THE IRRELEVANCE OF "DEMARcationIST" PHILOSOPHIES OF SCIENCE FOR THE SOCIOLOGY OF KNOWLEDGE

by

PAUL A. MITTENDORFF

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Paul A. Mittendorff
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APPROVAL

Name: Paul A. Mittendorff
Degree: Master of Arts
Title of Thesis: The Irrelevance of "Demarcationist" Philosophies of Science for the Sociology of Knowledge

Approved in 1983 by the Examining Committee:

Chair: Noel Dyck
Senior Supervisor Keith Dixon
Supervisor Hamish Dickie-Clark
External Examiner Phil Hanson, Department of Philosophy

Approved for Graduation, 2006:

Noel Dyck, Professor,
Department of Sociology/Anthropology

Keith Dixon, Professor Emeritus,
Department of Sociology/Anthropology

Phil Hanson, Professor,
Chair, Department of Philosophy

Dara Culhane, Professor
Chair, Graduate Program,
Department of Sociology/Anthropology

John Pierce
Dean of Arts & Social Sciences

Jon Driver
Dean of Graduate Studies
Simon Fraser University

Date Approved: October 4, 2006
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ABSTRACT

Sociology of Knowledge is influenced by theories of philosophy which demarcate science from non science. It is argued furthermore that scientific enquiry can be divided into a context of justification and a context of discovery.

Within the context of justification, notions such as "observation language", "theory", "axiomatization" etc are sufficient to fully explain the results of scientific enquiry.

The context of discovery includes human circumstances but these cannot contribute to the justification of science as a claim to knowledge.

I argue that the arguments presented are crucially flawed and cannot serve to justify any principled demarcation or division of contexts.

I argue that scientific knowledge may well be presented in an artificial language but this language cannot be demarcated from ordinary language.

Scientific enquiry can be placed in a broader cognitive outlook supported by broader cultural practices and ordinary language.

Keywords: Sociology of Knowledge; scientific enquiry; context of justification; context of discovery
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Examining Committee:

CHAIRPERSON PERSON: Noel Dyck

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Keith Dixon
Senior Supervisor

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Hamish Dickie-Clark

-----------------------------
Phil Hanson
Professor
Department of Philosophy
Simon Fraser University

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Date Approved
ABSTRACT

The main aim of the thesis is to demonstrate that some influential philosophical theories which seek to demarcate the realm of "science" from that of "non-science" are insufficiently grounded and therefore irrelevant to a properly conceived Sociology of Knowledge.

Chapter One focusses upon "positivist" conceptions of science. For positivists observation is ideally theory-independent. Scientific knowledge then can be reduced to an empirical "bedrock" vouched for through an appeal to "indubitable experience". No satisfactory arguments have been adduced to explicate "theory independent observation" or "indubitable experience".

Chapter Two examines Karl Popper's repudiation of positivism. Popper argues that observation is always theory-dependent. The "empirical base" of science changes with the adoption of successive theories. Popper's aim is twofold. (1) to account for the rationality of successive theory changes and (2) to demarcate science from non-science through an appeal to falsification.

Chapter Three examines the response of Thomas Kuhn to Popperian methodology. Kuhn denies the Popperian notion
of "rationality" which depends on the acceptance of supra-historical methodological rules. Kuhn argues that methodological rules are "paradigm" dependent. Kuhn attempts to demarcate science from non-science by locating a "paradigm" within a scientific community, independent of the larger cultural context.

Chapter Four examines Imre Lakatos' response to Kuhn's challenge. Lakatos proposes a sophisticated version of falsificationism. He argues that his notion of "research programme" can serve as a demarcation criterion and that it can account for the rational growth of science. Each of the "philosophies of science" discussed invoke criteria of demarcation. Each suggests that science is, in key respects, autonomous. On present evidence the case for demarcation is unproven.

In Chapter Five I discuss the reception of Kuhn in the sociology of science. For Kuhn there is a hesitant recognition that what constitutes "scientific knowledge" is an open question unrestricted by epistemological dictat.

In the conclusion I argue that "knowledge" is embedded in "ordinary language" which is in itself parasitic upon forms of life and social activity. An artificial language will retain an irreducible link with ordinary language.
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INTRODUCTION

There are several ways to approach the justification of scientific theories as knowledge claims—empiricism, conventionalism and a form of Neo-Kantianism.

Empiricism is based on the belief that the senses are both the origin and the justification of our knowledge claims. The increasingly theoretical and mathematical nature of physics make the adoption of a pure empiricism problematic: theory and mathematics are not obviously reducible to descriptions of sensations.

Conventionalism accepts theory and mathematics in their own right but regards them as structures produced by our own imagination. It denies that theories correspond to a reality that the theories purport to describe. To preserve a realistic interpretation one may adopt a form of Neo-Kantianism. One accepts as legitimate certain concepts and regard them as true. They are a priori, independent of experience but true nonetheless. The advent of relativity theory, however, denied the legitimacy of some accepted a priori concepts.

Logical Positivism, as a philosophy of science, is an attempt to provide a justification of scientific knowledge claims within an empirical context. Developments in the
philosophy of language created the expectation that the problem of reducing theory and mathematics to descriptions of sensations could be adequately dealt with.

The philosophy of Karl Popper is an attempt to provide criteria by which the progress of science can be rationally evaluated. Despite their differences both philosophies advocate a realist approach to science. Both demarcate science from non science and hold as possible a rational reconstruction of science.

Recently the dominant views have been challenged by Thomas Kuhn. Kuhn questions the traditional accounts of scientific growth and denies the possibility of a rational reconstruction. Nonetheless he seeks to demarcate science from non science.

Scientific results are stated in a formal language. There is an increasing awareness of the linguistic character underlying scientific theories. It is not obvious that scientific results can be stated in a formal language independent of any intrusion of ordinary language. This in turn will reflect on the possibility to demarcate science from non science.
Mach, an important precursor of Logical Positivism, rejected the notion of informative apriori knowledge independent of possible experience. Mach insisted that scientific statements must be reducible to statements about sensations. Since all our testimony concerning the so-called world relies on sensations, Mach held that we can and must take these sensations and complexes of sensations to be the sole content of those testimonies. "A body, a physical object is nothing else than a complex, a more or less firm (we would say invariant) pattern of sensation i.e. of colors, sounds, sensations of heat and pressure etc."¹

Relativity theory denied the validity of concepts of absolute space and time and made the adoption of a Neo-Kantian framework difficult to maintain. The increasing mathematical and theoretical nature of physics made the adoption of a Machian point of view equally difficult to maintain: mathematical and theoretical principles do not readily breakdown into complexes of sensations such as color, heat, pressure etc. Developments in mathematics how-
ever, created the expectation that mathematics could be reduced to logic and that logic could be interpreted as "non-empirical but true" in virtue of the rules that constitute logic. This dissolves the problem inherent in the attempt to reduce mathematics to descriptions of sensations. Logical Positivism as a philosophy of science is an attempt to fulfill Mach's programme; to reduce our knowledge of the external world to the only reliable testimony we have, the senses.

The impetus for the development of logical positivism as a philosophy of science first came from developments in the philosophy of language. Frege is credited with the introduction of mathematical notions into the analysis of language. Both the notion of "function" and of "having a truthvalue" (i.e. a sentence is either true or false) were extended to apply to sentences in ordinary language. Russell became interested in ordinary language, which, he thought, had an untoward tendency to reify. Once ordinary usage had fixed the "objects" to which a word is attached, common sense would be influenced by the existence of that word and aided by custom and habit it would lead to the assumption that the word must or should correspond to something. "The influence of vocabulary", Russell wrote, "is towards a Platonic pluralism of things and ideas."²

Russell sought to exorcise this tendency by investigating the possibility of deriving from ordinary language a
more precise and formal language. Hampshire argues that according to Russell,

...there must be a bedrock level of knowledge-claims which so far are not questionable by reference to any other knowledge claims and which have to be validated simply by a matching of the proposition with experience.  

This bedrock level is reflected in Russell's view that "every philosophical investigation starts from certain data which we must assume as being, on the whole and in a certain sense, pragmatically true." Russell calls these data "hard data" and argues that they are of two sorts: "the particular facts of sense" and "the general truths of logic". The Russellian view of language is that underlying ordinary language there is a stricter language, the grammar of which consists of the general truths of logic. The vocabulary is constructed from the particular facts of sense. This elementary logical language, essentially a series of atomic propositions, can then be directly validated by the hard data of experience such as smells, shapes, colors, and sounds. We can arrive at those data of experience, or atomic facts, by analyzing our perceptions till we arrive at a "sense datum". A "sense datum" is, roughly speaking, "a phenomenal image which can be directly perceived in such a way that its qualities are incorrigibly known." A sense datum forms the basis for the construction of logically warranted facts. A language incorporating such facts would not be vulnerable to the vicissitudes of ordinary language--a tendency towards a
Platonic pluralism of things and ideas.

The justification of the view that there are "statements which are elementary in the sense that, if they are true, they correspond to absolutely simple facts" was adumbrated by Wittgenstein in the *Tractatus Logico-Philosophicus*. Wittgenstein's aim was to explore the limits of language, of all that could be said in factual language. "Inside this boundary would lie all the things that could be said in factual language." Wittgenstein thought of logic as the framework underlying all factual language and anyone who investigated this structure "would be investigating the essential nature of language, which dictates the limits of its possible development." Logical propositions were thought of as tautologies in the sense that they are non-empirical, but true in virtue of the rules of logic. Logical propositions "reveal the structure of language and so, at one remove, reveal the structure of the world. The idea is that these two structures are the same."

According to the *Tractatus*, the world consists of states of affairs. Objects are constituents of states of affairs and a particular combination of objects or things, rather than the mere iteration of objects or things, will obtain in a particular state of affairs. A state of affairs that obtains is a fact. Propositions are pictures of the facts to which they refer. Language can picture the world because, Wittgenstein held, it has in common with the world a
logical structure. Passmore argues that "for Wittgenstein the form of a language is that which is common to it and to the reality it depicts." Language is the totality of propositions. Propositions can be analyzed into elementary propositions. Elementary propositions can be compared directly to atomic states of affairs and a truth-value can be assigned to them. Propositions can be "calculated" to be true or false via their constituent elementary propositions. At the basic level of analysis, the elements of the proposition (that is the linguistic formulation) can be broken down into "units" or "elements". What the linguistic formulation of the proposition depicts, - the fact or the state of affairs - can be broken down into "objects". A particular concatenation of linguistic "units" (the proposition) will depict a particular concatenation of "objects" (the state of affairs). At the atomic level of analysis it can be said that the units of the proposition correspond directly to the objects of the state of affairs. This correspondence is of a truth functional nature. Ordinary language may stray into "nonsense" because the "units" of a proposition may form particular combinations to which we have learned to attach meaning even though that particular combination does not correspond to a combination of "objects" of a state of affairs and therefore does not obtain as a fact. Thus factual language is separated from non-factual language. Toulmin argues that,
The arguments of the Tractatus, had employed the notions of "atomic facts to correspond to "unit propositions" of an idealized formal language. But Wittgenstein had said nothing about how, in practice, one was to recognize "atomic facts" or "unit propositions".10

The members of the Vienna Circle remedied this omission. They equated Wittgenstein's "atomic facts" with the indubitable and directly known "hard data" of Russell's epistemology. The unit propositions were taken to be the ultimate carriers of knowledge, "each recording a single item of sensory evidence, vouchsafed by one single 'sensation' or 'sense datum'".11

The Tractatus was especially congenial to the logical positivist movement. It set out to determine the limits of factual language and it revealed logic as the structure underlying language and at one remove the structure of the world. It appeared to argue for the possibility of the construction of an artificial language in which the world could be described factually and without fear of straying into nonsense. In order to separate factual from non-factual language a meaning criterion, the verification criterion, was adopted. This criterion stipulates that the meaning of a proposition is the manner in which it is verified. The criterion distinguishes between factual and non-factual propositions or cognitive and non-cognitive propositions. It declares as meaningful only those propositions that can be verified through a reduction to the hard facts of sense. Meaningful sentences
have cognitive import and came under the aegis of the verification criterion. They were regarded as synthetic and could be known to be true or false. Meaningless sentences were devoid of cognitive import. No possible experience could verify them and as a consequence it could not be ascertained whether they were true or false. A special class of sentences were the truths and rules of logic. They were cognitively empty, but regarded as true because of their self-evidence. Since no possible experience could invalidate their truth, they were regarded as 'analytic'. Language was thus divided into two classes of sentences: analytic and synthetic sentences. The two classes were regarded as mutually exclusive and jointly exhaustive. It was thought that theoretical terms could be given explicit definitions and cast in propositional form; in that manner they could be seen to obey the verification criterion. The possibility of the construction of a logically warranted language appeared feasible. Logic would form the basic structure of this language and the verification criterion would vouchsafe for the cognitive integrity of the cognitive import.

In the initial formulation however, there are several problems. In the first place, the verification criterion cannot itself be verified. It falls outside the division of the two mutually exclusive and jointly exhaustive classes. Strictly speaking it is meaningless. In the second place, scientific laws, which are expressed in the form of "all x's
are y's" are, strictly speaking not verifiable. They rely
on a principle of induction that allows one to infer from
"a good many x's are y's" to "all x's are Y's". Such a
principle of induction itself is not verifiable. In the
third place, the notion of sense datum is obscure. It relies
on two not altogether obvious notions: "directly perceived"
and "incorrigibly known". The problem here is that
practically, even after we have removed the interpretative
elements, we can usually doubt what we see. But even if there
is a feeling of certainty, we have no guarantee that we
perceive "directly". Hamlyn argues that "the question what
we perceive directly is a metaphysical question, not a
practical one".12 Moreover, sensing sense data is essentially
a private experience while the facts of science are public
events. Arguments to justify the move from private to public
events therefore took on a different emphasis. The elimina-
tion of metaphysics per se was no longer stressed.
Rather the emphasis was placed upon the elimination of
metaphysics as conducive to the unification of the sciences.
Frank argues

The elimination of metaphysics, from science
was not for Mach, as we now understand, a
demand arising from some anti-metaphysical
mood, but the only means of making possible
the unification of the sciences.13

From this point of view, Frank argues, "it is of secondary
importance whether this unification be achieved in terms of
perceptions or whether the physical language is introduced."14
The principal thesis of physicalism is that the physical language is a universal language, that is, "that any branch of empirical science can be translated into it without change of context." Besides the terms of physics proper, everyday terms for material objects and processes, and, moreover expressions like 'cell aggregate', 'stimulus', 'amount of cattle', 'behaviour of chieftains', and so on are part of the physicalist vocabulary. The language of physics is an artificial language. It has a distinctive vocabulary, a set of basic concepts, a syntax or logic underlying the language and a set of rules of translating other sentences within physicalism and a set of rules to translate sentences of other languages into physicalist language. Passmore argues that, All empirical statements can be expressed in a single language asserts then that there is a single set of basic expressions into which all other expressions can be translated, and a single method of translation which can be applied to all empirical statements. The physicalist language is also one in which the form of that language can be described within that language. Ordinary language can stray into nonsense because it does not necessarily remain in the same medium in which the absolute correspondence between the "units" of the atomic proposition and the "objects" of the atomic state of affairs obtains. Physicalism stays in the same medium as it were. It needs no outside restraint to prevent it from straying into nonsense. Neither does it need any outside justification.
for the factual claims generated within the language. Physicalism essentially retains the distinctions which characterise the logical positivist views as a philosophy of language. The division of all statements into analytic and synthetic statements with the contention that these two classes are mutually exclusive and jointly exhaustive remains. The verification criterion, however, underwent changes. With the adoption of physicalism the emphasis became on "confirmable", and "testable" in terms of "reports of basic observational data about objective situations involving material objects or physical measuring instruments." The intent behind the criterion remained the same however, to vouchsafe cognitive integrity. The singular importance which logic has within logical positivism, received its emphasis with the notion of axiomatics.

Axiomatisation is essentially an explication of all the logical apparatus underlying a scientific theory. A fully axiomatised scientific theory would consist of a list of logical terms, with rules to form axioms and theorems, a list of axioms and theorems, a list of theoretical terms and a list of correspondence rules. The paradigm example of axiomatization is the formalisation of Euclidean geometry. Euclidean geometry had been taught through the centuries as a concrete deductive theory. Various authors had modified the theory to a certain extent but they were essentially presenting the same body of knowledge. These differences in
presentation can be ignored as long as the main emphasis remained on the content of geometry. These differences take on a new significance as soon as this content is ignored. Customarily we regard a statement as axiomatic if what the statement refers to is self-evident, if it is beyond doubt. By divorcing logical content from cognitive (concrete) content, one can focus more clearly on the relationship between logical content and cognitive content. The truth or falsehood of an axiom, in the modern sense, is strictly governed by logical considerations independent of cognitive content. In the case of geometry it became apparent that some of the axioms did not deductively follow from the sequence in which they were presented, but were independent. A fruitful axiomatisation will reduce the content of a theory to a more compact axiomatic basis. It will display systematic interconnections between various concepts in the pre-axiomatic stage. It became possible to provide Euclidean geometry with a more economic axiomatic base. By providing a different set of axioms and a different set of concepts of geometry not based on our ordinary perception and experience of space, it became possible to construct different types of geometries. These geometries found their concrete application in Einstein's theory of relativity. The example of geometry served as an indication how our ordinary "empirical prejudices" could be intertwined with logical content. It was thought that if we could successfully divorce the content
of a theory from its logical form, we could obtain different concrete interpretations. Blanche argues that,

...these concrete realizations of an axiomatic system are called models. The original theory will be one of the models but not the only one. (An) axiomatization (thus) lends itself to different realizations which can be taken from fields of study far removed from the original domain.19

The axiomatisation of Euclidean geometry served as an example of this approach. The singular emphasis axiomatisation received stemmed not only from the exhibition of the logical structure, it also served as a tool both to demonstrate and to achieve the unity of science by paring down cognitive content to a set of basic concepts through logical manipulation and so exhibit the axiomatic unity (of e.g. geometry) in disparate fields of application.

The philosophical advantages of axiomatisation are controversial. It may well bring out the logical relationships between families of concepts, provide a standardization of terms and a degree of objectivity. It also may make clear what is assumed. But there is a strong suggestion however, that epistemological issues such as the unification of the sciences can be solved through axiomatization. One may argue against this view that axiomatization is essentially an expository device. It determines a set of sentences and exhibits the logical relationships between them. A particular theory may well have different sets of axioms underlying it. It is not immediately clear which one, if any, would be the
privileged set. One would have to do an extended analysis and any illumination may well come from this analysis rather than the particular set of axioms selected.

The separation between content and form brings to a focal point the importance of the analytic-synthetic division and the function of the verification criterion as a meaning-criterion. The structure of a scientific theory is its logical structure. The content of a scientific theory is provided by correspondence rules. It was thought that the meaning of a theoretical term could be captured by explicit definitions. Explicit definitions would be in terms of "reports of basic observable data." These reports would then be stated in correspondence rules and these would obey the verification criterion. The success of axiomatisation as an epistemological tool depends on the strict separation of content and form, or, the division into analytic and synthetic propositions. If the analytic-synthetic division did not hold then the separation between cognitive content and logical structure could not be made justifiably. Part of the structure could well have been cognitively important and so resist axiomatisation. Similarly, if theoretical terms could not be wholly and explicitly defined and captured in correspondence rules their cognitive integrity could not be vouchsafed for by the verification criterion. If the cognitive integrity of a proposition is no longer in its entirety vouchsafed for then we could not be confident that nonsense or metaphysics could not intrude under a cognitive disguise.
Correspondence rules serve several functions: they provide explicit definitions and they provide the cognitive import for a scientific theory. For example: a correspondence rule defines mass (a theoretical term) as the result of performing measurements $M$ on an object $O$ under certain circumstances $S$ (where $S$ and $M$ are specified using observation terms). This specifies an empirical procedure for determining mass, defines "mass" in terms of that procedure and does so in a way to guarantee the cognitive significance of the term "mass". If they are successful correspondence rules eliminate theoretical terms. They can be translated into observation terms and logical language. In that manner theoretical terms can be seen to be "metaphysically innocent". The functions of the correspondence rules are intimately related because of the requirement that they be expressable in mathematical logic - a requirement for axiomatisation. Difficulties arose however, in providing explicit definitions for disposition terms. Disposition terms such as 'fragile' or 'breakable' indicate a tendency or potentiality to break. This feature cannot be captured in mathematical first order logic. A further problem is that of specifying necessary and sufficient conditions for the applicability of a theoretical term. Routinely scientists will use different procedures for measuring the same theoretical concept. Strictly speaking, this would mean that each time new correspondence rules would have to be formulated, with the unwelcome consequence
that each time a new theory would be created since each theory by definition has its own correspondence rules. The result would be an awkward usage of "new theory". The course followed to remedy such objections was to relax the requirement of explicit definition and the requirement to specify the necessary and sufficient conditions for observation. The introduction of "reduction sentences" was thought to remedy the situation. A reduction sentence only stipulates a test condition which is applicable under certain circumstances and does not define a theoretical term completely. This must be regarded as unsatisfactory because "by allowing correspondence rules to be reduction sentences it can no longer be required that correspondence rules completely define the meaning of a theoretical term." If correspondence rules only partially define the meaning of a theoretical term, then the verification criterion can do its work only partially. It becomes then important to argue that the residual meaning not captured in correspondence rules and so not cognitively vouchsafed for by the verification criterion can be shown to be analytically or linguistically true. Considerable effort was expended to show that this was the case but the results remained ambiguous. Rather than explicating these attempts, I shall very briefly discuss Quine's classic paper "The two dogmas of empiricism". In this paper Quine argues for the rejection of both the analytic-synthetic division and the verification criterion
which are at root identical dogmas.

