A CRITICAL STUDY OF KARL POPPER'S THEORY OF FALSIFICATIONISM: A SEMANTIC APPROACH

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CERTIFICATE

This is to certify that Mrs. Milan Buqui Desai has satisfactorily prosecuted her course of research under the conditions prescribed by the University.

The thesis entitled "A Critical Study of Karl Popper's Theory of Falsificationism: A Semantic Approach" is the ressult of her original work under my supervision. The conclusions of her study are the result of her own researches. To the best of my knowledge no part of this work has been presented to any University for any other degree.

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DECLARATION

The contents of the thesis are my findings of research done under the guidance of Dr. A.V. Afonso. I hereby declare that the thesis or part thereof has not been published anywhere or in any other form. It has not been previously submitted by me for a degree of any University.

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CONTENTS

		Page No.
ABSTRACT	... ...	i
PREFACE	... ...	ii
CHAPTER I - POPPERIAN METHODOLOGY		1
1.1	Empiricism as the Rationale of Falsificationism	1
1.2	Abandonment of the Principle of Empiricism	13
1.3	The Popperian Concept of Evidence ...	28
1.4	The Neo-justificationist Position ...	34
1.5	The Issue of Stochastic Independence ...	44
1.6	Corroboration or Probability? ...	57
CHAPTER II - THE THESIS OF THEORY-LADENNESS		
2.1	Popper's Theory of Universals ...	64
2.2	The Bedrock of Conventionalism ...	71
2.3	The Duhem - Quine Thesis of Holism ...	78
2.4	The Incommensurability Sydrome ...	91
CHAPTER III - UNIVERSALS REVISITED: THE LOGIC OF IDENTITY		111
3.1	The New Theory of Reference ...	111
3.2	The Psychology of Perception ...	122
3.3	The Thesis of `Primitive' Classification: Similarity or Identity?	137
CHAPTER IV - IDENTITY AS THE LOGIC OF SCIENTIFIC DISCOVERY		151
4.1	Identity as Functional Dependence ...	151
4.2	Representation & Reduction: The Changing Faces of Realism	164
4.3	The creativity of Identity ...	182
REFERENCES	... ...	196

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ABSTRACT

The project attempts a critical study of Popperian methodological falsificationism from an analytical perspective. The critique of theory-ladenness shows that it leads to the abandonment of empiricism; instead an argument is built up for universals as names which indexically indicate the **same** kind in all possible worlds.

The intuitions of indexicality and of identity underlying the new theory of reference converge in Leibniz's Principle of the Identity of Indiscernibles; leading to the 'primitive' classification of kinds based on identity.

Identity as a primitive principle of classification, which can only be extensionally exemplified and not intensionally defined, replaces facts with existents, which evades the pitfalls of Cartesian foundationalism. As a mode of inference, logical identity as opposed to logical equivalence transcends truth-functional logics.

The rejection of the bivalence principle (of truth/falsity) is based on the 'fact' that (1)laws in physics exemplify functional dependencies which are expressible as symbolic mathematical identities (ii) theoretical structures are symbolic (mathematical) representations, and (iii) theoretical growth employs as mode of inference Leibniz's law which is non truth-functional.

This leads to the conception of Leibniz's law as 'creative' mode of inference for scientific discovery; where 'creativity of identity' is characterized by (a) the intrinsic creativity of the (fact) free proliferation of theoretical assumptions (b) the 'conceptual reshuffling' of phenomena wherein new classificatory structures transcend the old; and (c) the conjectural character of both (symbolic) premisses and conclusions.

Finally, identity as primitive principle of classification and identity as creative (non-truth functional) mode of inference interprets referential realism in its own terms; it presupposes only existents, i.e. the 'minimal form' of ontological realism.

PREFACE

I was introduced to the philosophy of science by Popper's great book *The Logic of Scientific Discovery*. This has been fortunate, for the work, as indeed the entire corpus of Popper's aachievement, is imbued by a sense of the freedom and creativity of the human intellect, particularly in its relation to science. This freedom and fecundity is however, preventd from degenerating into unreason, by the control of both logic and (aim of) truth. The fundamental intuition therefore, underlying Popper's conception of science is both its creativity and its rationality. I have made this intuition my own.

