments for the existence of private languages have already been repudiated by Wittgenstein with his formulation of the concept of the public "language game". For in this interpretation, the rules of empirical-analytical scientific discourse are established within the total context of the language game of science. The meanings of scientific terms are similarly defined within the context of this language game and are anchored to a correspondingly specific form of life: the system of instrumental action. We are back at this point, to the criticisms of Kuhn, Feyerabend and Habermas, which all substantially agree with Apel's critique of the observational reductionist model of scientific discourse. However, it is precisely at this point where Feyerabend, for example, retreats to a theoretical reductionist model of scientific discourse, that Apel's critical

insight is most fully revealed. For his rejection of observational reductionism as a model of scientific discourse does not predispose him to accepting a theoretical reductionist model of scientific discourse.

For the critical concept of "language game", when applied to all forms of discourse, has traditionally suggested various levels of relativism: historical, cultural and epistemological. Indeed, these consequences are impossible to avoid, for the premises of Wittgenstein's later method of investigation assumed the relativism and incommensurability of different forms of life. For these philosophers of science, therefore, who share Wittgenstein's preoccupation with an analysis of the language games of discourse, it is evident that the meaning of presuppositions can only be explicated within the rules of the total language game. In the hands of such philosophers as Peter Winch (1958), the analytic philosophy of language has been converted into a programme for the future growth of social scientific knowledge which is essentially indistinguishable in its consequences from that of Feyerabend. The existence of multiple language games, like the existence of multiple theories, is a condition that denies the possibility of common selection procedures for choosing one language game over another. But, as in the case of Feyerabend, the ultimate reduction of all discourse to the

internal structure of the rules of the language game conceals the historical content of discourse both as a theoretical object and as a form of social practice. The analytic philosophy of language thus ends in specifying an abstract method for the investigation of language games which succeeds in obscuring the historical content of discourse.

> Winch, however, due to his Wittgensteinian presupposition that language games determine the limits of understanding and that any question can be asked meaningfully only within a specific language game, arrives at a kind of 'methodology' of different cultural systems. A further consequence is radical relativism in philosophy, or in social science as he sees it.
>
> Accordingly to Winch, the different cultural systems, e.g. the forms of a magical interpretation of the world, can only be understood in terms of their immanent rules, i.e. in the context of their own social forms of life. If we think of our all too hasty explanations or archaic and exotic conceptions of the world in terms of Western, especially scientific standards of knowledge, we can only approve of Winch's demand as a conditio sine que non for sociological research of cultures. Is not a concrete system like the Western Christian religion really a compound of very different language games...which were nevertheless united through continuous argument with each other. And the external critique of religious forms - not admissable according to Winch - was it not always a part of the concrete dialogue of Western Christian religion...
>
> Apel, (1967:54).

Perhaps, more significantly than even this, Apel later concludes that the real problem with the Wittgensteinian concept of language game is that it conceals the existence of

change and perpetual historical process.

> If we...apply Wittgenstein's principles to the concrete history of the human mind, thus admitting all language games to be understandable - also the meta-physical ones, which really should not have functioned, according to Wittgenstein - then we see that these principles do not fit here. The language games of the concrete history of the human mind are not understandable just by and in themselves in the course of history, producing - through permanent dialogue - the reflection and critical questioning of their own principles (as well as the critique and eventually revolutionary corrections of the corresponding forms of life).
>
> Apel, (1967:55).

In place of the mechanistic conceptualisation of the 'language game' which necessarily has to correspond to an appropriate 'form of life', Apel insists upon a more historical interpretation of the language game. According to this interpretation language games may be seen as <u>dialectical units</u> which coordinate the use of language and an understanding of the world, with a practical form of life. (Apel 1967:56). Thus, for Apel, it remains conceivable for a language game from the past to exist with a form of life from the present, for the process of historical change generally results in such discrepancies.

The notion of dialectical unity, suggested by Apel, however, is also incorporated into a model of scientific discourse. It is presented as a solution to the assymmetry of either observational or theoretical reductionism, and as a

method for dissolving the objectivist illusion that has traditionally haunted scientific discourse. For whereas the logical empiricists have argued for the recognition and the application of a universally reconstructed language of logic and mathematics to express all propositions in the scientific mode of discourse, and while the anti-positivist philosophers have argued for the recognition of the fundamental distinction between the discourses of the empirical-analytic and the historical-hermeneutic sciences, Apel following Habermas, insists on the mutual interrelationship of both types of discourse.

> From this I conclude that philosophy of science in a broad sense, including the humanities, has to take into account two quite different, but complementary, leading interests of cognition. Only one of them is that of science in the narrow sense of the modern logic of science, that is, the leading interest of describing and explaining objectified data of the world. I would assume a close connection between this interest of cognition and instrumental labour, that is, operating on nature as an environment to be adapted to by experimental behaviour, learning by trial and error, and so on. In any event, the relation of scientific cognition in this sense to the practice of life is, nowadays, a technological one. Now, the other leading interest of cognition, complementary to the interest of an objectifying science, is, in my opinion, the interest in improving communication in own dimension of intersubjectivity.
>
> Apel, (1972:28).

This, of course, is a logical continuation of the argument already encountered in Habermas' work, that the discourse of the empirical-analytic sciences and the historical-hermen-

eutic sciences arise from the bases of different forms of social practice: instrumental and communicative action. Both Habermas and Apel, therefore, by revealing the internal inconsistencies of each of these two models of discourse which includes both forms. The rejection of both the reductionist variations on the binary language model of scientific discourse reopens the possibility of formulating a genuinely dialectical model of scientific discourse.

Thus, for Habermas and Apel, the first step towards the conceptualisation of a dialectical model for the use of language in the sciences is the recognition of the complementarity of the traditionally separated systems of discourse in the empirical-analytical sciences and in the historical-hermeneutical sciences. This solution stands in marked contrast to that of Kuhn, whose programme for paradigm closure is designed to generate only one dominant system of discourse within a specific discipline. It is also distinct from Feyerabend's programme for paradigm proliferation, for unlike the principle of "anything goes", the programme for paradigm complementarity establishes the relationship between theoretical science and social practice, and thus provides a framework for the formulation of selection procedures to choose between multiple theories which may compete for the explanation of a given event. The programme for paradigm complementarity is premised

upon the assumption that both the reductionist variations in the use of language are mutually inseparable in reality and should, therefore, be unified in both the theory and practice of scientific discourse.

> Objective explanation of facts and intersubjective communication about what is to be explained are instead 'complementary' aspects of human knowledge - in the sense in which N. Bohr used the word. They exclude each other and they presuppose each other. Nobody can just 'understand' without presupposing factual knowledge which could be stated explicitly as 'explanations'. On the other hand, no natural scientist can explain anything without participating in the intersubjective communication described above.
>
> Apel, (1967:23).

Apel thus makes clear that the programme for complementarity is particularly designed to transcend the reductionist conceptualisations of scientific discourse.

> I will call this first part, for the sake of brevity, the Thesis of Complementarity, and I will explicitly state my claim that this thesis refutes every kind of scientific reductionism.
>
> Apel, (1972:30).

In conclusion therefore, Habermas and Apel present their solution to the problematic use of language in scientific discourse in the following terms. Their criticism of the "discourse of the empirical-analytic sciences includes the recognition that these sciences are based on an observational (reductionist model of scientific discourse. It is largely on the basis of this restricted and dogmatic use of language that the empirical-analytic sciences have preserved the "objecti-

vist illusion" in their conceptualisation of the structure of natural reality. For where the protocol sentences of scientific discourse are assumed to offer a neutral language of descriptive report, the propositions which contain the sentences are seen as reflecting the structure of natural reality "as it really exists". In other words, they designate ontological categories in the framework of scientific language construction and application. There is, therefore, consensus in the works of Habermas and Apel that an equation exists between the observational reductionist model of scientific discourse and the objectivism which characterises the language of the empirical-analytic sciences.

On the other hand, the historical-hermeneutic sciences, which are based on a theoretical reductionist model of scientific discourse preserve another form of objectivism in the social sciences. Instead of the objectivism that corresponds to the belief in the possibility of a stable observational language, the historical-hermeneutic sciences assume the possibility of elucidating the original meaning of previous semantic systems through a reconstruction of the original language game and the form of life from which it was derived.

In both cases, Habermas and Apel maintain, the empirical-analytic sciences and the historical-hermaneutic sciences, the certification of evidence is not regarded as problematic

either as a stable observation language or as a previous historical form of discourse. In the case of the empirical-analytic sciences, the presupposition of an observational reductionist model of scientific discourse is a precondition for formulating the theory of a universal language of science which is incorporated into a programme for the growth of scientific knowledge, as that of paradigm closure.

In the case of the historical-hermeneutic sciences, the presupposition of a theoretical reductionist model of scientific discourse is the precondition for formulating a relativistic theory of scientific language, which is incorporated into a programme for the growth of scientific knowledge, as that of paradigm proliferation. Both theories of science, and the reductionist models of discourse that underlie them are individually inadequate for the construction of a satisfactory model of scientific discourse.

CHAPTER IV

THE MULTIPLE LANGUAGES OF SOCIOLOGICAL DISCOURSE

The Case for a Paradigm Language

The intention of this last chapter is to show how the foregoing analysis of the relations between models of scientific discourse and programmes for the growth of scientific knowledge may be applied to the special area of Sociology.

The case for constructing a paradigm language of observation and/or theory in Sociology has been advanced by a number of writers representing diverse theoretical, epistemological and methodological positions. Not every writer presented in this section, however, has made the need for the construction of a sociological paradigm language, or metalanguage, an explicit and programmatic demand. Some writers, it is true, have formulated the demand for a uniform language of sociological discourse as a necessary precondition for the development of a general theory of social behaviour (or social action). But other writers have contributed to the discussion on sociological discourse from the more modest perspective of clarifying the present state of terminology used in Sociology from logical, empirical and theoretical standpoints.

Whatever the motivation and justification for these different contributions, they have together been included under the present heading for the reason that the general implica-

tion of these criticisms point clearly in the direction of constructing a more standardised and paradigmatic language of theory and/or observation.

Toward a Criticism of Sociological Terms

One of the landmark texts in the literature of theoretical sociology to seriously address itself to the problems of re-evaluating the logical and theoretical status of sociological discourse was Zetterberg's, On Theory and Verification in Sociology, (1954). For it was in this work that Zetterberg drew general attention to the evident lack of consensus within the discipline over the use of even the most basic terms in sociological discourse.

> ...the actual number of sociological laws is subject to debate, because different sociologists cannot agree on how stiff to make criteria for calling a general statement about societal life a sociological law. Furthermore, there is a lack of agreement about the precise language and formulation of these laws. Any inventory of the laws of sociology becomes, therefore, subject to some convictions and preferences not shared by colleagues in all details.
>
> Zetterberg, (1965:12-13). (emphasis supplied).

Zetterberg's main task throughout this work was to present a preferred method for the construction of what he regarded as 'scientific' theory in Sociology, and to point out the inadequacies of what has traditionally passed as classical sociological theory. Toward this end he recommended a programme of logical formalisation for the construction of

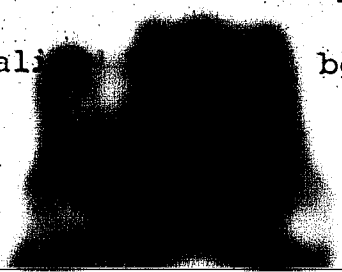
theoretical propositions in Sociology, which has subsequently come to be called the 'Axiomatic Method'.

The starting point for any formal theory construction in Sociology, according to Zetterberg, is the proper definition of scientific terms. Unlike the more exact sciences, however, the community of social scientists have not evolved a common terminology with standardised definitions.

> Sociologists have spent much energy in developing technical definitions, but to date they have not achieved a consensus about them that is commensurate with their effort. At present there are so many competing definitions for key sociological notions such as 'status' and 'social role' that these terms are no more valuable than their counterparts, 'position' and 'social relation', in everyday life.
>
> Zetterberg, (1965:30).

However, when it is recognised that one of the necessary functions of a scientific sociological discourse is the translation of terms from original historical and cultural sources into a technical vocabulary that is universalisable, then the need for a systematically defined sociological terminology becomes readily apparent. (Zetterberg, 1965:31,44).

The process of definition, for Zetterberg, is one of the most important ways of logically reconstructing the discourse of Sociology. For the logical analysis of sociological terms, which is a necessary precondition for the construction of formali[illegible] begins with a preferred method



of definition.

Following Hempel (1952), therefore, Zetterberg distinguishes between the logical and extra-logical components in any scheme of definitions. Words such as 'and', 'or', 'not', 'imply', et cetera, are part of the universal logical structure which governs the operation of all scientific terms. While words such as 'entropy', 'reinforcement', 'homeostasis', 'social system', et cetera, refer to complex properties, events, or relations, defined in the terms of a particular scientific theory language. In the ideal case of scientific theory construction, Zetterberg suggests, the extra-logical terms in a definition, or scheme of definitions, should be incapable of further reduction. That is, they should have the the status of primitive terms, which resist further definition and stand as axioms within a propositional system.

> In an ideal theory it should, furthermore, be possible to find a small group of extra-logical words, the 'primitive terms' which in different combinations with each other and with logical terms can define all other extra-logical terms of the theory, the 'derived terms'. Any derived term, in short, is obtained by combinations of the primitive terms and the logical words.
>
> Zetterberg, (1965:47).

Zetterberg's programme for the construction of formal theories in Sociology, therefore, starts from the premise that the complex terms of sociological discourse are reduc-

ible to primitive, or axiomatic terms. From such a set of axioms, Zetterberg concludes, it is possible to systematically deduce a general system of theoretical propositions.

> The fundamental idea of axiomatization was that, by strict propositional logic, one could derive all the empirical statements of a science from a set of axioms. There axiomatized theories would, in turn, make possible other deductions. By testing the consequence of a set of axioms, one could gradually build a tested, axiomatized deductive theory.
>
> Mullins, (1973:218).

As an example of the axiomatic method, Zetterberg introduces several 'theorems' formulated from Durkheim's theory of the division of labour, from which he proceeds to specify the basic concepts. Such theorems, for example, include:

(i) The greater the division of labour the greater the consensus.

(ii) The greater the solidarity, the greater the number of associates per number.

(iii) The greater the number of associates per number, the greater the concensus.

(iv) The greater the division of labour, the greater the solidarity.

From these theorems, Zetterberg isolates a number of basic concepts such as, 'division of labour', 'norm', 'consensus', 'behaviour', 'member', 'group', 'solidarity', et cetera. On the basis of these primitive terms, Zetterberg

derives more complex formulations through the process of nominal definition. Using the primitive terms as the axiomatic basis of his theory language he is able to derive, for example:

Uniformity - the proportion of members whose behaviour is the norm of the group.

Deviation - the proportion of members whose behaviour is not the norm of the group.

The key to Zetterberg's programme for the formalised construction of sociological theory, therefore, lies in his method of axiomatisation. For it is only by defining the primitive terms of the theory language that we may acquire the 'building blocks' for the construction of the more abstract and derivative terms needed for the formulation of generalised theoretical propositions. The process of translating the terms of a sociological theory language into the terms of an observation language, Zetterberg also identifies as a process of definition. Thus for concepts which have been nominally defined in the theory language it is possible to yield corresponding concepts in the observation language through the process of operational definition. Some examples are provided.

NOMINAL DEFINITION:	Degree of division of labour in society.
OPERATIONAL DEFINITION:	Number of occupations in society.

NOMINAL DEFINITION: Degree of rejection of deviants

OPERATIONAL DEFINITION: Rate of execution, incarceration or criminal prosecution.