Quine distinguishes two kinds of analytic sentences. An example of the second kind is: "No bachelor is a married man." (2). The characteristic of such a statement is that "it can be turned into a logical truth by putting synonyms for synonyms". Thus example (2) becomes "No unmarried man is married" (1), which is an example of the first kind. The relevant feature of this class is that "it is not merely true as it stands but remains true under any and all reinterpretations of "man" and "married". Examples of class (2) however, stand in need of explication of the notion of synonymity. Quine considers several notions of definitions as a means of converting class (2) statements into class (1) statements. The lexicographer's notion of definition is dismissed as it "affirms a relation (of synonymity) antecedent to the exposition in hand". The philosopher's notion of definition both formal and informal are also wanting; "we find that definition...hinges on prior relations of synonymy." Quine then examines the suggestion "that the synonymy of two linguistic forms consists in their interchangeability salva veritate (that is, without alteration of truth value in all contexts.). But he concludes that "interchangeability 'salva veritate' is meaningless until relativised to a language whose extent is specified in relevant respects that is, when we assume that "we already made satisfactory sense of analytic". Quine then considers "semantic rules" in
artificial languages as a guide, but discovers that these rules even in artificial languages do not define synonymy or else invoke similarly obscure notions such as "analytic for that particular language". Quine concludes that here "semantic rules" are of interest "only insofar as we already understand the notion of analyticity". They are of no help in gaining this understanding. Almost as if coincidence would have it, Quine considers as a last possibility the verification criterion "as a method of infirming or confirming a statement. An analytic statement is that limiting case which is confirmed no matter what". Quine distinguishes a (naive) form a reductionism where "every idea must either originate directly in sense experience or else can be compounded of ideas thus originating"; and an attenuated form of reductionism "whereby the primary vehicle of meaning came to be seen no longer in the term, but in the statement". Even so, Quine argues,

...the notion lingers that to each statement, or each synthetic statement there is associated a unique range of possible sensory events such that the occurrence of any of them would add to the likelihood of the truth of the statement, and that there is associated another unique range of possible sensory events whose occurrence would detract from that likelihood. This dogma of reductionism is ultimately connected with the other dogma, that there is a cleavage between the analytic and the synthetic...My present suggestion is that it is nonsense to speak of a linguistic component and a factual component in the truth of any individual statement...The unit of empirical significance is the whole of science.
Although Quine's argument cannot be regarded as conclusive, for he has only entertained a limited range of notions of analyticity and found those wanting, it has generally been accepted that this distinction cannot play the role attributed to it within Logical Positivism. It has generally been accepted that the division of language into analytic and synthetic statements is not mutually exclusive and jointly exhaustive. Quine's suggestion "that it is nonsense to speak of a linguistic component and a factual component in the truth of any individual statement", has led to the view that correspondence rules do not adequately define theoretical terms and that these terms cannot be introduced into a theory in a piecemeal fashion. Putnam, who argues in favour of the retention of a revised notion of analytic and synthetic, argues that there is a large class of statements, including most principles, definitions, and highly developed laws of science, which are neither analytic nor synthetic, but are nonetheless essential to the theory. Hempel argues that theoretical terms such as "pressure", and "volume", are not introduced in a piecemeal fashion. Rather these terms are introduced by setting up a theoretical system formulated in terms of them and by giving this system an experimental interpretation. This in turn confers empirical meaning on the theoretical constructs.

In "Epistemology neutralized" Quine restates his opinions. He argues that an individual statement about
bodies has no fund of "experiental implications it can call its own". But a substantial mass of theory, has experiental implications. Sometimes an experience implied by a theory fails to come off. Ideally, we declare the theory false. But the failure falsifies only a block of theory as a whole, a conjunction of many statements. The failure shows that one or more of those statements is false but it does not show which. The predicted experiences, true and false, are not implied in any one of the component statements of the theory rather than another. Quine clearly rejects a sentence by sentence approach to science. Theories, that is, conjunctions of many statements, can be falsified, but this does not throw light on which individual statement is false. Because individual statements do not have "an experiental fund to call its own", translation of e.g. English into Arunta must proceed as a "body". "If the English sentences of a theory have their meaning only together as a body, then we can justify their translation into Arunta only together as a body". Quine would reject the view of a single unified language of science into which we can translate, statement by statement, all empirical statements. Since Quine rejects the dogma of reductionism "of the sort where every sentence is equated to a sentence in observational and logico-mathematical terms", he also rejects the possibility of a rational reconstruction of science, which would establish the metaphysical innocence of theoretical terms. The impossibility
of such a reconstruction, dissipates according to Quine
"the last remaining advantage that rational reconstruction
seemed to have over psychology."34 The new epistemology "or
something like it simply falls into place as a chapter of
psychology and hence natural science".35

Quine's view of science contains some distinct
differences compared with both the views of the Logical
Positivists and Popper whose view will be discussed in the
next chapter. Both these philosophies have in common the
assumption that there is a sharp boundary or demarcation
between science and non-science. They both assume that
science consists of logically connected statements each one
of which can be justified, either by verification or falsifi­
cation. They both propose a "rational reconstruction of
science", although each view contains its distinct value.
Quine in contrast proposes that science is "like a field of
force". The boundary condition of that field can be subjected
to experience. At the boundary of that field or at "the
sensory periphery of the body scientific", a statement has
empirical content and "wears it on its sleeve". Inside the
field, receding from the periphery, a statement does not have
this direct empirical content, although conjunctions of
statements will have empirical consequences. But in the face
of contrary experience, we cannot single out which statement
is false. "Any statement can be held true come what may,
if we make drastic enough adjustments elsewhere in the system".36
Quine's position derives historically from Duhem. It is associated with the view that some empirical statements, when no contrary experience occurs, in due course acquire the character of conventions. A contrary experience will initially not reflect on these statements and other alterations will be made. Eventually however, the modifications will become too cumbersome and the system will "crumble".

Conclusion

Traditionally, in empiricist circles, the senses are regarded both as the source and the basis for the acceptance and justification of our knowledge claims. "Theory" is regarded as a potential source of error. The logical Positivist movement can be seen as an attempt to reduce the theoretical principles of science to an economic account of descriptions of sensations. Logic was regarded as analytic, true in all possible worlds, but not informative about the actual world of matter of fact. It was thought that the actual world could be described by a logical axiomatic calculus, which would connect with and describe the world through logically reconstructed facts. A fact could be logically reconstructed with a sense datum as its basis. A sense datum could be directly perceived and incorrigibly known. It was where perception and the world it depicted met in truth; a "piece of knowledge" was that which one could not, logically, be mistaken about. Failure actually to
formulate and identify a satisfactory notion of sense datum led to the adoption of physicalism as the language of science. Physicalism is an artificial language designed to 'stay in the same medium' it depicted. The structure of that language depicted the structure of the world, while the verification criterion restrained the vocabulary from straying into nonsense. Axiomatisation was seen as an especially helpful method to exhibit the logical structure of scientific theories, and to unify disparate theories by showing a common compact axiomatic base. Correspondence rules were thought to provide explicit definitions of theoretical terms stated in observation terms. Once verified to be the case, the meaning of theoretical terms would remain invariant through the further developments of science. Quine revealed the analytic-synthetic division to be at root identical to the verification principle. He rejected both dogmas and proposed his own view of science. Within this view there is a notable lack of concern to demarcate science from non-science. There is no sharp border between science and non-science.
CHAPTER TWO

KARL POPPER ON SCIENCE AND RATIONALITY

Central to the problem of the justification of scientific knowledge is the status of the empirical basis upon which science rests. The empirical basis rests essentially on personal experience. How do we justify this experience? Within logical positivism personal experience is regarded as both the source and the justification of the validity of our knowledge claims. This is reflected in the persistent attempts to construct atomic propositions which would correspond directly with atomic facts. If an atomic proposition does not say more than what is experienced, that proposition can be regarded as secure since it carries its own evidence. It can be known to be true or false. Complex propositions can be known to be true or false via their atomic statements. We essentially have a realm of propositions and a realm of facts. The guiding metaphor is that language—that is, the totality of all factual propositions—"mirrors" 'reality', which is the totality of all possible facts. The two realms are bridged by observation and at the atomic level of analysis, the atomic proposition and the atomic fact correspond absolutely. The linguistic formulation does not say more
than what is experienced. In this manner the logical positivists adhere to the general principles of empiricism.

Empiricism basically claims that our knowledge originates in and is validated through experience. It affirms a connection between the way a proposition derives its meaning and the way in which it is validated. The particular logical positivist solution is to argue that at the atomic level of analysis a particular "unit" of proposition corresponds absolutely to a particular "unit" of perception. The empirical foundations of science was held to consist of proven, true facts. Persistent failure actually to formulate atomic propositions and link them with primitive (visual) experience so that one could not be logically be mistaken about them, led to the abandonment of this approach. It illustrates however the predicament of the logical positivist approach: experience is appealed to validate knowledge derived from experience but it is an empirical fact that we can be mistaken about our experiences.

The effective abandonment of the justification of the empirical content of science along empiricist lines was initiated by Neurath. Neurath held that "protocol sentences", as a basic unit, were held to be corrigible and that their truth could be decided by convention. With changes, Popper pursues this line of argument. Popper examines anew the question how to justify the empirical base of science and he sees three options: dogmatism, infinite regress and psycholo-
For Popper the empirical base of science consists of "basic statements". If we demand a justification of "basic statement" by way of reasoned argument, we are led to an infinite regress. The argument to justify the statement will employ premisses assumed to be true. The justification of those premisses will require an additional argument and so on. To stop the regress we can say that the statement originates in experience in such a manner that we cannot be mistaken about it. This is essentially the logical positivist approach. Popper however, rejects this approach as too psychologistic. Another approach to stop the regress is to accept the statement as true by convention. Popper rejects this approach also: it leads to dogmatism as we may come to accept the statement solely on authority. In opposition to the logical positivist who asks, in a sense, how can we be certain about our experience, Popper asks, how can we best criticise our experience. This question reflects the tenor of Popper's approach. Popper is not interested in proven, true knowledge but in criticising the knowledge we accept. He argues that we can best criticise our experiences if we cast them in the form of basic statements.

Popper holds that "basic statements are...statements that an observable event is occurring in a certain individual region of space and time". Furthermore, "every test or theory...must stop at some basic statement or other which we decide to accept". Dogmatism can be rejected because basic
statements are to be accepted after intersubjective testing done by anyone who has learned the relevant technique and who will then be able to decide that the statement is acceptable as basic. Infinite regress does not obtain since no theory is trying to prove a basic statement to be true. Psychologism is also rejected since

...we do not attempt to justify basic statements by particular experiences. Experience can motivate a decision...but a statement cannot be justified by them no more than by thumping on the table.\(^3\)

To be sure there remains an element of conventionalism in the decision to accept a basic statement as basic and in a second, closely associated, decision to relegate a set of accepted basic statements as unproblematic background knowledge, but this conventionalism remains innocuous: both decisions are open to revision should the need occur.

As a consequence for Popper "the empirical basis of objective science does not rest on rock bottom. The structure of its theories rises, as it were, above a swamp".\(^4\) Basic statements and theories are not mutually exclusive. "Agreement upon the acceptance or rejection of basic statements is reached as a rule, on the occasion of applying a theory, the agreement, in fact, is part of the application which puts the theory to the test".\(^5\) As long as a theory withstands detailed and severe tests and has not been superseded by another theory, Popper argues, "we may say that it has proved its mettle or that it is "corroborated" by past
The fundamental point Popper makes in "The Logic of Scientific Discovery" is, that this corroboration of a scientific theory ought not be established by verifying the theory, but rather by falsifying the theory.

Popper holds that,

Natural laws, do not assert that something exists or is the case; they deny it. They insist on the non-existence of a certain state of affairs, prescribing or prohibiting, as it were, these things or state of affairs: they rule them out.

Because natural laws rule out certain states of affairs, we should in order to corroborate a theory, insist on falsification. By proving the existence of the ruled-out state of affairs can we prove the theory to be false. By failing to prove the existence of the ruled-out state of affairs we can corroborate the theory. If we corroborate a scientific theory by verification, we are only obliged to look for states of affairs which are not ruled out by the theory. But this approach can lead us to overlook the existence of a ruled-out state of affairs. It allows us to enlarge the theory, by additional ad hoc hypothesis, so that the ruled-out state of affairs can be accommodated by the theory. Verification is thus a sure way to dogmatism. Falsification, on the other hand, forces us to search out the ruled-out state of affairs, and avoids dogmatism. As a consequence a scientific theory can never prove to be a fundamentally true theory. All theories can only be constructed as 'not yet falsified' theories, and 'not yet falsified' does not imply 'fundamentally
true'. Thus, Popper holds,

...science is not a body of knowledge, but rather a system of hypotheses: that is to say, a system of guesses or anticipations which in principle cannot be justified, but with which we work as long as they stand up to tests, and of which we are never justified in saying that we know that they are 'true' or 'more or less certain' or even 'probable'.

Popper distinguishes three criteria for the growth of knowledge.

(1) the requirements of simplicity. This is a difficult principle to describe but it has one important consequence: it allow us logically to analyse the testability of the theory.

(2) the requirement that the theory should be independently testable. The theory must not only explain the facts it was designed to explain, it must lead to the prediction of phenomena which have not so far been observed.

(3) the requirement that the theory should pass some new and severe tests.

The first two requirements are "formal requirements and could be dealt with by logical analysis. The third requirement, however, is a "material" requirement and demands empirical success. It is essential if science is to progress. There are three good reasons for the third requirement and they reflect Popper's views on scientific growth.
(1) Successful predictions are at least a necessary condition for the truth of an independently testable theory.

(2) We have no reason to regard a new theory better than an older one until we have derived from the new theory predictions which were unobtainable from the old theory. A crucial experiment is necessary to confirm the new theory.

(3) A crucial experiment which would confirm a new theory can create new criteria for independence of explanation. The result is that what was problematic knowledge becomes now background knowledge.

The third requirement may be divided into two parts: "first we require of a good theory that it should be successful in some of its new predictions; secondly we require that it is not refuted too soon". Popper's reasons for these requirements are,

...the successful new predictions which we require the new theory to produce are identical with the crucial tests which it must pass in order to become sufficiently interesting to be accepted as an advance upon its predecessor, and to be considered worthy of further experimental examination which may lead eventually to its refutation. 10

Popper's view of science is initially best viewed in contrast to logical positivism. Popper associates the role of observation in logical positivism with psychologism, that is, an appeal to experience or to feelings of certainty about what one sees or experiences. Popper argues that a
statement such as "there is a glass of water" cannot be correlated with specific sense experiences. Such a statement is best viewed as theoretical. Terms such as glass and "water" transcend specific sense experience. Such terms draw on theories which have specific meaning to these terms. Psychologism is closely associated with the attempt to generalise from observed true specific instances to general statements. Popper rejects such attempts on the grounds that they require a principle of induction. "All swans are white" cannot be proven true; it requires a principle of induction which stipulates its truth. The principle of falsification however, can prove "all swans are white" false. All that is needed is one black swan. We cannot prove true, but we can prove false. Moreover if one realizes how little part the process of generalizing from observed instances actually plays in science, the problem of induction disappears Popper would want to argue. Rather than induction, Popper proposes deduction as the language of criticism. A logical property of deductive argument is that, if the premisses are true, the conclusion must also be true. If the conclusion turns out to be false and we accept this as the case, then we know that at least one of the premisses must be false.

Popper's concern is primarily to account for the growth of knowledge and to exhibit the rationality behind the choices in succeeding scientific theories. The rational reconstruction of science, for Popper, is the reconstruction
of the growth of science in such a manner that the rationality behind theory choice becomes fully perspicuous. In adopting this focus, Popper clearly differs from the logical positivists. The rational reconstruction of science for the logical positivists consists in exhibiting the empirical base of science to be a logical structure with proven knowledge claims. But both Popper and the Logical Positivists agree that the justification of their view of rational reconstruction takes place in the context of justification. It is the task of the philosophy of science to provide this reconstruction and it is claimed that the resulting reconstruction provides an adequate understanding of science.

The division of science into the context of justification and the context of discovery comes from Reichenbach. The context of discovery is "about" science. It may well include aspects of the sociology of science or the psychology of perception. It may include references pertaining to general sociological, economical or psychological factors, which may hinder, or facilitate, the institutionalization and further development of science. It may also take the form of an enquiry into the psychological aspects that accompany great discoveries in science. But the logical analysis and the justification of knowledge claims takes place within the context of justification. According to this view the question how we arrive at our scientific knowledge claims and what socio-cultural factors contribute to their acceptance or
rejection is independent of the question as to what sort of evidence is adequate for the acceptance or rejection of knowledge claims of science.

Connected with this view of justification of science is the question of how science is demarcated from non-science. Science, in Popper's view, is a series of basic statements, logically connected with each other and each one susceptible to the verdict of experiment. Given the division of science into the two contexts and given the conviction that the philosophy of science can legitimately confine itself to the context of justification, the study of the growth of knowledge is nothing but the study of the logical relationships between those statements. The sort of questions that are asked within the context of justification are: "Can a statement be justified? And if so how? Is it testable? Is it logically dependent on other statements?" What has disappeared entirely is the question of the origin of our knowledge claims. From the logical positivist perspective, the question concerning the origin of knowledge is dealt with within the context of justification. This is in part a consequence of the empiricist view that experience can function as both the source and the justification of a knowledge claim. In part it is a consequence of the aim of the logical positivist programme, to provide a logically warranted basis for the empirical foundation of science. The verification criterion, seen as a demarcation criterion, puts
the question of cognitive validity and the truth of those claims, within the sphere of the context of justification. The adoption of a different demarcation criterion, the falsification criterion, puts the emphasis on testability. The question of the origin of our knowledge claim is relegated to the context of discovery. The questions which arise in this context are irrelevant to the problem of justification of scientific knowledge. It is at this point that Kuhn and Hanson disagree. They argue in one way or another, that the questions we ask are not independent of the manner in which we justify them. I shall return to their views in Chapter Three.

Although the problem of finding a demarcation criterion may not appear to present any difficulties, in fact it does. It must include, but not uncritically, what scientist think science is. It must not exclude too much of what has passed for science in the history of science. In order to point out the differences between scientific statements and non scientific statements, we are already adopting a certain attitude towards what we think science is. Whichever feature we would choose to indicate the character of science, we would have to accept it as a norm. A demarcation "is not based on any natural features of a sentence, i.e. any demarcation criterion must be accepted as a norm". The acceptance of such a criterion must be based on agreement as to which ends it serves and how adequate it does that.
Amsterdamski argues that although it is not always recognized,

...a demarcation criterion also draws a
demarcation line between science and its
methodological rules. It turns out that,
because of their normative character, the
criterion of demarcation as well as other
methodological rules (rules of accepting
and refuting the claims of science, of
interpreting experimental results, of
constructing concepts and theories etc.) do
not belong to the set of statements denoted
as scientific. 13

Although both the demarcation and the methodological rules
are to be regarded as norms, they are not merely norms. Both
the demarcation criterion and the methodological rules are
derived from a certain period within the history of science
and contain descriptive elements. In that sense they can be
regarded as more or less descriptive. Moreover, Amsterdamski
argues,

...scholars who construct normative methodology,
and here Popper may serve as a good example,
are usually careful to ensure not only that
their methodological constructs lead to the
realization of certain values, which are hidden
behind the choice of a certain method, but also
that they have some relation to reality and do
not constitute a completely arbitrary set of
decrees. 14

Thus even though there will be discrepancies between the
proposed reconstructed model and the actual course of science
as it develops, it may be profitable to look into the source
of the discrepancies. If we can argue that the discre-
pancies which do occur do not find their source in the recon-
structed model of the growth of science, then there may be a
possibility that the proposed methodology and reconstructed
model could serve their normative function. This function is to exhibit the rationality behind theory choice in science, to show the rationality of the growth of science and to vindicate the claim that the reconstruction of science within the context of justification provides an adequate understanding of science. The substantial premisses involved behind the assumption that the philosophy of science can restrict itself to the context of justification, are that in the process of the evolution of knowledge there are no connections between the accepted manner of justification of statements and the manner of arriving at them.