Some re-reading and years later, however; and particularly in the wake of the critique of the Popperian position by philosophers both within the Popperian tradition and without; an angst has developed. This unease has to do, not merely with the erosion of the philosopher's position, but with the wider sense of crisis pervading the philosophy of science. It seems that at the heart of the scientific endeavour lies unreason; and its creativity after all is only another name for anarchy. The current metaphor for science today is, well, metaphor.

I have traced the root of this problem to the thesis of theory-ladenness which is at heart, a theory regarding universals. This has lead me to explore the semantics of natural kinds; and in the seminal work of Saul Kripke, I think I have discovered clues which leaad to a hidden treasure. For the fundamental intuitions underlying the thesis of naming are indentity and indexicality. These seemingly unrelated notions converge in the metaphysic of Leibniz, in particular in his Principle of Identity of Indiscernibles.

This principle, I think is a key which turns many locks. In particular it sets us free from the *tyranny of conceptual frameworks*; and other confusions engendered by the web of theory-ladenness. For the relation of identity, as captured by Leibniz's Law is a rather marvellous concept. It is at the same time tautologous and completely empirical. This is on account of its nature as a `primitive' relation which can never be intensionally defined but only extensionally exemplified. As a principle of classification it restores to science, I think, its empirical basis; as a mode of inference it satisfies the intuitions of both the creativity and the rationality of science, which inspired this research. Finally, it demands a minimal form of realism, namely referential realism whose ontological presuppositions are bare existents.

These ideas can be made much clearer, I think if the (guided) journey of exploration which leads from Popper's philosophy to the thesis of identity, is set forth in its chronological and logical order.

`Popperian Methodology' (Chapter I) examines the rationale of Popper's position of methodological falsificationism. Here, I have tried to show that, and how, the spectre of theory-ladenness (Popper's theory of universals) haunts his methodology from the very outset; and leads to the abandonment of the principle of empiricism which is the cornerstone of Popper's position. Induction, however, is also infected by the malaise of theory-ladenness, whilst probabilism wilts under the Popperian attack.

This sets the stage for the Weltanschuungen philosophers: Kuhn, Feyerabend (and Lakatos) are all philosophers in the Popperian tradition, and this perhaps, best equips them to expose the internal contraditions, engendered by *`The Thesis of Theory-Ladenness'* (Chapter II), in Popper's position. This is the critique from ***within***; the critique from without assumes the form of the Duuhem-Quine thesis of holism; but this challenge, I think, can be deflected by a slight modification of Popper's logical schematism of ***modus tollens***. (As long as the `holistic' philosophers operate from within the `statement' view of theories, I do not think they pose a serious threat.) The critique from within however, completely undermines Popper's position. I must confess that Feyerabend's influencce has been something of an eye-opener *and* a liberating influence. I do not think however that his thesis of incommensurability applies to science. It is in an attempt to justify this intuition that *`In Universals Revisited: The Logic of Identity'* (Chapter III). I reinterpret the thesis of theory-ladenness. This reinterpretation is in the light of the `new' theory of reference developed by Saul Kripke (to whom I am particularly indebted); as well as in the light of developments in cognitive science. The latter is a notoriously slippery field; but it has helped me, I think, to sift the grain from the chaff in the reference theorists' position. The insight that emerged has come as something of a revelation: I think that the concept of *`primitive classification'* and of identity as *the* primitive principle for scientific classification, is the most significant result of my research.

This insight marks a watershed, from which flows the conception of identity as a `creative' mode of inference. In *`Identity as the Logic of Scientific Discovery'* (Chapter IV), I compare the non-truth-functional concept of logical identity with the truth-functional concept of logical equivalence. The contrast has been liberating: In Leibniz's Principle of the Identity of Indiscernibles we have a *valid mode of inference* which *does not base itself on the bivalence-principle* (of truth/falsity). Thus at last, (it seems) we may free ourselves from the tyranny of truth-functional logics. Furthermore the examples of theoretical struccture (within my competence) which I have examined, seem to exemplify Leibniz's Law(s).

