2.1 POPPER'S THEORY OF UNIVERSALS

Popper's thesis of the theory-ladenness of observation can be formulated as consisting of two major assertions: (i) All universal concepts (general terms) both in science and in everyday language are 'theoretical' in the sense that the application of these terms in empirical contexts depends upon a law or a theory. There is no distinction in this respect between singular observation statements and universal statements of law. Since both contain universal terms both are 'theory-impregnated', albeit in varying degrees (ii) Since 'theoretical' concepts i.e. universal concepts are intensionally defined by laws or theories, these concepts cannot be correlated with anything given in experiences; moreover the theories which constitute the principle of application of universal terms imply ampliative inference to future and counterfactual behaviour, which is unwarranted; hence no statements in science, in particular no basic statements even, can be justified with reference to facts or experience.

The crucial statement of Popper's position is found in 'The Logic of Scientific Discovery'. Referring to the view that science is the systematic presentation of our sense-
experiences, Popper ([1972] p. 94) maintains: "This doctrine founders in my opinion on the problems of induction and of universals. For we can utter no statement that does not go far beyond what can be known with certainty 'on the basis of immediate experience' (This fact may be referred to as the 'transcendence inherent in any description). Every description uses universal names (or symbols or ideas); every statement has the character of a theory or a hypothesis. 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 (an 'immediate experience' is only once 'immediately' given; it is unique). By the world 'glass' for example, we denote physical bodies which exhibit a certain law-like behaviour; and the same holds for the word 'water'. Universals cannot be reduced to classes of experiences, they cannot be 'constituted'.

Popper's argument for the theory-ladenness of universal terms assumes the form of a criticism of the nominalistic theory of meaning which construes non-logical predicates as names. This view interprets universals as 'extensionally or enumeratively' defined; but according to Popper such a theory of meaning is totally inadequate for the language of science because statements in an extensionalist language can only be analytically true or false. Thus Popper says:
nominalism is the doctrine that all non-logical words are names - either of a single physical object, or shared by several such objects. This view may be said to interpret the various words extensionally or enumeratively; their 'meaning' is given by a list or an enumeration of the things they name. We may call such an enumeration an 'enumerative definition' of the meaning of a name; and a language in which all (non-logical) words are supposed to be enumeratively defined may be called an 'enumerative language' or a 'purely nominalistic language'. Popper goes on to maintain that such a purely extensionalist language is useless for science because the truth or falsity of its sentences can be decided simply by comparing the defining lists or enumerations i.e. as soon as, the words occurring in it have been given their meaning. Popper concludes that the language of science 'must make use of genuine universals i.e. of words, whether defined or undefined, with an indeterminate extension, though perhaps with a reasonably definite intensional meaning. This intensional meaning of all 'genuine non-extensional universals' is determined according to Popper, by theories.

1. The nominalist theory of meaning will be revived in Ch.III in connection with the new theory of reference; (of Kripke, Putnam et al); wherein Popper's criticism will be considered in greater detail.

2. The above discussion makes it clear that Popper is opposed to meaning analysis only in the sense of extensionalist meaning'. Since the intensional

contd.
Popper [1969] emphasizes this point by maintaining that 'all universals are dispositional'. Again, Popper [1983] counters Berkeley's contention that scientific theories are only instruments (for calculation) because scientific concepts are 'occult' or (extensionally) meaningless, by the assertion that all universal concepts, including those in ordinary language, are occult or abstract in precisely Berkeley's sense. Popper reiterates 'all universal terms incorporate theories'. These arguments lead to Popper's first thesis vis. that concepts in science cannot be correlated with anything given in experience.

meaning is determined on Popper's own view, by scientific theories, criticism of this 'theoretical meaning' should be legitimate. This point is often obfuscated by Popper's use of 'meaning' to cover only the 'extensional meaning' of universal terms. It may be clarified by reference to Popper's ([1972] p. 441) statement: 'I may perhaps sum up my position by saying that, while theories and the problems connected with their truth are all important, words and the problems connected with their meaning are unimportant'.

3. The criterion of a term being dispositional is law-like behaviour under certain conditions. Popper thinks that the problem of (operationally) defining dispositional terms is insoluble. It is insoluble both because sentences containing dispositional terms are open to an indefinite number of tests; (i.e. the testing of such sentences is inconclusive) and also because the attempt at definition leads to circularity). Thus the sentence 'x is soluble in water' can be tested by dissolving it in water; then recovering it, and so on. Also the result of the test is stated using the term 'water' which itself is dispositional and has to be tested by substances that dissolve in it. This leads to circularity. Popper concludes that attempts at explicit definition in terms of operational tests are futile.
The doctrine that science is founded on our sense-experiences also founders, according to Popper, on the problem of induction. Since the intension of terms in science is given by empirical laws and theories, the application of these terms in empirical contexts involves ampliative inference to law-like behaviour. Thus the statement 'Here is a glass of water' is open to an indefinite and inexhaustible number of tests-chemical tests for example—because water like anything else, is recognizable only by its law-like behaviour. But ampliative inference based on laws and theories would be justified only if laws could be established as true, or at least as probable. However Popper's criticism of induction and of neo-justificationism establishes that empirical laws can never be conclusively verified or even rendered probable. Hence ampliative inference based on these laws is unjustified. Popper concludes ([1972] p. 424) "... since every law transcends experience.... which is merely another way of saying that it is not verifiable—every predicate expressing law-like behaviour transcends experience also: this is why the statement 'this container contains water' is a testable but non-verifiable hypothesis transcending experience'. It follows that no statement in science, in particular no basic statement even, can be justified by experience. To the anti-nominalist and the anti-inductivist arguments, Popper appends one more—the anti-psychological
argument: since statements can be justified only by statements no statements can be justified by experience. This is because the relationship between experience and statements is causal not logical, whilst justification can only be a logical concept. In this connection Popper ([1972] p. 105) maintains: 'I admit, again that the decision to accept a basic statement, and to be satisfied with it, is causally connected with our experiences - especially with our perceptual experiences. But we do not attempt to justify basic statement by these experiences. Experiences can motivate a decision; and hence an acceptance or a rejection of a statement, but a basic statement cannot be justified by them - no more than by thumping the table. Popper himself does not elaborate on this position, but if this argument is analysed further, it reveals an enthymemetic premise whose implications cut both ways: Causal laws are not equivalent to logical principles because causal laws cannot be verified by induction. If a causal law could be so established as conclusively true, it would exemplify a Humean necessity (i.e. of universal concomitance) which Popper would perforce have to acknowledge as equivalent to logical i.e. truth functional necessity; for (as is clear from Popper's [1972] discussion of William Kneale's criticism of his position). Popper himself recognizes no principle of necessity apart from Hume's principle of universal concomitance. Hence if
empirical laws could be established as true, there would be no distinction in principle, between these true statements of causal connections, and the principles of logic; i.e. between causal and conceptual (logical) necessity. But universal statements cannot be verified as true, because of the invalidity of induction. Hence a sharp distinction must be drawn between logical and causal connections; accordingly experience which is only causally related to statements cannot logically justify these statements. The anti-psychologistic argument is thus at heart, the anti-inductivist argument, this time in relation to singular statements; and Popper's version of the analytic-synthetic distinction can be seen to be defended on anti-inductivist grounds. 4.