Zetterberg's general recommendation for the present state of sociological discourse, therefore, is for the logical reconstruction of the foundations of sociological theory languages through the axiomatisation of their basic terms. In this way Zetterberg was convinced that he was establishing the necessary preconditions for the growth of an hypothetico-deductive science of society.

From the above account, it is evident that Zetterberg conceives of a model for the formal construction of sociological theories in similar terms to the observation-reductionist model of scientific discourse previously examined in this work. The injunction to logically reconstruct the foundation of theory languages on the basis of axiomatised primitive terms owes much to the logical empiricist programme for the growth of scientific knowledge. In this sense, Wittgenstein's early dissolution of the complex predicates of discourse into their constituent elementary propositions, as well as the various proposals by Naurath, Carnap, Nagel and others for the construction of scientific observation language, together share with Zetterberg a common frame of reference.

Zetterberg's programme for the axiomatisation of socio-

logical discourse, however, has not gone totally unchallenged. Among the more critical responses to Zetterberg's work from quantitative sociologists have been those of Duncan (1963), Costner and Leik (1964), and Blalock (1964, 1966).

One of the more interesting criticisms of Zetterberg's work has been made by Charles Lachenmeyer in his book, The Language of Sociology, (1971). In this work Lachenmeyer repeatedly demonstrates the failure of sociologists to evolve a language which is capable of scientific precision. To the extent, therefore, that Zetterberg's work has advertised the need for the logical systematisation of sociological terms, Lachenmeyer is in general agreement. However, it is evident, even from a cursory reading of Lachenmeyer that Zetterberg's programme for the logical reconstruction of sociological theory languages falls considerably short of establishing adequate criteria for the precise use of sociological terms.

> Zetterberg claims that several distinct advantages accrue from the axiomatisation of sociological theory. By and large, these advantages are genuine, and I will not make explicit reference to them...What I want to emphasise is that as beneficial and for as faulty as certain aspects of the axiomatization of sociological theory may be, it is at best a partial solution to the definitional-theoretical problems of sociology we have seen.
>
> Lachenmeyer, (1971:87).

Lachenmeyer argues that Zetterberg's axiomatic method suffers from several significant limitations which substan-

tially detract from its value as a method of for theory construction in Sociology. These limitations, which are related to the general level of imprecision of sociological theory languages, are classified by Lachenmeyer within the context of his own investigations into the logical status of sociological discourse.

One of the basic limitations of Zetterberg's programme, suggests Lachenmeyer, is the uncritical acceptance of core terms from sociological discourse as primitive terms within an axiomatised propositional system. This would be legitimate only if such core terms carried precise definitions universally accepted within the discipline. But this is not the case, and such terms as 'group', 'social system', 'norm', 'solidarity', et cetera, have traditionally have been defined with a looseness and imprecision that makes them inimical to fuller logical axiomatisation.

A further related limitation of the axiomatic method uncovered by Lachenmeyer resides in the fact that many of the primitive terms introduced by Zetterberg do not designate observable properties, relations or events. Such terms as 'group', 'social system', 'norm', et cetera, refer to abstract concepts which would require the use of complex definitional chains before their object predicates could be stated in observable terms. Thus Lachenmeyer concludes that Zetterberg's

programme for the reconstruction of sociological discourse has neglected to specify the conditions under which the construction of a valid sociological observation language may be undertaken. Such an observation language, Lachenmeyer suggests, remains a necessary precondition for any system of scientific discourse.

The failure to achieve a language of theory and research in Sociology capable of scientific precision and of reduction to an empirically based observation language has resulted, in Lachenmeyer's view, from a series of logical fallacies which are endemic in present sociological discourse. A significant part of Lachenmeyer's programme for the reconstruction of sociological discourse is, therefore, directed toward a clarification of these sources of error in the use of sociological terms, in the expectation that a reconstructed sociological language may potentially free itself from such imprecision.

Lachenmeyer discusses four sources of error in the use of sociological terms that arise from common logical fallacies, and he suggests that any programme to control the imprecision of sociological discourse should start by minimising these sources of error. Most of these logical fallacies are endemic in ordinary language discourse, and their ubiquity in sociological discourse shows that the languages used by sociologists are closer to ordinary language than to scientific

language.

The first source of imprecision in much sociological discourse is the problem of vagueness. To Lechenmeyer, a term may be said to be vague when the range of object predicates forming a term's referential meaning has not been specified.

> In extreme cases, a vague term may have multiple, equipropable, specifiable object predicates. The term 'fucking' (in the vernacular) is often used so excessively in any (slang) linguistic statement that it has become vague in the extreme sense.
> Lachenmeyer, (1971:30).

Terms which are used without specification of the range of the object predicates are commonplace in sociological discourse. Lachenmeyer cites as an example the celebrated debate over the implications of the structural-functionalist perspective of society. He suggests that much of the controversy stemmed from the vagueness of terms such as 'function' and 'social structure'. For an assertion such as 'Every social structure has a function' has sometimes been interpreted to mean that "the established ways of doing things serve a purpose which justifies their continuance", (Lachenmayer, 1971:31). The problem of vagueness occurs, therefore, when the object predicates of a nominal remain unspecified and equiprobable.

The second source of imprecision in sociological discourse is the problem of ambiguity. A term may be said to be

ambiguous, suggests Lachenmeyer, when it carries multiple equally legitimate meanings. Whereas the problem of vagueness is caused by too many possible specifications of meaning, the problem of ambiguity is caused by too many actual specifications of meaning.

> Any term is ambiguous when more than two but a finite number of object predicates have been specified as equiprobable members of the set comprising the referential meaning.
> Lachenmeyer, (1971:32).

In sociological discourse, an excellent example of an ambiguously used term is provided by Kroeber and Kluckhohn's (1952) investigation into the multiple specifications of the meaning of 'culture'.

The third source of imprecision in sociological discourse is the problem of opacity. A term may be said to be opaque when there is no referent object of the sort represented by the term's object predicates. Traditional examples of opacity have been such terms as 'unicorn' or 'phoenix' for which no referent objects exist. However, as Lachenmeyer makes clear, the problem of opacity in sociological discourse is more commonly due to the improper use of legitimate terms, rather than through any confusion over the validity of a term's referential function.

> ...a term is used opaguely if it is used as if it designated directly observable objects, properties, or relations when, in fact, it does

> not and cannot without prior definition.
> Lachenmeyer, (1971:33).

Thus terms such as 'status', 'role', 'social system', 'society', et cetera, are constantly used as though they designated directly observable things, when in fact they refer not to empirical realities but to inferential realities. For this reason, therefore, Lachenmeyer is unable to accept Zetterberg's programme for language formalisation because it includes many opaque terms as primitive terms within the axiomatised language system. The problem of opacity, suggests Lachenmeyer, has prevented the development of an empirically based observational language in sociological discourse. Such a language is a necessary precondition for the further formalisation of sociological theory.

The fourth source of imprecision in sociological discourse, discussed by Lachenmeyer, is the problem of <u>contradiction</u>.

> Contradiction is a special case of ambiguity that occurs when a term has two different, equiprobable object predicates specified as its referential meaning and these object predicates are logically inconsistent.
> Lachenmeyer, (1971:34).

A term is thus in contradiction when both an assertion and a counter-assertion can be specified as parts of the referential meaning. The example Lachenmeyer gives is the <u>sui generis</u> fallacy involved in the definition of a group as both

the sum of its members and as more than the sum of its members. Such a use of a term, for Lachenmeyer, is contradictory because it contains logically inconsistent object predicates.

From Lachenmeyer's account of the significant sources of imprecision in sociological discourse it is evident that he can only regard Zetterberg's programme of axiomatisation as a very partial solution to these problems. For Zetterberg fails to appreciate that the sources of imprecision in sociological discourse are related to the routine use of even the most central terms in the vocabulary of the discipline. Any significant reconstruction of the foundations of sociological discourse must, therefore, by Lachenmeyer's standards, minimise the interference of factors such as vagueness, ambiguity, opacity and contradiction, and lay the basis for a genuinely empirical sociological observation language.

General criticism of the imprecision of sociological terms has become a commonplace in much current literature on sociological theory. More serious and analytical criticism of the present state of sociological discourse is not so easy to find, but is represented in a rising tradition of critical literature which includes such writers as Lundberg (1939), Blumer (1954), Scriven (1956), Bergman (1956), and the more recent contributions of such writers as Klima (1972), A. Kuhn

(1961), as well as those works already acknowledged.

Naturally enough, criticism of the present state of sociological discourse can be broken down into the different perspectives carried by the various critics of the discipline. Thus, those writers, such as Lundberg, Catton, Guttman, Homans, Zetterberg, Lachenmeyer, who have been influenced by the logical empiricists, tend to share a similar perspective on these problems. They also tend to share, to a lesser or greater extent, a similar view on the solutions to these problems. For even in a case where differences may arise between writers of a similar perspective, such as that between Zetterberg and Lachenmeyer, it should be evident that both writers have been concerned with related problems of definition, systematisation and axiomatisation, and for that reason write within a common tradition.

Another writer within this tradition who has written a series of papers on the problems of convential sociological discourse is Alvin Leyton (1956-1958a; 1956-1958b). In much the same way as Zetterberg and Lachenmeyer, Leyton argues for the construction of an empirically based observation language and for the elimination of those core primitives which fail to designate direct observables. He attributes the present absence of such an empirical data language to the relatively recent historical emergence of Sociology as an organised dis-

cipline. At the same time, however, the position is argued that without the emergence of a common observational language in sociology, the development of a theoretical science is impossible.

> The science of sociology is less than a hundred years old, and its results have been remarkable for that short space of time. But it awaits the development of its methodology. There must be more attention to the purely logical problem of the relation of the various fundamental social categories to each other. Most investigators feel free to use their own categories; and where these are expressed in common terms the old linguistic confusions arise, with all sorts of ill-defined penumbras of values and meanings avoiding the clarity so essential to any degree of rational thought.
>
> Leyton, (1956-1958a:398).

This argument is shared by many other commentators and theorists on the growth of scientific knowledge in Sociology. To examine the implications of the demand for a single theory and/or observation language in Sociology, we turn now to a brief review of several recent advocates of this position.

Toward the Construction of a Uniform Language

The greatest impetus for the recognition and/or construction of a uniform observation language for the discourse of Sociology came from the early programme of the logical empiricists. As we have already seen, the concerns of this school for the logical precision of language were carried into sociological discourse by such writers as Lundberg,

Zetterberg, and Lachenmeyer. However, in addition to the immediate need for the logical clarification of scientific systems of discourse, the logical empiricist programme also formulated the demand for a common language of observation to be recognised and/or constructed for each of the special sciences. The background principle underlying this demand for the development of common observation languages was, of course, the programme for A Unified Science to which all the original members of the Vienna Circle remained committed. Perhaps the major consequence of this programme for Sociology was the rejection of the earlier tradition of Idealist philosophy and historiography represented by such writers as Hegel, Dilthey, Windelband and Rickert in Germany, and Collingwood in England, which had always insisted that the natural sciences and the cultural sciences were separate forms of knowledge with their own appropriate methods of inquiry and their own systems of discourse. It was, therefore, against the legacy of these ideas, as much as anything else, that the programme for a Unified Science struggled in Sociology. It is with this polemic in the background, that the significance of the demand of the logical empiricists for a common language of observation in Sociology can be most easily understood.

(i) Neurath and the Logical Empiricists

The general significance of the logical empiricist pro-

gramme for a uniform language of observation for the discipline of Sociology can, perhaps, best be illustrated in the writings of Otto Neurath, who was both a sociologist and an original member of the Vienna Circle. A brief examination of his ideas on the status of sociological discourse reveals,

(i) the division within the Vienna Circle over the epistemological status of observation terms,

(ii) the general agreement on the need for a physicalistic (behaviouristic) observation language purged of 'metaphysical' expressions.

Neurath starts at a point of departure from the classical position of the Vienna Circle towards the epistemological status of observation terms. For whereas (the early) Carnap, Feigl and Schlick as well as (the early) Wittgenstein and Russell accepted, in one form or another, a position of logical atomism based on a correspondence theory of meaning and truth, Neurath did not. Wittgenstein, as we have already seen, claimed that it was the case that the elementary propositions of ordinary language discourse corresponded to the atomic facts of the world. It was through language, therefore, that knowledge was confirmed by reality, for elementary propositions which were incapable of a complete verification of their object predicated by their object referents were classed by Wittgenstein as cognitively insignificant and,

therefore, meaningless.

For Neurath, on the other hand, the form taken by an observation language for any of the special sciences was best expressed as a series of protocol sentences.

> Observation-statements, if formulated very carefully, may be called 'protocol-statements'. We shall use the term 'protocolist', as equivalent to 'protocol writer'. We may always ask for the 'protocolist-name' and for the details - that will depend on the discussion in question.
>
> Neurath, (1944:5).

Protocol sentences are, therefore, the terms and expressions of the observation language which are used to record the empirical phenomena (properties, relations or events) of the subject domain of the special sciences. However, Neurath emphatically rejects the claims made by other members of the Vienna Circle that the elementary (or primitive) terms of the observation language stand in a priviledged relationship to the world, or that they function as 'logical pictures' or reality. It is on the basis of this misapprehension propounded by Carnap, Wittgenstein and others, suggests Neurath, that the false idea of protocol statements as 'incorrigible' and stable elements of a scientific language system, not in need of verification, has gained support.

> There is no way of taking conclusively established pure protocol sentences as the starting point of the sciences...There are no sentences in the universal slang which one may characterize as 'more primitive than any others. All are of equal primitiveness. Personal nouns, words de-

> noting perceptions, and other words of little primitiveness occur in all factual sentences, or, at least, in the hypotheses from which they derive. All of which means that there are neither primitive protocol sentences nor sentences which are not subject to verification.
>
> Neurath, (1959:201, 205).

Neurath's objection to the position of logical atomism is based on a number of considerations, chief among them is his claim that the criteria for the verification of a proposition cannot reside in any mystical correspondence to the atomic 'facts of the world', but rather in the systematic agreement with other relevant propositions. This position was advocated by Neurath as a consequence of his rejection of Carnap's principle of methodological solipsism. This principle, it will be remembered, suggested that the process of verifying propositions could only be accomplished by subjecting them to the immediate private empirical experience of the individual. Such immediate experience, and the protocol language representing it, was regarded by Carnap as the empirical basis of scientific discourse, the primitive extra-logical terms of which were not in need of verification.

For Neurath, the protocol sentences of scientific discourse cannot be meaningfully be said to correspond to any extra-linguistic criteria, or to have any priviledged access to the facts of the 'real world'. They cannot, therefore, be assigned any fundamental status in the system of scientific

discourse for they are, indeed, as hypothetical and as subject to change as the theory languages which are logically derivable from them.

This rejection of the classical correspondence theory of meaning and truth leads Neurath to formulate what Schlick (1959:214) has called 'the coherence theory of truth'. Thus the test of cognitive significance for propositions is no longer achieved through the traditional reduction process to elementary propositions and correspondence with the 'facts', but is now a test of the consistency relations between the protocol sentences and non-protocol sentences (theory sentences) of a system of scientific discourse. (Neurath, 1959: 203-204).

In the case where a new protocol sentence fails to agree with the available system of propositions, Neurath points to the logical possibility that either the protocol sentence or the system of non-protocol sentences may have to be discarded, unlike Carnap for whom protocol sentences were incorrigible. (Neurath, 1959:203).