These premisses would have to state that the understanding of science (and of its growth) is nothing more than the understanding of its logical structure (or of the logic of its development, that is, the methodological rules according to which old theories are replaced by new ones) and that the history of science and the sociology of knowledge have nothing to contribute to this understanding.\textsuperscript{15}

Kuhn and others strongly disagree with these premisses and I shall delay the discussion on the adequacy of the methodological rules till Chapter Four. But the demarcation criterion is also a norm and I shall discuss now whether the discrepancies which arise from the normative model of testing by experience according to Popper and the actual notion of experiment, stem from the model or from actual scientific practice.

The empirical character of science for Popper does not rest on any specific appeal to experience. Rather,
Popper will admit a system as empirical or scientific only if it is capable of being tested by experience. "These considerations suggest not verifiability but falsification of a system is to be taken as a criterion of demarcation."\(^{16}\) It is thus important that the notion of "testing by experience" achieves what it is designed to, that is, to falsify a theory and to explicate the notion of "empirical". A theory is put to test by identifying the basic statements which support it. We can arrive at a decision to hold a statement as basic through an experiment that puts the theory to test. A basic statement and the theory of which it is part are not independent of each other. Testing can go on, but at a certain point it is decided to regard as "basic" a particular statement. A basic statement is arrived at by convention. For Popper, however this is an 'innocuous' convention since we can always reopen the testing. A basic statement furthermore is not basic in a logical positivist sense of absolutely basic; it may remain more or less complex depending on the decision made by the relevant community. A theory under test consists essentially of a class of basic statements and a class of potential falsifiers. If one of the potential falsifiers obtains that is, if we find a state of affairs that the theory rules out the theory is falsified and ought to be rejected. The theory to be tested should be risky. The class of potential falsifiers should not be empty for that way the theory cannot be falsified. A large class of poten-
tial falsifiers means that the theory rules out a great many states of affairs. Failure actually to falsify such a theory tells us a lot. It corroborates the theory itself and tells us what is not the case. Corroboration of a theory does not allow us to think that the theory is therefore more "probable" or more "certain" for the next test could, and ideally should, be the test that actually produces a falsifier. In actual fact however, the notion of "testing by experience" and "theory" are more complex. I shall describe the notion of theory more fully in Chapter Six, and will mention here only that theories, in an important sense, constitute "idealisations".

A theory is developed to answer a variety of questions about certain puzzling phenomena. These questions involve requests for descriptions, explanations and predictions about the phenomena. These phenomena are described from within certain relevant parameters adopted with a view to answer the questions the theory is supposed to answer. These parameters "isolate" the phenomena and provide "idealised" accounts of them. The assessment of the truth of the explanations the theory provides, based on the "idealized" accounts of the behaviour of the phenomena, comes from experimental verdict. This involves having to select a suitable experiment. An experiment "mediates" between the domain of the actual phenomena and the domain of the "idealised" phenomena. One has to design an experiment which
can be used to argue that it depicts a state of affairs that is representative of the behavior of the actual phenomena and also representative of the behavior of the "idealised" replicas. What is tested is a hierarchy of theories: the actual theory, a theory of experiment and design as being adequate to the task at hand and a theory of data, which has to be manipulated to be seen to represent both the actual phenomena and the "idealised" replicas. A falsification may reflect on any of the aspects involved. As Amsterdamski puts it;

What is confronted with the obtained results is not simply the theory under test, but the theory in conjunction with various theoretical assumptions (background knowledge) which participate in the interpretation of the obtained results and determine the decision whether to accept or refute the observation statement. 17

When the experiment delivers a falsification it is not clear what is falsified, the theory under test or part or all of the background assumptions. The concept of falsification is thus a weak one--something is falsified but it is not exactly clear what it is. At this point Popper introduces another convention. After it is decided, by way of experiment and convention, what is to be counted as background knowledge and what as the theory under test, we are not allowed to make changes to accommodate the falsifying instance. This is known as the prohibition against ad-hoc change. That to test a theory is actually to test a theory in conjunction with various other theories or theoretical assumptions, is a well
established point of view. Quine, as quoted in Chapter One, argues that in the face of one contrary instance there is much leeway in the system of interconnected statements as to which one we can alter. Korner argues:

As Poincare and Duhem have shown—and as in the case of more complex theories is generally admitted—it is possible to replace a conjunction which in the light of certain tests has in some respects been found defective, by more than one conjunction which is free of this defect.18

It is precisely at this point that actual scientific practice has been at odds with the prohibition against ad-hoc change. History of science indicates that scientists sometimes reject the theory and sometimes make changes in the background knowledge. Either way may lead to success or failure. It is clear then, that the notion of "falsification" gains its strength from a convention and not from the notion of "testing by experience". "Testing by experience" turns out to be too opaque to do anything other than deliver a weak sense of falsification. To adopt the strong sense of falsification seems neither here nor there. Scientists, as a matter of fact, do not abandon their theories as readily as Popper would wish to argue and sometimes they are right and sometimes they are wrong.

Earlier in this Chapter, I argued that the notion of "testing by experience" ought to explicate the notion of "empirical" in the manner in which it is used in the Popperian scheme. This is necessary since Popper rejects
the logical positivist interpretation of "the empirical base of science". In this view experience, through observation, is regarded as both the origin and as a source of justification of the empirical base of science. Popper has rejected this view of observation and experience on the grounds that it is too psychologistic. As a consequence a new sense of "the empirical base of science" must be provided and a new connection between experience and science ought to be provided. But it is not clear that the notion of "testing by experience" achieves this. Experience is connected with basic statements but the first thing we notice, is that acceptance of a basic statement is arrived at by appeal to a convention. We may notice here that a theory and a basic statement are not independent of each other and that acceptance of a basic statement also involves accepting a theory. We may prefer this circularity to an infinite regress of which the acceptance of a basic statement is supposed to stop. But when it comes to locating the point at which experience gains its purchase in "basic statement" or "theory", we are confronted either by a circularity or a convention. The view that experience may gain its purchase through the notion of "refutation", is not clear. Here, one may assume that our expectations of regularities via basic statements are thwarted by a falsifier. The falsifying statement disrupts these expectations and it is at this point that experience gains its purchase. But this line of argument
relies on a strong sense of falsification and this is only obtained by convention.

The difficulty in locating the empirical basis of science in Popper's philosophy, stems from his rejection of the logical positivist view of observation as providing both a source and a basis for the justification of the empirical base of science. Observation for Popper is primarily theoretical: "All terms are theoretical, some are more theoretical than others." Popper argues that the statement "here is a glass of water" cannot be verified by any observational experience. The reason is that the universals which appear in it cannot be correlated with any specific sense experience. Popper concludes from this that, rather than appeal to experience to accept a basic statement, we should decide, upon testing, what is to count as a basic statement. Ayer argues that the weakness of this argument is that although we may agree that "observation transcends" the experience on which it is based, it does not follow that we are left with no reason for accepting it". Although we may be, at times, mistaken about the description of an experience, this does not prevent it "from being true that my "having this observational experience" supplies me not only with a motive, but also with a ground for accepting the interpretation I put upon it". The reason for accepting this view lies in the fact, that "my command of the language which I am using fundamentally consists in my having acquired
the habit of accepting certain statements as the result of having the appropriate experiences".²² Ayer thinks that

One's right to be sure of the truth of a statement is founded, not on one's having the right to be sure of the truth of some other statement, but directly on the fact that one is having the experience in question.²³

Ayer rejects Popper's argument that in general we do not test the validity of an assertion by tracing its origin, but by a critical examination of the asserted facts themselves.

Ayer regards this view as mistaken:

What must be meant here by examining the facts is making observations which lead one to accept or reject certain basic statements. If these observations give us no authority for accepting any basic statement, they do not constitute a test of anything, so that there is nothing to be gained by examining the facts: if they do give us this authority, our claims to know basic statements are validated by their sources.²⁴

Ayer's argument seem particularly relevant when it comes to observing an actual "ruled out state of affairs". It is at this point that our expectations in previous regularities have been thwarted. Presumably the occurrence is novel and it is here that, if one is to assert the falsifier to be the case, that observation—must play the role Ayer recommends it does—to accept the falsifying statement as the case because it describes the appropriate experience. Quinton in "The foundations of Knowledge" also sees difficulties with the view that experience can only motivate a decision to accept a statement:
...the vital difficulty is this: either the fact that an observational belief is motivated by experience is a reason for accepting it, in which case experience is not just a motivation; or else no belief is justified at all. Unless experience actually supports the beliefs that it prompts us to hold why should we choose to adopt them in preference to those which are prompted by wishful thinking or the desire to save ourselves trouble or any other emotional factor?25

It is not clear however whether Popper has remained consistent in his view of the role observation plays. Earlier I have quoted Popper as saying that a theory should resist for a given time, all attempts at falsification in order to give the theory time to establish as successful its new predictions. Amsterdamski comments; "this means of course that the author (i.e. Popper) acknowledges the cognitive value of positive evidence.26 Popper also stipulates certain criteria for evaluating the new theory—"the new prediction which we require the new theory to produce are identical with the crucial test which it must pass...."27 Combined with the requirement that the new theory should resist falsification for a specified time, this amounts to arguing that a new theory is confirmed i.e. verified at least until it is decided, by way of a convention one presumes, that the theory is ready to be falsified. Popper admits that there is a "whiff of verificationism" here but he prefers that to instrumentalism.28 The point is however whether this amounts to assigning value to positive evidence qua positive evidence and if so, how this is different from assigning at
least a temporary role to observation in the sense Ayer argues for it.

Conclusion

The philosophy of Popper can best be seen in relation to logical positivism. Within logical positivism, as part of their empiricist heritage, experience plays a role as a source for and as a validation of knowledge claims. The logical positivists were concerned to find a link between the realm of experience and the realm of statements. Popper, in a sense, dissolves this problem by confining himself entirely to the realm of statements. Epistemology, for Popper, has nothing to do with knowledge of, or the meaning of statements but is solely concerned with the justification of the acceptance of those statements.

The essential problems in science are, according to Popper, the problem of demarcation and the problem of induction. Both these problems are solved with the adoption of "falsification" as a demarcation criterion. This dissolves the problem of induction because we do not attempt to prove true, but we try to prove false. If we can prove the existence of one contrary instance we can show the generalization to be false. Moreover, the notion of "testing by experience" allows Popper to regard himself as an empiricist. The empiricism of Popper lies solely in the veracity of the notion "testing by experience". It is through experimental
verdict that we falsify—produce a state of affairs inconsistent with the theory under test. The empirical base of science, for Popper, has nothing certain about it; the basic statements remain on "indefinite probation", their justification is the main problem. The justification of those statements lies in thorough testing and an appeal to methodological criteria concerning acceptance or rejection of evidence. Both the falsification criterion and the methodological rules can be considered as norms. Adoption of these norms will serve certain ends. Comparison of these norms with actual scientific practice will be helpful in judging whether these ends could conceivably be realized. It is not clear however, whether the notion of falsification achieves what it has to: to falsify and to elucidate what is meant with the empirical base of science. Falsification relies on a straightforward sense of "testing" and "theory". What appears to be involved however, is a hierarchy of theories each developed with specific aims. It is not clear whether a theory under test can be separated from background knowledge by means of an innocuous convention. Once the decision is made, we are not allowed to make changes in the background knowledge. If a falsification obtains, the theory must be rejected. Judging from actual scientific practice, it is not clear whether the empirical falsification of a scientific claim is either a sufficient or a necessary condition for their elimination. With an equal measure of success scientists reject or hold to their refuted theories.
There is, furthermore, a difficulty in locating the empirical base of science. "Testing by experience" brings us no further since we are confronted by either a circularity or a convention. No appeal to experience can be made even in the ordinary sense that such an appeal may provide grounds for accepting a particular claim. Popper's reply to Ayer remains "inconclusive". "Our experiences", Popper argues, "are not only motives for accepting or rejecting an observational statement, but they may even be described as inconclusive reasons." Neither is it clear whether Popper has remained consistent in his rejection of inductivism. He argues that a good theory should resist falsification for a while. Agassi, in "Science in Flux" argues that Popper here attributes value to positive evidence "and is in the same boat as the inductivist philosophers". For some time at any rate passing a test means that we accept verification as legitimate.

In agreement with the logical positivists, Popper divides science into the context of justification and the context of discovery. Popper's aim is to exhibit the rationality of the growth of science by exhibiting the rationality of theory-choice in succeeding scientific theories. This amounts to exhibiting the logical relationships between the statements which are accepted in the context of justification. The question of the origin of the scientific statement is relegated to the context of
discovery and independent of its justification. In the next chapter I shall discuss "The Structure of Scientific Revolutions", by Thomas Kuhn. Kuhn argues that the scientific questions which we ask and the justification of the answers which we provide, are not independent of each other. He achieves this through an appeal to a notion of "paradigm" which combines a sense of cognitive origin with a sense of justification of those cognitive knowledge claims.
CHAPTER THREE

THOMAS KUHN ON SCIENCE

The aim of both the philosophy of Popper and of the Logical Positivists is to provide a rational reconstruction of science. For the Logical Positivists the aim of the rational reconstruction is to secure a proven empirical base for science. This would serve to exhibit that scientific knowledge is based on experience and cognitively vouchsafed for. For Popper the aim of the rational reconstruction of science is to exhibit the rationality behind theory choice in the growth of science. His aim is to exhibit this rationality as the consequence of adopting certain supra-historical methodological rules. Kuhn rejects the notion of rational reconstruction in either sense. There is no clear sense of "rationality" derivable from the view of Kuhn even though Kuhn attempts to demarcate science from non-science. Kuhn introduces several new concepts such as "paradigm", "anomaly" and "normal science" and he puts an emphasis on the "conceptual" aspect of observation in contradistinction to both Popper and the Logical Positivists. I shall discuss this emphasis on the "conceptual" by first outlining the differences
in outlook in the growth of science, I shall then proceed
to discuss Kuhn's view and their implications.

The notion of theory within Logical Positivism is
basically atomistic. A theory is essentially a series of
propositions each one of which confronts experience directly
and can be fully justified by an appeal to the verification
criterion. A fully axiomatised theory consists essentially
of a series of primitive terms, definitions and rules of logic
adequate to construct the logical structure of the theory
and a series of correspondence rules which give explicit
definitions of theoretical terms. Correspondence rules,
stated in observation terms, provide the cognitive import to
a theory. Scientific progress consists in recognizing new
phenomena in a particular domain. When it is realized that
the particular theory cannot provide an adequate solution
to the new problems, a new theory is developed with new
methods of experimenting. This will result in a new set of
correspondence rules and the result will be a new theory.
The old theory is not invalidated however. Once a theory
is confirmed it is relatively immune to disconfirmation.
The new theory pertains to new phenomena, employs different
experimental techniques and has a different set of correspon-
dence rules. Once this theory is confirmed it too will be
relatively immune to disconfirmation. Progress thus consists
of expanding the scope of a well-confirmed theory to a
wider scope, using different experimental methods and so
developing a new theory. The new theory and the old theory are closely related however: the new theory employs no descriptive terms that are not used with approximately the same meaning as in the old theory.

A second form of scientific progress within Logical Positivism occurs when a particular theory is absorbed into a more inclusive or comprehensive theory, "for example, the reduction of thermodynamics to statistical mechanics or the reduction of Kepler's laws to Newtonian dynamics".¹ In order for this type of theory reduction to occur, it must first be established that the theoretical terms of the theory to be absorbed, can be reduced to, with the help of additional assumptions and supportive evidence, the theoretical terms of the more inclusive theory. In both manners of "theory reduction" there are then deductive relations established between the theoretical terms of the different theories. It is here that the notion of meaning invariance throughout scientific progress finds its origin. The development of science consists in

...the extension of (accepted) theories to wider scopes, the development of new highly confirmed theories for related domains, and the incorporation of confirmed theories into more comprehensive theories.²

Within the Popperian framework a theory can be broken down to "basic statements". They are justified (for the time being) by an appeal to experimental verdict, methodology and convention. Theories are in this sense atomistic. A
theory is essentially a series of basic statements. Science grows by refuting theories and inventing new ones. New theories have to account for the failure of the old theory, explain its facts and predict new phenomena. These theories should then be tested and criticized and the one with the greatest empirical content, that is, the one which rules out the most states of affairs, should be accepted. Tests may then further corroborate this theory if we fail to obtain a ruled-out state of affairs. Corroboration however, does not mean that we may legitimately accept the theory as more "probable"; the next test can always be and ideally should be the test that refutes the theory. Science grows through successive theories, each one explains the previous one and each one has excessive empirical content. Although observation is theory dependent, that is, always from a theoretical point of view, observation does not interfere with theory comparison. Within the Popperian framework it is a relatively straightforward affair to compare theories, corroborated or refuted, for content comparison. Science thus grows in an orderly and gradual way. Old facts may fade away when their probation is revoked by the new facts, but they retain their historical links in as much as the new theory must explain the old facts. Observation does not involve any reference to experience in the sense that the occurrence of the experience does not influence at all the decision to accept the basic statement as justified. It is perhaps as a consequence of
PoPuer's hostility towards "psychologism" that any consideration of "cognition" and "meaning" in the acceptance or the justification of a basic statement is absent. The origin of a statement and its meaning are at best interesting problems, but not germane to the problem at hand: how can we best learn from experience?

For Kuhn, observation is theory-laden, but here theory-laden means context-dependent. It involves a reference to prior interpretations that cannot be abstracted from perception to arrive at a sense datum. Prior interpretation forms an essential part of observation. Observation is a kind of "seeing that". The notion of "seeing that" is developed by Hanson. Hanson invites us to look at a well known perceptual puzzle: the rabbit-duck drawing where one sees the drawing either as a duck or as a rabbit. Hanson argues that "the difference in what is seen cannot be due to either differing visual images, or to any interpretation superimposed on the sensation". Hanson concludes that "seeing is a theory laden undertaking. Observation of X is shaped by prior knowledge of X. This sense of seeing is seeing that".³ "Seeing that" also involves being able to give further information about what it is "that" we see. It involves a linguistic component "without which nothing we observe could have relevance to our knowledge, even though there is nothing linguistic in what forms in the eye or the mind's eye".⁴ Hanson argues further that facts are not
observable or picturable entities but are stated in a language and they are 'language-relative'. As a consequence, in physics at least, conceptual differences or differences in the meaning of terms employed in the language may facilitate or hinder the ability to determine or grasp certain facts. In particular in physics the ability to see the facts often depends on the concepts employed in one's language. The meaning of a term, Hanson argues, "is a function of the conceptual pattern it enters into, although not all theoretical terms need enter into a pattern". That will depend on the context, but if a term carries an explanatory burden in a given context, it enters into a conceptual pattern in that context. Physical theories provide patterns within which data appears intelligible: they constitute a "conceptual Gestalt".

A theory is not pieced together from observed phenomena, it is rather what makes it possible to observe phenomena as being of a certain kind and as related to other phenomena. Theories put phenomena into systems.

This sense of "seeing that" puts an emphasis on observation that is absent in both the philosophies of science of Popper and the Logical Positivists. It emphasises that observation is theory laden. A theory in a sense guides observation into a conceptual Gestalt. Observation in the sense of "seeing that" carries with it a sense of interpretation which will be fulfilled as the theory becomes more articulated. Theoretical terms which enter into this pattern
acquire traces of this conceptual pattern. Facts associated with the theory are linguistically formulated and will also carry conceptual traces. Consequently, theories articulated along different conceptual patterns cannot be readily compared for factual content. Facts and theoretical terms are not as a matter of course severed from their conceptual context. As a consequence scientific growth need not be accumulative in the Popperian sense or in the sense of logical positivism.