Popper's argument from logic however, cuts both ways. Since no causal law in general, and the causal connection between experience and statements in particular, can ever be verified, the relationship between experience and statements is not a logical one. This means that experience does not conclusively (deductively) imply any statement.

4. This analysis is explicitly supported by Popper's [1983] criticism of the Positivists' interpretation of scientific theories as inference-tickets. There Popper says quite clearly that the problem of the truth of a universal statement is exactly equivalent to the question of its validity as a (logical) principle of inference; and that therefore (because induction is invalid) nothing is gained by replacing the one formulation by the other.
Therefore, Popper argues that experience cannot justify statement because justification is a logical concept. But then refutation is also a logical or syntactic concept which can hold only between statement and statement and not between statements and experience precisely because of the causal i.e. non-deductive relation between observation and (basic) statements. Falsifications of basic statements therefore, which do not follow conclusively i.e. deductively from experience cannot constitute tests for these statements. Popper ([1972] p. 104-105; [1983] p. 109) would therefore appear to be wrong in insinuating that basic statements can be tested albeit inconclusively against experience. The point is that in terms of deductive logic (which is the only kind that Popper recognizes; and upon which he bases his causal-logical distinction) inconclusive testing is no testing at all. Inconclusive testing does not constitute refutation, any more than does inconclusive verification constitute justification (of either basic statements or of theories). It follows that basic statements cannot be either justified or refuted (tested) by experience; and science is therefore not even negatively under the control of experience.

2.2 The Bedrock of Conventionalism

The deeper implications of Popper's thesis of theory-laden observation can be coherently analysed as ensuing from
the form his own arguments take. Thus (1) the anti-
inductivist argument (which encompasses the anti-
psychological or anti-causal argument) leads to the
abandonment of the principle of empiricism; and an
inexorable convergence upon the methodology of
conventionalism. This is emphasized by Lakatos. (2) At the
same time, Popper’s rejection of the nominalist or
extensionalist theory of meaning (of general terms), and his
construal of intensional meaning as determined by empirical
laws/theories leads to Feyeraband’s thesis of meaning
variance and of incommensurability. These implications are
elaborated in the ensuing sections.

Lakatos [1976] emphasizes the role of convention or
agreement (according to accepted procedures) in Popper’s
development of the falsificationist position. Since the
thesis of theory-ladenness undercuts the empirical basis,
decision by convention or consensus is required to (1)
delimit the set of basic statements. (2) Delimit the set of
accepted basic statements. (3) The ‘unproblematic’
background theory which in the context of testing a
hypothesis functions as the ‘observational theory’ or theory
of observation. (4) Decisions rendering statistical theories
falsifiable. (5) In regard to theories hedged in by a
ceteris paribus clause; when the combination is refuted,
decisions are required to direct the arrow of the modus
tollens to either the specific theory under test or to any
of the unspecified ceteris paribus conditions.

As regards (1) and (2), Lakatos follows Popper in repudiating the *naturalistic doctrine of observation* viz. the assumption that there is a natural, psychological borderline between theoretical or speculative propositions on the one hand and factual or observational (or basic) propositions on the other hand. This doctrine follows from the tabula rasa theory of mind of classical empiricism (and from the extensionalist theory of meaning which distinguishes between 'observational' concepts and 'theoretical' concepts); but it ensues from the arguments of Kant and of Popper [1969] that there can be no sensations unimpregnated by expectations, by theories, hence there is no natural demarcation between observational and theoretical propositions. The delineation of the set of basic statements therefore requires conventional assent which encompasses the 'observational' theory which constitutes the principle of application of terms in these statements. Moreover since no empirical theories, in particular no corroborated theories even, can be construed as true or even as probable, the choice of observational theories must perforce be arbitrary. Again since the truth-value of basic statement, cannot be established indubitably, (because the relationship between experience and statements is only causal not logical) basic statements must be accepted by consensus.

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Lakatos argues that these methodological decisions reduce the concept of falsifiability to mere inconsistency between the observational theory and the hypothesis under test. Popper ([1972] p. 111) himself emphasizes "science does not rest upon a solid bedrock. The bold structure of its theories rises, as if were, above a swamp'. But Lakatos ([1976] p. 220) carries the argument much further. "This 'basis' can be hardly called a 'basis' by justificationist standards. There is nothing proven about it - it denotes 'piles driven into a swamp'. Indeed if this 'empirical basis' clashes with a theory. The theory may be called 'falsified', but it is not falsified in the sense that it is disproved. If a theory is falsified, it is proved false; if it is 'falsified' it may still be true. If we follow up this sort of 'falsification' by the actual elimination of a theory, we may well end up by eliminating a true, and accepting a false theory'.

What Lakatos is trying to get at is that theory dependence reduces the concept of falsifiability (and of verifiability) to a merely syntactic notion of the relationship between theory and theory (rather than between theory' and experience). Hence the problem is not the (potential) clash between theory and facts; but rather the clash between two high-level theories: between an interpretative theory to provide the facts and an
explanatory theory to explain them. Since neither theory is warranted by experience (since no theory is validated by experience). Lakatos thinks Popper is wrong to construe such a refutation as 'real' and to reject the hypothesis under test as falsified. Such 'falsification' amounts merely to ratifying an 'empirical basis' which is only established by convention.

The same conclusion emerges when we consider Popper's comparison of his own empiricist methodology with that of the conventionalist. Popper ([1972] p. 72-84) maintains that theoretical systems which are axiomatically articulated can be interpreted either (a) as conventions or (b) as empirical systems. This distinction turns on the decision to treat non-logical terms as either implicitly defined by the axioms or as defined 'empirically'. At the same time Popper admits that owing to the problem of theory-ladenness the notion of 'empirical' definition of universal terms is fraught with difficulties. He, therefore rests his case (for a empirical interpretation) on the decision 'to adopt a rule not to use undefined concepts as if they were implicitly defined' (Popper [1972] p. 75). But this rule is then further interpreted as the explicit definition of concepts of an axiom system in terms of 'a system of lower-level universality' whose concepts in turn are established by 'usage'.

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Theorin are true because they are accepted. It is not what they are accepted because they are true.