Finally if we assimilate the two aspects of identity, i.e. identity as a `primitive' principle for scientific classification, and identity as a creative mode of inference. I think we may succeed in satisfying both our intuitions regarding science - i.e. its empiricism and its (logically controlled) creativity.

I must however emphasize that the analysis at this stage is very tentative and preliminary, and much more work needs to be done in terms of substantiating/extending my insights by a detailed analysis of actual theoretical structures from science. It may well be that problems will crop up (as Popper always insisted they do); but I think the direction of research is clear.

In conclusion, I must make my acknowledgements: My greatest debt of gratitude is to my guide Dr. A.V. Afonso, who bestowed on me the great gift of ***freedom***, freedom to work and think independently. But in the proper Popperian tradition, this has been a controlled freedom; for he has always been there to both guide and checck my intuitions (when they ran too wild).

Again, I must acknowledge my debt to the Indian Council of Philosophical Research, New Delhi, whose (Junior) Research Fellowship has supported this research. They have been most co-operative in extending the period of fellowship. In particular, I should like to thank Dr. Mercy Helen for her understanding and patience. I also thank the authorities of the I.C.P.R. Library, Lucknow. I should like to acknowledge the help and encouragement of Prof. Amitabh Gupta, I.I.T., Bombay. I should also like to thank Prof. R. Sundara Rajan and his wife, for their kindness and helpful suggestions, on my visit to the University of Pune. I am also grateful to Dr. S.B. Kulkarni (Central University, Hyderabad) who has been a source of inspiration; and whose warm hospitality I enjoyed on my visit to the library at Hyderabad. My thanks to the Librarian, Mr. Navelkar and staff of the Goa University Library for help rendered. Many thanks to Mr. Mahadev Khanapuri for the excellent typing and presentation of this thesis.

I must also thank my husband for his forbearance. Finally, I dedicate this work to my children, Urvashi and Siddhesh.

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Milan B. Desai

CHAPTER I

POPPERIAN METHODOLOGY

1.1 Empiricism as the Rationale of Falsificationism

Popper ([1972] p. 312,27-48) terms the criterion of falsifiability as `a Criterion of the Empirical Character of Theoretical Systems'. Its rationale follows, according to him, from the logic of the situation; the situation in question being the conception of scientific hypotheses or systems of theories as universal statements of unrestricted universality, which are in some sense, empirical. But since the form of statements which might be `known by experience' is that of singular statements, the question of the truth of scientific theories which are universal statements, reduces to the problem of the (in) validity of inductive inference. This constitutes Hume's problem. In *The Logic of Scientific Discovery* Popper formulates this problem as follows: `Now it is far from obvious, from a logical point of view, that we are justified in inferring universal statements from singular ones, no matter how numerous; for any conclusion drawn in this way may always turn out to be false: no matter how many instances of white swans we may have observed, this does not justify the conclusion that *all* swans are white'. Later Popper ([1983] p. 32) reformulates this, more sharply and briefly as follows: (i) `There can be no valid reasoning from singular observation statements to universal laws of nature, and thus to scientific theories. This is the principle of the invalidity of induction. (ii) We demand that our adoption and our rejection of scientific theories should depend upon the results of observation and experiment, and thus upon signular observation statements. This is the principle of empiricism', According to Poper, Hume realised that the clash between the two principles is only apparent; for he accepted both, and dissolved the `clash' by abandoning rationalism. Hume accepted that all our knowledge of laws is obtained from observation by induction, and he concluded that since induction is logically invalid, this shows that we have to rely on `habit' rather than on reason. In the process Hume thus belittled human rationality. The Positivists' `solution' on the other hand (including that of Wittgenstein, Mill and Schlick amongst others) consisted in belittling scientific laws and theories as `pseudo-statements' or `inference-tickets'.