Within Kuhn's philosophy the notion of "gestalt" or "conceptual pattern" finds its culmination in the notion of "paradigm". In its bare bones a paradigm appeals to a notion of "regularities (which) were consequences of immediate experience...when seen through the paradigm of which these conceptions are a part",7 Experience does not confront a paradigm in the same manner as a sentence confronts experience in logical positivism or Popper. A paradigm forges, as it were, a certain conceptualization on experience and the "regularities" which a particular paradigm discloses are consequences of "the perceptual features that a paradigm so highlights that they surrender their regularities almost upon inspection".8 Science grows by changing paradigms, but this is not an orderly affair. When the scientist adopts a new paradigm he undergoes a shift in expectations pertaining to the conceptualized experiences in the old paradigm. Experiences that seemed irrelevant now seem surprising and demand
explanation. The concept of paradigm brings together two aspects of behaviour of the scientist that in Kuhn's view cannot be separated: the questions which he asks and the criteria he sets up to judge whether or not a given explanation is reasonable. The notion of paradigm so conceived is intended as a critical tool against some of the basic assumptions within the Popperian and the logical positivists' approach. Kuhn rejects the "sentence-by-sentence" approach to science. A paradigm is a holistic notion. It constitutes the basic unit of science. The acceptance of a new paradigm does not come about through an appeal to a crucial experiment. Given the conceptual differences it would not be clear what would be tested and how to evaluate the data. New paradigms do not so much invalidate old facts as render them obsolete. Kuhn specifically rejects the division of science into the context of justification and the context of discovery. He does this by arguing that the manner in which we come to formulate scientific questions is not independent of the manner by which we solve them and judge them adequate. Kuhn arrives at his criticism of both philosophies of science through a detailed historical analysis. Clearly he rejects notions of rational reconstruction as both adequate explanations of science and of the growth of science. Since the notion of paradigm carries the burden as a tool of criticism and as a vehicle for expressing Kuhn's own views, it, unfortunately, soon becomes too inclusive. Virtually all
commentators complain about the heterogeneous factors that are included in the notion of paradigm. In an otherwise sympathetic article, Masterman in "The nature of a paradigm" discerns twenty one different usages. She divides them into three main groups: First, metaphysical or meta paradigms, where paradigm is associated with "something which determines a large area of reality", an "organizing principle governing perception itself". Second, sociological paradigms, where paradigm is associated with "a concrete scientific achievement" and "a set of political institutions". Third, "artifact or construct paradigm", where paradigm is associated with "supplying tools", as "actual instrumentation". In the following pages I shall outline Kuhn's thought primarily from the point of view of "meta paradigm". Primarily, because it is in this sphere that Kuhn specifically criticises Popper and the Logical Positivists and advances his own views. It is in this sphere also that Kuhn rejects the division of science into two contexts and argues for the inclusion of a sociological point of view. Secondarily I shall be concerned with the more specific issues which Kuhn sees as of sociological import. To conclude, I shall argue that despite the interesting counterproposals Kuhn makes, his own views are too wrought with ambiguity to adopt as a coherent philosophy of science.

Kuhn sets out to describe science in close connection with the actual history of science rather than "from a study
of finished scientific achievement as these are recorded in the classics". Close inspection of the historical records of the research activity itself reveals that it is difficult to answer questions such as: "When was oxygen discovered, who conceived first of the idea of energy conservation?" Simultaneously, Kuhn argues, historians confront growing difficulties in "distinguishing the "scientific" component of past observation and belief from what their predecessors had readily labelled "error" and "superstition". The historian faces a dilemma. If he regards the out of data beliefs as myths, "then myths can be produced by the same sort of methods and held for the same sort of reasons that now lead to scientific knowledge". If, on the other hand they are to be called science, "then science has included bodies of belief quite incompatible with the theories we hold today". Kuhn argues that the historian must make the latter choice, but this choice "makes it difficult to see scientific development as a process of accretion." Lately, historians of science have attempted "to display the historical integrity of that science in its own time". A study of science from this perspective will bring out that "methodological directives by themselves are not sufficient to dictate a unique substantive conclusion to many sorts of scientific questions". It brings out that "what differentiates different school is not a failure of method, but incommensurable ways of seeing the world and of practising
Observation and experience restricts the range of admissible scientific belief. The restriction of this range accounts, according to Kuhn, for the efficiency and the direction in which the research activity proceeds at any given time. The efficiency and the direction of such research finds its culmination in "normal science". Normal science is characterized in the way observation and experience are restricted. There is a commitment to certain beliefs of seeing that—-a commitment to certain ways of seeing the world. This involves, according to Kuhn, a certain degree of arbitrariness but this arbitrariness ensures that novelty will not be suppressed for long. This novelty comes in the form of an "anomaly". An anomaly precipitates a crisis. The old way of looking at the world will be felt as too restrictive. The restricted view under which normal science had been conducted, will be re-examined and will culminate in a new way of "seeing that"—-a new paradigm.

The history of science does not in Kuhn's view, vindicate, a picture of accumulative growth either according to Popper or according to the logical positivists. Rather, science progresses during the period of normal science where the scientist adopts a somewhat restricted view of what is to count as scientific. This enables him however to surge ahead with the problems at hand. During this period the scientist does not aim at conceptual novelty but provides solutions for existing puzzles. The adoption of the paradigm
more or less ensures that there are solutions to these puzzles. Only the scientist's own lack of ingenuity could prevent him from actually solving the puzzle. Normal science, however, may culminate in a feeling that something is amiss. There may be an awareness of an anomaly or of a problem which resists solution. A period of crisis sets in and new proposals are entertained but not accepted until a new paradigm is accepted as such. Rather than gradual growth we have a kind of seriality. Science is discontinuous because different paradigms will restrict "observation and experience" differently.

A paradigm is largely a promise of success discoverable in selected examples. A given speciality has at a given time "a set of recurrent and quasi standard illustrations of theories in their conceptual, observational and instrumental applications." The members of the corresponding community learn their trade by studying them and practising them. A paradigm functions by telling a scientist about the entities that nature does and does not contain and the ways in which these entities behave and it prepares the scientist to accept these fundamental ontological categories. A paradigm carries with it "standards governing permissible problems, concepts and explanations."

Throughout the "Structure of Scientific Revolutions", Kuhn invokes "nature" in such a fashion that paradigms may "fit" or "clash" or "reveal". One of the foci of normal
science under a paradigm is "that class of facts that the
paradigm has shown to be particularly revealing of the
nature of things". Scientific discovery starts with the
awareness of an anomaly, "with the recognition that nature
has somehow violated the paradigm-induced expectations that
govern normal science". Concerning paradigm change, Kuhn
argues,

...the decision to reject a paradigm is
always simultaneously the decision to accept
another and the judgment leading to that
decision involves the comparison of both
paradigms with nature and with each other.

An anomaly, Kuhn writes, "can emerge only to the extent that
(the scientist's) anticipation about nature and his instru-
ments are wrong". In order to exemplify that paradigms
are, in a sense, constitutive of nature, Kuhn argues that
when scientists look at the same sort of thing under differ-
ent paradigms they really see different things.

Regularities that would not have existed for
an Aristotelian (and that are in effect
nowhere precisely exemplified in nature) were
consequences of immediate experiences for the
men who saw the swinging stone as Galileo did...
when seen through the paradigm of which these
conceptions are a part. The falling stone,
like the pendulum exhibited its governing laws
almost on inspection.

By "immediate experience" is meant "the perceptual features
that a paradigm so highlights that they surrender their
regularities almost on inspection". In a sense then a
paradigm forges perception into an awareness of immediate
inspectable regularities where a previous paradigm revealed
no such regularities. Part of the notion of paradigm includes uninterpreted theory and techniques and ways of measurement. A paradigm is a promise that the initial immediately accepted regularities can be extended through puzzle solving to other phenomena and problems in the field. It constitutes a promise that normal science will lead to the explication and formulation of new theories, techniques and ways of measurement. The most compelling question is "which paradigm should in the future guide research on problems which neither competitor can yet claim to resolve completely?"  

The initial conception of a paradigm is in a sense about nature and displays this in immediate inspectable regularities. These serve as exemplars, that is, examples by which the student can learn to solve problems. He learns to see a particular problem "as like a problem he has already encountered." Having grasped the analogy between two or more distinct problems, he can interrelate symbols and "attach them to nature in ways that have proved effective before". These analogies will induce the student to learn "a similarity relationship". But we should not ask "similar with respect to what". That request, according to Kuhn, is a request for a rule and Kuhn argues "that the temptation to seek criteria (or at least a full set) should be resisted". As a result the decision to accept a new paradigm cannot proceed by piecemeal comparison of concepts, statements and theories. In part it must be based on faith; the scientists
that accept it "must have faith that the new paradigm will succeed with the many large problems that confront it knowing only that the older one has failed a few."30 This is not to suggest however that new paradigms triumph ultimately through some mystical aesthetic, Kuhn argues but only that there is no single argument that can or should persuade all scientists. "What occurs is an increasing shift in group allegiances."31

The success of a paradigm is in the beginning only a promise. Normal science consists in the actualization of that promise. This is achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the predictions of the paradigm and by further articulation of the paradigm itself. Kuhn refers to this type of work as "mopping up": "an attempt to force nature into the preformed and relatively inflexible box that the paradigm provides".32 Normal science, as Kuhn has it,

...possesses a built in mechanism that ensures the relaxation of the restrictions that bound research whenever the paradigm from which they derive ceases to function effectively.33

There are three foci for scientific investigation in normal science. One focus consists of that class of facts that the paradigm has shown to be particularly revealing of the nature of things. The second focus consists of a smaller class of facts that can be directly compared with the predictions of
the paradigm theory. The third focus consists of empirical work undertaken to articulate the paradigm theory resolving some of its ambiguities and permitting the solutions of problems to which it previously had only drawn attention. Theoretical work in normal science consists simply in the usage of existing theory to predict factual information of intrinsic value: ...these problems of application account for what is probably themost brilliant and consuming scientific work of the eighteenth century." Another set of theoretical problems aims at "clarification by reformulation", "reformulations of a paradigm occur repeatedly in all of the sciences".

The most striking feature of normal science is that "it does not aim to produce major novelties conceptual or phenomenal". Even when normal science is concerned with paradigm articulation, and reformulation it does not aim at unexpected novelty. With the adoption of a paradigm the scientific community acquires "a criterion for choosing problems that can be assured to have solutions". One of the reasons why normal science seems to progress so rapidly is "its practitioners concentrate on problems that only their own lack of ingenuity should keep them from solving. Although a problem may have an assured solution, "there must be rules that limit the nature of acceptable solutions". These rules all derive from paradigms, "but paradigms can guide research even in the absence of rules". Kuhn extends
the notion of "game", originally introduced by Wittgenstein, to apply to "various research problems and techniques that arise within a single normal scientific tradition". The notion of "game" and "family resemblance" are introduced to make the point that there may not be a single set of attributes common to all games; rather, we apply the term "game" when we recognize an activity which resembles sufficiently, i.e. has a family resemblance with, activities which we call "games". Research problems and techniques within a normal science tradition do not have in common "some fully discoverable set of rules and assumptions". They may relate instead by resemblances and by modelling to parts of the scientific corpus which the scientific community recognizes as among its established achievements.

Although normal science is eminently successful in the accumulation of facts, it does not aim at conceptual novelty. Yet, new phenomena are discovered and radically new theories have been invented by scientists. The history of science suggests, Kuhn argues, "that the scientific enterprise has developed a uniquely powerful technique for producing surprises of this sort" A surprise of that sort comes in the form of an "anomaly". The role of an anomaly is to precipitate a crisis in the belief of the efficacy of the current paradigm and the associated belief that there exists a solution to the puzzles of normal science. The awareness of an anomaly starts "with the recognition that nature has somehow
violated the paradigm induced expectations that govern normal science". An anomaly is the sort of fact that demands more than an adjustment of the theory. It prepares the way for a genuine discovery. Kuhn argues that "discovery is a process and must take time", because, "both observation and conceptualization, fact and assimilation to theory, are inseparably linked in discovery". An anomaly loosens, as it were, the conceptual grip a paradigm holds. The conceptual categories which the paradigm prepared through restriction of observation and experience, are seen to be inadequate to explicate the anomaly. The anomaly demands a reconceptualization of experience. Genuine discovery is of this sort. An anomaly is not a counterinstance and does not falsify a paradigm. A decision to reject one paradigm is simultaneous a decision to accept another. This decision "involves the comparison of both paradigms with nature and with each other". At first however, the anomaly initiates theory proliferation and ad-hoc modifications designed to fit the anomaly in the existing paradigm. But as the anomaly resists solutions of this sort a crisis of confidence sets in, precipitating the conditions for the acceptance of genuine novelty which demands reconceptualization. This culminates in the acceptance of a new paradigm which holds the promise for a solution of the anomaly and which will guide research in the future. But the normal science tradition that emerges from a scientific revolution "is not only incompatible, but often actually incommensurable
with what has gone on before". \textsuperscript{47} "Successive paradigms", Kuhn argues, "tell us different things about the population of the universe and about that population's behaviour". \textsuperscript{48} Since paradigms provide both "the source of the methods of the problem fields" and "the standards of solutions", as a consequence,

to the extent that two scientific schools disagree about what is a solution they will inevitably talk through each other when debating the relative merits of their respective paradigms. \textsuperscript{49}

The paradigm has both a cognitive and a normative function. It provides the conceptual boxes normal science will fill in and it provides the standards and rules for the acceptance of the puzzles which will fill the conceptual boxes. This conception of paradigm with its associated emphasis on cognition, is close to the empiricist interpretation of experience (observation) as providing the source and the justification for the acceptance of knowledge claims. Even the language with which Kuhn employs is reminiscent of the language of the logical positivist: "regularities" are consequences of "immediate experience". To be sure, Kuhn argues that we need not be concerned with this "immediate experience". Nevertheless, a paradigm forges experience and observation so that we observe regularities where previously we would observe none. Moreover, even though we should not ask for a complete set, rules for acceptance and rejection will be provided as the paradigm becomes articulated. Clearly, a paradigm by restricting "experience and observation" and by
providing rules for acceptance, can be seen as both a source of and a justification of, knowledge claims. Kuhn however, introduces the notion of "conceptual". This notion is appealed to argue that observation, or seeing that, draws on a prior notion of "gestalt" or "conceptual pattern". Theories also are infected by this notion of "gestalt". A paradigm makes it possible to observe phenomena as being of a certain sort. A paradigm can never be just a series of propositions each one of which expresses a fact. Scientific growth is not reducable to "paradigm reduction". Facts cannot readily be separated from their paradigmatic context. Kuhn also explicitly denies that rules i.e. correspondence rules could be extricated from a paradigm to guide research independently of a paradigm. In fact Kuhn actively resists demands for the explication of rules. The notion of paradigm, then, serves specifically to argue against correspondence rules, against theory reduction and against an interpretation of observation as theory-independent. Furthermore the notion of paradigm militates against a sentence-by-sentence approach to science. The meaning of a theoretical term becomes context dependent and changes radically with the adoption of a new paradigm.

The notion of paradigm also runs counter to Popperian philosophy. Paradigm as both a source for and providing justification of the acceptance of knowledge claims, has clear overtones of psychologism. Kuhn also argues against falsification on the grounds that scientists do not abandon
their theories after one falsification. Despite superficial similarities, an anomaly is not to be construed as a falsifying instance. An anomaly carries with it a sense in which it defies solution within the particular paradigm that generated it. An anomaly demands reconceptualization and it precipitates genuine discovery which takes time. Although observation does not, in Kuhn, appeal to feelings of certainty, it appeals to a sense of "gestalt" that has as a consequence that observation is context dependent and that meaning acquires an importance it does not have in Popper's philosophy. For Popper, it is of crucial importance that observation remains theory neutral. The notion of experiment in Popper depends on a few, in Popper's view, innocuous conventions. These conventions are brought in to play where the experimenter has to decide when a basic statement is to be accepted as basic and to separate a theory under test from background knowledge. It is not clear however, what sense can be attached to background knowledge and theory under test within a paradigm. The initial solved problems, or exemplars, serve as example for problem solving. This is accomplished through "similarity", or "resemblance" relations. It is through these "resemblance relations" that the initial sense of "conceptual reinterpretation" and the sense of regularities that are consequences of "immediate experience" retains their integrity. As a consequence it would appear that theoretical and empirical aspects of the paradigm are all permeated by
this initial conceptual aspect. Since observation too is conceptually impregnated, it is not clear whether falsification has any purchase at all. The role of experiment seems confined to some form of verification. The very idea of normal science where the scientist specifically avoids novelty is anathema to Popper, who has emphasized a risk taking strategy.

Undoubtedly the main point of discontent is Kuhn's denial that there are paradigm independent methodological rules that can be appealed to choose rationally between paradigms. This denial undoubtedly stems from Kuhn's rejection of the division of science into the context of discovery and the context of justification. The context of justification is solely concerned with the logical relationship between statements accepted as scientific. Scientific growth is seen as the study of the logical relationships between these statements and it is argued that this is an adequate picture of science. It is at the point where scientists accept radical new theories that Kuhn argues that the historical record indicates substantial disagreement with Popper's normative model of scientific growth. Scientists, according to Kuhn, do not abandon their paradigm unless a new one has been found and this can only occur when a crisis undermines their confidence in the old paradigm. At that point it is realized that the rules of the paradigm are inadequate. But until a new paradigm has been found,
scientists will conduct research according to the old rules. A new paradigm is adopted largely on the promise that it will guide future research and solve the problems which actually confront the paradigm. There is thus a period in which scientists will work with theories even though these are not justified in the sense Popper requires. At the point where an old paradigm becomes ineffective and a new paradigm is proposed, logic, experiment and methodological rules become ineffective. At such points the disagreement is not about facts qua facts, but about the different interpretations they support. Although there is little doubt that Popper's contention that observation is always from a theoretical point of view has contributed to our understanding of what is involved in the "empirical base" of science, his rejection of "experience" as a ground for the acceptance of a basic statement, has forced him into a certain type of conventionalism. Kuhn argues that not only theoretical considerations but also conceptual considerations are of importance in observation and the role it plays in science. The manner in which paradigm and experience are connected and the manner in which paradigm operates both as a cognitive and a methodological norm, puts experience back into a supportive role in epistemology. The question is, can the notion of paradigm vindicate this new found emphasis?

When one tries to get a grip on the notion of paradigm, one is immediately beset by difficulties. Not only does it have at least twenty one different usages which
can be subdivided into three distinct classes, one is also not sure whether the whole of science is governed at one time by a specific paradigm, or whether particular disciplines are at specific times governed by a particular paradigm. But the main source of difficulty is that at strategically important places, Kuhn asserts that something is the case rather than presenting an argument. At various occasions we are told that paradigms "fit" or "clash" with nature, provide us with "conceptual boxes", tell us "different things about the universe" and so on. Nature, presumably as coincidence would have it, also plays a role independent of a paradigm: "awareness of an anomaly begins with the recognition that nature has somehow violated the paradigm induced expectations that govern normal science". But a paradigm is also about practitioners: "a paradigm governs, in the first instance, a group of practitioners". But now we have a crucial ambiguity: either a paradigm governs a group of practitioners or it governs a subject matter. If it governs a subject matter then nature may well exert its influence, even though more is needed in the way of an argument to justify this influence. If a paradigm governs, in the first instance a group of practitioners then talk about nature is incidental and it would be coincidental if each time a paradigm is changed this would be due to nature exerting its influence. The notion of paradigm is loose enough to accommodate a notion of normal research that could go on indefinitely. A paradigm, Kuhn
tells us never solves all the problems with which it is confronted. Problems which cannot be solved immediately may be set aside for later. Most anomalies, Kuhn argues in the Postscript, are resolved by normal means anyway. If it is only scientific opinion that has the decisive influence, then research could go on indefinitely. Kuhn rejects demands to provide us with an explicit set of rules to evaluate the notion of the "similarity" or "resemblance" relation prior to the research by which the "immediate perceptible regularities" of the initial solved problems are extended to other problems and situations in the paradigm. If these rules evolve as normal science progresses, as Kuhn indeed suggests, it is not obvious that the integrity of these initial "regularities" is maintained and that scientific opinion cannot be beguiled into perceiving regularities where in fact there are none. It is indeed only an appeal to nature which could prevent this from becoming the case. But it is not clear whether these criticisms, cogent as I think they are, accomplish anything. This is because it is not clear whether we are presented with a philosophy of science in which the notions of paradigm, anomaly, and so on, are an integral part, or whether we are presented with a view of perception in which these same notions are embellishments.

In his Postscript to the "The STructure of Scientific Revolutions", Kuhn elaborates upon his views of perception. Kuhn argues that past experience is embodied in
the neutral apparatus that transforms stimuli to sensations. It is this past experience that gives perception its integrity. An appropriately programmed perceptual mechanism has survival value, Kuhn argues. Few ways of seeing withstand the test of group use and are worth transmitting from generation to generation. Because these ways of seeing have been selected for their 'success' over historic time "we must speak of the experience and the knowledge of nature (my emphasis) embedded in the stimulus-response route". Perhaps knowledge is the wrong word, Kuhn argues, but it shares with (real) knowledge the following characteristics:

...it has been transmitted through education, it has, by trial, been found more effective than its historical competitors in a group's current environment; and, finally, it is subject to change both through further education and through misfits with the environment.\(^{53}\)

When Kuhn discusses the anomalous card experiment, the normal series of cards are referred to as "conceptual categories prepared by prior experience".\(^{54}\) Surely ordinary language will in the first instance provide conceptual categories consistent with the "tacit knowledge" and the environment as it is passed on from generation to generation. We can think of ordinary language as providing the categories by which we can through observation, locate things and events. Such categories acquire their meaning by implying that certain statements will be found to apply to that which has been observed to be the case. The notion of "world-view" will find its application in the first instance in ordinary
language. The question then becomes whether we should regard an emphasis on the "conceptual" as a pervasive condition underlying also artificial languages applying to restricted environments, or whether this condition is an incidental one. If we regard the condition as an incidental one then our position will be similar to that of Popper's. We can break-out of our frameworks and compare them. This is not Kuhn's intent. If we regard the condition as pervasive then there is a sense in which ordinary cultural influences will remain part of one's (scientific) world-view. In that case a paradigm cannot serve as a demarcation criterion.