Thus Neurath's criterion for the verification of propositions is based on relations of consistency and coherence within the total system of available propositions. In this sense, his position seems to approximate that of (the later) Wittgenstein, although Neurath never actually conceptualises

the systems of scientific discourse as 'language games'. It is also for his 'coherence theory' of meaning and truth that Neurath has been most criticised by other commentators, (Schlick, 1959; Scheffler, 1967; Russell, 1962). For it has been argued that because Neurath rejects any extra-linguistic (factual) basis for the verification of propositions, he therefore possesses no criteria to select between conflicting or contradictory sets of sentences. Indeed, Neurath himself seems to intimate that in such cases the choice of propositional systems is more or less arbitrary.

> A social scientist who, after careful analysis, rejects certain reports and hypotheses, reaches a state, finally, in which he has to face comprehensive sets of statements which compete with other comprehensive sets of statements. All these sets may be composed of statements which seem to him plausible and acceptable. There is no place for an empiricist question: Which is the 'true' set? but only whether the social scientist has sufficient time and energy to try more than one set or to decide that he, in regard to his lack of time and energy - and this the more important point - should work with one of these comprehensive sets only.
>
> Neurath, (1944:13).

For this retreat into relativism Neurath is soundly berated by both Schlick and Scheffler.

The real significance of Neurath's impact on the discourse of Sociology, and one which he shared with the other members of the Vienna Circle on all branches of scientific discourse, was his programme for the empirical reconstruction

of the language of observation. For like Carnap, Wittgenstein, Schlick et al, Neurath saw the need to purge the languages of the special sciences, including Sociology, of the metaphysical terms and expressions that arise through the incorrect use of language. The protocol statements which form the language of observation should, therefore, according to Neurath, be formulated exclusively in those terms which designate aspects (i.e. properties, relations, or events) of observable physical behaviour.

> Without meaning to say that every sociologist must be trained in behaviourism, we can still demand of him, if he wishes to avoid errors, he must be careful to formulate all his descriptions of human behaviour in a wholly straightforward physicalistic fashion.
>
> Neurath (1959:299).

Neurath further argues (1944:6) that the systems of discourse in all cultures have protocol statements formulated in physicalistic (or behaviouristic) terms and that it is only on the basis of these protocol statements that propositions from one system of discourse may be translated into that of another system. For the only point of natural equivalence between different cultural language systems, in Neurath's view, lies in the concrete protocol statements of the culture and the collective practical experience that underlies them.

Neurath, therefore, recommends to sociologists use of an observation language based on the physicalistic categories

of objective behaviour. Such an observational language, he suggests, is able to make use of the protocol sentences that exist in all cultural languages, for the purposes of comparison and translation; and also the common use of a physicalistic observation language in all branches of the special sciences is seen by Neurath as a necessary precondition for the growth of a Unified Science.

On these points, Neurath stands in general agreement with the other members of the Vienna Circle, and is thus propounding a fundamental tenet in the doctrine of logical empiricism. For whereas Schlick, Carnap and Russell may have disagreed with Neurath over the epistemological status of protocol sentences, or the proper method for their justification, all of them agreed on the need to eliminate 'metaphysical' expressions from scientific discourse. For these reasons, therefore, the logical empiricist programme for the logical reconstruction of language of Sociological discourse had as its most important demand the construction of a paradigm language for the social sciences based on the physicalistic categories of objective human behaviour.

(ii) The Modern Legacy of Positivism

The necessity for a logical analysis and reconstruction of scientific systems of discourse, first raised by the logical empiricists, has remained an abiding issue in the disci-

pline of Sociology. However, it is now the case that demands for the elimination of logical imprecision from sociological discourse have increasingly been supplemented by additional demands for the construction and/or recognition of a common language of observation and/or theory.

Interestingly enough, although both of these demands were originally identified with the general programme of the logical empiricists for the growth of scientific knowledge in areas such as Sociology, more recent formulations of these demands have drawn their inspiration from altogether different philosophical sources. Thus to many sociologists, the work of an alledged arch-positivist such as Thomas Kuhn has been used to justify the need for a common (or paradigm) language of observation in Sociology. From our previous account of Kuhn's theory of science, it will be apparent that we have already anticipated this particular interpretation of Kuhn's work as The Programme for Paradigm Closure. It remains only to document some more recent examples of the demand for a paradigm language in Sociology, and to complete our theoretical analysis of this position.

One commentator who has recognised the monistic implications of Kuhn's work for the future construction of sociological discourse is H. Martins, (1972). In a paper discussing the role of paradigms in the future growth of scientific

knowledge in Sociology, Martins has expressed his conviction that Kuhn's work is generally regarded as prespective by many social scientists, and for this reason may be seen as a new justification for some of the classical demands originally raised by the logical empiricists.

> Although Kuhn's theory of science is clearly incompatible with logical empiricisms at the epistemological level, its influence on the social sciences is only too likely to reinforce that of logical empiricist canons. Formalization, quantification, extrusion of soft data, social behaviourism, 'ethical neutrality', the discouragement of epistemological debate and close links between systemmatic theory and the history of thought - all these prescriptions, prohibitions and preferences can be justified on standard neopositivist grounds, such as inter-subjective agreement, extrusions of extra-scientific valuations, predictive power.
>
> Martins, (1972:52).

Thus the demands for a greater consensus over the use of sociological terms, or over the selection of a common methodology for testing hypotheses, are increasingly likely to be justified on the basis of Kuhn's Programme for Paradigm Closure.

> Kuhnianism in social science is beginning to act as a functional equivalent and substitute for philosophical positivism. To some extent this 'Kuhn effect' is the result of the reception of Kuhn in a milieu saturated with positivism, but it also partly reflects the substance of Kuhn's analysis.
>
> Martins, (1972:52-53).

Another example of the more recent criticisms of socio-

logical discourse which draws inspiration from both the logical empiricists and from Kuhn's work is that of Rolf Klima (1972). Klima has become known for his criticism of the imported ideas of Feyerabend into the discourse of Sociology by writers such as Spinner (1973). Feyerabend, it will be recalled, is well known for recommending a programme of theoretical pluralism (described in our section on Paradigm Proliferation) to the natural sciences. Klima's main objection to using Feyerabend's theory of science as a programme for the growth of knowledge in Sociology is that it is unworkable at the present time. For in order to identify a case of theoretical pluralism, says Klima, it is necessary for the competing theories to have been formulated according to common methodological rules. It is only on this condition that theories may be said to 'compete' at all in their knowledge claims about reality. It is only on the basis of a common set of methodological rules that some theories may be eliminated and other theories selected according to generally agreed upon empirical criteria. Because Sociology has not achieved such general methodological consensus, concludes Klima, it is impossible to speak meaningfully about 'theoretical pluralism'. On the contrary, the existence of diverse schools of theory and method signal only the existence of a "quasi, apparent or pseudo pluralism", which does not represent a real

"competition of ideas", but only a "confrontation of standpoints". This situation argues Klima, is not conducive to the growth of scientific knowledge in Sociology because the truth conditions of sociological propositions are not subjected to common criteria of confirmation.

The more interesting aspect of Klima's position, for our present purposes, is his criticism of what he calls the state of "methodological dissensus" within sociological discourse. Upon further examination of Klima's critique, it appears that what he is largely descrying is the absence of a commonly shared (or paradigm) observation language in Sociology.

> Given the still prevailing 'essayistic' manner in which sociological theories are formulated, often not only the logical relations between the propositions, but indeed the meaning of the terms from which these propositions are constructed, remain in the dark.
>
> Klima, (1972:70).

As he further makes clear, the development of a paradigm language of observation in Sociology may be regarded as a necessary precondition for the proper construction and testing of theoretical propositions. Until such time as a paradigm language emerges in sociological discourse, sociological theory is doomed to eclecticism.

> What is now often camouflaged by the rich, sometimes colorful and flowery, sometimes excessively abstract and empty but impressive, descriptive vocabulary of the sociological language, is that

> most sociological theories - and especially the formalised ones - do not contain observables. The problem of connecting the theoretical terms with the observables (or "indicators") is usually treated under the catchwork 'operationalization' in a non-theoretical therefore arbitrary manner. The validity of the operations, considered as measurements of the theoretical terms, is most often based, not on well corroborated auxillary measurement theories, but rather on intuition, or at best, on some statistical ad-hoc tests of the internal consistency of several 'indicators'. But as long as the validity of sociological measurements remains as uncertain as is the case in most empirical studies, one cannot speak of satisfactory theory-testing, and one cannot yet talk about 'theoretical pluralism' in Sociology in a meaningful way.
>
> Klima, (1972:71).

Other proposals have been advanced to extend the construction of a uniform language of observation by systematising observation terms taken from the most developed social sciences. Thus Alfred Kuhn (1961), suggests the construction of a uniform language of information and knowledge which would develop a set of conceptualisations and a vocabulary from four interrelated fields of research: philosophy, information and communication theory, psychology and neurophysiology. He suggests that there exist certain isomorphic parallelisms in the conceptual structure of these special disciplines which can be expressed by the construction of a uniform language. Kuhn provides as an example of such a parallelism that existing between the neurological term, "network formation", the term "code formation" used in information/communi-

cation theory, and the psychological term "concept formation". In this way, he argues for the possibility of constructing a data language which can maximise the powerful theoretical advances achieved in all the above research fields.

The attraction of a unified language of observation for the discourse of Sociology has not only exercised the logical positivists. Luckmann (unpublished), for example, has argued for the development of a paradigm observation language based on a phenomenological, rather than behaviouristic criteria. Other writers, equally opposed to the logical empiricist programme for the reconstruction of a physicalistic language have also suggested the development of phenomenological data languages in sociological discourse, but most of these writers (Blumer 1956, Bruyn 1966, Denzin 1970, Phillips 1973), have stopped short of supporting the construction of paradigm languages.

For the construction of a paradigm data language in Sociology is based on a particular theory of the growth of scientific knowledge which we have called a Programme for Paradigm Closure. We shall now turn to examine alternative proposals for the development of sociological discourse which derive from different theories of scientific development.

The Case for Plural Languages

The case for the recognition or construction of multiple

languages or observation and theory in sociological discourse has been made by a number of commentators of different theoretical and methodological viewpoints. Their basis of unity, and reason for their inclusion in this present chapter section, lies in their opposition to the Programme for Paradigm Closure, and to the related programme for a unified language of observation or theory for the social sciences.

Part of this resistance to the programme for a unified language of science derives from the residual influence of an earlier tradition of idealist philosophy, a tradition which has always separated the natural sciences from the social sciences on the basis of the content of their subject domains. For it was held by such writers as Dilthy, Windelband and Collingwood that whereas the natural sciences investigated the properties of the natural world as an independent object of inquiry, the social sciences are in the distinctive position of having man both as the subject and as the object of scientific inquiry. For this reason, ran the essential argument, the methods of inquiry as well as the system of discourse of the natural sciences are inappropriate for use in the scientific investigation of human society. This debate over the presuppositions of scientific method is too well known to require further elaboration here, and our main interest touches only peripherally on the classical

questions that have traditionally been raised by it. A comprehensive documentation of these issues and an examination of their relevance for Sociology is provided by Radnitzsky (1968, 2 vols.). As earlier stated, however, these background polemics should not totally be lost from sight for they form the broader context in which more detailed debates over the epistemological status of theoretical terms become more readily intelligible.

We may say, as an introduction to this final section, that whereas the logical empiricists and other supporters of the programme for paradigm closure in the social sciences have started from the premise of the actual or potential unity of different systems of scientific discourse based on commonly constructed or recognised languages of observation, the opponents of this programme start from the contrary premise of the relativity of different systems of discourse. The premise of the relativity of systems of discourse, as a basis for opposition to observational-reductionist models of scientific discourse has been expressed in the literature of theoretical Sociology, (i) as a general critique of the logical empiricist call for a uniform language of the sciences, and (ii) as a specific critique of the programme for a paradigm language in Sociology. We shall, therefore, briefly proceed to review these two cases of relativism and show their

consequences for a model of scientific discourse within a discipline such as Sociology.

The Historical and Cultural Relativity of Systems of Discourse

One of the earliest and most important assaults on the programme for a unified science and for a uniform language of science came from a source that had been very close to the original Vienna Circle. For in the Tractatus, (1923), Wittgenstein had presented a theory of meaning and truth which was to significantly influence the school of logical empiricism for many years to come. With the completion of the Philosophical Investigations, (1953) however, it became evident that Wittgenstein had turned his back on some of the fundamental tenets in the early doctrine of logical empiricism, such as the theory of logical atomism and the correspondence theory of meaning and truth. In his rejection of these theories of meaning, Wittgenstein was led to a total renunciation of all models of ordinary and scientific discourse based on what we have called 'observational-reductionism', and proceeded to construct an alternative model of discourse which has exercised some considerable influence among social scientists.

After the rejection of his earlier formulation of the correspondence, or picture theory of language functions as set down in the Tractatus, Wittgenstein turned to a theory of

language based on the criteria of use and coherence. The meaning of terms of propositions, he now insisted, was not to be found in their correspondence to the facts of the empirical world, but rather in the part they played within a particular 'language game'. Language games for Wittgenstein are simple or primitive forms of language which are held together by the institution of particular rules of language usage. Thus the set of rules governing the use of language in one language game may be quite different and alien to the set of rules used in another. The problem of understanding (or elucidating) the meaning of a term or proposition, therefore, is a problem of exposing the actual set of rules operative in a particular language game, as well as interpreting the significance that the place of the term or proposition has within this set. The meaning of a term or proposition is, therefore, a specification of the use to which it is put within the language game as a whole. The rules used in language games, writes Wittgenstein, derive from the particular 'forms of life' that support them. Forms of life are thus institutions of human activity characterised by customs, practices and uses (P.I. #199). The relationship between the meanings of terms or propositions and the language game of which they are a part has been illuminated by the metaphor of a game of chess.

> To understand what a piece in chess is one must understand the whole game, the rules defining it, and the role of the piece in the game... Similarly we might say, the meaning of a word is its place in a language game. To put it in another way, the meaning of any simple word in a language is 'defined', 'constituted', 'determined', or 'fixed'...by the 'grammatical rules' with which it is used in that language...
>
> Fann, (1969:73).

One of the major implications of Wittgenstein's later philosophy, therefore, for a model of scientific discourse is the relativity of different historical-cultural languages of observation and theory. Unlike Neurath, who also retreated in a similar direction away from a correspondence theory of meaning, Wittgenstein did not recognise any basis for commonly shared meanings between alien cultural language games expressible in protocol sentences or any other form of metalanguage. For Wittgenstein strictly held to the view that the meanings which different and alien language communities attach to the terms of their languages, cannot, in any sense, be extrapolated from their particular language games. Thus the meaning of the observational terms of any particular language community, or the protocol sentences constructed from such terms, are intelligible only when the rules governing the use of these terms are revealed. They may only be revealed within the total context of the language game. Unlike Neurath, or any of the more orthodox members of

the Vienna Circle, therefore, Wittgenstein renounced once and for all any further speculation or consideration of a "common-sense language', 'a universal slang', or 'a universal physicalistic language' which might have operated as an observational metalanguage between different historical and cultural language communities.

Wittgenstein's later conclusions on the function of language were introduced into Sociology by Peter Winch (1958). In the same way as Wittgenstein, Winch held the view that the meanings different language communities attach to the terms of the language cannot be extrapolated from their particular language games. They cannot be fitted into a larger and more inclusive language game where the meanings taken from specific games can be independently and objectively evaluated on a transcendental scale of meaning. For Winch and Wittgenstein, at least such a transcendental dimension of meaning is unavailable, and the concepts of all societies lie locked into their particular language games where they can only be elucidated in their own terms.

> There is just as much point in saying that science, art, religion and philosophy are all concerned with making things intelligible as there is in saying that football, chess, patience and skipping are all games. But just as it would be foolish to say that all these activities are one part of one super-game, if only we were clever enough to learn how to play it, so it is foolish to suppose that the results of all those other

> activities should all add up to one grand theory of reality as some philosophers have imagined); with the corollory that it was their job to discover it.
>
> Winch, (1958:19).