In fact, it is for considerations instrumentalism to be a degenerate version of conventionism. Cf. Fabričekan and methodology.
The regression to conventionalism is clear; for 'usage' is always a matter of linguistic convention. Again, since all concepts including every-day concepts established by usage are theory-laden, the theories with which they are laden i.e. the theories which constitute their principle of application, must also be a matter of acceptance by convention. Furthermore, on pain of infinite regress (or else of circularity) some theories must implicitly define their concepts, thus rendering these theories tautologically or analytically true. This converges upon the position of methodological conventionalism.

This argument can be further elucidated as follows: According to Popper ([1972] p. 78-84) the conventionalists (chiefly represented by Poincare' and Duhem) hold scientific theories to be analytically or tautologously true, in the manner of definitions. In fact, Popper classifies conventionalists with instrumentalists because Poincare' [1976] regards theories as both irrefutable and (more strongly) as having no physical meaning; whilst Duhem ([1976] p. 1-40) considers that 'the sole purpose of physical theory is to provide a representation and classification of experimental laws'. Thus Popper says: 'According to the conventionalist point of view, laws of nature are not falsifiable by observation, for they are needed to determine what an observation and more especially, what a scientific measurement is'. The conventionalist
position, according to Popper, cannot be faulted on grounds of logic; only it betrays a conception of, and aims for science, which are at variance with Popper's own views. Popper, therefore proposes to combat conventionalist methodology with his own empiricist (falsificationist) methodology; the fundamental rule for which is to desist from the implicit definition of concepts. But owing to theory-ladenness the attempts at explicit definition culminate (as has already been noted) only in conventionally delineated 'observational concepts', some of which must perforce be implicitly defined by theories which are thus rendered tautologous in precisely the conventionalists' sense of the term. We must conclude that the falsifiability (of 'higher-level theories') is relative to conventionally held ('lower-level') 'observational theories'; and that Popper's empiricist methodology therefore rests on a bedrock of conventionalism.

It is important to note that this thesis of conventionalism of the 'empirical basis' is not the trivial one of linguistic usage; but concerns the methodological issue of the role of observational theories in the context of testing. It is suggested that the thesis of theory-ladenness leads to a conception of this role as the semantic one of implicitly defining the terms of the hypothesis under test. This renders observational theories as tautologously true in the context of testing.
2.3 The Duham - Quine Thesis of Holism

The foregoing analysis elucidates a controversy which has dogged methodological falsificationism over several decades. It was initiated by Pierre Duhem ([1976] p. 1-40) who maintained that the falsification of a hypothesis is never free of ambiguity because hypotheses in science are never tested in isolation, but only as a system; so that if the combination is refuted, no one of the group of hypotheses can be singled out as responsible for the erring prediction. Duhem, therefore argued that the falsification of individual hypothesis is inconclusive. Moreover since hypotheses cannot be individually verified either, Duhem concluded that only scientific systems in their entirety can be characterized as empirical. Duhem was concerned primarily with the testing of theories in physics which necessarily involve instruments and hence the 'theory of the instrument'. But since all natural sciences invoke instrumental techniques to a greater or lesser extent, for testing their theories, Duhem's holistic thesis embraces the corpus of natural science. Duhem also adduces to his argument from the logic of the experimental situation, examples drawn from the corpus of theoretical physics.

Quine ([1976] p. 41-64) extends Duhem's argument radically when he maintains that it is only systems as a whole, including the laws of logic that collectively face
the tribunal of sense-experience. Quine's defence of this radical conventionalist thesis assumes the form of a criticism of the analytic-synthetic distinction, including the attempt to base the distinction on the verification theory of meaning. Quine's thesis and its criticism (especially by Strawson) are too well-known to require representation. Instead certain points relevant to the argument being developed in this thesis can be made: If, Quine is looking for a naturalistic interpretation of the analytic-synthetic distinction, he is not liable to discover it. This is because the distinction is *lingistic*, it marks the semantic role that certain statements play in specific contexts. In particular, observational theories in the context of testing hypotheses are analytic because they implicitly define the terms of the hypothesis. If this role and the distinction based on it are denied, then neither refutation nor verification goes through. For if the results of experiment (which are interpreted by the observational theory) are questioned, then there are no acceptable results to compare with the predictions from theory. Refutation therefore is either conclusive or else it does not go through. Observational theories therefore, in the context of testing are irrefutable i.e. analytic; and falsifiable theories are synthetic with respect to these. The analytic-synthetic distinction therefore is contextual not nonexistent.
This position will be developed and reflected in the re-interpretation of the structure of *modus tollens* argument in science. But first we consider Popper's response to the Duhem - Quine thesis. Popper ([1972] p. 75-77) agrees (mistakenly) that it is a system of theories to which the *modus tollens* of classical logic applies. The form of the falsifying inference is \((t \rightarrow p, p) \rightarrow t\) or in words 'If \(p\) is derivable from \(t\), and if \(p\) is false, then \(t\) also is false'. Popper says: 'By means of this mode of inference we falsify the whole system which was required for the deduction of the statement \(p\). i.e. of the falsified statement. Thus it cannot be asserted of any one statement of the system that it is, or is not, specifically upset by the falsification...'. Nevertheless Popper distinguishes between *levels* of universality; new higher-level hypotheses might relatively safely be regarded as falsified in relation to well-corroborated lower-level hypotheses.

Popper [1972] also considers the conventionalist objection that no theoretical system is ever conclusively falsified for it can always be saved by the logically admissible procedure of 'introducing ad hoc auxiliary hypotheses, by changing ad hoc a definition, or by simply refusing to acknowledge any falsifying experience whatever'. Popper proposes that 'the empirical method shall be characterized as a method that excludes precisely those ways of evading falsification which are logically
admissible'. Thus Popper argues that it is not *systems* but
*methods* which are empirical/non-empirical.

Later Popper ([1969] p. 238-239) argues specifically
that the Quine - Duhem thesis, 'the holistic view of tests'
both 'does not create a serious difficulty for the fallibilist
and the falsificationist' and also that 'on the other
hand... the holistic argument goes much too far'. Evidently
he thinks it does not create a difficulty since while the
falsificationist does take for granted a vast amount of
traditional knowledge.

He does not accept this background knowledge as
established, nor as fairly certain, nor yet as probable. He
knows that even its tentative acceptance is risky, and
stresses that every bit of it is open to criticism even
though only in a piecemeal way. We can never be certain
that we shall challenge the right bit; but since our quest
is not for certainty, this does not matter...... Now it has
to be admitted that we can often test only a large chunk of
a theoretical system and sometimes perhaps only the whole
system; and that, in these cases, it is sheer guess work
which of its ingredients should be held responsible for any
falsification'.

However the holistic argument goes much too far since
it is possible in 'quite a few cases to find which
hypothesis is responsible for the refutation; or in other
words, which part or group of hypotheses was necessary for the derivation of the refuted prediction.