Conclusion

Kuhn approaches science primarily from a historical point of view and argues that there are certain periods in science when there is relative peace and quiet and periods which we can liken to revolutions. The quiet periods are periods of normal science. During these periods scientists do not aim at novelty and are relatively sure that a solution for their puzzles exists. It is a period of fact accumulation and theory-articulation. These periods are followed by periods of crises. Normal science is faced with problems it cannot solve and the confidence in the ability of normal science to provide solutions for the puzzles it confronts is shaken. Eventually a new period of normal science research returns and again there is confidence in the puzzle
solving ability of the new tradition. Kuhn argues that neither the rational reconstruction of science prescribed by logical positivism nor by Popper can account for this puzzling phenomenon. Kuhn introduces the notion of paradigm to explain these interruptions in the growth of science.

The striking characteristic of the resolution of a crises is that scientists adopt a new paradigm which has solved only a few pressing problems. It faces many problems for which there is only a hope of a solution. Moreover, scientists will continue to work with the paradigm even though initially it may be refuted by experimental evidence. Eventually a paradigm will solve a good many problems with which it is confronted; some however will never be solved and in due course will be regarded as insignificant. Historical examples of theories which, when formulated, faced considerable obstacles include those of Copernicus, Newton and Einstein. All these theories were contradicted by the best available experimental data of their times. The nature of the accept­ance of such radical new theories involves a radical new interpretation of the known facts. The notion of paradigm is advanced to solve these problems, through an appeal to the veracity of the conceptual aspect of paradigm. "Paradigm" involves an appeal to the conceptualization of experience with which the scientists confront the scientific world. Adoption of a new paradigm involves a reconceptualization of that experience and of the scientific world. The notion of
paradigm also serves to argue against a theory-neutral or theory-independent observation language. Facts are not only theory-impregnated, they are also conceptually impregnated. Since methodological rules, according to Kuhn, are paradigm dependent, we cannot reconstruct science along Popperian lines.

Although an appeal to the notion of paradigm may well help to explain what is involved when scientists adopt radical new theories which seem to demand new interpretations of "experience", "experimental evidence", "facts" and so on, this is not the sole intent of Kuhn. Kuhn also argues that "paradigm" can serve as a demarcation criterion, even though this involves a circularity: "A paradigm is what members of a scientific community share, and, conversely a scientific community consists of men who share a paradigm". Further vicious circularities arise when it transpires that a paradigm is about nature and also in the first instance about scientific communities. There is furthermore, a disturbing tendency to appeal to mechanisms and techniques where reasoned arguments would be more appropriate. Although one may well agree with Kuhn about the historical variability of methodological rules and with the view that facts are also conceptually impregnated, Kuhn's attempts to locate the source of the historical variability and the source of the conceptual import solely within the scientific community is ill-conceived. Nowhere becomes the artificiality of demarcation
criteria more apparent than when Kuhn attempts to argue for a privileged interpretation for the notion of anomaly in a small area of the scientist's experience, so that, just for that small area we should accept the analogy that scientists who hold different theories with regard to their conceptualized experience of that small area, are like "the members of different language culture communities".\textsuperscript{56}

In the next chapter I shall discuss Lakatos' attempt to accommodate Kuhn's criticism within the Popperian framework.
CHAPTER FOUR

THE RESEARCH PROGRAMME OF IMRE LAKATOS

In Popper's view science is essentially a superior form of knowledge. To support this claim Popper seeks to demarcate science from non-science. On his view science consists essentially of a series of basic statements each one of which has the same logical status: each one obeys the falsification criterion. To demarcate science from non-science and to argue that science is essentially a series of "units" each one of which has the same logical status, also involves having to adopt a strategy to deal with counter-instances to the accepted empirical base of science. A counter-instance threatens the accepted base of science and must be accommodated in order that the empirical base continues to consist of such "units". Popper may be regarded to all intents and purposes as an empiricist because his demarcation criterion refers to "testing by experience" and because experimental verdicts play a strong role in his philosophy.

It has been argued in Chapter Two that the Popperian position requires a certain 'innocuous conventionalism'. The very idea of having to accept a convention within the realm demarcated as science, however, detracts from the notion that
science can be demarcated from non-science.

For Popper the most important aspect of science is its growth. The superiority of science, qua knowledge claim, does not lie in an appeal to a proven empirical bedrock. Indeed, it is one of Popper's most significant contributions to argue that this alleged "bedrock" is essentially theory-impregnated. In a sense the empirical base remains on probation—a status which can be revoked when new theories are adopted. What is important is to be clear about the criteria of justification of the acceptance of basis statements. The superiority of science qua knowledge claim lies in the growth of science: the growth of science is rational because the acceptance of different theories can be shown to proceed according to certain supra-historical methodological rules. The acceptance of new theories is unsullied by cultural or socio-psychological factors. The exhibition of the rationality in the growth of science takes place in the context of justification. Here, only statements accepted as scientific penetrate and the questions asked pertain solely to the logical relationships between these statements. Scientific growth is essentially a series of changes in the content of these statements. We remain in the same framework throughout the course of science. The restriction of the growth of science to the context of justification constitutes a proposal that the growth of science may be confined to a study of the logical relationships between
statements and that, as a consequence, the rationality derived from this study is independent of cultural influences.

Kuhn has challenged this position. He has argued that logic and methodological directives are insufficient by themselves to dictate a unique conclusion to many scientific questions. Kuhn denies the legitimacy of the division of science into the two contexts. He reintroduces within the philosophy of science the question of the origin of a scientific claim and the manner in which it is justified. He argues that the manner in which the questions are asked is not independent of the rules of evidence which are formulated to accept the solutions to those questions. By arguing furthermore that the notion of paradigm draws on a "conceptual Gestalt", or "world-view", Kuhn introduces cultural and psychosociological factors within the sphere of the justification of science. Kuhn appears confident that such factors can be explained solely from within the scientific communities and that an appeal to these factors need not be a deterrent to demarcate science from non-science. Kuhn's denial that theory choice in science proceeds upon Popperian lines brings back into focus the justification of the notion of theory. In the introduction it was argued that the increasing theoretical nature of science made it difficult to justify science on pure empiricist grounds. Theory plays an important role in scientific knowledge, but it cannot be derived from experience. The logical Positivists "dissolved"
this problem by arguing that theory is parasitic on observation. Theory in their view could be reduced to observation statements. Popper rejects this solution and argues that observation is theory-impregnated. Thus the notion of theory is reintroduced in the justification of science. Popper proposes that we can still demarcate science from non-science and regard science as empirical if we can reconstruct the growth of science in the context of justification. The importance of theory can then be admitted and at the same time the acceptance of particular theories can be justified in logical terms.

Popper's proposal is an important attempt to come to an adequate understanding of "theory" without taking recourse to (non-innocuous) conventionalism or to (synthetic) a-priorism. Popper however relies on a notion of "testing by experience" where experience remains unaffected by the theoretical innovations in science. Kuhn denies this. He argues that experience or "testing by experience" itself is paradigm-dependent and is already conceptualized through the adoption of a (scientific) world-view before tests are undertaken. By appealing to a notion of a (scientific) world-view and a notion of tacit (inherited) knowledge, shaped and selected by the environment and embedded in the neurological system, Kuhn brings the notion of theory to the nether realm of the historical-cultural and the socio-psychological.
Lakatos, in the "Methodology of scientific research programmes", attempts to formulate a revised notion of rational reconstruction which aims to stay within the Popperian framework and which aims simultaneously to answer Kuhn's criticism. Lakatos begins by enumerating the decisions a scientist working within the Popperian framework has to make. First, the scientist has to decide when to count a basic statement as "basic". Second, the scientist has to decide which basic statements can be regarded as unproblematic background knowledge. Third, the scientist when he is working with probabilistic theories, must specify rejection rules which render statistically interpreted evidence inconsistent with the probabilistic theory. Fourth, when the scientist tests a theory, he tests it in conjunction with a ceterus paribus clause. A ceterus paribus clause "holds all other circumstances equal". When a falsification obtains, the scientist must decide whether to reject the theory or to re-evaluate the ceterus paribus clause. Furthermore, Lakatos argues, the notion of "crucial experiment" within the Popperian framework is dissonant with the actual history of science. A crucial experiment is a "two cornered fight between theory and experiment". The only interesting outcome is "conclusive falsification". History of science suggests, Lakatos argues, that "tests are at least three cornered fights between rival theories and experiment". Furthermore "some of the most interesting experiments result, prima
facie, in confirmation rather than falsification."² Lakatos proposes sophisticated methodological falsificationism (SMF) as an elaboration on and improvement of naive methodological falsificationism (NMF). SMF differs from NMF in the following manner:

(1) A theory is acceptable or scientific only if it has corroborated excess empirical content over its predecessors. That is, only if it leads to the discovery of novel facts. There are two clauses:
a) the theory has excess empirical content. This can be checked by prior logical analysis.
b) some of this excess content is verified. This can be checked only empirically and may take an indefinite time,³ (my emphasis).

(2) A theory is falsified if and only if another theory T' has been proposed with the following characteristics:
a) T' has excess empirical content over T. That is, it predicts novel facts; facts improbable or even forbidden by T.
b) T' explains the previous success of T. That is, all the refuted content of T is included (within the limits of observational error) in the content of T'.
c) some of the excess content of T' is corroborated.⁴ Requirement 2(a) is leveled against the "conventionalist stratagem" of devising auxiliary, ad hoc, hypotheses. The
point Lakatos makes in answer to this, is that "any scientific theory has to be appraised together with its auxiliary hypothesis, initial conditions, etc. and especially with its predecessors, so that we may see by what sort of change it was brought about. Then, of course what we appraise is a series of theories rather than an isolated theory."  

Lakatos then distinguishes between progressive and degenerative problem shifts. A problem shift is progressive if it is both theoretically and empirically progressive if it predicts novel facts and some of these novel facts have been confirmed. A problem shift is degenerative if it is neither theoretically nor empirically progressive. We may accept a problem shift as "scientific" if it is (only) theoretically progressive. The distinction between progressive problem shifts and degenerative problem shifts enables us to appraise progress in scientific explanation. If we reinterpret a theory and a counter example in such a manner that no novel facts are explained with it, then it is only a linguistic reinterpretation. Insistence on a new theory T' before T can be said to be falsified forces us to re-evaluate the relationship between a theory and its empirical basis. "Falsification is not simply a relation between a theory and its empirical basis, but a multiple relation between competing theories".  

Lakatos also re-evaluates the concept of counterevidence: "Counterevidence to T(1) is a corroborating instance to T2 which is either inconsistent
with or independent of T1. Consequently, Lakatos writes, this shows that "crucial counterevidence" or "crucial experiments" can be recognized as such among the scores of anomalies only with hindsight, in the light of some supersed-ing theory.⁸

Lakatos then mitigates the conventional aspect of the decision as to which statement to count as basic and which basic statements to regard as "unproblematic background knowledge" by arguing that "whether a proposition is a "fact" or a "theory" in the context of a test situation depends upon our methodological decision. The "empirical basis of a theory", Lakatos writes "is a mono-theoretical deductive structure". We may use it as a first approximation, but in case of "an appeal by a theoretician" we must use a pluralistic model. In a pluralistic model the clash is not between theories and facts", but between two high level theories. There is an interpretative theory which provides the facts and an explanatory theory which explains the facts. These theories may be "out of phase" and the problem is which theory to consider as the interpretative one and which as the explanatory theory. The "appeal procedure" is not final and only postpones the final decision. It would appear that this is Lakatos way of agreeing that the Popperian notion of falsification is actually a weak one. This can be overcome by looking at the empirical base as a series of theories. Rather than rejecting a theory when it is falsified we can
now argue that either the interpretative theory or the explanatory theory is "out of phase" and we can re-evaluate the whole situation and try again. Even so, in the end we must make a decision nonetheless. We can try to reduce the conventional element "but we cannot possibly eliminate it".9 We may notice furthermore that "falsification" has undergone a substantial change. Originally a falsification meant that the theory had to be rejected. Now Lakatos requires that a new theory be found first and that the theory has excess content over the previous theory.

Lakatos then introduces notion of "research programme". Scientific research programmes consist in part of methodological rules. Some tell us what paths to avoid, the "negative heuristic" of the programme; some tell us which paths to pursue by way of hints and suggestions, the "positive heuristic". A research programme has a "hard core". It is not clear what exactly a "hard core" is. The actual hardcore of a programme", Lakatos argues, "develops slowly by a long preliminary process of trial and error. In this paper this process is not discussed."10 The example Lakatos gives of a research programme is one which adopted "Cartesian metaphysics". This programme adopted the mechanistic theory of the universe according to which the universe is a huge clockwork with push as the only motion. This view functioned as a "powerful heuristic principle". It discouraged work on scientific theories such as the "essentialist" version of Newton's
theory of action at a distance. It encouraged work on auxiliary hypotheses explaining apparent counterevidence to the whole programme. The latter aspect is the "positive" heuristic, the former is the "negative" heuristic of the programme. An actual example of an already formulated "hard core" is "Newton's three laws of dynamics and his law of gravitation". This hard core is held irrefutable by decision of its protagonists. To strengthen the hard core scientists must use their imagination in developing a "protective belt". It is this protective belt that bears "the brunt of the tests". The protective belt consists first of auxiliary hypotheses which become in due course articulated into theories. The protective belt is guided by the positive heuristic, "a set of partially articulated suggestions or hints on how to change, develop the "refutable" variants of the research-programme, how to modify, "sophisticate" the "refutable" protective belt".

The positive heuristic, Lakatos argues, "sets out a programme which lists a chain of ever more complicated models simulating reality, following instructions which are laid down in the positive part of his programme". These guesses articulations and model building activities eventually result in the articulation of theories within the protective belt. A research programme which has gotten off the ground, has a relatively well-articulated hard core. This hard core is held to be irrefutable by the decision of the scientists involved. Rather they direct their attention to the articu-
lation of the protective belt. "It is this protective belt" Lakatos argues, "that gets adjusted, and readjusted, or even entirely replaced, to defend the thus hardened core."\textsuperscript{14} (emphasis added). This, in my view, does not follow at all unless one wants to adopt some form of inductivism. Although particular facts do not support the hard core but theories, still it requires a sense of inductivism to argue that the protective belt hardens the core.

The whole notion of protective belt seems a bit like the normal science tradition. There are rules of the game, ways of doing research, model building according to instructions which are part of the programme. It is designed to give the scientist considerable freedom on how to interpret negative experimental results. We may be frustrated "by a long series of refutations". In fact, refutations have become irrelevant. "Their existence is fully expected", Lakatos argues, "the positive heuristic is there as a strategy for predicting (producing) and digesting them".\textsuperscript{15} Earlier Lakatos has assured us that theoretical scientists have a long term research policy which anticipates "refutations". Rather it is "verification" which keeps the programme going.

New theories in the protective belt are accepted if they show a progressing problem shift. A new theory must have excess empirical content, it must predict novel facts. This requirement, in my view, is significantly altered later: "until now we have assumed that it is immediately
ascertainable whether a new theory predicts a new fact or not. But the novelty of a factual proposition can frequently be seen only after a long period has elapsed. This is because a fact is not merely the consequence of observation, but it must also be explained. A new fact is the result of several new theories. A new fact must be "explained" by an explanatory theory, while it is "delivered" by an interpretative theory. The problem is, as I have suggested earlier, which theory to regard as interpretative and which as explanatory and no doubt this takes times. It may take decades, Lakatos notes. The example which Lakatos adduces to vindicate his claim that the novelty of a fact is not merely observational is Balmer's formula. Balmer merely observed. Bohr predicted the same observational effect but was in due course able to give a theoretical interpretation of Balmer's formula which Balmer himself was unable to do. "Given that Bohr's programme had shown its heuristic power, its hard core would itself have become well corroborated and therefore qualified as an observational or interpretative theory." There are several comments to make. This interpretation of a novel fact makes a mockery out of the requirement of "theoretical problemshift": such a problem-shift must predict "some novel, hitherto unexpected fact". But the novelty can only be ascertained with hindsight after decades of work," it may even take an indefinite time. A novel fact is a novel fact if it is not only discovered
but also explained. "We should ignore", Lakatos argues, "the insolent priority claims of amateur fact collectors".\textsuperscript{19}

In effect, both the requirements of theoretical and empirical problemshift can only be evaluated with hindsight. We can speak of facts only with an articulated protective belt which hardens the hard core, which then, as it turns out, can serve as an interpretative theory, while the protective belt presumably can function as the explanatory theory. To argue that the "demarcation between progressive and degenerating problemshifts sheds new light on the appraisal of scientific or, rather, progressive explanations..." is misleading. It is premature to argue about facts (let alone require them) if only the "hardened" hard core can function as an interpretative theory. Moreover, theory change takes place in the protective belt. It is just in this belt that we may disregard long series of refutations. In this sphere theoreticians have long term strategies for dealing with refutations. Theoreticians may even refuse to be drawn into observation.\textsuperscript{20} It would be better to speak of phenomena which is what Lakatos does, or at least Bohr does, when Lakatos quotes him.\textsuperscript{21}

Lakatos addresses himself to some of the weaker points in the Popperian programme, in particular the notion of "crucial experiment". Lakatos argues that "experiment" must be seen as a three cornered fight between rival theories. He correctly points out that a theory which delivers the
"fact" and the theory which explains the "fact" should be on more or less the same level qua theory. The following considerations are germane: the role of deductive logic and theory neutral observation language. The advantage of deductive logic is that if we know that the premisses are true then the conclusion must be true also. If an experimental verdict shows the conclusion to be false, we know that at least one of the premisses is false. But in order to take advantage of the deductive argument, we must be sure that we are dealing with an observation language which is neutral with respect to the theories which deliver the premisses and the theories which deliver the experimental verdict. Possibly because this assumption has come under criticism by Kuhn, Lakatos introduces his modification which eliminates the question of a theory-neutral observation language. What is to count as an observation-language is methodologically decided upon. In order to make this decision a fair amount must be known about them. An experiment delivers a falsification. Should we regard one of the premisses in the deductive argument as false and reject the associated theory? In Lakatos view we need not, we may regard a theory out of phase and Lakatos regards this as an inconsistency. This is, in my view, because an experiment may well address certain aspects of the same phenomena which either the "explanatory" or the "observational" theory does not address; they may be out of phase in that respect. As a consequence the conclusion in a deductive argument may be false not because one of
the premisses is false, but because they are stated in an observation-language which is not neutral, but inconsistent with respect to either the theories of observation or explanation. Furthermore, as I have already indicated, Lakatos may admit in this circumspect manner that the notion of falsification is a weak one. We do not have to make the decision to reject the theory or make suitable alterations in the background knowledge immediately. We may notice that in the process the empirical base of science has been replaced by a "series of theories" and that what is to count as a "fact" or what is to count as "observational", has been drawn into the methodological realm. But the same problem re­appears for Lakatos on the level of research programmes. Lakatos never tells us what happened when an interestingly testable version of a programme is developed. The most that he can tell us is that the title of crucial experiment can only be awarded with hindsight. Amsterdamski argues that there is a problem here. Suppose we encounter an anomaly. Since Lakatos does not tell us when we may remove the structure of "irrefutability" from the hardcore, the only way we can deal with the anomaly is to introduce a new theory into the protective belt. Suppose that after we have done this the promised empirical content does not materialize. The programme is degenerating and no new theory emerges immediately. Do we abandon the programme, that is, regard it as falsified, or do we keep looking for a new theory? "This is", Amster-
damski argues, "the same difficulty with which the falsificationist model had to deal on the level of theories".  