The consequences of the philosophical presupposition of the relativity of different language communities for a model of scientific discourse are evidently critical. For the clear implication of Winch's work is to deny to systems of scientific discourse any advantages over ordinary language systems of discourse when the object of their investigation is human society. From this position, some commentators (Saran 1965, MacIntyre 1967, Cohen 1968, Gellner 1968, Jarvie 1972) have concluded that for Winch, the enterprise of social science remains an impossibility.

For Winch, the relativity of different language communities presumes the necessity for alternative models of discourse to those of the natural sciences.

> ...for whereas in the case of the natural scientist we have to deal with only one set of rules, namely those governing the scientific investigation itself, here what the sociologist is studying, as well as his study of it, is a human activity and is therefore carried on according to rules. And it is these rules, rather than those which govern the sociologists investigation, which specify what is to count as 'doing the same kind of thing' in relation to that kind of activity.
>
> Winch, (1958:87).

In other words, a model of scientific discourse based

on a standardised observation language is incapable of elucidating the meaningful content of alien language communities. For Winch, therefore, the paradigmatic status of the languages of science must be overthrown before a meaningful elucidation of the languages of other cultures can begin.

> The trouble is that the fascination science has for us makes it easy to adopt its scientific form as a paradigm against which to measure the intellectual respectability of other modes of discourse.
>
> Winch, (1964:308).

Thus according to Winch, any scientific investigation into a primitive or alien cultural community must begin with the scientists own participation in the language community under study. For the modern observation languages of the contemporary social sciences are based upon the forms of life which support such scientific cultures and are, therefore, restricted in their range of meaning to these cultures. Even when the sociologist or anthropologist begins to test the behaviour of an alien cultural community in the terms of a technical language of observation developed within his own scientific language community, Winch presumes a prior familiarity with the language of the community under study.

> ...although the...student of society, or of a particular mode of social life, may find it necessary to use concepts which are not taken from the forms of activity which he is investigating, but which are taken rather from the context of his own investigation, still these

> technical concepts of his will imply a previous understanding of those other concepts which belong to the activities under investigation.
>
> Winch, (1958:89).

The ultimate consequence of the Winch-Wittgenstein presupposition of the relativity of different language communities for a model of scientific discourse is a criticism of knowledge rather than the construction of alternate models of scientific discourse. The traditional claims of logical empiricism, contained in the programme for a unified science, are criticised for their inapplicability to the social sciences. For the observation language of the scientist which is taken as the empirical bedrock of his system of discourse is not universalisable to other language communities which are supported by different forms of life. The terms and propositions of such alien language communities may only be elucidated in the total context of the language game of which they remain an integral part.

Gellner (1968) in a highly critical review of Winch's work has proposed a number of conditions under which the relativistic presuppositions are invalidated. One of these conditions is that the forms of life which may once have been separate and distinct from that of our own scientific culture have now become increasingly integrated into our dominant form of life. Winch's continued insistence on the incommen-

surability of different forms of life and the untranslatability of different language games, writes Gellner, amounts to a new presupposition of 'collective solipsism': He adds an irritable footnote:

> For most modern thinkers, relativism is a problem: for Winch and Wittgenstein, it is a solution.
> Gellner, (1968:399).

Another writer greatly influenced by the philosophical investigations of Wittgenstein was Benjamin Whorf. In his book, Language, Thought and Reality, (1956) Whorf formulates his own version of a relativistic theory of language usage. For Whorf, the content of any system of discourse only acquires meaning through the particular form of language usage, that is through the particular set of grammatical rules in operation. Thus for Whorf, the syntactic structure of language, as much as the available categories and classes of propositions, determines the relationship of a language community to its particular material environment. In his investigations into the systems of discourse of alien and primitive language communities, therefore, Whorf always placed great importance on the grammatical structure of foreign languages, for it seemed to him that these structures revealed the hidden logic of an alternative form of life. For the same reasons as Wittgenstein and Winch, Whorf was also sensitive to what he regarded as the limitations of scientific systems of discourse, and was

critical of any claims for their universalisability.

> What we call 'scientific thought' is a specialization of Western Indo-European type of language, which has developed not only a set of different dialectics, but actually a set of different dialects. THESE DIALECTS ARE NOW BECOMING MUTUALLY UNINTELLIGIBLE...Every language and every well-knit technical sublanguage incorporates certain points of view and certain patterned resistances to widely divergent points of view...These resistances not only isolate artificially the particular sciences from each other, they also restrain the scientific spirit as a whole from taking the next freat step in development - a step which entails viewpoints unprecedented in science and a complete severence from traditions. For certain linguistic patterns rigidified in the dialectics of the sciences, often also embodied in the matrix of European culture from which these sciences have sprung, and long worshipped as pure Reason, per se, have been worked to death...
>
> Whorf, (1956:246-7).

Whorf's preoccupation with the grammatical structures of ordinary language discourse and his implication that these structures restrict the range of meaningful content, has drawn sharp criticism from commentators such as Feuer (1953). But his writings clearly dovetail with those of Wittgenstein and Winch on the common presuppositions of the relativity of different historical-cultural systems of discourse, and on the common opposition to any programme for a uniform language of science.

A more recent derivation of these presuppositions in Sociology may be seen in the rising tradition of Ethnometho-

dology. For in the same way as Wittgenstein, Winch and Whorf, Garfinkel (1967) has also insisted that the meaningful structures of social action may only be elucidated in terms of the rules which govern a particular form of life, and which are expressed in the relevant language communities. For Garfinkel and for other ethnomethodologists such as Cicourel and Sacks, the observation languages developed within the system of sociological discourse are determined by the imminent rules of the language game, and cannot be translated into the technical language game of the sociologists. In their own programme for sociological research, therefore, the ethnomethodologists have imported the critical presuppositions of the later Wittgenstein into the discourse of Sociology. It is probably fair to say that much of their work has taken form of a <u>criticism</u> of established sociological discourse, rather than the construction of an alternative model of discourse.

One other important tradition in Sociology to have upheld the presupposition of the relativity of different historical-cultural systems of discourse, and last tradition to be mentioned here, is that of the Sociology of Knowledge. For the Sociology of Knowledge is generally considered to be an outgrowth of Karl Marx's theory (and critique) of ideology, in fact, the central presupposition of those sociologists

working within this tradition has frequently been cited as Marx's famous dictum 'It is not the consciousness of men that determines their existence, but on the contrary their social existence determines their consciousness'.

It was the ambition of Karl Mannheim (1936), however, to convert the Marxian theory of ideology into an inclusive theory of knowledge which would be capable of logically justifying its own foundations. For Mannheim, one of the main problems of the Marxian theory of ideology was the logical contradiction that he believed was entailed in the two related claims of (i) the possibility of a criticism of previous ideologies (and systems of discourse) through exposing the historically redundant material interests they represented, and (ii) the possibility of a scientific Marxian theory of ideology and its independence form any historically or culturally limited context.

Mannheim believed, therefore, that Marx had fallen into the classical trap of the German Historical School, and one which could be traced back to the earliest forms of philosophical skepticism, by adopting a relativistic method of criticism which necessarily invalidated his own position and simultaneously liquidated any objective criteria for testing the truth conditions of knowledge claims.

In seeking to avoid what he regarded as the unreason-

able consequences of a presupposition of the relativity of different historical and cultural systems of discourse, therefore, Mannheim replaced the relativistic premise with a presupposition of the relational character of systems of discourse.

> If we wish to emancipate ourselves from this relativism we must seek to understand with the aid of the sociology of knowledge that it is not epistemology in any absolute sense but rather a certain historically transitory type of epistemology which is in conflict with the type of thought oriented to the social situation... A modern theory of knowledge which takes account of the relational as distinct from the merely relative character of all historical knowledge must start with the assumption that there are spheres of thought in which it is impossible to conceive of absolute truth existing independently of the values and position of the subject and unrelated to the social context.
>
> Mannheim, (1936:79).

The justification of propositions within a system of discourse is, for Mannheim, always made with criteria that are imminent within the system. This is a position that parallels that of Wittgenstein and his followers, and Mannheim's definition of relationism is seen to be very similar to Wittgenstein's definition of the language game.

> Relationism signifies merely that all of the elements of meaning in a given situation have reference to one another and derive their significance from this reciprocal interrelationship in a given frame of thought. (1936:86).
> ...Relationism does not signify that there are no criteria of rightness and wrongness in a discussion. It does insist, however, that it

> lies in the nature of certain assertions that they cannot be formulated absolutely, but only in terms of the perspectives of a given situation.
> Mannheim, (1936:283).

Thus it may be seen that the presuppositions of the relativity of different historical and cultural systems of discourse is shared by intellectual traditions within Sociology that have otherwise been considered as separate tendencies. The analytic philosophy of language, the cultural relativism of linguistic anthropology, the historical relativism of the German idealist school and its modern representation in the Sociology of knowledge, as well as the rising tradition of Ethnomethodology, all presuppose a fundamental criticism of the programme for a unified science and for a uniform language of sociological discourse. A conclusion shared by all these theoretical and methodological tendencies is that the observational-reductionist model of scientific discourse is inappropriate and unworkable for the social (and cultural) sciences.

To these examples we could also have added the Hermeneutic School of Sociology, some of whose representatives have been mentioned in a previous chapter. The relativistic presuppositions that form the basis of the above writers opposition to the observational-reductionist model of scientific discourse have in turn been criticised by other

commentators. A useful discussion of the more common criticisms of the relativistic position is provided by Cunningham (1972).

The general conclusion of these separate traditions of social theory is that the social sciences cannot operate with a model of discourse that has been developed in the research contexts of the natural sciences. For in rejecting the correspondence theory of meaning and truth, or any other justification for constructing an observational metalanguage, we are left with the necessity of formulating an alternative model of scientific discourse.

We shall now examine the alternative models of scientific discourse that have been proposed as a result of the renunciation of the programme for paradigm closure, and the rejection of the call for a paradigm language of observation and theory. The completion of this final stage of our analysis will bring the chapter to a close.

The Relativity of Observation Language in Sociological Discourse

The significance of the existence of multiple languages of theory and observation in the present system of sociological discourse has been apparent to many recent commentators on the discipline. To some (Kuhn 1970a) as we have seen, such pluralism has been interpreted as an indicator of underdevel-

disciplines must pass on their way to achieving full scientific status. To others (Masterman 1970), the pluralism of the social sciences has suggested the need for a revision in the Kuhnian theory of scientific development, to include a recognition of the existence of multi-paradigm sciences.

We shall argue that there have been at least two identifiable alternatives formulated in opposition to the observation-reductionist model of scientific discourse, and the related programme for paradigm closure. One of these, the programme for paradigm proliferation and the related theoretical-reductionist model of scientific discourse has advocated a strategy of theoretical pluralism as a condition for the progressive growth of scientific knowledge. The other alternative, one we have called paradigm complementarity, has advocated the overthrow of both reductionist models of scientific discourse and their replacement by a model in which the relationships between the languages of observation and theory are conceptualised in dialectical terms.

We shall now proceed to briefly trace the relationship of these alternative programmes and models to the system of sociological discourse.

Pluralism in Sociology

The adoption of a programme of paradigm proliferation in Sociology necessarily presupposes the renunciation of a

programme of paradigm closure. For many observers, the most problematic aspect of the programme for closure has been the model of scientific discourse related to it. It has become apparent to many writers, that the recognition of construction of a paradigm language of observation in sociological discourse is neither possible nor desirable. Thus Dixon (1973) identifies the binary language model of scientific discourse and dismisses its relevance as a suitable model for the conceptualisation of sociological discourse.

> Thus the idea that one can operate in scientific terms with two levels of language - a formal theoretical language and a data-language linked by rules of correspondence - appears inadequate to meet the subtle interplay of theory and data. The separation of theory and data can only lead to a distortion of what occurs in the development of a science.
>
> Dixon, (1973:12).

His rejection of this model for the conceptualisation of sociological discourse is based on two main considerations. The first is, that whereas in the physical sciences it may be correct to say that observational statements are already highly interpreted in theoretical terms, it is not correct to say this for the social sciences. For the social sciences do not possess any overall 'touchstone' theories whose component terms may be treated as hard data. More importantly, however, Dixon argues further that the empirical data languages of Sociology are not theoretically interpreted in the same sense as

the data languages of the physical sciences because social data is interpreted within the framework of culturally understood meanings.

> Social data is interpreted not theoretically but within a framework of culturally understood meanings which make nonsense of the behaviourists claim that an uninterpreted observation language is possible...
>
> I have argued, then, that the attempt to construct a neutral data base which will operate as a foundation for theory is in principle not possible.
>
> Dixon, (1973:13).

From this analysis, Dixon concludes that any programme to construct a paradigm language of observation for the discourse of Sociology would already have had to select a paradigm language of theory.

The problem of the relationship between the languages of observation and theory has also been the subject of a recent exchange between other writers of opposing views. Thus Hummell and Opp (1968) have argued that the subject domain of Sociology, defined as that class of propositions which designates collectivities, is reducible without remainder to the subject domain of psychology, defined as that class of propositions that designates individuals. Hummell and Opp make clear that one of their central presuppositions is the existence of a paradigm observation language in psychology, to which observation statements in Sociology may be reduced.

> Therefore you might say that the 'reductionist' proposes a certain type of descriptive language, that is a language similar to that used in behaviouristically oriented psychological theories. This proposal is combined with the claim that this language is rich enough to express those facts which are of interest to Sociologists.
>
> Hummell and Opp, (1968:226).

In response to this clearly (if not simplistically) formulated programme of reductionism for the social sciences, Spinner (1973) has argued that the case for reductionism rests upon a false premise: that the content of the special sciences' subject domains can be defined by reference to paradigm languages of observation or theory. For Spinner, there is no science that exhibits a sufficiently precise or stable language of observation or theory to warrant the generalisation of that language to paradigm status.

> ...there is no such thing as a distinctive 'vocabulary' or 'language of science' which is characteristic of a certain scientific discipline or of a science as a whole and, in addition, sufficiently clear, autonomous, stable, and unambiguous to provide a precise, unequivocal demarcation criterion.
>
> Spinner, (1973:23).

In the social sciences, the possibilities of constructing a common language of observation seem even more remote, at the present time, than in the physical sciences. For, as Spinner recognises, the definition of the data-base of a discipline such as Psychology will differ according to which one

of a number of observation languages is selected to provide the terms for this definition. Thus the multiple languages of observation in the systems of discourse of the social sciences, ranging from phenomenological observation languages to behaviouristic observation languages, presuppose altogether different data-bases and different classes of available facts.

> This relativity is due to the plurality of possible and even actual psychological and sociological perspectives, theories, and systems as well as philosophical systems thereof...which one among the actual plurality of partly incompatible vocabularies is the right vocabulary, entitled to become the proper basis of our definition...?
>
> I am inclined to assume, without seeing any further need for argument, that we will get substantially different results in delimiting the proper domain of psychology depending on whether fix on the vocabulary - with, alas, the rather distinctive cluster of basic terms - of a mentalistic or physicalistic, a materialistic, physiological or hermeneutic...psychology, or on whether we start out from a Freudian or Skinnerian conceptual scheme for the analysis of what is regarded as the proper domain of psychology.
>
> Spinner, (1973:24).

In response to Hummell and Opp, therefore, Spinner argues that the content of the subject domain of the special sciences cannot be defined in terms of any paradigm language of observation. In place of the observational-reductionist model of scientific discourse, therefore, Spinner argues that the content of scientific subject domains is only definable

in terms of the prevailing theories of a discipline. "Theories of a very fundamental and comprehensive kind", he writes, "define implicitly the proper domain of any scientific discipline" (1973:29).