Sandra Harding ([1976] p. xv) queries: Does the fact that in some cases scientists reach intersubjective agreement, at least temporarily, as to which of their theories to revise, save Popper’s falsificationism from the Duhem-Quine thesis? The latter would appear to challenge not this uncontroversial sociological fact but the notion that it is tests which \textit{logically determine which} part of our web of hypotheses and beliefs should be counted as refuted. Harding concludes: we must still ask how Popper has succeeded in deflecting the challenge posed to his falsificationism by the ‘holistic view of tests’.

In several publications Adolf Grunbaum ([1976] p. 116-131, 260-288) has challenged Duhem’s thesis that the falsifiability of an isolated empirical hypothesis is unavoidably inconclusive. According to Harding Grunbaum regards the Duhemian thesis as a conventionalist ploy to be found also in Einstein, Poincare’ and Quine. In his [1960] essay, Grunbaum argues that the Duhem-Quine thesis is both a logical non-sequitur and furthermore false. First he argues that conclusive falsifying hypotheses are possible. To deny this, Grunbaum says, Duhem would have to prove on general logical grounds that for any empirical finding (e.g. \( \sim 0 \)) there is a set of non-trivial auxiliary hypotheses from
which, together with the target hypotheses in question, the findings could be deduced. But this Duhem cannot guarantee. Thus Duhem committed a logical error. But furthermore, Grunbaum thinks that the history of science reveals that conclusively falsifying experiments have been accepted as a matter of fact; and Grunbaum thinks this refutes the Duhemian thesis. In his response Quine himself suggests that he finds the Duhem-Quine thesis as challenged by Grunbaum tenable only if taken trivially.

Harding points out that several philosophers have been provoked by the Duhem-Quine-Grunbaum-Popper controversy to partisan stands. Laurens Laudan, Carlo Giannoni and Gary Wedeking all emphasize that there are two versions of the Duhem-Quine thesis. There is a stronger one held not by Duhem but probably by Quine; the weaker one, actually held by Duhem, is untouched by Grunbaum’s attack.

Laudan points out that Grunbaum wrongly presumes that the burden of proof is on the scientist who refuses to indict a particular hypothesis (when the combination is refuted) to show that his hypothesis can be saved by some suitable auxiliary hypothesis. But Duhem did not make this strong claim but only the weaker one that those who deny the target hypothesis must show that there does not exist an auxiliary hypothesis which would make the target hypothesis
compatible with the unforeseen experimental results. Unless such a proof is forthcoming, Laudan emphasizes, 'a scientist is logically justified in seeking some rapprochment between his hypothesis and the uncooperative data'. Laudan also points out that Grunbaum's purported counter-examples involve sets of theories and not isolated hypotheses; Moreover Grunbaum's assumption that in a particular case the auxiliary hypothesis being highly probable forces a scientist to relinquish the target hypothesis, is fallacious; for 'highly probable' 'the demands of prudence do not carry logical weight'.

Carlo Giannoni's ([1976] p. 162-175) interpretation of Duhemian conventionalism is of particular interest from the point of view of the argument being developed in this thesis. Giannoni considers the broader implications of the Duhemian thesis for our conception of scientific knowledge. According to him the Duhemian thesis is not an epistemological thesis regarding our knowledge of the world but a semantical thesis regarding the meaning of scientific words and of scientific language. But to say this is not to trivialize the issue, he thinks, for the thesis is required

5. It might be pointed out that Duhem would insist that given conceptual ingenuity such a proof is impossible. This is because Duhem, unlike Popper, does not require of auxiliary hypotheses that they be well corroborated. In fact Duhem emphasises that auxiliary hypotheses actually used in science are often untested and sometimes even physically meaningless.
by the very notion of scientific discovery.

The main argument can be elaborated thus: Giannoni first agrees with Sandra Harding that whilst Quine's criticism of the analytic-synthetic distinction is vigorous, his criticism of the true-false dichotomy is relatively thin. In fact it is Hempel ([1974] p. 65-88) who, according to Giannoni, provides some of the philosophical underpinning for the Quinean thesis. That our statements about the external world face the tribunal of sense-experience not individually but only as a corporate body. Because of the problem of theoretical terms Hempel arrives at the same conclusion as Quine viz. that the unit of empirical significance is not the terms of statements of science, but the theories of science. Hempel's argument is that since the theoretical terms of science cannot be explicitly or operationally defined, they must be introduced by the theories themselves. The theory consists of several statements which partially define the term implicitly; also 'observational' consequences can be deduced from the theory which bestow upon it (collectively) an empirical significance. But since each statement of the theory individually contains theoretical terms which are only partially defined implicitly; these individual statements cannot be tested in isolation but only in conjunction with the other theories which define these terms. Therefore, theories by virtue of containing theoretical terms must face
Giannoni extends Hempel's analysis to the part of science (theoretical physics) with which Duhem himself was particularly concerned viz. physical theories involving measurements, and in particular derivative as opposed to fundamental measurements. In regard to these Giannoni points out that the operations involved only partially define the concepts. For the intensional meaning or the principles of application of these concepts is given by the 'theories of the instruments' which are themselves natural laws. Being causal laws, they hold only under certain (unspecified) conditions i.e. on the condition that there are no perturbing influences. Now we can never be certain in any particular case that this condition is satisfied. Hence if the experimental findings are negative, the blame could be attributed as much to these auxiliary hypotheses (which constitute the principle of application of the terms) as to the hypothesis under test. Falsification in science is ambiguous therefore, precisely because and to the extent that terms in science are theory-laden. Therefore Giannoni considers the Duhemian thesis to be fundamentally a semantic thesis although it has epistemological and ontological consequences and is therefore non-trivial.

6. The ontological consequences of the 'Duhemian thesis hinge, according to Giannoni, on the decision to adopt either a nominalistic or a realist approach to the symbols (terms) of science.
Giannoni's basic argument viz. that falsification is ambiguous because the theories with which terms in science are laden are natural laws - is flawed by the conflation of a distinction which Duhem himself first makes and then confuses: Thus Duhem ([1976] p. 4-5) distinguishes between experiments of application and experiments of testing. He says: 'You are confronted with a problem in physics to be solved practically; in order to produce a certain effect you wish to make use of knowledge acquired by physicists; you wish to light an incandescent bulb; accepted theories indicate to you the means for solving the problem; but to make use of these means you have to secure certain information; you ought, I suppose, to determine the electromotive force of the battery of generators at your disposal. You measure this electromotive force, that is what I call an experiment of application. This experiment does not aim at discovering whether accepted theories are accurate or not; it merely intends to draw on these theories. In order to carry it out you make use of instruments that these same theories legitimate; there is nothing to shock logic in this procedure. In experiments of testing however when a physicists doubts a certain law/hypothesis, to justify these doubts he derives from the hypothesis (under a cloud) certain experimental consequences. This derivation invokes laws from the corpus of physics, mathematics etc. both as (explicit) premises and as (implicit) principles of
deduction. If the combination is refuted then according to Duhem no isolated hypothesis can be indicted. Hence in experiments of testing falsification is ambiguous.