Lakatos develops his notion of research programme to indicate continuity between the adoption of theories: "It is a succession of theories which is appraised as scientific, the members of such theories are usually connected by a remarkable continuity which welds them into research programmes." But the first thing we notice is that in fact there is little continuity within and between research programmes. There is at the center of the programme a hard core that is held irrefutable be methodological decision. This hard core is surrounded by a protective belt of which Lakatos claims that it hardens the core. Nevertheless there is a disconnection and this disconnection remains throughout the course of the programme. The structure to hold the core irrefutable is responsible for the discontinuity between research programmes. The problem of the discontinuity between programmes may well lie with Lakatos stipulation that falsification on the basis of experiment only is no longer sufficient to reject the theory: we must first find another one. If we apply this rule to level of research programmes we find that in fact we cannot falsify the programme: the negative heuristic forbids this. Thus we cannot look for a new programme unless we violate this rule first and regard the programme as falsified anyway. As Amsterdamski argues:
On the basis of Lakatos' rules, the attempt to save the old program are as equally justified as a rejection of the program and the search for a new one. No methodological rules can, of course, decide the question of how long the attempts to save the old programme remain rational.²⁴

Internally there is a discontinuity which is created by the requirement that new theories predict new facts and the assessment that it may take decades to develop an interpretative and explanatory theory to explain them. We can remove this discontinuity by allowing "phenomena" rather than "facts" to fulfil the role of empirical content. This would merely indicate that the positive heuristic is nothing but a series of informed guesses. This is probably all one can go by and hope that some of these phenomena will become facts in due course. The goal behind a research programme is to devise a protective belt around the hard core which becomes in this manner solidified. The "rationality of intermittent problemshifts", as Lakatos calls it, depends on the ease with which the protective belt is articulated. This in turn depends on the actual force of the positive heuristic. The scope of the positive heuristic is enormous, we may disregard long series of refutations. There are hints, suggestions for the formulation of hypotheses and ways of model building which stimulate reality. It nonetheless takes decades to develop articulated explanatory and interpretative theories. It is difficult to see why this should be different from Kuhn's suggestion that rules develop as the paradigm becomes
articulated. Since the positive heuristic may be developed around metaphysical principles, are these rules research-program specific or are these rules to be understood in the same sense as "methodological rule" in the Popperian framework? After all the positive and the negative heuristic are methodological rules.25

Lakatos also argues that he stays within the Popperian framework. This claim is difficult to take seriously. Lakatos has steadily reinterpreted key notions in the Popperian framework. I have already indicated that falsification can only occur if a new theory is actually found. Experimental verdicts themselves are no longer sufficient. Furthermore falsification in Lakatos has become "repairing an inconsistency". I have indicated in Chapter Two that the connection which Popper makes between the empirical base and experience could benefit from an explication of the notion "testing by experience" and "basic statement" where we found that the decision to accept a "basic statement" is decided by convention. It has been argued that if our conjectures clash with reality we know at least that they are false. Experience clashes with reality if our expectations of certain regularities are thwarted through falsification. Lakatos removes even this tenuous connection by arguing that falsification has become "repairing an inconsistency". The notion of testing has become emasculated: relatively few experiments are important. The connection between experience and the
justification of the acceptance of the empirical base of science all depends on a relatively unambiguous application of "basic statement" "testing" and "methodological rule" as does the very notion of "rational reconstruction". Lakatos systematically obfuscates these concepts and alters their relationship to each other. In particular by altering "falsification" into "repairing an inconsistency", by replacing "basic statement" with "a series of theories" and by practically eliminating "testing", Lakatos removes the tenuous connection between experience and the empirical base of science. It is only because of the strong role of "experimental verdict", that we may regard Popper as an empirical minded philosopher. By altering the focus of "experiment" and "testing by experience" Lakatos needs accordingly to reassess where experience and science connect—in particular since he insists that "experience remains, in an important sense, the impartial arbiter in scientific controversy". In fact however, Lakatos argues that "whether a proposition is a "fact" or a "theory" in the context of a test situation depends on our methodological decision". Lakatos places what could serve as the basis for a reinterpretation of the connection between experience and theory firmly within the methodological realm.

It is precisely this question, whether experience remains an independent arbiter during scientific controversy, that is the central question that Kuhn poses. Lakatos has
eliminated this question with the elaborate notion of "research programme". The basic aim of such a programme is to articulate a series of interpretative theories and a series of explanatory theories, to generate and explain facts from an adopted point of view. This point of view is a metaphysical one; Cartesian metaphysics is advanced as an example. The "hard core" is "metaphysical", by decision, in the Popperian sense, that is, it is decided that no falsifiers, even though they may exist, can penetrate the hard core. The hard core serves, if adequately hardened by the protective belt, as an interpretative theory. The protective belt, when adequately articulated, serves as an explanatory theory. There is a circularity here: the articulation of the protective belt serves as a justification to regard the hard core as hardened thereby allowing it to function as an interpretative theory. This, of course, is no problem, for Lakatos. In the final analysis what is to count as an interpretative theory and what is to account as an explanatory theory is a matter of convention anyway.

The difficulty concerning the empirical base which confronted 'naive' falsificationism, cannot be avoided by 'sophisticated' falsificationism either...some conventional line must be drawn at any given time.28

Lakatos claims to have incorporated "paradigm" into research programme and claims to have shown that this results in a continuous growth of science picture. But he has failed to do that. Not only are the research programmes internally
discontinuous, no argument for the continuity of different programmes has been presented. Internally continuity is achieved only by equivocating between phenomena and novel facts, while the notion of competing or succeeding research programmes is too obscure to serve as a demonstration of continuity. Neither is it at all obvious why observation, organized around different metaphysical principles, can remain neutral and not result in what Kuhn calls incommensurabilities, at the very least, of key concepts. Kuhn argues that perception is influenced by prior education and that observation is not theory neutral, and as a consequence meaning, in particular of key concepts does play a role in Kuhn. Lakatos does not address himself to this problem but merely claims that "the rationally reconstructed-growth of science takes place essentially in the world of ideas in Plato's and Popper's third world, in the world of articulated knowledge which is independent of the knowing subject."29 Since this third world is used to discredit the psychological claims Kuhn makes and is in effect the only argument against the usage of psychology in the reconstruction of science, it is well worth to briefly outline Popper's views.

The first world is the physical world or the world of physical states. The second world is the mental world, or the world of mental states. The third world is the world of the intelligibles or the world of ideas in the objective sense: the world of theories in themselves, and their
logical relations, or arguments in themselves and problem situations in themselves.\textsuperscript{30}

The worlds are so related that the first two can interact and the last two can interact. The second world can be seen as the mediator between the first and the third world. The mind as an object of the second world may be linked with objects of both the first and the third world. By these links the mind establishes an indirect link between the first and the third world. An example may be the intervention of technologists. They effect changes in the first world by applying certain consequences of theories in the third world. These theories were developed by other men who may have been unaware of any technological possibilities inherent in their theories. One day, Popper suggests, we will have to revolutionize psychology by looking at the human mind as an organ for interacting with the objects of the third world; for understanding them, for contributing to them and for bringing them to bear on the first world.\textsuperscript{31}

The third world is man made but, Popper suggests, it is possible to accept the reality or, the autonomy of the third world and at the same time to admit that the third world originates as a product of human activity. Larger parts of the third world are the unplanned products of human action. Unsolved problems, for example, the unsolved theory of prime numbers, are clearly autonomous. They are in no sense made by us, they are discovered by us, and in this sense
they exist undiscovered, before their discovery. About the problem of understanding i.e. the subjective act of understanding, Popper advances three theses:

1. Every subjective act of understanding is largely anchored in the third world.

2. Almost all important remarks that can be made about such an act consists in pointing out its relations to third world objects.

3. That such an act consists in the main of operations with third world objects: we operate with these objects almost as if they were physical objects.

Popper's central thesis is that any intellectual significant analysis of the activity of understanding, has mainly, if not entirely to proceed by analyzing our handling of third world structural limits and tools. The metatheorist of the third world may contrast a theory with radically different competing theories. Some of the third world structural units that constitute the metatheory may be utterly dissimilar from those that constitute the theory (e.g. Galileo's) to be interpreted or understood. In this manner Popper proposes to replace psychological explanations by the analysis of third-world relations: "in place of psychological explanatory principles we make use of third world considerations mainly of a logical character; and my thesis is that from such an analysis our historical understanding can grow."
Popper's articulation of the third world concept is basically a restatement of his belief that in studying the growth of knowledge we may neglect the connection between their origin and their methods of justification. What is repeated in so many words is that we can always explicate the presuppositions behind our conceptual framework and critically discuss them. Unsolved problems are independent of the conceptual framework in which they are stated. They exist, in a sense undiscovered, before their discovery. Third world understanding consist mainly in the logical manipulation of these problems. The third world concept does not address itself to the problem Kuhn poses. Lakatos by embracing this third world simply removes the problem from discussion. Kuhn argues that "meaning" is important and is connected with science in a psychology of perception which is shaped by a particular paradigm which one learns through education. Kuhn argues that only with difficulty, in a "radical translation situation" the psychological colouring of scientific concepts can be removed, only to be replaced by a different one. Popper argues that "logical manipulation" is sufficient as an explanation and is sufficient to exclude psychological considerations. But here it is not clear what logic means: logic does not pick out one state of affairs over another, it does not remove psychology from science as Lakatos claims the third world argument does. At best Lakatos reiterates Popper's view the force of which lies in
the rearticulation of previous beliefs. It is thus an alterna­
tive to Kuhn, but not a refutation.

Conclusion

Lakatos attempts to provide an interpretation of
"rational reconstruction" which would rescue this notion from
the unwarranted intrusion of non-logical considerations
within the sphere of the context of justification. The
context of justification provides a focus for the acceptance
of the importance of "theory" within science as well as a
justification of the acceptance of particular, successive
theories within science. This justification is provided solely
through an appeal to methodological and logical considerations
which are entirely independent historical-cultural circum­
stances. The central assumptions for the success of such a
reconstruction are that observation (or experience) and the
methodological rules remain neutral with regards to the
successive changes in accepted theories within science. Kuhn
has denied that observation (or experience) remains neutral,
rather experience becomes "incommensurably" conceptualized
under new paradigms, while paradigms generate paradigm
specific methodological rules. This, according to Kuhn,
explains why scientists will adopt radical new perspectives
even though a new perspective may be confronted by counter­
instances which would seemingly deny its efficacy to explain
known facts. Lakatos accepts this aspect of Kuhn's thought,
but denies that this cannot be accounted for in a revised notion of the context of justification.

Lakatos introduces the "research programme" in order to demonstrate this. Lakatos' aim is twofold: to improve and stay within the Popperian framework, and to counter Kuhn's arguments against the "two contexts" in science. Lakatos is not successful in either objective. He systematically alters beyond recognition various key concepts of the Popperian framework. Concepts such as "testing by experience", "experiment" and "methodological rule" are, within Popper, crucial to establish a particular connection between "experience" and "science". Their perspicuity contributes to our understanding and of what is involved in "the rational reconstruction of science". In particular by attenuating "testing", Lakatos ought to have addressed himself anew to the problem of the "empirical base" of science. He does this by declaring that a certain amount of conventionalism is inevitable. He appears to mitigate this conventionalism by widening the scope of methodology.

An example of a research programme is one inspired by the metaphysical view of Descartes that the universe can be regarded as a clockwork. An example of a hard core within this metaphysically inspired programme is Newton's law of gravity. It seems now that Lakatos embraces metaphysics of the variety Popper sought to exclude from science, even though Popper acknowledges the merit of metaphysics. Moreover
such metaphysical views as in Lakatos' example also function as cosmological views and have, ordinarily, typically an interpretative effect on observation (or experience). Lakatos avoids the issue of the interpretative effects of the adoption of such views by arguing that it is sufficient to hold the "core" irrefutable by decision in order to articulate a protective belt which in turn hardens the core. Within the protective belt the notion of progressive and degenerating problemshift are intended to give scope to the "rationality" behind theory choice. In fact, this rationality, as Lakatos admits, only becomes apparent after decades of work. There is furthermore no continuity between research programmes. The honorific title of "crucial experiment" can only be awarded with hindsight. Lakatos seems to replace the rational reconstruction of science with one that can take place only decades after the adoption of the initial revolutionary theory, after the question of whether experience could remain an independent arbiter is no longer of pressing concern. But this is not the question Kuhn posed.

Lakatos argues that his account "implies a new criterion of demarcation between "mature" science and "immature" science consisting of mere patched up pattern of trial and error". Like Kuhn, Lakatos departs from a sentence-by-sentence view of science. The unit of science, the research-programme serves as a demarcation criterion. This need not be a particular deterrent were we presented
with a notion of research programme the articulation of which could be seen to proceed along empirical lines—the notion of "empirical" accompanied by an explication of how experience connects with theory. But the articulation of a research programme for Lakatos could not proceed without an appeal to conventionalism. The claim that a research-programme could serve as a demarcation criterion is a hollow one. Lakatos' flight into the third world comes as no surprise but it cannot be taken as an adequate answer to the tasks he set himself to accomplish.

In the next chapter I shall discuss the reception of Kuhn in the sociology of science. Prior to Kuhn the sociology of science was dominated by the "Mertonian paradigm". This paradigm accepts the dominant view of science and treats "methodological rules" as functionally equivalent to norms.
THOMAS KUHN AND THE SOCIOLOGY OF SCIENCE

The sociology of science prior to Kuhn is mainly based on the Mertonian paradigm. Merton is credited as the founding father of the sociology of science,

...others before him had investigated the social aspects of science; but only Merton and his group made a conscious effort to establish a definition of the area, a conceptual framework and a program of research and they were the first to make a conscious effort at obtaining recognition for the field as a branch of sociology.¹

The theoretical framework of Merton's analysis is the structural functionalist perspective. Merton applied the Weberian hypothesis about the importance of the protestant Weltanschauung in aiding the development of capitalism in Europe to the emergence of science in the same historical period and culture area. He concluded that puritan values had been important in the establishment of science as an institution in 17th century England. Like Weber, Merton did not point to any causal connection but emphasized that puritan religious values were a strong factor predisposing its adherents towards experimental science.
The emergence of modern science as an institution, in Merton’s approach is the result of the emergence of a particular value-complex embedded in a more general but congenial Weltanschauung. In later works Merton takes the institutionalization of science for granted and concentrates on the way in which science, as a system of social relationships operates. This approach emphasizes the social rules, the norms of science and the rewards that are built into the social system of science in order that it should work efficiently on the premise that the system continues to operate in roughly the same manner. Merton introduces the notion of the "Ethos of Science": "an affectively toned complex of values and norms which is held to be binding on the men of science". These norms are expressed in the form of prescriptions, preferences and permissions. They are legitimized in terms of institutional values. These imperatives transmitted by precept and example and reinforced by sanctions are in varying degrees internalized by the scientist. Although the ethos of science has not been codified it can be inferred from the moral consensus of scientists, in writings on the scientific spirit and in moral indignation directed toward contraventions of the Ethos. Merton specifies four basic moral imperatives which provide the foundation for the social relations in science and observance of them provides the key to scientific progress as social progress.
(1) Universalism prescribes that knowledge claims in science should be evaluated and accepted or rejected according to impersonal cognitive criteria. Careers and opportunities in science should be based on achievement and competence only.

(2) Communism or communality reflects the fact that there is common ownership of goods in the scientific community.

(3) Disinterestedness represents a focus on pure science and a disinterested search for truth.

(4) Organized skepticism is both a methodological and an institutional mandate.

Merton and his associate Barber also include the idea of "an essential autonomy" i.e. freedom from external controls (religious, economic or political) as a condition for the effective functioning of science. An emphasis of "originality", "objectivity", "individuality" and "generalization", are other norms which are emphasized as functional in the writings of Merton and his associates. Both the actual norms as well as their status (i.e. whether they are normative (moral) or cognitive) have come under considerable scrutiny. Stehr for example in "the Ethos of Science Revisited" argues "that there is a "theoretical break" in the work of Merton and therefore in the sociology of science". The four basic moral imperatives of science, Stehr argues, are linked in distinct ways to the cognitive development of science."
The norms do not merely regulate the behaviour or the social relations of members of the scientific community "they enhance the goals of science which is the continuing extension of certified cognitive knowledge claims". The normatively prescribed social relations of science therefore complement if not implement the development of knowledge in science and vice versa. But, Stehr argues, when Merton's emphasis shifts to a more detailed inquiry into the normative basis of the social relations of science, he neglects to clearly separate the interdependent realms of the intellectual and the social domains of science. The theoretical break, the dissociation of the cognitive and social domains, was obscured by the dominant philosophies and histories of science. As Stehr argues:

The Ethos of science is assimilated, on the one hand, to a particular theory and history of knowledge and, on the other hand, to a particular model of the social relations of science - a model that emphasizes the normative basis and control of social action - cognitive divisions are de-emphasized because the theory of the institutional basis of science and its cognitive development reinforce each other.

Given this shift in emphasis, the Mertonian model has been regarded mainly as a justification for empirical research into the social relations of science and the institutionalization of the social norms, which can be regarded as institutionalized when they are positively linked to the distribution of reward. The reward is conceived of as social recognition and is closely associated with the system of formal informa-
tion. Professional recognition and personal reputation can then be used to obtain scarce resources such as research funds and academic promotion.

Although Merton at one time recognized a cognitive dimension and a social dimension within the scientific community, subsequent research took for granted the complementarity between the normative structure of the community and the cognitive aims of science. The influence of Kuhn in the sociology of science shows itself primarily by the attacks on this aspect of the Mertonian paradigm. Thus Whitley argues: "by assuming that the cognitive aspect is non problematic for sociology, sociologists of science have implicitly adopted a view of scientific knowledge". Merton, according to Whitley, reduces scientific knowledge to the method which produces it, "just as the logical positivists reduced the meaning of a proposition to the way it is verified". The kinds of issues that are discussed by Merton, according to Ramasubban, are exceptional cases. They make reference to eminent men: Newton, Einstein, Bacon, etc.; "while this area in the sociology of science is interesting, it would be wrong to believe that what is true of these exceptional cases would be true of all participants in scientific activity". Leslie Sklair objects to Merton's norms on the ground that "the analysis is intended to apply to science as a social institution universally in all societies - whereas it appears to hold good only for science
in a certain type of society".\textsuperscript{11}

In a study of the "Cambridge group", a successful research group in radio astronomy, Mulkay discovered that the Cambridge group had delayed publication, and that they had published insufficient data to allow other groups to undertake supplementary research. They were in certain respects secretive, they could have passed on their results before publication to colleagues in neighbouring laboratories to obtain advice. Mulkay discovered however, that all these deviations from "Merton's norms" could be countered by reasonable answers pointing to accepted practices and norms in the field. Mulkay concludes:

In science we have a complex moral language which appears to focus on certain recurrent themes or issues: the procedures of communication, the place of rationality, the importance of impartiality and of commitment and so on. But no particular solutions raised by these issues are firmly institutionalized.\textsuperscript{12}

Mulkay argues that these (i.e. Merton's) norms can best be understood as an "ideology". "These values were said to be derived from the nature of scientific knowledge and, although they were realized most fully in the scientific community, they were portrayed as being the fundamental values of American society".\textsuperscript{13}

Mulkay acknowledges Kuhn's influence: "There is a close resemblance between Kuhn's paradigm and what I (i.e. Mulkay) have called cognitive and technical norms".\textsuperscript{14} Mulkay's criticism of Merton has been to emphasize the
cognitive and technical norms implicit in the usage of paradigm, where Merton has de-emphasized these concerns. These criticisms Stehr argues "for the most part amount to an alternative delineation of a set of norms as constitutive of scientific practice. Kuhn's approach is perceived as a reversal of Merton's approach" Kuhn, in the postscript of the The Structure of Scientific Revolutions mentions Warren Hagstrom's "The Scientific Community" approvingly. Hagstrom however, is exclusively interested in the social relations and the community structure within research communities and their bearing on innovation and control. He stays well within the Mertonian paradigm. In a collection of essays by Kuhn, "The Essential Tension", Kuhn stresses the continuity between his concerns and those of Merton's.16

That Kuhn and Merton's views may be regarded as continuous becomes clear if one acknowledges, as H. Martin's does, that paradigm change is endogenous:

Kuhn's account of science may be 'irrationalist' and 'sociological' (or partly so) but it holds 'external' variables constant. It is internal in the sense that its only explanatory variables pertain to the culture of science and the psychology of scientists.17

Although Martins, King, Dolbey, Barnes and Whitley all argue that Merton held a "perfect rationality for scientists" (Whitley), "a logical positivist view" (King, Barnes, Dolbey) this is, in a sense, irrelevant with regards to Merton's functionalist sociology of science. All Merton needs to argue is that his conception of science is independent of external
factors such as socio-economic or cultural factors. But the particular philosophy of science, as long as it is consistent with an "internal" approach need not matter. Thus Whitley in "Black Boxism", who examines Kuhn's influence in British sociology is only partially correct when he claims that "by emphasizing the cognitive aspect of science and linking changes in cognitive structures to socio-psychological phenomena, Kuhn legitimated sociologist's revolt against Merton". Whitley overlooks that although Kuhn does emphasise cognitive structures, they remain independent of the larger (external) cultural context.