Thus Spinner identifies the reductionist model of discourse, as well as a stage theory (or accumulation theory) of the growth of scientific knowledge. These are, of course, the elements of the programme we have already identified as the programme for paradigm closure. As an alternative to this programme, Spinner recommends a programme for the growth of scientific knowledge based on the recognition and development of theoretical pluralism. For in the same sense as Feyerabend, Spinner has argued that theoretical pluralism is a condition for the effective criticism of knowledge, and that the criticism of theories, rather than the reduction of theories, is a necessary process underlying any future programme for the progressive growth of sociological knowledge.

> If one grants, in accordance with theoretical pluralism, that the most severe and effective criticism of theories (and of whole sciences, too) consists in their confrontation with contradictary global alternative which are quite capable in, principle, to invalidate and to <u>replace</u> criticised ideas, what a backward Sociology needs is a Psychology (or any alternative, <u>including a new, more</u> advanced Sociology) which stands in its way as a stumbling-block, not one that nicely accommodates it be reductive incorporation.
>
> Spinner, (1973:65).

Another writer who has rejected the relevance of the observational-reductionist model for sociological discourse is Derek Phillips (1974). Phillips has also drawn attention to what he regards as the theoricised character of data-language in the discourse of Sociology. The certification of any class of social phenomena (properties, relations or events) as part of the legitimate data-base of the science, is contingent upon the different presuppositions contained in the various observation languages. These presuppositions, argues Phillips, are derived from the main theoretical orientations of the discipline, and cannot be justified in terms of empirical experience.

> Whether or not something is regarded as a 'fact' (as well as whether something is regarded as 'something') is dependent upon different sets of presuppositions. That is, there is no such thing as 'pure experience', no such thing as facts that are recorded directly 'from nature'. Theoretical presuppositions are always involved. There can be no scientific knowledge free of suppositions, and it is therefore impossible to get to the bottom in any verification process.
>
> Phillips, (1974:229).

Thus although most sociological research programmes are guided by explicit procedural rules, these rules are incapable of deciding whether particular phenomena, or classes of phenomena, are legitimately part of the content of the subject domain. Such issues are only resolved by utilising one or another 'implicit theory'. For the distinctions between

what may be certifiable as 'factual', and what may not, are originally distinctions that are settled within a particular framework of theory. Only then are such distinctions translated into an appropriate observation language. For this reason, concludes Phillips, the programme for a paradigm language of observation would reverse the natural processes of theory construction and theory criticism.

The programme for theoretical pluralism in sociological discourse has also received support from Gross (1960, 1961). In discussing the relative immunisation of the several theoretical orientations in Sociology from each other, Gross concludes that there is insufficient use of alternative theories as a basis for criticism. "One kind of language", he writes, "is taken as the standard bearer and all others are evaluated by it" (1960:441). For Gross, therefore, the ideal programme for the growth of sociological knowledge is one that would institutionalise the differences in multiple languages of theory and observation and use these differences as a basis for the criticism of sociological theory.

> In brief, does not every form of knowledge, every form of criticism, rest upon contrast and comparison? To characterise anything must it not be collated with qualities or events that serve as opposing standards of appraisal and evaluation.
>
> Gross, (1961:135).

Madsen (1970) has also recognised the relativity of observa-

tion languages in the social sciences, and their direct relationship to theoretical presuppositions. Thus he proposes an alternative to the binary language model of scientific discourse which he argues, would more adequately represent the actual functions of the languages of science. His alternative model, therefore, carries three levels: the observation language, which is basic from an epistemological point of view; the hypothetical language; and the metalanguage, which is basic from a historical and psychological point of view. Although Madsen's conclusions are drawn from an analysis of the discourse of Psychology, there are also evident implications for Sociology.

> The important fact is that psychologists - and all other scientists - have certain philosophical presuppositions, and they influence their way of theory construction and their selection of methods, data-language and field of search. And only by <u>explicit</u> formulation of the philosophical presuppositions can they be criticised. Thus if a psychologist has a materialistic philosophy and a Darwinian view he will be inclined to prefer hypothetical terms with physiological surplus meaning, use a behavioural data-language and perform experiments with rats or other animals. On the other hand, if a psychologist has a dualistic philosophy and a 'humanistic' view of man he will be inclined to employ hypothetical terms with mentalistic surplus meaning and perhaps use a phenomenological data-language in research with human beings as subjects.
>
> Madsen, (1970:150).

In conclusion, therefore, it is evident that many recent commentators have supported a programme for the growth of so-

ciological knowledge based on theoretical pluralism. Some writers (Friedfichs 1970, Gouldner 1970, Lammers 1974) have suggested that an earlier development of sociological theory, during the nineteen-fifties and early nineteen-sixties, showed a pattern of paradigm closure around the language of structural functionalism. Whether such an application of the terms 'paradigm' or 'theory' are consonant with the original context of their application by Kuhn, is an issue that cannot be settled here. Most writers at the present time, however, are agreed upon the non-paradigmatic status of sociological discourse, as is evidenced by the considerable fragmentation of theoretical orientations. Attempts to classify the main identifiable 'theories', or theoretical schools, have ended in a puzzling variation in the number and type of different theories. Thus Sorokin (1966) lists four such theoretical schools, Martindale (1960) lists five, Wallace (1969) nine, Timasheff (1968) seven, Mullins (1973) four and Ritzner (1975) lists three main schools.

The increasing popularity of a programme for theoretical pluralism in Sociology reflects a growing disenchantment, among many sociologists, with the earlier programmes formulated by the logical empiricists. The failure of social scientists to construct a paradigm language of observation for the discourse of Sociology has resulted in a critical re-eval-

uation of the pr[illegible]ppositions of the programme for paradigm closure.

The alternative programme for paradigm proliferation has been advanced on the basis of a theoretical reductionist model of scientific discourse. Although this has been justified in terms of the necessity for theoretical pluralism as a basis for the criticism of knowledge, theoretical-reductionist model, itself, is not free from criticism. The central problem for this model of scientific discourse remains the problem of selection procedures. For the position offers no rational criteria for the selection of one scientific theory over another. Agassi (1972) has also noted the logical dilemma of the binary language model of scientific discourse.

> The truth is that both theories, 'we start from observation', and 'we start from theory' are both uncomfortable and abstract. The more phenomenolofical alternative is due to Michael Polanyi. "We start science not by observing and not by thinking", he says, "we start science by going to college and taking courses in science".
>
> Agassi, (1972).

It is in order to suggest some escape from the horns of this dilemma that we turn now to a final analysis of the programme for paradigm complementarity.

Complementarity and the Way Ahead

Our investigations into the two main logical variations of the binary language model of scientific discourse have

shown both these variations to be based on reductionistic premises: at one end of the continuum we find the ultimacy of observation, at the other end, the ultimacy of theory.. It has also been further shown that each of these reductionist models of scientific discourse forms the basis of a related programme for the growth of knowledge. Thus a commitment to the belief that theoretical languages should be ultimately reducible to a series of observation statements which corresponds to a designated class of stable object referents, leads naturally to scientific programme dedicated to the construction of paradigm languages within the special sciences, as a precondition for the eventual unification of the languages of science.

At the other end of our continuum, however, it has been shown how a commitment to the alternative belief that observation terms are only meaningful when defined within the context of a particular theory, and the terms from different theories are mutually untranslatable, leads naturally to a scientific programme dedicated to the construction of multiple languages of theory and observation within the special sciences, as a precondition for the permanent relativisation of the languages of science.

Thus from such an ideal-typical reconstruction of models of discourse and theories of scientific development, it is

possible to throw some light on what more recent writers have begun to see as one of the most important problems of science: the growth of scientific knowledge.

> It is quite reasonable to demand of a philosophy or theory of science which claims to provide research programmes that it should be able to give us some relevant information about the most important problem of science: the problem of the growth of scientific knowledge.
>
> Spinner, (1973:44).

The results of the foregoing investigation have shown that both the reductionist models of discourse which fall at opposite ends of the observational-theoretical continuum are inadequate to conceptualise the real relationships between science as a theoretical object and science as a form of social practice. For this reason, therefore, both these programmes must be regarded as unsatisfactory for they neither present a reasonable theory of scientific change and development, nor do they provide any guidelines for the practical aims of scientific research. If these omissions are regarded as serious ones in the natural sciences, how much more so must they be in the social sciences, where controversy over these fundamental questions is sharper, and where the need for clarification is much greater.

The programme for the growth of knowledge previously introduced as the programme for paradigm conplementarity, offers some possibilities of escape from a model of contending

forms of reductionism.

It will be recalled that in their theories of science, Habermas and Apel identified three forms of scientific knowledge, the first two of which: empirical-analytic knowledge and historical-hermeneutic knowledge, correspond more or less to the two main variations on our binary language model of scientific discourse. The third form of knowledge proposed is that of critical-emancipatory science which is based upon a dialectical model of scientific discourse. The attribute of such a model of discourse, explains Apel, is that it presents a method for mediating between alternative aims of knowledge: explanation and understanding.

In order to clarify the status of the critical-emancipatory form of science, Apel (as, of course does Habermas), presents the case of psychoanalysis as an example.

> Now, I myself have come to the conclusion that psychoanalysis is neither a natural science nor a purely hermeneutical science. Rather it incorporates a peculiar methodological model which constitutes the very heart of a branch of humanistic social science which I would call critical-emancipatory social science. The point of the model is, in my opinion, the dialectical mediation of communicative understanding - especially human self-understanding - by the quasi-naturalistic objectification and explanation of human behaviour and human history.
>
> Apel, (1972:33).

The case of psychoanalysis is instructive as an example of the dialectical relationship between the models of dis-

course. For one stage of the psychoanalytic process makes use of a semi-reductionist model of discourse (what Apel calls 'quasi-naturalistic objectification), through which the personal reports of the patient are certified as relevant evidence for further analysis. At this stage, therefore, the personal reports of the patient supply the observational evidence for his pathological condition. In this situation the patient is both seen to be, and treated as, an independent object of observation, and once expressed, his personal reports retain only that meaning provided by the theoretical framework of the analyst. In Apel's words, the analyst objectifies the content of the patient's communication "by repressing them into a reified pseudo-language of neurotic symptoms".

At the same time, however, the analyst must also seek to interpret and understand the personal reports of the patient in the patient's own terms of reference. Thus the analyst participates in two sets of language games: the first related to the discourse of a special science, the second related to the ordinary language discourse of the patient. It is by dialectically mediating these two systems of discourse, suggests Apel that the analyst succeeds in transferring his role as commentator and interpreter to that of the patient. The patient acquires the perspective to retrospectively reinter-

pret the meaning of his own past personal reports and to practically reorganise his own basis for self-understanding.

> The proper aim of psychoanalysis as a method - this is the crucial point in my opinion - does not lie in the nomonological deduction of predictions which could be tested by observation, but in their restitution of communication between the analyst and analysand on a higher level. Thus the object of analysis should himself confirm the hypothesis of the analyst and even supplement them by his self-understanding.
>
> Apel (1972:34).

What, one may still ask, is the relationship of the above exposition to general theories of scientific development, models of scientific discourse, or programmes for the future growth of scientific knowledge? We can, perhaps, draw a general conclusion from Apel's example and show its relevance to the class of issues that have concerned us.

The case of psychoanalysis shows clearly how an objectified domain of observation (such as neurotic behaviour) which may be accurately describable by psychoanalytic protocol statements can also (i) interact with the investigative system of discourse, and (ii) transform itself into something else. The significance of this case lies in the fact that the system of relationships which provided the evidence for neurotic personal reports is capable of change. Thus the observation predicates that at first designate a class of neuritic symptoms will become progressively meaningless as the object referents themselves are <u>de-reified</u>, until they are no

longer certifiable as facts or as evidence.

The contradiction between the 'correspondence' and 'contextual' theories of meaning in the binary language model of scientific discourse is thus a product of a non-dialectical form of conceptualisation. For Apel and Habermas (and even Mannheim), an observational-reductionist model of discourse may co-exist within a progressive contextial relativisation of all models of discourse.

The reification of observational terms which occurs in an observational-reductionist model of sociological discourse is thus in practice the reification of actual social relationships. Lukacs (1971) has described the relationships between the reification of social life and the objectification of the social sciences. The progressive transformation of the conditions that reify social relationships must also result in the progressive transformation of those systems of sociological discourse, that have as their object the description of these conditions. It is clear, therefore, that any adequate model of scientific discourse, or any related programme for the growth of knowledge, should represent the changing and dialectical relation of science as a theoretical object to that of science as a form of social practice.

Alternative conceptualisations to the binary language model are as yet only incompletely suggested in the critical

literature related to these issues.) Thus (as already mentioned) Madsen (1970) has proposed a trinary language model of discourse. Besides the original two levels of observation and theory, Madsen also suggests the recognition of a meta-language level of scientific discourse which functions as an 'argumentative structure' for the total model. It is at this level that the selection of theory and observation languages is made on the basis of meta-theoretical and metascientific criteria.

In another proposal for a multi-level model of scientific discourse, Lafferty (1970) has argued that the subject domain of the social sciences necessitates the construction of a model of discourse which can generate languages for a series of different epistemological programmes.

> How are we to know, describe, or conceptualize multi-level social reality? What linguistic framework shall we use to express the intra-level and trans-level ontological aspects? By what method will we grasp the different domains of 'lawfulness'? These are the basic issues which describe the epistemological tasks associated with the flux and permanence of the social world. I believe that the answers to these questions will be decided in relation to the ontological commitment to the various aspects of the multi-level complex. Depending on the research-guiding interest, this commitment will stress either the genetical, or the trans-level interactive, or the intra-level static domains. Each realm will be associated with its own explanatory mode; the epigenetic, the causal-conjunctive and the correlative. All three modes have their specific operational and theoretical

> characteristics; their specific methodological and conceptual languages.
>
> Lafferty, (1970).

He suggests that such a multi-level model of discourse should be capable of switching from objective measurement languages to "problem oriented contextual languages". "The idea of multi-level data structures", he concludes, "provides the syntax of such a language".

A similar tentative model for sociological discourse has also been outlined by Lemert (1973). Lemert proposes that a 'relativistic paradigm' be constructed to include the multiple orientations of theoretical Sociology, related to each other on the basis of complementarity, indeterminacy, and relativity. Through the integration of the multiple languages of theory and observation into a single paradigm, Lemert suggests that it is then possible to formulate a unified programme for the future growth of sociological knowledge.

> The analysis I have presented of the relativistic paradigm requires us to pause before uncritically accepting theoretical pluralism as a necessity. We have seen that the relativistic paradigm, which has developed in exactly the same ethos and time period as has sociological theory, makes quite a different response to the pluralism of modern culture. Relativism has incorporated modern pluralism within itself. The result has been a remarkably consistent integrated paradigm which has forged a single coherent perspective with a broad applicability to a number of methodologically diverse disciplines. Sociological theory, on the other hand, is responsible to a comparatively smaller range of methodological and topical con-

> siderations, yet has been unable to take the plurality of modern life into its thought structures. Simply stated: Both Sociology and the relativistic paradigm have arisen in a plural ethos. Sociology appears to have fragmented as a result. Relativism has transcended the pluralism and escaped fragmentation.
>
> Lemert, (1973:28).

Further discussion of alternative models of discourse to the classical binary language model has also seen a proposal by Maruyama (unpublished) to formulate a programme for the growth of knowledge based on the method of paradigmatology. Paradigmatology is the study of alternative systems of communication through the elucidation of the paradigmatic structures of reasoning and models of discourse. Maruyama has suggested that such a method could be used to investigate alternative cultural systems of communication, as well as scientific systems or other identifiable systems.