Popper's [1972] solution to the problem of induction is to drop the requirement in principle of the complete decidability i.e. verifiability and falsifiability of all genuine statements. He points out that we can quite consistently, interpret natural laws or theories as genuine statements which are *partially decidable*; i.e. which are for logical reasons, not verifiable but *in an asymmetrical way falsifiable only*. They are statements which can be empirically tested by being submitted to systematic attempts to falsify them. This insight into the logical asymmetry between verification and falsification which is implicit in the quantification logic structure of scientific inference is exploited by Popper to solve what he terms as `Kant's problem' and also as `Russell's problem'. The problem is a criterion for demarcating the empirical systems of scientific theories from the speculations of metaphysics (or lunatics). Popper ([1983] p. 54) points out that Russell appreciated the full force of Hume's demonstration of the invalidity of induction, and its implications for science. Russell formulated the problem as follows: `If Hume is right that ***we cannot draw any valid inference from observation to theory***, then our belief in science is no longer reasonable. For any allegedly scientific theory, however arbitrary, becomes as good or as justifiable as any other, because ***none is justifiable*** ..... Thus if Hume were right there would be `no difference between sanity and insanity, between the theories of science and the speculative fancies of metaphysics or lunatics'. According to Popper, Russell just fails to note the logical asymmetry between verification and falsification. He fails to register that `Hume's argument does not establish that we ***e may not draw any inference from observation to theory***: it merely establishes that we may not draw ***verifying*** inferences from observations to theories; leaving open the possibility that we may draw ***falsifying*** inferences.

Popper's solution to Hume's problem and to Russell's problem viz. to the problems of induction and of demarcation, would appear to be highly integrated. But they involve components which have somewhat varied implications. Whereas the problem of demarcation can be resolved by the recognition of logical asymmetry; the problem of induction is only dissolved if we dispense with the need for verified or justified knowledge. Undoubtedly, Popper ([1974] p. 981) offers his criterion of falsifiability as a proposal which marks no real distinction. betw. what? He says `Any demarcation in my sense *must* be rough .... For the transition between metaphysics and science is not a sharp one' Accordingly, he thinks the Positivists were mistaken in interpreting the problem of demaracation in a naturalistic way, as if it were a question of discovering a difference, existing in the nature of things. Instead Popper offers his criterion as a proposal or a convention, to be accepted for its power in resolving the problem of demarcation. But equally undoubtedly, the proposal is based on the *recognition* of logical asymmetry, i.e. on logico-epistemological properties intrinsic to the inferential structure of quantificational logic accepted by Popper. Hume's problem or the problem of induction, on the other hand, can only be dissolved on Popper's criterion, if we take the epistemological *decision* to dispense with the need or aim of justified knowledge;

what Popper terms knowledge with a capital `K' or science with a capital `S'. Popper's dissolution of the problem of induction, therefore, is not a solution for those unable to accept this.

Popper ([1983] p. 21,24,32,33) acknowledges this profound difference in epistemological attitudes which creates the chasm between the context of Justificationism and the context of ~~Rational~~ Criticism. Justificationist philosophies (and according to W.W. Bartley *all philosophies thus far have been justificationist philosophies*) assume the prima facie task of the theory of knowledge to show that, and how, we can *justify* our theories or beliefs. Popper's critical rationalism on the other hand, accepts the conjectural or hypothetical character of all knowledge, including scientific knowledge. Within this context of critical rationalism, the question of the justification of a theory is replaced by the problem of rational preference from amongst competing theories. The context therefore, presupposes theoretical pluralism and develops a concept of rational criticism which Popper sums up as `criticism of the claim of a theory to be true, and to be able to solve the problems which it was designed to solve'. This leads to the formulation of the principle of critical rationalism as `the demand that our adoption and our rejection of scientific theories should depend upon our critical reasoning (combined with the results of observation and experiment, as demanded by the principle of empiricism)'. Popper's final solution to Hume's problem then consists in (i) Acceptance of the supreme importance of theories i.e. universal statements, for explanation and problem solving in science (ii) Acceptance of the principle of the invalidity of induction: scientific theories can never be justified as true or probable (iii) Acceptance of the principle of empiricism: scientific theories must be adopted or rejected in the light of experimental/observational tests (iv) Acceptance of critical rationalism. Scientific theories are accepted or rejected in the light of the results of rational criticism ***and*** the results of observation/experiment. These four points summarise Popper's dissolution of the logical problem of induction.