Mulkay and King are chastized by Whitley for not paying attention at all to epistemic concerns (Mulkay) and for adopting an extreme relativist theory of knowledge (King) because King ignores the epistemological difficulties in Kuhn and considers insuperable the question of deciding between the validity of "cognitive structures". But Whitley is also ambiguous with reference to epistemological concerns. Kuhn, he argues, "has denied the possibility of progress, he does not explain how science progresses if the truth content of a paradigm is incommensurable with its successor". In a footnote Whitley argues that "epistemological rational" means rational according to a particular theory of scientific knowledge. "It is important to understand," Whitley argues, "that there is more than one way of making sense of an event so that we can improve our current rationality and not allow
it to become sterilized into dogma". Whitley conveniently
forgets that the notion of progress in terms of increased
truth content itself rest on very tenuous assumptions.
Whitley quite correctly argues that there are difficulties
with Lakatos and Popper's criteria for reconstruction and
concludes "some criterion by which we can judge the current
scientific elite's decision is necessary if scientists are
to be considered partial (my emphasis) rational".
Herminio Martens also sees Lakatos as trying to meet the
Kuhnian challenge: "by rationally distinguishing between
progressive and degenerative problem shifts it becomes
possible both to identify genuine crises and to vindicate
the rationality within limits (my emphasis) of paradigm
retention." I have already outlined my criticism of Lakatos
and his historical reconstruction. Briefly, the history of
science is reconstructed rationally by adopting conventiona-
lism. There is a constant tendency to equivocate between
rationality (reconstructed) and using this rationality as a
criterion for decision between progressive and degenerative
problemshift only to have actual practice belie the
reconstructed norm and thus necessitating a new norm.
Scientific theories must eventually fit the facts. Other-
wise, they would not be satisfactory - but the continuance of
theories fitting the facts rely on a principle of induction.
Any rationality which is derived from a historical reconstruc-
tion and used as a norm to rationally decide between problem-
shifts sidesteps this principle. Either the theory-fact-fit continues to hold or it does not. If it does there is no problem. If it does not both the theories and the facts must be re-examined, but any rationality derived from a previous fact-theory-fit is a fortiori beside the point. "Partial" rationality in this context is neither here nor there. Martin's and Whitley are (quite rightly) dissatisfied with Lakatos' and Popper's retreat into the third world. Yet the rationality of Lakatos is dependent on this concept for its force. "The rationally reconstructed growth of science takes place essentially in the world of ideas, in Plato's and Popper's third world."24

There is in both Martins and Whitley a persistent tendency to emphasize the irrationalist features of Kuhn. "That Kuhn's account of scientific revolutionary episodes is irrationalist is now almost taken for granted".25 This view probably derives from the view attributed to Kuhn that changes in paradigms are matters of conversion of psychosociological persuasion etc. Although these views have contextual support, there is also, certainly since the Postscript, a view discernable in Kuhn which is less, if at all, irrational and this view also has contextual support. Briefly, in the postscript Kuhn argues that translation of paradigms is possible if both antagonists engage in a radical translation situation. Kuhn considers culture prior to scientific activity. The influence of culture becomes less
discernable once the student goes through the educational channels and becomes acquainted with "exemplars". Here psychology takes over - but it is not psychology tout court, but only psychology of perception. Kuhn argues that training forges perception into "immediate recognition of regularities" and also tries to argue that these are constitutive of nature. Kuhn also quotes the "anomalous card experiments" where we sometimes see something which in fact is something else. But prior knowledge and expectation conspire that we see something that in fact is something else. In other words, given a certain tradition of knowledge claims, we impute a visual analogy on some sensory stimulus when in fact we ought not to. The point is that we do not know and cannot know any better.

By introducing a psychology of perception based on similarities and expectations, Kuhn provides an alternative to both the logical positivist and Popperian accounts of perception and its influence on observation language. One of the consequences of this alternative is a denial of a theory neutral observation language. Disagreement with those who hold an opposite view is hardly "irrational".

Kuhn's initial reception in the sociology of science has been welcomed by those who saw Merton's paradigm as devoid of cognitive concerns. Kuhn's emphasis on cognitive content is interpreted as anti-Mertonian. But as I have tried to argue, both Kuhn and Merton argue for an "internal development thesis" and thus their positions are not
inconsistent with each other. Each has a different "internal development thesis" with regard to science. This, indeed, may lead to a different emphasis on "cognitive" and "institutional" norms.

A different approach, derived from, but not explicit in Kuhn is the approach that questions the difference between the "internal" and "external" development of science theses. By "internal" is meant that the history and development of science is not influenced epistemically by external factors such as socio-economic or cultural factors: these factors do not enter science in a cognitive significant sense. That this development derives from Kuhn is clear because of Kuhn's emphasis on cognitive and structural factors. But as Mendelsohn argues

...what is striking to the reader, however, is Kuhn's assumption of an almost total independence of the shape of the cognitive structures, or the content of the concepts, from institutional structures. It is not that the question of links between the conceptual, institutional and societal levels were examined and nothing of note detected: rather the question itself was never posed.26

Barnes, in "Scientific Knowledge and Sociological theory" argues that,"the extent to which scientific change is determined or influenced by "external" factors is a contingent matter requiring separate investigation for every particular instance".27 Barnes nonetheless presents an argument that establishes the relevance of the sociology of knowledge, that is, an enquiry into the social dimensions of
belief in scientific knowledge claims. Barnes argues that...

...facts may remain beyond the sphere of the sociology of knowledge, but the rest of science does not. Once beliefs are conceded not to derive completely from the restraints of reality no further a priori argument can be made against their sociological interpretation\(^\text{28}\).

Barnes denies that this claim ignores the possibility of rational theory choice - but "we must ask (again) where the initial set of theories comes from. If, as presumably it must be, it is drawn from, or inspired by, the scientist's general cultural resources, then the final, rationally chosen theory may be partially determined by social factors".\(^\text{29}\)

Barnes points out the difficulties of "theory" in Popper: "Popper with his claim that one can always break out of the constraints of any framework of beliefs and ideas is one of the philosophers who takes least account of the sources of theory".\(^\text{30}\) This is indeed important since Popper claims that theories may originate from myths, rough trial and error solutions, and also claims that scientific theories can be looked at as independent of cultural influences. At which point do they shed their cultural beginnings?

How cultural resources influence theory choice is a contingent matter. Not the scientists social commitments but their social position, or simply their society may determine their thought. The scientists may draw their ideas from a culture, largely embodying the set of ideas of a dominant intellectual class or their culture may in some sense reflect the total situation of their society and contains
beliefs all of a certain kind of form. Neither can the "facts" be treated as sociologically unproblematic. Since factual statements involve the categorization of experience, it follows that, if they are to be regarded as universally acceptable basis for further belief, there must exist a special theory independent observation language. However, observation terms function in living languages and they are always involved in general and theoretical statements. Their usage changes over time: "a "fact" does not constitute an unchanging reference point".

The sociologist then cannot treat "theory" or "facts" as a given unproblematic baseline. Neither can be treat the concept of rationality in an unproblematic fashion. Scientific rationality could be given special status "by arguing that they are held rational". Barnes considers several possible arguments in favour of such a move, but finds them wanting. Induction, as a possible model for rationally held belief is inadequate. The sociologist would find several models in existence, mutually incompatible yet all with arguments in their favour. "Moreover, he would find that all the models are recognized as inadequate so that none could be used with confidence to detect rationality". Barnes also discusses falsifiability but argues: "there is the difficulty of objectively establishing that a belief system is falsifiable. This is a particularly acute problem since inspection of the system itself will not normally suffice to solve it".
Barnes thinks furthermore that falsifiability is not naturally compelling; "there are many ground for thinking not. One is the existence of a very strong conventionalist tradition in the philosophy of science". With regards to "methodological conventions", advocated by Popper as rational to adopt. Barnes argues that the reason for their appeal is clear: it is that Popper is able to present what we already regard as great scientific achievements as the product of conformity to these conventions. "The logic of scientific discovery does not justify science at all; it is justified by science as Popper presents it". Barnes comments:

Philosophers of science have been insufficiently conscientious in preventing their general prescriptions from being taken as descriptions of science and they thereby have helped to sustain unwarranted and misleading conceptions of science and rationality within the social sciences.

After dismissing "facts" "theories" and "rationality" as providing a baseline for judging differing sets of beliefs as irrational, mistaken, not efficacious etc., Barnes argues that rather than through the notion of truth or rationality,

...the manifest variability of institutionalized natural beliefs is to be made intelligible by being set against an unproblematic baseline of normality. It is possible for the sociologist to identify normal patterns of belief by the investigation of human collectives in the light of existing sociological theory. We may claim that all institutionalized systems of natural belief must be treated as equivalent for sociological purposes.

The reason why Barnes does not pay too much attention to the internal-external dichotomy is that his own idea, which
obviously resembles that of Kuhn's, obliterates this, in Barnes' opinion, obtuse distinction. The scientist who has been trained successfully, possesses a number of routines, of acting and of thinking which are capable of being applied in a number of ways depending upon circumstances and how these circumstances are perceived. The importance of routine activity, activity which 'comes naturally' to the scientist is difficult to overestimate. One may regard a large part of scientific activity as explicable in terms of the development of a subculture along "natural lines": "this is the kind of activity which sometimes prompts historians of science to say that a field is developing under an inner logic. It is worth stressing an alternative naturalistic conception". Barnes is concerned with the constraints "reality" imposes, but argues that traditional notions such as "objectivity", "falsifiability" and "rationality" are not exacting enough to compel one to accept these notions as epistemologically prior to and unproblematic with regards to what could be called "cultural reality". Barnes suggests that cultural reality, that is, notions of cultural normality constitutes the restraining influence, rather than "reality" as depicted by epistemologists in some explication of scientific realism.

Mulkay, in "Science and the Sociology of Knowledge" is also concerned with various notions such as "fact", "theory" "observation" and so on, which are traditionally invoked to argue for the special status of scientific
knowledge. Mulkay, like Barnes, argues that the physical world need not be the only restraining influence on the "facts" and argues that "the products of science can be interpreted as social constructions like all other cultural products". Mulkay starts by examining some of the assumptions of the standard view of science, which have traditionally supported the belief that the conclusions of science were determined exclusively by an appeal to the physical world. "The principle of the uniformity of nature". Mulkay argues, cannot be regarded as an aspect of the natural world, but must be seen as an aspect of the scientists' methods for constructing their accounts of the world. "It cannot be used as grounds for treating the generalizations of natural science as definitive representations of a stable and uniform physical reality".

Mulkay sees observation as an active process in the sense that the observer creates and responds to a dynamic sequence of cues which are used to place hypothesized objects and events in terms of pre-established set of concepts. Observation furthermore involves interpretation: "these are two facets of a single process." Judgements of observation are guided by criteria of adequacy but these criteria are not explicit and impersonal, "they can be established only in an indirect and uncertain manner". All knowledge claims are judged, according to Mulkay, partly according to conventional criteria of adequacy which vary over time and from one group
or social context to another, and partly in terms of their consistency with an ever changing interpretative framework".46 Assessment of new knowledge claims is likely to provoke changes in currently accepted criteria and to foster opposition as members explore the implications of the claim. This process of assessment is also a process of reinterpretation.

In contrast to the standard view, scientific knowledge is not stable in meaning, not independent of social context and "not certified by the application of generally agreed procedures of verification."47 Scientific knowledge, "offers necessarily offers an account of the physical world which is mediated through available cultural resources and these resources are in no way definitive".48 This non-definitive character of scientific knowledge, the dependence of criteria of adequacy on available resources indicate: "that the physical world could be perfectly adequately analyzed by means of language and presuppositions quite differently from those employed in the modern scientific community".49 Although the external word exerts a constraint on the conclusions of the scientific community these constraints operate "through meanings created by the scientist to interpret that world".50 Since their meanings are revised and partly dependent on the social context in which they are formulated, "there is no alternative but to regard the products of science as social constructions like all other cultural products".51
implications of this view enable the sociologist to ask a much wider range of questions than from within the standard view of the sociology of science. This view assumed that the production of scientific knowledge can be explained by showing that general conformity is maintained to sets of cognitive and social rules, "the strict implementation of which guarantees an undistorted revelation of the real physical world." In contrast Mulkay advocates the view that "neither of these kinds of rules has a determinate meaning for participants, implementation therefore requires a continual process of cultural reinterpretation". The implementation of these rules and the cultural reinterpretation of them are established by a process of negotiating, that is, "by the interpretation of cultural resources in the course of social interaction". This negotiating is a rather informal process about which little is known, partly because it is covered up by the transition from private speculation to formal demonstration. There is in practice a continuous cultural exchange between science and the wider society. Interpretative resources Mulkay argues, ...enter science mainly through informal thinking...they are not generated by the facts of nature, nor by the social life of a segregated community alone. They must be understood at least in part as a product of the social processes of society at large. Both Barnes and Mulkay argue that the traditional notions of "facts", "observation" and so on, can no longer support the standard view of science. Rather these notions 

must be located in a cultural context, where "cultural reality" or "cultural normality" constitutes the restraining influence on the "facts". Ravetz is in general agreement with the rejection of formal methodological rules. He emphasizes a painstaking and careful notion of constructing contact with the external world and extracting consent from it. The judgements which guide the scientist are social in origin.

Ravetz argues that until recently scientific research activity and the resulting knowledge have been studied separately. The result has been an impoverishment in our understanding of science. Ravetz sees the scientist as a very special sort of craftsman, "for the objects he is dealing with are highly artificial". The world of nature is too complex to be understood in terms of the concepts generated from ordinary experience. In order to approach the external world the scientist constructs "intellectual objects". The scientist's goal is to establish properties of classes of things and events of these intellectual objects. But "these classes are inaccessible to direct view and testing, it is only through the theory in whose terms the experiment is conducted and interpreted that these properties can be inferred". The constructed intellectual objects find their roots in common sense, but "the need for systematic coherence weighs at least as heavily as the retention of the original link". This link should not be severed. Established
scientific disciplines, Ravetz argues tend "to create a world of their own and then live totally within it, (this) is now recognized as an obstacle for the solution of practical problems involving science and technology.\(^{59}\)

Through the course of an enquiry the intellectually constructed objects may change before an acceptable description is found, although this description is never final. "What is meant by such terms as "molecule", "force", "acid" or even "iron" is in constant flux".\(^{60}\) To ask for a precise meaning would be inappropriate: "each object carries with it a complex burden derived from its history of use and adaptation".\(^{61}\) Ravetz sees this as a consequence of the well-known thesis

\[\ldots\text{that the basic categories of our experience are incapable of precise definition. Concepts such as 'cause', 'change', and the like have in them an inexhaustible supply of subtleties and ambiguities...even basic concepts such as number defy all attempts at conclusive explanation.}\]^{62}\]

The intellectual constructs are embedded in deep aspects of human experience of coping with the world. Our practical command of them "has developed through millenia of experience, no single formalization can capture that body of completely tacit inherited knowledge."\(^{63}\) These deep aspects remain obscure and in a sense there is obscurity at the foundation of knowledge. Explication of these obscurities is not the scientist's concern. The intellectual objects of a scientific argument,
are used as the components of an argument intended to relate to something other than the objects themselves, namely the external world. Hence a more ready and rough control of the properties of these objects will be adequate for the argument.64

Contact with the external world is tenuous and difficult to extract. In the first instance "data are a record of the point of contact with the external world and as such constitute the foundation in experience of scientific knowledge."65 At this point Ravetz emphasises the craft knowledge inherent in scientific practice. Ravetz sees the tools of the craftsman as analogous to the intellectual objects of the scientist. The craftsmanship of the scientist is gained from experience in manipulating the intellectual objects in a variety of contexts. This type of knowledge, intuitive and derived from experience, is essential in judging what data can be used to construct points of contact with the external world. Collecting sound data is only the beginning. The next stage in the construction of the link between the intellectual objects and the external world is the refinement of data into the production of "information" with the aid of mathematical tools. Again, Ravetz emphasises that the scientist must make "judgments of reliability", gained from experience in manipulating data into information. It is at this stage that "information" gained from experimental data becomes the basis for the inference that the intellectual objects do have the properties which the theory in whose terms the experiment is conducted stipulates it has. To be
satisfied, Ravetz argues,

...that one's descriptions are really those of the inaccessible classes of things and events which are being approached through the experiment is another judgment. This is even less capable of being reduced to a routine exercise then the judgments of reliability and so requires an even more refined craftsman's skill.66

The argument which is used to support the particular assertion in view of the evidence obtained, cannot be conclusive. Contact with the external world is unnatural and indirect, "the reports of that contact do no more than serve as a basis for evidence embedded in an argument whose pattern cannot be formally valid".67 To support the argument subsidiary arguments will be brought forward. These arguments will include probabilistic, inductive, confirmatory, or analogical inferences, "all drawing their strength from previous well attested reports but all lacking demonstrative certainty".68

When the scientist has used his craft knowledge to good avail and published his research material, the information he provides may become factual. For data to become evidence in a particular experiment and for information to become factual, is, in Ravetz view, a historical process, conditioned at all stages by socially erected criteria of adequacy. These criteria do not come at the beginning of a scientific enquiry, but "they are the outcome of a lengthy process of work starting with the individual phase of the investigation of a problem followed by the social phase of
testing and through use, of its solution". When a fact is submitted to the scientific community the result is tested, first informally and then formally. If the fact originated from a genuine novel situation its acceptance will take time. Genuine novelty carries its own standards of adequacy and judgements as to the strength of the evidence presented in support of the claim made. If such a fact survives the initial criticism and if the standards of adequacy in its support are accepted, it "may become the material around which a new school comes to be". The original problem may well become transformed into versions that are more powerful. The original fact may also become transformed to produce "extensions and standardized versions performing a variety of functions in its own field and others". The standardized fact may then suffer the fate of becoming vulgarised. This vulgarisation may obscure the profound insight and novelty which accompanied its formulation and acceptance.

The character of scientific knowledge in Ravetz' view is constantly undergoing change. Ravetz denies that it is possible to formalize the character of scientific research or to postulate a specific set of methodological rules which would apply to the practice of science. Ravetz writes:

The diversity in form and function of any piece of knowledge and the obscurity at its foundations are not a denial of its reality, but a necessary condition for its existence. The unity behind the diverse appearances lies not in a unique, but hidden structure of meaning, but rather in their common ancestry and their continued mutual interactions.
Ravetz realizes that an objection to his view might be that it applies only to a small fraction of scientific knowledge, that it unduly restricts what we call knowledge and that it can be applied only in retrospect. He argues that

...the things that we know by a nearly immediate, unreflective sensory experience are particular, temporary and shallow. Knowing that the book fell on the floor is not the same as knowing that its falling is due to the mutual gravitation of itself and the earth: the former is undubitable but scientifically insignificant, the latter is profound but obscure in its foundation and always subject to evolution.72

The authors discussed in the second part of this chapter all emphasize the social and cultural influences in the construction of scientific knowledge. They all deny the legitimacy of the "two contexts" view of science. This influence is obviously due to Kuhn although the authors all deny that the social-cultural influences can be confined to a particular scientific community and can be demarcated from the wider cultural context. Yet prior to Kuhn, perspectives which could have led to an examination of cultural influences upon the end products of science were available. Phillips discusses these influences and offers an opinion as to why they were ignored.

Phillips, in "Epistemology and the Sociology of Knowledge: the contribution of Mannheim, Mills, and Merton", argues that both Mannheim and Mills anticipated some of Kuhn's contributions. Mannheim, Phillips argues "is enormously
sensitive to the influence of people's social positions upon what they can perceive, what they define and accept as knowledge and truth, as well as their views, opinions, goals and values." But Mannheim also suggests that natural science "is largely detachable from the historical-social perspective of the investigator". This view, Phillips comments, "no doubt served to encourage the development of a sociology modelling itself on the natural sciences". C. Wright Mills argued that "a specialized language constitutes a veritable a-priori form of perception and cognition, which are certainly relevant to the results of enquiry. Different technical elites possess different perceptual capacities". Mills, like Mannheim, according to Phillips, sought in vain to avoid the relativist's dilemma: that either the relativist's own assertions are relative, and, therefore, lacking truth value; or his argument is unconditionally true, and, consequently, relativism is self contradictory. Phillips wonders why the epistemological implications of Mannheim and Mills (and the early Merton who also held relativistic views) have been ignored or, "at least not taken as a topic for sociological inquiry". The real question is, Phillips argues, why did the sociologists fear relativism?

The answer lies in the associated assumption of positivist inspired philosophies: the hierarchy of the sciences. According to positivist edict sociology is at best a marginal science. Only by faithfully adopting the
positivist doctrine could there by any hope of sociology becoming a real science, with real knowledge to impart. When sociologists themselves wanted sociology to become a "real" science, it is not surprising Phillips argues that "there was no great enthusiasm for viewpoints that tended, if taken seriously, to throw into question the very cognitive stability of sociology itself". A sociology of science which ignores the content of science and stays within the hierarchy of the sciences, can ultimately ignore the content of sociology qua science, as soon as sociology becomes a science. The content of sociology as a science becomes then automatically justifiable in the context of justification. Its independence qua knowledge claim, of the socio-cultural circumstances of the particular investigator is established. Thus, as Phillips argues, "the maturity of sociology--its own self awareness is ultimately at issue here". The central question remains according to Phillips, "whether we can maintain a relativist viewpoint and defend one's own as rational". Phillips here, in my view, overestimates the force of the relativist's dilemma. The dilemma arises only from a very strict positivist interpretation of language as consisting solely of propositions which are of a truth functional nature and from a view of knowledge as justified within the context of justification. In the concluding chapter, I shall present a view of language as embedded in a "form of life", were giving grounds" or giving a justifica-
tion comes to an end "not as a kind of seeing on our part", but lies in "acting and doing". This will not completely eliminate "relativism", but it will be seen to be a general condition underlying human activity. This is, in my view, a consequence of the fact that the "basic categories of our experience are incapable of precise definition". A language embedded in a form of life can be seen in the first instance as a theory of knowledge, where our basic categories are given, through activity, a measure of clarity and preciseness.