These then have been some of the tentative outlines for the conceptualisation of sociological discourse which have attempted to avoid the implicit problems of reductionism retained in a binary language model. For an adequate model of scientific discourse has, on the one hand, to avoid the reification of observation statements, while on the other hand avoiding their total relativisation. Such a model, it has been suggested, must account for the relationship between the status of science as a theoretical object and its status as

a form of social practice. When this perspective is adopted it is seen that the relationship between scientific theory and observation is a fundamentally dialectical one. For this reason, reductionist models are incapable of conceptualising this relationship and should be rejected in favour of a dialectical model.

As an unofficial postscript to this thesis, it must be acknowledged that the decision to confine the foregoing investigation to the problematic of scientific discourse imposes critical limitations on the final outcome. Such a decision presupposes that the resolution of the problem of contending forms of reductionism in the languages of science is locatable within an alternative model of scientific discourse. However, to adopt such a conclusion would be to further perpetuate the problem as it has been formulated until now, and to postpone indefinitely any more practical clarification of these issues.

For any useful understanding of the growth of scientific knowledge clearly cannot be limited by an exclusive preoccupation with models of scientific discourse. What is more importantly required is an historicised political economy of those societies that have produced science (among other things), so that scientific discourse, as a rationally ideologised belief system, and the scientific practice that corre-

sponds to it, may be seen as inseparable dimensions of an historically specific form of human activity.

For this reason, therefore, the stated problematic of this thesis, and the typologies constructed for its elucidation, break down in the face of historical reality. There can be no resolution of the problem of reductionism in scientific discourse unless it is a practical solution, a solution found in the historical activity of scientists, and upon which these abstract models of discourse and programmes of knowledge are necessarily based.

This conclusion is hardly a novel one. It goes back at least to 1845 when Marx first published his Theses on Feuerbach. For in his criticism of epistemology Marx sorted out his differences with both the philosophical traditions of idealism and traditional materialism. His criticism of these philosophies contains much that is relevant to our present investigation, for both these traditions have modern legacies in the philosophy of science. His pronouncement on the historical status of epistemological problems (such as we have undertaken to investigate) should stand as the real conclusion of this work.

> The question whether objective truth is an attribute of human thought is not a theoretical but a <u>practical</u> question. Man must prove the truth, i.e. the reality and power, the "this-sidedness" of his thinking in practice. The

dispute over the reality or non-reality of thinking that is isolated from practice is a purely <u>scholastic</u> question.

SELECTED BIBLIOGRAPHY

Achinstein, P.: "Theoretical Terms and Partial Interpretation". British Journal of the Philosophy of Science 14: 89-105, 1963.

Achinstein, P.: "On the Meaning of Scientific Terms". Journal of Philosophy 61: 497-509, 1964.

Achinstein, P.: "The Problem of Theoretical Terms". American Philosophical Quarterly 2: 193-203, 1965.

Achinstein, P.: Concepts of Science. Johns Hopkins Press, Baltimore, 1968.

Agassi, J.: "Towards an Historiography of Science", in History and Theory, Studies in the Philosophy of History, Beiheft 2, 1963.

Agassi, J.: "The Confusion between Science and Technology in the Standard Philosophy of Science". Technology and Culture 8: 348-366, 1967.

Agassi, J.: "Sociologism in Philosophy of Science". Metaphilosophy 3: 103-122, 1972.

Andreski, S.: Social Sciences as Sorcery. Andre Deutsch, 1972.

Alexander, P.: Sensationalism and Scientific Explanation. Routledge, London, 1963.

Apel, K.O.: "Analytic Philosophy of Language and the Geisteswissenschaften", in Foundations of Language, Supplementary Series, 4: D. Reidel, Dordrecht, Holland, 1967.

Apel, K.O.: "The a priori of Communication and the Foundation of the Humanities". Man and World, 3-37, 1972.

Ayer, A.J.: Language, Truth and Logic. 2nd Ed., Gollancz, London, 1946.

Bachelard, G.: The Philosophy of No: A Philosophy of the New Scientific Mind. Translated by G.C. Waterston, Orion Press, New York, 1968.

Barber, B.: "Resistance by Scientists to Scientific Discovery". Science CXXXIV: 596-602, 1961.

Barnes, S.B. and Dolley, R.G.A.: "The Scientific Ethos: A

Deviant Viewpoint". European Journal of Sociology 11: 3-25, 1970.

Barnes, S.B.: "Sociological Explanation and Natural Science: A Kuhnian Reappraisal. Archives of European Sociology XIII: 373-391, 1972.

Ben-David, J. and Collins, R.: Social Factors in the Origins of a New Science". American Sociological Review XXXI: 451-465, 1966.

Benson, C.R.: "Problems in Terminology, Concept Formation and Theory Construction. Sociological Focus 4.: 107-125, 1971.

Bernstein, B.: "Public Language" Some Sociological Implications of a Linguistic Form". British Journal of Sociology 10: 311-326, 1959.

Bierstedt, R.: "Sociology and Humane Learning". American Sociological Review 25: 3-9, 1960.

Birnbaum, N.: "Science, Ideology and Dialogue: The Amsterdam Sociological Congress. Commentary XXII/6, 1956.

Blalock, H.: Causal Inferences in Non-Experimental Research. University of North Carolina Press, Chapel Hill, N.C., 1964.

Blalock, H.: "[redacted]ication Problem and Theory Building: The Case of [redacted]consistancy". American Sociological Review 31: [redacted]66.

Bloor, D.: "Two P[redacted]ms for Scientific Knowledge". Science Studie[redacted] 101-115, 1971.

Blumer, H.: "What [redacted]Wrong with Social Theory?" American Sociological R[redacted]ew 19: 3-10, 1954.

Blumer, H.: Sociol[redacted]ical Analysis and the "Variable". American Sociologic[redacted] Review 21: 683-690, 1956.

Bochenski, J.M.: The Methods of Contemporary Thought. Harper Torch Books, 1968.

Boudon, R.: "The Sociology Crisis". Social Science Information 11: 109-139, 1972.

Braithwaithe, R.B.: Scientific Explanation. Cambridge Univ-

ersity Press, 1953.

Braybrooke, D.: Philosophical Problems of the Social Sciences. MacMillan, 1965.

Bright, W.: "Language and Culture". International Encyclopedia of Social Sciences 9: 18-22, 1968.

Brodbeck, M.: "Philosophy of the Social Sciences". Philosophy of Science 21: 140-156, 1954.

Brody, N. and Oppenheim, P.: "Tensions in Psychology between the Methods of Behaviourism and Phenomenology. Psychological Review 73: 295-305, 1966.

Brody, N.: "Methodological Differences between Behaviourism and Phenomenology in Sociology". Psychological Review 74: 330-334, 1967.

Brown, R.: Explanation in Social Science. Aldine, 1963.

Bruyn, S.T.: "Rhetorical Devices in Sociological Analysis". Sociological Quarterly 5: 101-112, 1964.

Bruyn, S.T.: The Human Perspective in Sociology. Prentice Hall, N.J., 1966.

Buchdahl, G.: Metaphysics and the Philosophy of Science. Basil Bladewell, Oxford, 1969.

Butchvarov, P.: "Meaning-as-Use and Meaning-as-Correspondence". Philosophy 35: 314-325, 1960.

Carnap, R.: "Testability and Meaning". Philosophy of Science 3: 420-471, 1936.

Carnap, R.: "Testability and Meaning". Philosophy of Science 4: 1-40, 1937.

Carnap, R.: "The Methodological Character of Theoretical Concepts", in H. Feigl and M. Scriven, Minnisota Studies in the Philosophy of Science 1: The Foundations of Science and the Concepts of Psychology and Psychoanalysis, 38-76, 1956.

Carnap, R.: "The Elimination of Metaphysics through Logical Analysis of Language", in Logical Positivism, edited by A.J. Ayer, Free Press, Illinois, 1959.

Carnap, R.: Psychology in Physical Language, in Logical Positivism, edited by A.J. Ayer, Free Press, Illinois, 1959.

Cavell, S.: "The Availability of Wittgenstein's Later Philosophy". The Philosophical Review LXXI: 67-93, 1962.

Cervin, V.: "Linguistic Problems in Recent Social Research". Synthese 10: 279-281, 1956-1958.

Child, J.: "On the Theoretical Dependence of Correspondence Postulates". Philosophy of Science 38: 170-177, 1971.

Cicourel, A.: Method and Measurement in Sociology. Free Press, New York, 1964.

Cohen, M.: "The Social Sciences and the Natural Sciences", in The Social Sciences and Their Inter-relations, edited W.F. Ogburn and A. Goldenweiser, Houghton-Mifflin, 1927.

Cohen, P.: "The Very Idea of a Social Science". Problems in the Philosophy of Science, edited by I. Lakatos and A. Musgrave, North-Holland, Amsterdam, 1968.

Colodny, R.G. (Ed.): Beyond the Edge of Certainty. Prentice Hall, N.J., 1965.

Colodny, R.G. (Ed.): The Nature and Function of Scientific Theories. University of Pittsburgh Press, 1970.

Cooley, C.H.: Sociological Theory and Social Research. New York, pp. 290-308, 1930.

Costner, H.L. and Leik, R.K.: "Reductions from 'Axiomatic Theory'". American Sociological Review 29: 819-835, 1964.

Coulter, J.: "Decontextualized Meanings: Current Approaches to Verstehnde Investigations". The Sociological Review 19: 305-306, 1971.

Cunningham, F.: Objecting in Social Science. Toronto University Press, 1973.

Dallmayr, F.R.: "Critical Theory Criticized: Habermas' Knowledge and Human Interests and its Aftermath". Philosophy of the Social Sciences 2: 211-222, 1972.

Dallmayr, F.R.: "Reason and Emanicipation: Notes on Habermas". Man and World 5: 1972.

Dawe, A.: "The Two Sociologies". The British Journal of Sociology 21: 207-218, 1970.

Denzin, W.: "Symbolic Interactionism and Ethnomethodology: A Proposed Synthesis". American Sociological Review 34: 922-934, 1969.

Denzin, N.K.: The Research Act: A Theoretical Introduction to Sociological Methods. Aldine, Chicago, 1970.

Deutscher, I.: "Language, Methodology and the Sociologists". Sociological Focus 32: 1-12, 1969-1970.

Dixon, K.: Sociological Theory: Pretence and Possiblity. Routledge and Kegon Paul, London.

Dodd, Stuart C.: "How Momental Laws can be Developed in Sociology". Synthese 14: 277-299, 1962.

Dolby, R.G.A.: "The Sociology of Knowledge in Natural Science". Science Studies 1: 3-21, 1971.

Dressler, D. and Korber, G.W.: "A Comment on the Language of Sociology". Pacific Sociological Review 5: 36-40, 1962.

Duncan, O.P.: Axioms or Correlations? American Sociological Review 28: 452, 1963.

Emmet, D. and MacIntyre, A.: Sociological Theory and Philosophyical Analysis. Macmillan, 1970.

Erfat, A.: "Power to Paradigms: An Editorial Introduction". Sociological Inquiry 42: 3-33, 1972.

Fann, K.T.: Wiggenstein's Conception of Philosophy. B. Blackwell, 1969.

Fararo, T.J.: "The Nature of Mathematical Sociology: A Non-Technical Essay". Social Research 36: 75-92, 1969.

Feuer, L.S.: "Sociological Aspects of the Relation between Language and Philosophy". Philosophy of Science 20: 85-100, 1953.

Feyerabend, P.K.: "Attempts at a Realistic Interpretation and Experience". Proceedings of the Aristotelian Society, New Stories LVIII: 143-170, 1958.

Feyerabend, P.K.: "Patterns of Discovery: N.R. Hanson". Philosophical Review 69: 247-252, 1960.

Feyerabend, P.K.: Knowledge without Foundations. Oberlin Ohio, 1961.

Feyerabend, P.K.: Explanation, Reduction and Empiricism, in H. Feigl and G. Maxwell (Eds.), Minnesota Studies in the Philosophy of Science, III: 23-97, 1962.

Feyerabend, P.K.: "On the 'Meaning' of Scientific Terms". Journal of Philosophy 62: 266-274, 1965.

Feyerabend, P.K.: "Reply to Criticism", in Boston Studies in the Philosophy of Science, edited by R.S. Cohen and M.W. Wartofsky, v.2, pp. 223-261, 1965.

Feyerabend, P.K.: "On the Improvement of the Sciences and the Arts, and the Possible Identity of the Two". Boston Studies in the Philosophy of Science 3: 387-415, 1967.

Feyerabend, P.K.: "The Theatre as an Instrument of the Criticism of Ideologies". Inquiry 10: 298-312, 1967.

Feyerabend, P.K.: "How to be a Good Empiricist...a plea for tolerance in matters epistemological", in The Philosophy of Science, edited by P.H. Widditel, Oxford University Press, 1968.

Feyerabend, P.K.: "Problems of Empiricism: Part Two", in The Nature and Function of Scientific Theories, edited by P.G. Colodny, University of Pittsburg Press, 1969.

Feyerabend, P.K.: "Against Method". Minnesota Studies in the Philosophy of Science, 7, 1970a.

Feyerabend, P.K.: "Consolations for the Specialist", in Criticism and the Growth of Knowledge, edited by A. Musgrave and L. Lakatos, Cambridge University Press, 1970b.

Fine, A.: "Consistancy, Derivability and Scientific Change". Journal of Philosophy 64: 231-240, 1967.

Floistad, G.: "Social Concepts of Action" Notes on Habermas' Proposal for a Social Theory of Knowledge". Inquiry 13: 175-198, 1970.

Frank, P.: "Metaphysical Interpretations of Science". British Journal of the Philosophy of Science I: 60-76, 77-91, 1950.

Frank, P.: "The Logical and Sociological Aspects of Science". Contributions to the Analysis and Synthesis of Knowledge. Proceedings of the American Academy of Arts and Sciences 80: #1, 1951.

Frank, P.: "Contemporary Science and the Contemporary World View. Daedalus 65-76, Winter 1958.

Frank, A.G.: "The Sociology of Development and the Underdevelopment of Sociology". Catalyst, Summer 1967.

Friedrichs, R.: "A Sociology of Sociology". Free Press, New York, 1970.

Friedrichs, R.: "Dialectical Sociology: Toward a Resolution of the Current 'Crisis' in Western Sociology". British Journal of Sociology 23: 263-274, 1972.

Gallie, W.B.: "What Makes a Subject Scientific?" British Journal of the Philosophy of Science 8: 118-139, 1957.

Garfinkle, H.: Studies in Ethnomethodology. Prentice Hall, 1967.

Gellner, E.: "Concepts and Society". Transactions of the Fifth World Congress of Sociology. International Sociological Association, v. 1, 1962.

Gellner, E.: "The New Idealism: Cause and Meaning in the Social Sciences", in Problems in the Philosophy of Science, edited by I. Lakatos and A. Musgrave, Amsterdam, pp. 377-432, 1968.

George, F.H.: "Language, Philosophy and Empirical Science". Synthese 11: 63-71, 1959.

Gervirth, A.: "Can Man Change Laws of Social Science?" Philosophy of Science 21: 229-241, 1954.

Giedymin, J.: "The Paradox of Meaning Variance". British Journal of the Philosophy of Science 21: 257-268, 1970.

Giglioli, P.P. (Ed.): Language and Social Context. Penguin, 1972.

Glazov, N.: "Notes Towards a Sociocultural Interpretation of American Sociology". American Sociological Review 545-553, 1946.

Goodwin, G.: "On Transcending the Absurd: An Inquiry in the Sociology of Meaning". American Journal of Sociology 831-846, 1971.