The fundamental question now arises: in what way, logically and physically (i.e. in the physical conditions of experimentation) do experiments of application differ from experiments of testing. Both invoke a corpus of accepted theories\(^7\), both could fail. Thus in the example cited from Duhem, the incandescent bulbs might fail to light. But such a failure would be imputed by Duhem, not to the falsity of 'accepted' theories but only to the inapplicability of the conditions which define the experiment. Yet in the context of the testing of (new) hypotheses Duhem interprets refutation as the possible falsity of auxiliary hypotheses. This inconsistency arises from Duhem's failure to realise that what distinguishes the experiment of application from the context of testing is a difference in epistemic (or conventional) attitudes: Experiments of application invoke 'accepted' theories, whereas in the experiment of testing there is always a hypotheses under test. But then there is

\(^7\) Duhem [1976] does argue that many of the theories invoked as premisses or as principles of inference in an experiment of testing are themselves untested, untestable or even physically meaningless. But he misses the point that unless these auxiliary hypotheses are granted the derivation does not proceed and the inference does not go through. 'Acceptance' in the context of both experiments of application and of testing marks a logical distinction whether or so.
also in the latter case a corpus of accepted theories, which in the context of testing is treated as 'unproblematic' background knowledge'. The distinction from epistemology (or from convention), therefore does not merely demarcate the context of testing from the context of application. It runs right through the heart of the experiment of testing, isolating the hypothesis under test, from the rest of the corpus of 'accepted theories'; and making it a sitting duck for the arrow of the modus tollens. If Duhem can find nothing in logic to shock in the inferential procedures adopted by experiments of application which invoke only 'accepted theories', then he must grant similar licence to that part of the system of theories which also invokes only 'unproblematic' background knowledge.

But an even stronger case than the argument from epistemic attitudes, can be suggested. The distinction between application and testing (of theories) is logical - it marks a difference in semantic function between hypotheses under test and 'accepted theories'; the distinction renders these background theories as necessarily true in the particular context of testing. To appreciate this we have only to keep in mind that (owing to theory-ladenness), the semantic function of background theories is to constitute the principle of application of terms involved. This means that in the context of testing a hypotheses, the background theories are in that context
irrefutable or analytically true, because they define in that context what constitutes an observation or instance of the hypothesis (under test). These theories themselves therefore can, in the semantic context that they define, encounter no counter instance. Within this context it is only the hypothesis under test that is vulnerable to falsification because it is dissociated from the semantic function of defining the theoretical terms. This precisely is the point of Popper's methodological injunction; to wit, that the axioms (of the theory under test) must not be construed as implicitly defining the meanings of theoretical terms.

The foregoing analysis suggests that the structure of modus tollens inference in science is exemplified by the following schema:

1. \( H \land A \rightarrow A \land P \) (where \( H \) is hypothesis,
2. \( A \land \overline{P} \) A the auxiliary assumptions,
3. \( A \land \overline{P} \rightarrow \overline{H} \) and \( P \) the prediction).

This formulation emphasizes that falsification goes through only relative to epistemological assumptions, whose semantic function renders them tautologically (conventionally) true in the context. If these assumptions are not granted, then the inference simply does not go through, so that inconclusive falsification is a logical non sequitur. We must conclude that falsificationism is contextual, and that the empiricist
methodology embraces a bedrock of conventionalism. Gerard Radnitsky's review of Andersson [1991] suggests that Andersson endorses this interpretation of the structure of modus tollens arguments in science. According to Radnitsky, Andersson considers it appropriate to view a falsificationist argument as an argument whose premisses consist of the antecedent conditions A and a negated unconditional prediction. This interpretation is preferable to the customary one according to which the premisses consist of a simple basic statement, mainly (but not exclusively) because thereby the relationship between falsification and the deduction of predictions is clearly shown: \( 'A, H \rightarrow P' \) being metalogically equivalent to \( 'A, \overline{P} \rightarrow \overline{H}' \). Hence, Radnitsky considers Andersson's explication of the concept of a falsifying argument to be wider than Popper's. The point however is not of greater generality, but of incorporating epistemic presuppositions into the formal structure of the inference.

2.4 The Incommensurability Syndrome

In point of fact although Popper's formal schema (of the modus tollens argument) does not reflect this, his general position especially in later years, has always stressed that the growth of knowledge takes place only against a theoretical background of accepted belief. Thus Popper ([1983] p. 153-157) maintains that Kant was right in
teaching that any growth of knowledge needs a theoretical framework which must precede the growth. But Kant was wrong, according to Popper, in believing that this conceptual framework could not possibly be transcended in its turn. Again Hegel was right in pointing out that the framework too was subject to growth, and could be transcended. But he was wrong in suggesting that truth is essentially relative to some framework; and that it is not our active criticism which forces a change in ideas and belief; rather than the criticism being dependent upon an independently evolving framework.

The latter view leads according to Popper, to the 'myth of the framework' i.e. to the pessimistic Kantian doctrine that we are hopelessly enslaved by the conceptual framework we (physiologically) inherit. A special form of this philosophy of human bondage is linguistic relativism (à la Benjamin Lee Wharf [1956]): the thesis that human languages incorporate in their structures, beliefs, theories and expectations; from whose ideological fetters we cannot break out by criticism, because all criticism presupposes language. But Popper ([1983] p. 15-30) thinks that although the thesis of theory-ladennes indicates that we cannot do altogether without some form of theoretical framework; yet (relative) freedom can be attained through criticism, both immanent and transcendent, of the most varying frameworks. He also thinks moreover, that (Popper [1983] p. 57). "There
is no reason whatever to think, as some people do, that, Wharf or anybody else, has shown the incommensurability of sets of beliefs (or that all assertions are relative to irreducibly different sets of fundamental beliefs'). Yet when we bring to bear upon the issue the foregoing analysis of the semantic function of conceptual frameworks (i.e. of the background 'observational theories') an analysis moreover to which the thesis of theory-laden observation inexonably leads; then it is clear that shifts in framework from one epistemic context to another, do involve semantic variance and incommensurability. This is Feyerabend's thesis.