Conclusion

Kuhn's Philosophy of science in a sense constitutes a promise to be able to create a sociology of science which could be concerned with the empirical content of science. Kuhn achieves this by focussing on science as both a process and an activity. The concept of paradigm brings together two aspects of behaviour which cannot be divorced from an understanding of science. These aspects are the questions which the scientist asks and the criteria he sets up to judge whether or not a given explanation is reasonable. Since a particular paradigm involves a particular image of the world and a particular interpretation of perceptual experience, by switching allegiance from one paradigm to another the scientist transforms his own behaviour. Experiences that seemed irrelevant now seem surprising and demand explanation. This general shift in expectations involves a
shift in the scientist's perception of the world around him. His reaction to this experience changes and his conceptualization of that experience also changes. But not only "the conceptualization of that experience" changes. Methodological procedures are part and parcel of the rules which a scientific community accepts as part of a dominant paradigm. During periods of "normal" scientific activity the scientist tackles only those problems which are assumed to have a solution within the context of these rules. It is, according to Kuhn a hopeless task to try to provide logical justification for these rules.

Kuhn's perspective constitutes a shift away from a "formal" or an "analytic" emphasis upon what serves as a satisfactory explanation. The formal approach emphasises internal structure and coherence in scientific theories, but ignores the interpretation to be given to experience. Experience is regarded as a set of statements about reality which are accepted as "factual" in a sense that remains undefined. Explanation is regarded as a formal connection between different factual statements. The promise which is extended by Kuhn is that we may come to understand science as consisting of routine activities and behaviour not too dissimilar from the routine activities which sustain our view of everyday reality. We can then see scientific knowledge and come to see scientific knowledge as socially constructed, distinguishable and separable, but not to be
severed from the roots of common-sense knowledge. But Kuhn moves away from this possibility by locating a paradigm exclusively within scientific communities, and, it seems by doing so severes the ties with ordinary, natural language.

Barnes, Mulkay and Ravetz agree with the tenor of the notion of "paradigm" but disagree with Kuhn that "paradigm" can be fruitfully separated from the wider cultural context. Barnes and Mulkay deny that traditional notions of "objectivity" or "rationality" are compelling enough to extract consent of the acceptance of scientific knowledge claims. If traditional ambitions to anchor epistemological notions of "objectivity" or "rationality" are inadequate, where can we fruitfully locate them? Barnes and Mulkay argue that notions of cultural reality or cultural normality are adequate in this respect. The exactness behind these notions, disappears, but the arguments supporting this exactness are wanting anyway. Ravetz argues that they lie in deep aspects of human experience of coping with the world which has resulted in a body of tacit inherited knowledge. The need for science to retain these roots is as important as the need for systematic coherence. In the conclusion I shall argue that the foundations of knowledge may be located in a conception of "form of life". A "form of life" is a matrix of activities—ways of acting and doing. It is through this matrix of activities that we gain purchase on nature. Ordinary language and the conceptual framework it provides is anchored upon this matrix of activities.
CONCLUSION

The philosophies of science discussed can each be characterized by their view on language, meaning and observation. Within logical positivism a scientific theory may be regarded as a calculus consisting firstly of axioms and theorems, primitive terms which cannot be defined and defined terms which are formed from the primitive terms. Secondly, there is a set of rules, usually deductive, which govern the formation of theorems and a set of correspondence rules which links the propositions contained in the theory with empirical phenomena. On this view the formal truth of the calculus lies in an appeal to the laws of logic, while the observation statements, which connect the theoretical with the empirical are vouchsafed for by the verification criterion. Once a fact is verified, once it is confirmed, it is relatively immune to disconfirmation. If we are to regard such a formal calculus as a language, albeit an artificial one, then, at least initially, the logical positivist's view of the language of science is one of a closed and coherent system, independent of the natural language. The laws of logic and the rules of deduction form the syntax of this artificial language, while the cognitive import, gained through the correspondence rules, provides the semantics of the language. Within
this closed system, meaning is entirely accounted for and
the artificial language is free of the vissicitudes of
ordinary language, a tendency to reify or to regard as real
what in fact is metaphysical. The language of science so
conceived serves as a mirror to nature and there exists a
harmony between the realm of propositions and the realm of
facts. There is a unique correspondence between proposi-
tions and facts and it is here that the meaning invariance
through scientific change finds its origin. Quine has
stressed the indeterminacy of translation and stressed the
links between artificial and natural languages. As Amster-
damski puts it;

Artificial languages are islands floating
on the ocean of natural languages. The
unity of science, which in fact is a multi-
tude of languages, can never be achieved;
it would be possible only if we could
connect all these languages into a coherent
linguistic system corresponding to experi-
mental data.¹

Although Popper specifically disagrees with the
logical positivist preoccupation with formal languages and
in general regards the study of linguistic usage and lang-
language systems at best interesting but not germane to the
problem at hand, the growth of knowledge, he implicitly adopts
a view of language. At the very least it is denied that an
emphasis on the conceptual aspect of language would make it
problematic to separate background knowledge from the theory
under test, to assess the factual content of rival theories and to require of newly adopted theories that they explain the facts of a refuted theory.

Such a view of language is denied by Kuhn, who argues that observation is dependent on a linguistic framework and that a single observation sentence can have no meaning apart from its location in a broader conceptual and linguistic framework. There is, however, a further claim involved in focusing on language as an aspect of behaviour and that is an epistemic sense of "seeing that". "For most purposes", Mulkay argues, "we can think of observation as the act of locating things and events by means of categories; and such categories acquire their meaning by implying that certain statements will be found to apply to that which has been observed to be the case." There is a sense in which there is embedded in language and continually reinforced in social interaction, rules of judging what is synonymous what is the same, what is consistent. There is an emphasis on "the social character of our concepts, the links between our thoughts and speech, speech and communication, communication and social communities." I shall now outline a view of language which emphasizes the links between acting and doing and the agreements arising out of this.

If one argues that science is a superior form of knowledge, then the empirical basis of science constitutes the foundation of knowledge. To provide an answer to where
these foundations may lie it is necessary at least to be aware of how language and thought connect with experience and reality. Conway, in "Wittgenstein on Foundations", argues that "traditionally philosophy sought to secure a firm foundation by arguing that thought and discourse mirror or somehow correspond to an objective independent reality". This account rests on a specific view of language, thought and reality. "The necessary foundation of thought and language rests in a reality already constituted before active human participation". Ordinary language may well obscure this reality but it remains possible to construct a language which could describe this reality. This view rests upon an interpretation of experience that is passive. On this account it is no deterrent that we learn ordinary language in a context of activities in a particular world. It remains possible to discover a real world behind the phenomenal world—the world we see. Kant has argued "that our notions do not regulate themselves according to things but things according to our notions". It follows from this that the objects of experience are only our objects, the objective world is only objective for man and the "real world" behind the phenomenal world must remain hidden. Once one acknowledges the contributing factor of experience, the phenomenological shaping of things according to our notions, as they enter experience, the traditional account of language and the independent reality becomes irrelevant. The foundations of knowledge can
no longer lie in an independent reality but must be found in the subject. To secure the foundations of knowledge from mere whim, Kant posited the "transcendental subject" as "the ultimate ground of all objectivity, (it) is an abstract correlate of pure knowledge."\(^7\) The transcendental subject is independent of the vissicitudes of biology, history, culture and so on, which mold the ordinary subject and his way of thinking. But the question is, Conway argues, "can we speak meaningfully of a transcendental subject, radically distinguishable from the empirical, psychological, bodily subjects?"\(^8\)

The question of a passive sense of experience and an active sense provided by the mind also play their role in the empirical base of science. Within logical positivism (passive) experience can be appealed to justify the acceptance of basic propositions. With Popper the active ingredient is provided by the notion of theory, by our conjectures which we put to the test. The rationality of this aspect is grounded in the rationality of the context of justification. If we reject an appeal to experience to provide the empirical basis of science and the context of justification as providing grounds for acceptance of new theories, where should we look for foundations and in what sense should we interpret them? A tentative answer is provided by adopting a view of language which consists of "language games" and which, in the first instance, derive
their meaning from patterns of activity on which they are grounded. The phenomenal aspect, the sense in which our notions shape the things which enter experience, are found to lie in the workings of the language. To investigate the workings of the language is to investigate the structure of the world.

When we engage in particular activities we commit ourselves implicitly to certain propositions by thinking and doing things in a particular way. Such propositions presuppose that seeing, doing, remembering and so on have a place in our "form of life". These activities and ways of doing are in the end "the ultimate basis for our way of speaking and thinking about the world". Wittgenstein claims, Conway argues, "that the human form of life, the human way of being and doing in a particular world, is the ultimate ground". Language through these types of activities attaches to the world, unfolds the world and grounds the world. In a demand for justification we will reach certain propositions which are themselves bound up, and grounded in ways of acting and doing. They cannot be further justified. Giving grounds, justifying come to an end, but the end is not certain propositions striking us immediately as true i.e. "it is not a kind of seeing on our part; it is our acting which lies at the bottom". To ask for further grounds beyond such a matrix of activities or to ask that such grounds be further justified is to misunder-
stand their position and role. A direct attack on reality is misplaced and will simply generate wrong language games with misleading rules.

Pitkin gives the following definition of "form of life": "because they are patterns, regularities, configurations, Wittgenstein calls them forms; and because the patterns occur in the fabric of human existence and activity on earth he calls them forms of life". This initial definition leaves open four possible ways of interpreting "forms of life". Gier distinguishes first, the language game account, where forms of life are identical with language games; second, the behaviour-package view, where "forms of life" are behaviour packages correlated with language games; third, the cultural-historical view—here "form of life" is a way of life, a world view, fourth, the organic account which emphasises form of life as something typical of a living being reacting in complicated ways to the environment. Gier favours the organic account but sees no conflict between this account and the cultural-historical account. The reason is that language and culture are typically human ways of dealing with the environment. But "form of life" is not identical with "language game". Gier argues: "we reach bedrock in forms of life that are basic human activities, not just linguistic ones".

Phillips in, "Wittgenstein and Scientific Knowledge", clearly agrees with the organic-cultural historical view of
form of life, which he also considers as bed-rock. Phillips treats the ways of acting, seeing, doing and so on, which are the ground of "form of life" as "facts of nature". Various language games are partly dependent on certain contingent "facts of nature": "that human beings talk, use language, agree in judgments and reactions and share certain common interests". In a sense, language is embedded in activities that form the basic matrix of the "form of life": "it is a product of the facts of human and physical nature". But language is also "a producer of new meaning and new forms of human activity". Phillips argues that "Wittgenstein's conception of the facts of nature provides an important prior grounding for language". But Wittgenstein denies, according to Phillips, that the facts of nature are completely the creation of our language and that language is uniquely determined by the external facts of nature. This balance is struck by appealing to the "form of life" on which the language is grounded. It is a basic appeal to acting and doing and so on in response to finding ourselves as a human species in a complicated environment. This acting and doing presupposes some purchase on that environment. The language grounded on that activity will reflect this grasp on nature. Our conceptions of nature are not mere creations of language. Although "language has enormous freedom to create its own reality it is nevertheless built on the facts of human and physical nature". But we cannot show this by pointing to
a fact of nature in the sense that the fact "mirrors" reality. Facts as entities now dissipate in activity within a form of life. As Kenny puts it "the datum on which language rests, the framework into which it fits, is given not by a structure of unchanging atoms, but by a shifting pattern of form of life grafted on a basic common human nature". 21

When we speak of the ordinary language game, grounded on a form of life we must include the acting and ways of doing as part of our reaction to nature. This grasp on nature is embedded in ordinary language. It provides a framework of some system of concepts in which we conceptualize these basic ways of doing. The structure of the world is not independently given. Conway, who asks rhetorically whether we can speak of a transcendental subject independent of biology, culture etc., sees in "form of life" a way of redefining the a-priori.

One must envision the a-priori as arising in experience rather than being imposed upon experience--In the light of the concept of the form of life, the limits and of thought and language are rooted in the conditions of persons in a human world. 22

Gier also argues that "forms of life are primarily the formal conditions, the patterns in the weave of our lives, that make a meaningful world possible". 23

An appeal to "form of life" as a context in which all meaning and thought are grounded avoids realitivism because rules of thinking do not rest on idiosyncratic judgments.
Rules of thinking are themselves grounded on a more primordial foundation. This "dynamic world-unmeshed thought" finds its expression in the everyday language game. Our everyday language game, in Phillips' interpretation, "Constitutes the very rockbottom of our knowledge and experience". It would not make sense to ask of our everyday language game whether it is true or false. It is in order as it is. Wittgenstein argues, in Conway's interpretation, "that there are certain propositions which "stand fast", "form an axis or scaffold" for all our thinking." We do not question, infer, deduce or even consciously try to justify these propositions. They express the fundamental given of a language game which are outside of discourse, but give discourse its shape. "Even if required to call such a foundation into question", Disco writes, in "Wittgenstein and the end of wild conjectures", "we cannot imagine (because our imagination moves on the paths of its language game) what a doubt would look like, let alone its consequences".

Our ordinary language has an ontological and epistemological primary, Phillips argues. The sciences, philosophy, sociology and so on can be seen as language games which are grafted onto the ordinary language games. In practice there exists a continual interaction between the specialized sciences (language games) and the ordinary language game. This interaction arises because the practitioners are in the first instance members of a society. This
primary training persists and permeates all future activity. Phillips argues, "the constructed, extra-ordinary language game, cannot exist in total isolation from concrete everyday language [which] is itself partly dependent on certain facts of physical and human nature". Science, then, as a specialized language community, can no longer be seen as directly addressing itself to an independent world. Rather it must proceed via our ordinary language game. This view of science emphasises the notion of "idealization of experience". In an important sense, the view of science which I shall now outline is non empirical.

Scientific theorizing is essentially a means of obtaining empirical knowledge. But there is more than one way of differentiating the world into empirical individuals, classes-cum-relations and criteria. Resemblances play an important role. Resemblances perceptible to the ordinary eye may be inexact and usually are; the notion of "shading" and "merging into" are important in, for example, distinguishing colours. An empirical continuum for example is continuously connected: "the structure of this connection is grounded in the indefiniteness of the connected parts or the inexactness of the connected classes". In a mathematical continuum a fundamental assumption is that "the elements of a continuous series are exact classes or definite individuals". Classical logic has no room for inexact classes. Ordinary observation, perception of the world reflects this inexact-
ness in the sense that "neutral candidates" can always be found where the "ordinary eye" does not distinguish between certain colors but where distinctions can nonetheless be made. "Theoretical predicates incorporated into a theory embedded in logic are exact and thus not instantiated in perception". Briefly speaking, Korner argues, "theoretical and empirical discourse are logically disconnected". The replacement of inexact empirical predicates with exact non-empirical predicates constitutes an idealization, rather than a description. A further idealization of experience is made when under the aegis of a particular theory, perceptual characteristics grounded in empirical reality are not considered relevant to that theory:

...in the theory of classical mechanics, the individuals, its particles are unlike things in possessing only mass and spatio-temporal position relative to a fixed origin. It is not simply that the presence of irrelevant characteristics is ignored, their absence is postulated.

A further assumption enforced by classical logic is the requirement that the individuals to which the predicates are applied are to be unchangeable, although no system of empirical propositions logically implies the existence of permanent empirical individuals. "Every substantive theory requires a fixed domain of permanent individuals."

Theories have as their subject matter a domain of phenomena and they are developed to answer a variety of questions about these phenomena. Some of these questions are requests for
descriptions, explanation and predictions about the behaviour of these phenomena. Certain parameters are adopted in a theory from which attempts are made to describe these phenomena. The adoption of certain parameters is influenced by the domain of the phenomena, the intended scope of the theory and the sort of questions it is supposed to answer. The theory assumes that the isolated phenomena are influenced only by the selected parameters. In actual fact this need not happen and in fact seldom occurs. What the theory then provides, given it is an adequate theory, is an idealized characterization of an isolated system and what the theory actually characterizes is not the actual phenomena within its scope, but idealized replicas. For a theory to be adequate the generalizations used to describe the "idealized replicas" must be true and these generalizations must also be capable of providing the answers the theory is committed to answer. Suppe argues:

It is perfectly conceivable that the proponents of different competing theories may disagree on what sorts of questions ought to be answered by an adequate theory for a particular range of phenomena, the theories being introduced to deal with different sets of questions; in such a case different facts will be relevant in assessing the adequacy of the competing theories.\textsuperscript{35}

If one assumes that one's prior knowledge and beliefs influence what one is able to correctly observe, it follows that differences in one's knowledge and beliefs affect the facts one can observe to be the case. Persons accepting different theories for a given range of phenomena thus may be able to
observe facts differently and disagree on what the facts are which a theory must accommodate. Once it is seen that theories can limit their descriptions of phenomena within their intended scope to "idealized replica's" in terms of selected parameters, one allows for the possibility of different empirically true theories for the same intended scope. The assessment of the truth of the generalizations describing those "idealized replicas" must be based on data obtained from experiments and this involves theories of experimental control and design; "there is room for legitimate disagreement over which experimental data represents (such) isolated systems and is relevant to the assessment of the theory's generalizations". 36 There is also no rational canon for determining which questions ought to be asked about a given range of phenomena. As a consequence there is no standard criterion for assessing competing theories: to argue so "would be to assume there was some objective criterion for deciding which questions ought to be asked". 37

The idealized replicas are linked to perception through 'as-if' identifications and may provide a source through which subjective factors enter. Korner in "Science and Belief", provides an example. "Einstein, while granting the predictive efficacy of classical quantum mechanics continued to search for a causal theory which would make the phenomena covered by quantum mechanics not only predictable, but also intelligible". 39 Einstein, in a sense refused to
complete the as-if identifications. In his view the statistical explanation of quantum phenomena distorted the causal and true nature of the actual phenomena. In this view it is clear that subjective factors may play a role in the various judgments which are made. In Chapter Two I discussed what is involved in "testing by experience". In "mediating" between actual experience and the "idealized replicas" representing certain aspects of that experience, judgments have to be made with reference to "the need for systematic coherence" and "the retention of the original link" with the roots of the "constructed intellectual objects" which carry "a complex burden derived from (their) history of use and adaptation". These judgments are made not through an "appeal to the facts of nature" but "by a process of informal thinking". This is not to argue that the "roots of the intellectual objects" can be traced, conceptually, to their origins in experience. It is to argue, however, that the deductive perceptual abstractions enforced by the logical apparatus of a substantive scientific theory are not irrelevant. The idealized replicas retain these perceptual "umbras and penumbras". Theory change may force a different set of deductive abstractions and may bring to the fore neglected aspects of those "umbras and penumbras". It will not do to argue that scientific knowledge must still be made whether idealized replicas can be seen to be consistent, for the purposes at hand, with actual perceptual experience.
The judgments made with the "as-if" identifications and the perceptual abstractions would lose their significance if it could be argued that the language of science is a closed and coherent system, independent of ordinary language. In that case it would be in principle possible to explicate all the presuppositions we have when we make the "as-if" judgments. Amsterdamski argues that "our knowledge never constitutes a closed and coherent system". Knowledge without presuppositions is impossible "since it is not provided either by experience or by the mind". Amsterdamski places the source of the presuppositions on which knowledge is based, but which cannot be explicitly stated, in the cognitive outlook of man upon the universe and upon himself. Global revolutions in science are characterized not only by the introduction of a new order in the sphere of human experience, but also by a new conception of "the cognitive experience". The conceptions of man as a knowing subject "codetermine...with the concepts that are necessary for the ontological characterization of the structure of the universe as an object for cognition...the common frameworks within which the cognitive effort is undertaken". One can argue, in my view that the conceptual framework of the cognitive experience finds its source in the form of life, where "common conceptual aspects, patterns of thought are rooted in the form of life, ways of being and doing in a human world". A "form of life" need not be static, it may be
malleable. Whichever direction this malleability will take, there will always be "a network of activities which grants a certainty and trustworthiness which is not a way of knowing but a way of being".45

It may be objected that this manner of speaking puts the question of "relativism" beyond the scope of enquiry. To a limited extent this is the case. To a limited extent it is unsatisfactory to posit the origin of a non-innocuous sense of "conceptual framework", "rules of thought" etc. in a realm where language and enquiry have no purchase. It seems to me however, that one's view of how language hooks on to the world is not an empirical question but rather "a first move to be made". If anything the view I advocate is closer to the actual history of science. With Kuhn it may be argued that facts are not solely arrived at through theoretical considerations but "gell" in a shared conceptual framework. This conceptual framework in my view does not find its origin in an exclusive scientific world-view. The acceptance of a common conceptual background against which facts are interpreted accounts for the historical phenomenon that scientists cannot agree on the interpretation of the facts even though they may agree that they are dealing with natural phenomena. Although the notion of presuppositions underlying a conceptual framework may appear opaque, it does not remain so. Revolutions in thought do occur and will, in a sense, reveal these assumptions which have facetly guided the "cognitive experience" of man.
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NOTES

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