Gouldner, A.: The Coming Crisis of Western Sociology. Basic Books Inc., New York, 1970.

Grene, M.: The Knower and the Known. Faber and Faber, London, 1966.

Grimshaw, A.: "Language in Society: Part 2". Contemporary Sociology 3: 3-11, 1974.

Gross, L.: Symposium on Sociological Theory. Row Peterson and Co., Evanston, Ill., 1959.

Gross, L.: "An Epistemological View of Sociological Theory". American Journal of Sociology 65: 441-448, 1960.

Gross, L.: "System-Construction in Sociology". Behavioral Science 5: 281-290, 1960.

Gross, L.: "Preface to a Metatheoretical Framework for Sociology". American Journal of Sociology 67: 125-143, 1961.

Gruender, D.: "Language, Society and Knowledge". Antioch Review 28: 187-212, 1968.

Grunbaum, A.: "Historical Determinism, Social Activism and Prediction in the Social Sciences". British Journal of the Philosophy of Science 7: 236-240, 1956.

Gumperz, J.: "Language and Communication". Annals of the American Academy of Political Social Science 373: 219-300, 1967.

Habermas, J.: "Technology and Science as 'Ideology'". Mimeographed, New School of Social Research, New York, 1969.

Habermas, J.: "On Systematically Distorted Communication". Inquiry 13: 205-218, 1970.

Habermas, J.: "Summation and Response". Continuum, Spring-Summer, 1970.

Habermas, J.: Toward a Rational Society. Boston, 1970.

Habermas, J.: Knowledge and Human Interests. Heinmann, London, 1972.

Hanson, N.R.: Patterns of Discovery. Cambridge University Press, 1958.

Hanson, N.R.: Perception and Discovery. Freeman, Cooper and Co., California, 1969.

Hanson, N.R.: Observation and Explanation. Harper and Row, 1971.

Hartung, F.: "Problems of the Sociology of Knowledge". Philosophy of Science 19: 17-32, 1952.

Hayek, F.A. (Von): The Counter-Reduction of Science. Glencoe, Illinois, 1952.

Hayek, F.A. (Von): The Confusion of Language in Political Thought with Some Suggestions for Remedying It. Institute of Economic Affairs, London, 1968.

Helmer, O. and Rescher, N.: "On the Epistemology of the Inexact Sciences". Management Science 6: 25-52, 1960.

Hempel, C.G.: "Fundamentals of Concept Formation in Empirical Science". Foundations of the Unity of Science. International Encyclopedia of Unified Science, edited by O. Neurath, R. Carnap, and C. Morris, University of Chicago Press, 1952.

Hempel, C.: Aspects of Scientific Explanation. Free Press, New York, 1965.

Hempel, C.: "Empiricist Criteria of Cognitive Significance: Problems and Changes", in Aspects of Scientific Explanation, Free Press, New York, 1965.

Hertzler, J.: A Sociology of Language, Random House, New York, 1965.

Hesse, M.: "Theories, Dictionaries and Observation". British Journal of the Philosophy of Science 9: 12-28, 1958.

Hesse, M.: "Is There an Independent Observation Language?", in The Nature and Function of Scientific Theories, edited by R.G. Colodny, University of Pittsburg Press, 1970a.

Hesse, M.: Duhem, Quine and a New Empiricism", in Knowledge and Necessity, Royal Institute of Philosophy Lectures, 3: 1968-1969, London, Macmillan, 1970b.

Hill, M.A.: "Jurgen Habermas: A Social Science of the Mind". Philosophy of the Social Science 2: 247-259, 1972.

Hobhouse, L.T.: Liberalism. Oxford University Press, 1964.

Hockney, D.: "Conceptual Structures", in Conceptual Change, edited by G. Pearce and P. Maynard, D. Reidel, Dordrecht.

Hull, R.T.: "Feyerabend's Attack on Observation Sentences". Synthese 23: 374-399, 1971-1972.

Hummell, H.J. and Opp, K.D.: "Sociology without Sociology". Inquiry 11: 205-226, 1968.

Hunter, J.F.M.: "Forms of Life in Wittgenstein's Philosophical Investigations". American Philosophical Quarterly 5, 233-243, 1968.

Husserl, G.: The Crisis of the European Sciences and Transcendental Phenomenology. Evanston, 1970.

Israel, J.: "Is a Non-Normative Social Science Possible?" Sartryck ur Acta Sociologica 15: 69-87, 1968.

Israel, J.: "The Principle of Methodological Individualism and Marxian Epistemology". Acta Sociologica 13-14: 145-150, 1970-1971.

Israel, J.: Alienation: From Marx to Modern Sociology. Allyn and Bacon Inc., Boston, 1971.

Jarvie, I.C.: Concepts and Society. Routledge and Kegan Paul, 1972.

Jevons, W.S.: Elementary Lessons in Logic. Macmillan and Co., London, 1905.

Joergenson, J.: "The Development of Logical Empiricism". International Encyclopedia of Unified Science 11: #9, University of Chicago Press, 1951.

Jordan, R.W.: "Husserl's Phenomondogy as an "Historical" Science". Social Research 35: 245-259, 1968.

Kant, I.: Metaphysical Foundations of the Natural Sciences. Bobbs-Merrill, Library of Liberal Arts, 1970.

Kaplan, A.: The Conduct of Inquiry: Methodology for the Behavioral Sciences. Chandler Publishing Co., 1964.

Kapp, K.W.: Towards a Science of Man in Society. Martinus Nijhoff, Hague, 1961.

Katz, F.E.: "Analytic and Applied Sociologists: A Sociological Essay on a Dilemma in Sociology". Sociology and Social Research 48: 440-447, 1964.

Kaufman, F.: "The Significance of Methodogy for the Social Sciences". Social Research 5: 442-463, 1938.

Kessell, F.: "The Philosophy of Science as Practised Identity or Dualism". American Psychologist 24: 999-1005, 1969.

Kienzle, H.J.: "Epistomology and Sociology". British Journal of Sociology, 21: 413-425, 1970.

King, M.D.: "Reason, Tradition and the Progressiveness of Science". History and Theory 10: 3-32, 1971.

Kleiner, S.: "Ontological and Terminological Commitment and the Methodological Commensurability of Theories". Boston Studies in the Philosophy of Science VIII: 507-518, 1971.

Klima, R.: "Theoretical Pluralism, Methodological Dissension, and the Role of the Sociologists: The West German Case". Social Science Information 11: #3-4, 69-108, 1972.

Klima, R.: "Scientific Knowledge and Social Control in Science: The Application of a Cognitive Theory of Behavior to the Study of Scientific Behavior, in Social Processes of Scientific Development, edited by Richard D. Whitley, London: Routledge and Kegan Paul, 1974.

Kneale, W.: "Scientific Revolution For Ever?" British Journal of the Philosophy of Science 19: 27-42, 1968.

Kockelmans, J.J.: "On the Meaning of Scientific Revolutions". Philosophy Forum 11: 263-264, 1972.

Korner, S.: Kant. Penguin, 1955.

Korner, S.: Experience and Theory. London, 1966.

Koertge, N.: "For and Against Method". British Journal of the Philosophy of Science 23: 274-290, 1972.

Kordig, K.: The Justification of Scientific Change. Dordrecht, Holland, 1971.

Kruger, M.: "Sociology of Knowledge and Social Theory". Berkely Journal of Sociology 14: 152-163, 1969.

Kuhn, A.: "Toward a Uniform Language or Information and Knowledge. Synthese XIII: 127-153, 1961.

Kuhn, T.S.: "The Function of Dogma in Scientific Research", in Scientific Change, edited by A.C. Crombie, London, 1963.

Kuhn, T.: The Structure of Scientific Revolutions. University of Chicago Press, 1970a.

Kuhn, T.: "Reflections on my Critics", in Criticism and the Growth of Knowledge, edited by A. Musgrave and L. Lakatos, Cambridge University Press, 1970b.

Kuhn, A.: The Logic of Social Systems. Jossey-Bass Publishers, 1974.

Lachenmayer, C.W.: The Language of Sociology. Columbia University Press, 1971.

Lafferty, W.M.: "Industrialization and Labor-Response: Notes toward the Construction of a Multi-Level Data Structure". Acta Sociologica 13: 1970.

Lakotos, I.: "Criticism and the Methodology of Scientific Research Programmes". Proceedings and the Aristotelian Society 69: 149-186, 1968.

Lakatos, I. and Musgrave, A. (Eds.): Problems in the Philosophy and Science. Amsterdam, 1968.

Lakatos, I. and Musgrave, A. (Ed.): Criticism of the Growth and Knowledge. Cambridge University Press, 1970.

Lakotos, I.: "History of Science and Its Rational Reconstruc-

tions", in Boston Studies in the Philosophy of Science VIII: edited by R.C. Buch et al, 91-136, 1971.

Lammers, C.: "Developmental Differences between Natural and Social Sciences". Social Processes of Scientific Development, edited by R. Whitley, Routledge and Kegan Paul, London, 1974.

Lazarsfeld, P. and Rosenberg, M.: The Language of Social Research. Free Press of Glencoe, 1955.

Lea, C.H.: Materials for a History of Witchcraft. 3 Volumes, New York, 1957.

Lee, H.N.: Percepts, Concepts and Theoretic Knowledge. Memphis State University Press, 1973.

Lemert, C.C.: "Social Theory and the Relativistic Paradigm". Presented to Annual Meetings of the American Sociological Society, New York City, 1973.

Lenhart, C.K.: "Rise and Fall of Transcendental Anthropology". Philosophy of the Social Sciences 2: 231-246, 1972.

Lenneberg, E.H.: "The Relationship of Language to the Formation of Concepts". Synthese 14: 103-109, 1962.

Lephin, J.: "Meaning Variance of the Comparability of Theories". British Journal of the Philosophy of Science 20: 69-75, 1969.

Leyton, A.C.: "The Role of the Irrational in Sociology". Synthese 10: 389-398, 1956-1958a.

Leyton, A.C.: "Semantic Aspects of Sociological Studies". Synthese 10: 270-278, 1956-1958b.

Leyton, A.C.: "Sociology and Obligations of Interpretations". Synthese 11: 177-196, 1959.

Lincrome, D.: "Reporting, Evaluating, Describing". Philosophy of Science 39: 179-194, 1972.

Lobkowicz, N.: "Interest and Objectivity". Philosophy of the Social Sciences 2: 193-210, 1972.

Louch, A.R.: "Concepts and Society: Review". Inquiry 16: 127-137, 1973.

Luchins, A.S. and Luchins, E.H.: "Two Philosophies of Science: A Study in Contrasts". Synthese 15: 292-316, 1963.

Luckman, T.: Science, Philosophy and Everyday Life. Unpublished.

Lukacs, G.: History and Class Consciousness. Merlin Press, London, 1971.

Lundberg, G.A.: Foundations of Sociology. Macmillan, New York, 1939.

Lynd, R.S.: Knowledge for What? Princeton University Press, 1939.

Lyons, J.: Introduction to Theoretical Linguistics. Cambridge University Press, 1969.

MacDonald, M.: "The Language of Political Theory". Avistotelian Society Proceedings 41: 91-112, 1940-1941.

Machamer, P.K.: "Observation", in Boston Studies in the Philosophy of Science VIII: 187-201, 1970.

Machamer, P.K.: "Feyerabend and Galileo: The Interaction of Theories and the Reinterpretation of Experience". Studies in History and Philosophy of Science I: 1-46, 1973.

MacIntyre, A.: "The Idea of a Social Science", in Aristotelian Society, Supplementary Vol. LXI, 95-114, 1967.

MacIntyre, A.; Against the Self Images of the Age. New York Schocken Books, 1971.

McCarthy, T.A.: "A Theory of Communicative Competence". Philosophy of the Social Sciences 2: 193-270, 1972.

McEvoy, J.G.: "A 'Revolutionary' Philosophy of Science: Feyerabend and the Degeneration of Critical Rationalism into Sceptical Fallibilism." Philosophy of Science 42: 49-66, 1975.

McEwan, W.F.: The Problem of Social Scientific Knowledge. Bedminster, 1963.

McMullin, E.: "The History in Philosophy of Science: A Taxo-

nomy". Minnisota Studies in the Philosophy of Science V: 12-67, 1970.

McNamara, R.: "Description and Theory: A Plea for Understanding". Sociological Analysis 31: 216-218, 1970.

Madson, K.B.: "The Languages of Science". Theory and Decision 1: 138-154, 1970.

Mannheim, K.: "Structural Analysis of Epistemology", in Essays in Sociology and Social Psychology, Ch. 1, Routledge and Kegan Paul, 1922.

Mannheim, K.: Ideology and Utopies. Harvest Books, 1936.

Martin, J.E.: "Theoretical Languages in Psychology. Philosophy of Science 38: 344-352, 1971.

Martingdale, D.: The Nature and Types of Sociological Theory. Boston, 1960.

Martingdale, D.: "The Roles of Humanism and Scientism in the Evolution of Sociology", in Explorations in Social Change, edited by G.K. Zollschan and W. Hirsch, Houghton and Mifflin, pp. 452-490, 1964.

Martins, H.: "The Kuhnian 'Revolution' and Its Implications for Sociology", in Imagination and Precision in the Social Sciences, edited by T.J. Nossitor, A.H. Hanson, and S. Rokkan, 1972.

Maruyama, M.: "Paradigmatology and Its Application to Cross-Disciplinary, Cross-Professional and Cross-Cultural Communication. Unpublished paper.

Masterman, M.: "The Nature of the Paradigm", in Criticism and the Growth of Knowledge, edited by I. Lakatos and A. Musgrave, Cambridge University Press, 1970.

Maxwell, G.: "Meaning Postulates in Scientific Theories", in Current Issues in the Philosophy of Science, edited by H. Feigl and G. Maxwell, 1961.

Maxwell, G.: "Theories, Frameworks and Ontologies". Philosophy of Science 28: 1961.

Maxwell, G. and Feigl, H.: "Why Ordinary Language needs Reforming". Journal of Philosophy 58: 488-498, 1961.

Maxwell, G.: "The Ontological Status of Theoretical Entities", Minnesota Studies in the Philosophy of Science 3: 3-27, 1962.

Mayo, B.: "The Existan[illegible] of Theoretical Entities". Science News 32: 7-18, 19[illegible]

Mayo, B.: "More about [illegible]al Entities". Science News 39: 42-55, 1956.

Miliband, R.: T[illegible] Stat[illegible] apitalist Society. Quarter Books, Londo[illegible]973

Mills, C.W.: "Two [illegible]yle[illegible] of Research in Current Social Studies". Philoso[illegible]y [illegible]Science 20: 266-275, 1953.

Mills, C.W.: The So[illegible]gical Imagination. Oxford University Press, 1959.

Mink, L.D.: "Comment[illegible] Stephen Toulmin's 'Conceptual Revolutions in Scienc[illegible]". Synthese 17: 92-99, 1967.

Moore, W.E.: "The Who[illegible] State of Sociology". American Sociological Review [illegible]: 715-718, 1959.

Morgenthau, H.: "Thought and Action in Politics". Social Research 38: 611-632, 1971.

Morgenthau, H.: Science[illegible]vant or Master? New American Library, New York, [illegible].

Mulkay, M.: "Some Aspects of Cultural Growth in the Natural Sciences". Social Research 36: 1-52, 1969.

Mulkay, M.J.: "Conformity and Innovation in Science". Sociological Review Monograph #18, University of Keele, 1972.

Mullins, N.C.: Theories and Theory Groups in Contemporary American Sociology. Harper and Row, New York, 1973.

Musgrave, A.E.: "Kuhn's Second Thoughts". British Journal of Philosophy of Science 22: 287-297, 1971.