Feyerabend [1976] emphasizes that (1) firstly, the choice of an observational theory is arbitrary. It usually indicates an irrational preference for the 'older entrenched' theory and an equally unwarranted bias against the 'younger' one. The test of a new theory against such an observational background theory represents at best an inconsistency, which could as well be remedied by jettisoning the observational theory instead of the theory under test. Interpreted as a refutation and rejection of the new theory, this process renders the context of testing as irrational as the context of discovery. (2) Secondly, since the observational theory constitutes the principle of application of terms in the test-situation, it defines the meaning of these terms in the context. Two tests of a
theory therefore, against the backdrop of different observational theories, are semantically incommensurable. Moreover in the case of cosmological theories like classical mechanics and special relativity, their tests against a common observational theory (which might even be the physiological theory incorporated in our sensory apparatus) are incommensurable, because the cosmological theories override the observational base, and interpret its concepts in their own terms. Feyerabend's argument requires careful sifting: To consider the last point first, if Feyerabend is right that cosmological theories override their observational base by interpreting concepts in their own terms; then this leads us straight to the heart of methodological conventionalism. For Popper emphasizes that it is precisely this feature viz. of implicit definition of theoretical terms by axioms of the theoretical system, which distinguishes conventionalist methodology from his own empiricist position. (Hence the methodological injunction against implicit definition). But Popper also emphasizes that methodological conventionalism is a logically unassailable position (although it involves the loss of both empiricism and commensurability) only it is at variance

8 Whilst Feyerabend not merely accepts but even welcomes semantic incommensurability (which permits the gay proliferation of incompatible life-styles) he is ambivalent on the issue of empiricism. However his attempts to retain both radical conventionalism (in Popper's sense) and vestiges of empiricism have met with criticism, especially by Dudley Shapere [1976].
with his own conception of science. Popper's position of methodological falsificationism is not really vulnerable to this criticism of Feyerabend.

Feyerabend, however is warranted in asserting along with Lakatos\(^9\), that the thesis of theory-ladenness reduces the concept of falsification to a merely syntactic one of inconsistency between theories. However, Feyerabend's proposed solution i.e. of jettisoning the observational theory would lead again to radical conventionalism, and is therefore shunned by falsificationist. Lakatos' [1976] solution on the other hand, consists in replacing (not

9 Lakatos [1976] in his development of 'sophisticated' methodological falsificationism, reinterprets refutation as a syntactic relation between succeeding theories in research programme. But, as he is at pains to stress, this concept of refutation like Popper's is syntactic; hence to compensate for the loss of the semantic concept (of falsity) his methodology emphasizes the criterion of 'novel prediction'. Like Popper's but to a greater extent, Lakato's concept of 'novel prediction is ambiguous. It is not clear whether theoretical novelty or empirical novelty is intended. Theoretical novelty involves the inferential relation between theory and prediction i.e. the prediction ought not to have been deduced from any prior theory; empirical novelty, on the other hand, emphasizes the novelty of the fact per se, regardless of whether it is deducible from any theory. In Popper's case atleast, the former interpretation would appear to be warranted especially on account of his emphasis on the theory-ladenness of all observation viz. that all observation is in the light of some theory. Thus (background) theory is not only constitutive of the facts, but also facts are significant i.e. only in the light of theory (i.e. they are deducible from some theory). This is Popper's [1969] 'searchlight theory' of knowledge, as opposed to the classical empiricists' tabula rasa or 'bucket theory of knowledge'.

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jettisoning) theories in quick succession at successive stages of a 'research programme'. In a research programme the particular hypothesis scientists are interested in is retained as 'hard core'; while the 'protective belt' of auxiliary hypotheses is adjusted, till consistency is restored between the hypothesis under test and the 'observational' theories. Lakatos' methodological stratagem however fails to overcome the problem of incommensurability. For if the 'hard core' theory is treated as observational, then competing research programmes with different 'hard cores' become incommensurable. If, on the other hand, the auxiliary theories define terms, then succeeding stages of the research programme are rendered incommensurable. The problem (of incommensurability) would therefore appear to be intractable.

A solution however is suggested by Mary Hesse [1974]. She points out that persons (and groups) holding different theories can agree over the results of experiments, if test consequences can be deduced from either theory, which are couched in an observational language that is neutral vis-a-vis both theories (though not neutral in relation to observation).

More recently Andersson [1991] shows in detail how 'unproblematic' test statements can be derived from problematic ones with the help of auxiliary hypotheses.
According to him Popper has always claimed that a critical discussion of theory-dependent test-statements is possible; but has only hinted at how this could be done. Andersson extends Popper's analysis to show that it is always possible from two theories that (claim to) describe the same sort of phenomenon but are allegedly incommensurable, to deduce further test statements until one arrives at test-statements that are unproblematic in the sense that they are neutral vis-a-vis the two competing theories 10.

The argument of A. Franklin, M. Anderson, P. Brock et al ([1989] p. 229-231) is in the same vein. They maintain: 'one of the interesting questions in exploring the complex interaction between experiment and theory is that of the theory-ladenness of observation. In its most radical form, incommensurability, Kuhn and Feyerabend have argued that experiment cannot distinguish between competing paradigms or theories. Briefly stated, the argument is that there can be no neutral observation language since all terms are theory-laden; thus we cannot compare experimental results because in different paradigms the terms describing these

10 In order to substantiate this claim, Andersson analyses some of Kuhn's and Feyerbend's case-studies and claims to show that for example, the Copernican and Ptolemaic theories turn out to be optically and dynamically commensurable; and that the phlogiston and the oxygen theory can be compared with each other. Andersson concludes that a falsificationist interpretation of the classical 'case studies' of Kuhn and Feyerabend is superior to the interpretations that Kuhn and Feyerabend have offered.
experimental results have different meanings even when the words used are the same. An example would be the term 'mass' which in Newtonian mechanics is a constant, while in Einstein's theory it is a function of velocity. It has already been argued by Franklin [1984] that in this particular instance, the change from Newtonian to Einstein mechanics, (a prime example for both Kuhn and Feyerabend) that a procedurally defined, theory-neutral (between the two competing theories) experiment can distinguish between the two theories.

Franklin et al consider that there are even circumstances under which the theory-ladenness of an experiment can be a virtue. Thus in their argument that different experiments provide more confirmation than repetitions of the same experiment, Franklin and Howson [1984] point out that the existing theoretical context may provide reasons why experiments which were once considered the identical are considered different with the advent of new theories. They also note that Dudley Shapere ([1982] p. 485-525) extends the idea of 'direct observation' to include theoretical beliefs.

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11 Thus, according to Franklin and Howson [1984] tests of the velocity addition law at speeds close to or small compared to the speed of light would be considered almost the same before 1905, when Newtonian mechanics which made no such distinction was the only theory. After 1905, when Einsteinian relativity became a serious competitor, such experiments would have been considered as quite different.
According to Franklin et al theory dependence creates no problem if the theory of the phenomena under test and the theory of the apparatus are distinct; Furthermore they maintain that the argument of Gillies ([1972] p. 1-24) notwithstanding; even when the two theories coincide vicious circularity can be avoided provided the theory of the instrument is disregarded and the instrument treated merely as a calibrated measuring device. Furthermore one could even use an instrument whose theory seems (to the naive observer) to refute the theory under test. They conclude: 'We do not wish to imply that there are no possible cases in which the theory-ladenness of observation prevents the testing of a theory; but we believe that examples from science should be presented'.

Franklin, Brock et al however miss the point of Feyerabend's argument and that of Kuhn (whose views will be shortly considered). Neither is concerned to maintain that theories cannot be tested against other theories.

12 However, Feyerabend's [1978] recommendation in this regard is that (in the interests of a variegated joyous existence) we desist from testing theories against other theories. His point is that since no theories can be tested by experience, each theory ought to be allowed to conceptualise its own experience, in its own way. This thesis holds for cosmological or global theories; to universal generalisations of a lower order of universality (which Feyerabend disparagingly characterizes as of the 'All ravens are black' variety) he is content to grant testability. But this testability is only against the backup of cosmological theories.
(variously characterized as 'accepted theories', 'theory of the instrument' etc) what Feyerabend and Kuhn do assert is that owing to theory dependence, theories cannot be tested against experience. A corollary is that when observational theories are tested, this is always against some other theory which then functions as the observational theory (semantic theory) which constitutes the principle of application of terms in the changed context. It follows that the two contexts are semantically incommensurable.

In attempting to draw conclusions from this wide-ranging and far-reaching controversy which extends beyond logic to embrace epistemological, linguistic and cultural issues (especially in the philosophy of Feyerabend); it is crucial to realise that protagonists are often arguing at cross purposes. Popper is interested in empirical theories; theories which can be tested against experience. Lakatos, Feyerabend, Kuhn, Quine and others focus, on the other hand, on the observational or semantic theory which defines the conditions of experience. Popper relegates this conceptual framework of observational theories to 'unproblematic' background knowledge in the context of testing. Popper ([1970] p. 56-57) believes however that the framework itself is testable, in some other context. But he fails to realise that the framework qua framework is never testable. For in the changed context where the old framework transmutes into
an 'empirical hypothesis' some other set of observational theories constitutes the fresh framework. In the context of testing frameworks the old framework is never the new framework; and so frameworks qua frameworks can never be empirically tested. This is the burden of the Weltanschauung philosophers; it is the contretemps to which the thesis of theory-laden observation leads.

Yet within the context of a single framework the falsification of hypotheses is possible. But as Eugene Freeman ([1974] p. 464) points out this is 'rule-bound' concept of falsification. Lakatos terms Popper's falsificationist methodology `quasi-empirical'. Finally

13 Lakatos ([1977 b] p. 29) distinguishes between two kinds of deductive systems, the 'Eulidean system' and the 'quasi-empirical' system. The distinction marks the different patterns of truth value flow in deductive systems: either the truth, flowing down from the top (a finite conjunction of axioms) through the safe truth-preserving channels of valid inferences, inundates the whole system; or the falsity, through the deductive channels, flows upwards from the bottom (a special kind of basic statement) to the top. Lakatos calls these two kinds of deductive systems the 'Eulidean system' and the 'quasi-empirical' system respectively. As the concept of 'quasi-empirical' relates only to the ways of transfer of truth-value in deductive systems, it should be differentiated clearly from the concept of 'empirical' in the usual sense. Lakatos himself makes this point quite clear. He says (Lakatos [1977 b] p. 29) 'This demarcation between patterns of truth-value flow is independent of the particular conventions that regulate the original truth value injection into the basic statement. For instance a theory which is 'quasi-empirical' in my sense may be either empirical or non-empirical in the usual sense. What can be claimed about a quasi-empirical theory is not to be true but at best to be well-corroborated.'
Kuhn ([1970] p. 13) points out that Popper describes as 'falsification' or 'refutation' what happens when a theory fails in an attempted application. Kuhn emphasizes 'Both 'falsification' and 'refutation' are antonyms of 'proof'. They are drawn principally from logic and from formal mathematics...'. These considerations suggest a rather radical thesis viz. that Popper's methodological falsificationist structure approximates Kuhn's conception of 'normal science' far better than it does the revolutionary picture evoked by Popper's own rhetoric or his examples from science.

This thesis can be defended in the following manner: Kuhn ([1970] p. 1-23) draws his distinction between normal science and revolutionary science based on the acceptance/non-acceptance of a paradigmatic theory which defines meaning, ontology and facts in the domain. Within the context of such a conceptual framework or Weltanschauung 'normal science' or normal research' proceeds by the testing of hypotheses which *premises* current theory as the rules of the game. The purpose of this testing is the *application* and *extension* of the paradigmatic theory, not its overthrowal. Normal research is therefore 'puzzle-solving' activity; and current theory is required to define the puzzles (problems) and to guarantee (given sufficient ingenuity) its solution. Extraordinary research or revolutionary science, on the other hand, calls into
question or doubt, the fundamental conceptual framework. It marks a period of crisis which is best described, according to Kuhn, by Popper [1969] himself as a critical discourse of claims, counter-claims and debates over fundamentals. Communication is possible at this stage, but (owing to theory-dependence) the discourse suffers from all the ills to which translation is prone. The crisis is resolved when scientists (in the light of the preceding critical discourse) once again adopt a common paradigmatic theory, whereupon normal science is resumed. But this choice (between competing paradigmatic theories) is not prompted by strictly empirical considerations and testing plays no decisive role. It is not difficult to see that rule-bound methodological falsificationism fits the mould of Kuhnian normal science far better than it does his concept of revolutionary science. Popperian injunctions to 'great or heroic science' notwithstanding. It is also not difficult to see that Kuhn [1970] himself recognizes only normal science as science at all, his protestations to be in sympathy with Popperian ideology notwithstanding. This is because the revolutionary interregnums between paradigm-shifts are marked by that debate over fundamentals which Kuhn considers as characteristic of philosophy and the arts, rather than of science. Thus Kuhn ([1970] p. 6-7) says: 'In a sense, to turn Sir Karl's view on its head, it is precisely the abandonment of critical discourse that marks
the transition to a science. Once a field has made that transition, critical discourse recurs only at moments of crisis when the bases of the field are again in jeopardy. Only when they must choose between competing theories do scientists behave like philosophers. That, I think, is why, Sir Karl’s brilliant description of the reasons for the choice between metaphysical systems so closely resembles my description of the reasons for choosing between scientific theories. In neither choice, ... can testing play a quite decisive role’.