Naess, A.: The Pluralist and Possibilist Aspect of the Scientific Enterprise. Allen and Unwin, London, 1972.

Nagel, E., Bromberger, S. and Grunbaum, A.: Observation and Theory in Science, Johns Hopkins Press, Baltimore:London,

1969.

Nettler, G.: Explanations. McGraw Hill Book Company, 1970.

Neurath, O.: "Foundations of the Social Sciences". International Encyclopedia of Unified Science II: #1, University of Chicago Press, 1944.

Neurath, O.: "Protocol Sentences", in Logical Positivism, edited by A.J. Ayer, Free Press, Illinois, 1959.

Neurath, O.: "Sociology and Physicalism", in Logical Positiism, edited by A.J. Ayer, Free Press, Illinois, 1959.

Nichols, C.: "Science or Reflection: Habermas on Freud". Philosophy of the Social Sciences 2: 261-270, 1972.

Nidditch, P.H.: The Philosophy of Science. Oxford University Press, 1968.

Northrop, F.S.C.: The Logic of the Sciences and the Humanities. Meridian, 1959.

Nowak, S.: "Observation and Understanding of Human Behavior in the Construction of Sociological Concepts and Theories. Revue Internationale de Sociologie 53: 439-454, 1969.

Ogburn, W.F.: "Three Obstacles to the Development of a Scientific Sociology". Social Forces 8: 347-350, 1929-1930.

Page, C.H.: Sociology and Contemporary Education. Random House, New York, 1964.

Pantin, C.F.A.: The Relations between the Sciences. Cambridge, 1968.

Park, P.: "The Cretan Dictum: A Functional Analysis of Sociology". American Sociologist 2: 155-157, 1967.

Park, P.: Sociology Tomorrow. New York, 1969.

Peak, H.: "Problems of Objective Observation", in Research Methods in the Behavioral Sciences, edited by L. Festinger and D. Katz, New York, 1953.

Pearce, G. and Maynard, P.: Conceptual Change, D. Reidel,

Dordrecht, Holland/Boston, U.S.A., 1973.

Peel, J.D.Y.: "Understanding Alien Belief Systems". British Journal of Sociology XX: 69-99, 1969.

Peursen, C.A. Van: Phenomenology and Analytical Philosophy, Duquesne University Press, 1972.

Phillips, D.: Abandoning Method. Jossey-Bass, 1973.

Phillips, D.: "The Madness in our Methods: A Three-Author Exchange in Four Parts". Sociology and Social Research 58, 1974.

Pieris, J.: "Speech and Society: A Sociological Approach to Language". American Sociological Review 16: 499-505, 1951.

Popper, K.R.: The Logic of Scientific Discovery. Hutchinson, 1968.

Psathas, G.: "Ethnomethods and Phenomonology". Social Research 35: 500-520, 1968.

Purtill, R.L.: "Kuhn on Scientific Revolutions". Philosophy of Science 34: 53-58, 1967.

Purtill, R.L.: "Toulmin on Ideals of Natural Order". Synthese 22: 431-437, 1971.

Putman, H.: "What Theories are Not", in Logic Methodology and Philosophy of Science, edited by Nagel, Suppres, Tanski, Stanford University Press, 1962.

Quine, W.V.O.: "The Scope and Language of Science". British Journal of the Philosophy of Science 8: 1-17, 1957.

Quinn, P.: "Methodological Appraisal and Heuristic Advice: Problems in the Methodology of Scientific Research Programmes". Studies in History of Philosophy of Science, 3: 1972.

Radnitzky, G.: Contemporary Schools of Metascience. 2 Vols. Akademeforfaget, Goteborg, 1968.

Ramsey, F.P.: The Foundation of Mathematics and Other Logical Essays. Routledge and Kegan Paul, London, 1931.

Reichenbach, H.: The Rise of Scientific Philosophy, University of California Press, 1968.

Reid, L.A.: "Religion, Science and Other Modes of Knowledge". Hibbert Journal 54: 2-14, 1955.

Rex, J.: Discovering Sociology: Studies in Sociological Theory and Method. Routledge and Kegan Paul, London, 1973.

Rickman, H.P.: Understanding and the Human Studies. Heinemann Educational Books Ltd., London, 1967.

Ricoeur, P.: "Husserl and Wittgenstein on Language", in Phenomonology and Existentionalism, edited by E.N. Lee and M. Mandelbaum, Johns Hopkins Press, Baltimore, 207-217, 1967.

Ricoeur, P.: "New Developments in Phenomenology in France: The Phenomonology of Language". Social Research 34: 1-30, 1967.

Ricoeur, P.: "Structure, Word, Event". Philosophy Today XII: 2-4, 1968.

Ritzner, G.: Sociology: A Multi-Paradigm Science. Allyn and Baher, Inc.

Robertson, R.: "The Sociocultural Implications of Sociology: A Reconnaissance", in Imagination and Precision in the Social Sciences, edited by T.J. Nossitor, A.H. Hanson and S. Rokkan, 1972.

Roche, M.: Phenomonology, Language and the Social Sciences. Routledge and Kegan Paul, 1973.

Rose, A.: Theory and Method in the Social Sciences. Univer- of Minnesota, 1954.

Rose, A.: "Varieties and Sociological Imagination". American Sociological Review 34: 623-630, 1969.

Rostow, W.W.: The Stages of Economic Growth. Cambridge, 1960.

Rudner, R.: The Philosophy of Social Science, edited by N.J. Englewood, Prentice Hall, 1967.

Russell, B.: Analysis of Mind. Allen and Unwin, London, 1921.

Sacks, H.: "Sociological Descripties". Berkeley Journal of Sociology VIII: 1963.

Salvemin, G.: The Historian and Scientist, Cambridge, Mass., 1939.

Sapir, E.A.: Selected Writings in Language, Culture and Personality, edited by Mandelbaum, Berkely, University of California Press, 1949.

Saran, A.K.: "Some Aspects of Positivism in Sociology". Transactions of the Fifth World Congress of Sociology. International Sociological Association, 220-223, 1962.

Saran, A.K.: "A Wittgensteinian Sociology?" Ethics 75: 195-200, 1965.

Saussure, F. de: Course in General Linguistics, edited by C. Bally and A. Sechehaye, McGraw Hill, 1966.

Savory, T.H.: The Language of Science: Its Growth, Character and Usage, Andre Deutsch., London, 1953.

Scheffler, I.: Science and Subjectivity. Bobbs-Morrill Co., Indianapolis, 1967.

Scheler, M.: Die Wissensformen und Die Gesellschaft, Francke Verlag: Born and Munchen, 1960.

Schlick, M.: Philosophy of Nature. New York, 1949.

Schlick, M.: The Foundation of Knowledge, in Logical Positivism, edited by A.J. Ayer, Free Press, Illinois, 1959.

Schloek, H. and Wiggins, J.W.: Scientism and Values. Van Nostrand, 1960.

Schroyer, T.: Alienation and the Dialectical Paradigm. Doctoral Dissertation, New School for Social Research, 1968.

Schroyer, T.: "Towards a Critical Theory for Advanced Industrial Society, in Hans Peter Dreitzel (ed), Recent Sociology #2, Macmillan, London, 1970.

Schroyer, T.: "The Dialectical Foundations of Critical Theory". Telos #12, 1972.

Schutz, A.: "The Social World and the Theory of Social

Action. Social Research 27: 203-221, 1960.

Scriven, M.S.: "A Possible Distinction between Traditional Disciplines and the Study of Human Behavior", in Minnesota Studies in the Philosophy of Science, v. 1, The Foundations of Science and the Concepts of Psychology and Psychoanalysis, edited by H. Feigl and M.S. Scriven, University of Minnesota Press, Minneapolis, 1956.

Scriven, M.S.: "A Study of Radical Behaviorism", in Minnesota Studies in the Philosophy of Science, v. 1, The Foundations of Science and the Concepts of Psychology and Psychoanalysis, edited by H. Feigl and M.S. Scriven, University of Minnesota Press, Minneapolis, 1956.

Scriven, M.S.: "Definitions, Explanations, and Theories", in Minnesota Studies in the Philosophy of Science, v. II, Theories and the Mind-body Problem, edited by H. Feigl and G. Maxwell, University of Minnesota Press, Minneapolis, 1958.

Scriven, M.: "Explanation and Prediction in Evolutionary Theory". Science 129: 477-482, 1959.

Sellars, W.: "The Language of Theories", in Science, Perception and Reality, London, 1963.

Sergerstedt, T.T.: The Native and Social Reality: An Essay in the Epistemology of Empirical Sociology. Bedminster, 1966.

Shapere, D.: "Meaning and Scientific Change", in Mind and Cosmos, edited by R. Colodny, University of Pittsburg Press, 1966.

Skinner, B.F.: "Critique of Psycoanalytic Concept and Theories". Minnesota Studies in the Philosophy of Science 1: 77-87, 1956.

Sklar, L.: "Types of Inter-theoretical Reduction". British Journal of the Philosophy of Science 18: 109-124, 1967.

Skolimowski, H.: Polish Analytical Philosophy, Routledge and Kegan Paul, London, 1967.

Skolimowski, H.: "Knowledge, Language, and Rationality". Boston Studies in the Philosophy of Science IV: 174-198, 1969.

Smart, J.J.C.: "The Reality of Theoretical Entities". Australian Journal of Philosophy 34: 1-12, 1956.

Smith, J.E.: "Hegel's Critique of Kant". The Review of Metaphysics 26: 438-460, 1973.

Smolicz, J.: "Paradigms and Models: A Comparison of Intellectual Frameworks in Natural Sciences and Sociology". Australian and New Zealand Journal of Sociology 6: 100-119, 1970.

Somerville, T.: "Logical Empiricism and the Problem of Causality in Social Science". Erkenntis 6: 405-411, 1936.

Sorokin, P.: "Sociology of Yesterday, Today, and Tomorrow". American Sociological Review 30: 833-843, 1965.

Sorokin, P.A.: Sociological Theories of Today. Harper and Row, New York, 1966.

Spector, M.: "Theory and Observation, I or II". British Journal of the Philosophy of Science 17: #1 and #2, 1-20, 89-104, 1966-1967.

Speirer, H.: "The Social Determination of Ideas. Social Research 5: 182-205, 1938.

Spinner, H.F.: "Science without Reduction". Inquiry 16: 16-94, 1973.

Stark, W.: Description and Theory: A Plea for Cooperation. Sociological Analysis 32: 209-215, 1970.

Stehr, N. and Simmons, A.: Modes of Discourse and the Development of Sociological Specialties. Presented at the Annual Meeting of the Pacific Sociological Association, Victoria, B.C., 1975.

Stern, A.: "Science and the Philosopher". American Scientist 44: 281-295, 1956.

Sztompka, L.: "Telelogical Language in Sociology". Polish Sociological Bulletin 20: 56-59, 1969.

Taviss, I.: "The Technological Society: Some Challenges for Social Science". Social Research 35: #3, 1968.

Taylor, S.: Conceptions and Institutions and the Theory of

Knowledge. Bodeman Associates, New York, 1956.

Tennessen, H.: "Humanistic"/"Scientific": A Chimerical Bichotomy". Unpublished paper, presented at the Annual Conference of the American Psychological Association, Waikiki Beach, Hawaii, September 1972.

Thompson, M.: "When is Ordinary Language Reformed?" Journal of Philosophy 58: 498-504, 1961.

Timasheff, N.: Sociological Theory: Its Nature and Growth. Random House, New York, 1968.

Tiryakian, E.: "Existential Phenomenology and the Sociological Tradition". American Sociological Review 30: 674-688, 1965.

Toulmin, S.: "Comments" Language and Knowledge". Boston Studies in Philosophy of Science IV: 199-207, 1966-1968.

Toulmin, S.: "Conceptual Revolutions in Science". Synthese 17: 75-91, 1967a.

Toulmin, S.: "The Evolutionary Development of Natural Science". American Scientist 55: #4, 455-471, 1967b.

Toulmin, S.: Human Understanding. Clavendon Press, Oxford, 1972.

Toulmin, S.: Foresight and Understanding. Harper Torchbooks, 1961.

Townsend, B.: "Feyerabend's Pragmatic Theory of Observation and the Compatability of Alternative Theories". Boston Studies in the Philosophy of Science VIII: 202-211, 1970.

Ublink, J.B.: "Model, Description and Knowledge". Synthese 12: 302-321, 1960.

Von Mises, L.: Theory and History, Chp. 11, New Haven, Conn. 1957.

Wagner, H.R.: "Types of Sociological Theory". American Sociological Review 28: 735-742, 1963.

Wallace, W.L.: Sociological Theory: An Introduction. Aldine, Chicago, 1969.

Walter, P.F.: "A Philosophy for Sociologists". Sociology and Social Research 31: 262-267, 1947.

Walton, K.L.: "Linguistic Relativity", in Conceptual Change, edited by Pearce and Maynard, D. Reidel, Dordrecht, 1973.

Warm, T.W.: Behaviorism and Phenomonology, University of Chicago Press, Chicago, 1964.

Warshay, L.: "The Current State of Sociological Theory: Diversity, Polarity, Empiricism and Small Theories". Sociological Quarterly 12: 23-45, 1971.

Whorf, B.L.: Language, Thought and Reality: Selected Writings of Benjamin Lee Whorf", edited by J.B. Carroll, M.I.T. Press, Cambridge, Mass., 1956.

Whitley, R.D.: "Black Borism and the Sociology of Science: A Discussion of the Major Developments in the Field", in The Sociology of Science, The Sociological Review Monograph #18, University of Keele, Keele, 61-92, 1972.

Wilson, F.: "Definition and Discovery". British Journal of the Philosophy of Science 18: 1967-1968; 19: 1968-1969.

Wilson, T.: "Normative and Interpretive Paradigms in Sociology", in Understanding Everyday Life, edited by T. Douglass, Aldine, Chicago, 1970.

Winch, P.: The Idea of a Social Science and Its Relations to Philosophy. Routledge and Kegan Paul, 1958.

Winch, P.: "Nature and Convention", Proceedings of the Aristotelian Society 60: 231-252, 1959-1960.

Winch, P.: "Understanding a Primitive Society". American Philosophical Quarterly 1: #4, 1964.

Winnie, J.A.: "The Implicit Definition of Theoretical Forms". British Journal of the Philosophy of Science 18: 223-229, 1967.

Wisdom, J.O.: "Four Contemporary Interpretations on the Nature of Science". Foundations of Physics 1: 1921.

Wisdom, J.O.: "Observations as the Building Blocks of Science in 20th Century Scientific Thought". Boston Studies in the Philosophy of Science VIII: 212-222, 1970.

Wittgenstein, L.: Tractatus Logico-Philosophicus. Harcourt and Brace, New York, 1923.

Wiggenstein, L.: Philosophical Investigations. Translated by G.E.M. Anscombe, Blackwell, Oxford, 1953.

Wittgenstein, L.: The Blue and Brown Books, Basil Blackwell, Oxford, 1969.

Wolff, K.H.: "The Sociology of Knowledge: Emphasis on an Empirical Attitude". Philosophy of Science 10: 104-123, 1943.

Woodger, J.H.: Biology and Language, Cambridge University Press, 1952.

Zaner, R.M.: "Theory of Intersubjectivity: Alfred Schutz". Social Research 28: 71-93, 1961.

Zaner, R.M.: "Theoretical Note: Criticisms of Tensions in Psychology between the Methods of Behaviorism and Phenomonology". Psychological Review 74: 318-324, 1967.

Zetterberg, H.: Social Theory and Social Practice, Bedminster, 1962.

Zetterberg, H.: On Theory and Verification in Sociology, Bedminster, 1965.