Watkins [1970] is critical of Kuhn for evincing what he considers an undue bias towards normal science; and Popper [1970] can express only pity for minds trapped in the (uncritical) routine of normal science. In defence Kuhn [1970] urges only that normal science is what is peculiarly characteristic of science; and also that revolutionary science presupposes a backdrop of normal science. But Kuhn might have gone much further. For he is trying to make the much stronger case that the very possibility of science presupposes a conceptual framework which is itself empirically untestable, and hence metaphysical in precisely Popper’s sense of the term. In fact it is Popper [1970] who sees quite clearly that Kuhn’s thesis (and all Kuhn’s arguments) are logical; although Popper considers the thesis to be mistaken. Thus Popper ([1970] p. 56-57) says: ‘I
regard the thesis as mistaken. I admit, of course that it is much easier to discuss puzzles within an accepted common framework, and to be swept along by the tide of a new ruling fashion into a new framework, than to discuss fundamentals, that is the very framework of our assumptions. But the relativistic thesis that the framework cannot be critically discussed is a thesis which can be critically discussed and which does not stand up to criticism'.

Popper ([1970] p. 56-57) dubs this thesis The myth of the framework'. He admits 'that at any moment we are prisoners caught in the framework of our theories; our expectations, our past experiences, our language. But we are prisoners in a Pickwickian sense; if we try, we can break out of our framework at any time'. Popper's prescriptions for freedom to escape into a 'roomier' conceptual framework rely on (i) a tradition of critical discourse, and (ii) the possibility of translation.

Critical discussion in the context of scientific theories consists (as has already been discussed in Ch.I) in the criticism of the claim of a theory to be true and to solve the problems it is designed to solve. Popper [1969] himself espouses Tarski's correspondence theory of truth; but acknowledges that owing to theory-dependence objective or ontological truth lies beyond our reach. Instead Popper
([1969] p. 232) offers the criterion of versimilitude which permits him to write: 'a later theory ... $t_2$ has superseded $t_1$ ... by approaching more closely to the truth than $t_1$ ...'. Furthermore, Kuhn ([1970] p. 265) emphasizes: "Also when discussing a succession of frameworks, he speaks of each later member of the series as 'better and roomier' than its predecessors; and he implies that the limit of the series, at least if carried to infinity, is 'absolute' or 'objective' truth in Tarsk's sense'. But Kuhn points out (and Feyerabend would endorse this) that the comparison of theories for their degree of versimilitude involves the comparison of their consequence-classes; and it is not obvious that these consequences can be expressed in a neutral observation language i.e. in a language that is neutral vis-à-vis experience and not merely neutral in relation to a commonly accepted framework. Kuhn concludes that versimilitude is a tenable intra-theoretical criterion within the accepted framework but it cannot adjudicate inter-theoretically between competing frameworks. Like Popper's other concepts, versimilitude belongs to the context of normal science.

Popper's second argument against the 'myth of the

14 versimilitude is a measure-theoretic comparative relation between two theories $t_1$ and $t_2$ such that $t_2$ has greater versimilitude than $t_1$ iff the truth content of $t_2$ is greater than that of $t_1$ and its falsity-content no greater. This criterion has been variously criticised in the literature.
Does Popper need to maintain that they be fully commensurable?
framework' invokes the possibility of translation. Kuhn [1970] does not deny this possibility but only emphasizes the compromises, inadequacies and failures of communication to which translation is prone. He invokes Quine's [1960] thesis of the indeterminacy of translation, and emphasizes that mere observation of linguistic behaviour does not easily yield the ontological categories deployed. Kuhn is here once again making the point that the conceptual categories of a language i.e. its set of intensional meanings, determines its ontology or world-view. Kuhn concludes that translation mediates but indifferently between languages, and *a Fortiori* between fundamental scientific theories which constitute conceptual frameworks. Scientific paradigms are therefore, at best only partially commensurable. Kuhn's argument can be presented from another angle which clarifies his position: If fundamental scientific theories constitute conceptual frameworks, then they are scientific taxonomies. In this context Ernest W. Adams and Williams. Admas ([1987] p. 419) maintain that the

15 Quine ([1960] p. 73 ff) points out that though the linguist engaged in radical translation can readily discover that his native informant utters 'Gavagai' because he has been a rabbit; it is more difficult to discover whether 'Gavagai' refers to 'rabbit', 'rabbit-kind', 'rabbit part' or whatever.

16 This has led Sheffler [1967] and others to accuse Kuhn of being an idealist. But if Kuhn is an idealist then so is Popper, for this position - i.e. of the conceptual categories determining the ontology, follows from the thesis of theory-ladenness.
appropriate criterion in relation to taxonomies is not that of truth/falsity, but that of adequacy/inadequacy in the light of purposes. They argue: "The scientific concept formation with which we are concerned is that which occurs when technical terms or systems of terms are introduced or deliberately modified by scientists in the pursuit of their scientific objectives. We will advocate a 'philosophy' of this sort of concept formation in which the purposes for which the terms are introduced and employed are central and various features of their introduction and use are explained 'functionally' in terms of these purposes. We will argue that many of the qualities that are thought to be definitive of the scientific are 'accidental features' that are fairly well-approximated in certain cases, but insistence that all scientific concepts should possess these qualities can also be counter productive to the actual and legitimate purposes of many scientific activities. Among these stereotypes are that scientific concept should be precise, objective and subject to observational determination (the latter two have been extensively criticised in the Kuhnian tradition, but we will criticise them here from a different point of view). The failure to recognize that these qualities are desirable only to the extent that they serve scientific purposes, and they are not ends in themselves, stems from the failure to recognize the purposes for which concepts are employed and from mistaking properties that are frequently approximated
for attributes that are essential to the scientific'.

The above thesis of Adams and Adams viz. that it is the aims and purposes (for which scientific taxonomies are deployed) that determine concept formation in science, illumines the Kuhnian position greatly. For aims and purposes presuppose scientific communities rooted in time and evolving with time. This is Kuhn's thesis of historicity viz. that the features of conceptual frameworks (of science) are to be discovered by historical research into the purposes and motivations of the members of the community. Secondly, since the frameworks are semantic i.e. conceptual, they reflect the goals of the community as a whole, and not of any individual scientist (since language is the common possession of the community). This is Kuhn's sociological thesis. Finally since collective goals and purposes are in the ultimate analysis shaped by the world-view or Weltanshauung of the culture (to which the scientists belong) the conceptual framework of science reflect the Weltanshauung. This is Kuhn's thesis of paradigms as metaphysical world-view. Since these theses follow from the logic of scientific conceptual systems as semantic taxonomies, Kuhn is right to rebut the charges of irrationalism and relativism levelled against him.

Nevertheless the conception of fundamental scientific theories as semantic taxonomies - a conception to which the
thesis of theory-ladenness inexorably leads—represents theory change in science as essentially non-empirical. Popper's methodology, by virtue of his acceptance of this thesis is implicated and undermined by the loss of the principle of empiricism. But the problem also afflicts the Weltanshauung philosophies as indeed it does all philosophy of science in the current century. An attempt is therefore made to reinterpret this thesis.