Does Popper really solve Hume's problem? Swann [1988] maintains that the point of the problem of induction is that justificationism, which he characterises as Narrow Rationalism (NR) must be rejected. According to him Popper grasps this, and it is Popper's strength. But then Popper proceeds to use the refuted theory NR, to attack induction. This is Popper's weakness, but Swann thinks that Popper's positive thesis i.e. his rejection of NR, is unaffected by this weakness. It would also remain unaffected by criticism of Popper's own theory of falsifiability or his views on probability.

Swann's arguments can be elaborated thus: First Swann considers possible criticism of Popper's rejection of justificationism (NR). In his ([1983] p. 28) Popper defends his solution to the problem of induction by saying that non-demonstrability `never worries the critical rationalist'! Spelli This lends itself to the stoic interpretion: Hume's discovery that all our theories about the world are without foundation is not a problem for Popper, simply because he does not *want* his theories to be justified. Swann says this interpretation is inadequate because what Popper is really getting at is that we do not *need* justified theories. So if Hume's problem is stated in the form: there are propositions on which we must act, and yet which cannot be justified, then Popper's solution is that `best' theories i.e. best in the light of the critical discussion, can do the job. The cynical see Popper as `solving' the problem of induction by calling these propositions `conjectures' when it is time to justify them, and `background knowledge' when it is time to act upon them. Swann's own interpretation makes use of Susan Haack's suggestion that in Popper, much of the work traditionally done by `beliefs' is done instead by `held' theories. Usage connects `held' with `justified' but Popper rejects this. According to Swann, Popper is right to do so because the usage stems from narrow rationalism.

Swann formulates Narrow Rationalism as the demand that theories about the future be deducible from the set

This is one version (inductivist) of NR

Swann's stand:

1) Popper attacks ~~???~~ NR
2) But he uses it in his attack on Inductivism
3) he uses it to defend his own position ie critical rationalism.

~~ACCT~~.

consisting of statements of past observations and necessary truths, in accordance with a principle of induction. Popper questions the status of this principle. Either it is a priori, which is unaccceptable, or else, if empirical it leads to infinite regress or is false. Popper therefore rejects the principle of induction. But if Popper's own method of falsificationsim (critical rationalism) is formulated as `accept the best theory in the light of critical discussion', then the defence of his method also presupposes the method. For Popper declares that he does not seek to justify his method, but retains it only till a better one is found. But this argument presupposes the method of critical rationalism. Swann emphasizes that this was precisely the point of Hume's criticism of induction.

Popper also employs NR to attack induction. In his [1969] he declares himself perfectly satisfied by Hume's demonstration of the invalidity of induction. But the demonstration amounts to showing that (i) inductive inference is not deductive and (ii) the principle of induction cannot be deduced from experience. From this it is inferred that the inductive principle is refuted! This is a narow rationalist argument against induction. Moreover Popper defends the method of falsificationsim because it presupposes no inductive inference but only the tautological transformations of deductive logic. This again is a NR argument, this time in favour of falsificationism. But

Swann argues that if Narrow Rationalism is conceded, then ex hypothesis, Popper's falsificationist empiricism cannot provide the rationality of science any more than induction or anything else can. This accords well with G.J Warnock's [1960] criticism of Popper in which he argues that *if* there is a problem of induction *then* Popper's view leads to a similar problem.

Tom Settle [1990] says Swann's case against Popper hangs on whether Popper uses NR, which he has successfully
Popper seems to do so
refuted in arguing against Hume. This seems to be because Popper claims that falsifiability satisfies empiricism's demand `that experiment alone can decide upon the truth or falsity of scientific statements'! ?? which is based upon falsification being deductive. Strictly, this point of Popper's is not quite right, as he himself concedes in later discussions of crucial experiments. Experiment alone does not decide the falsity of a theory, as Duhem pointed out. One has to choose what not to regard as under challenge, to make a refutation go through. Of course, when it does go through, it is deductively valid. But this means Popper's solution most certainly does not emmploy NR, as Swann alleges, since the decision as to what not to treat as under challenge, cannot be deductively warranted from experience. Settle concludes that Popper did not offer falsifiability to the world because it might satisfy NR; what led him to see the falsifiability of theories as a desirable and

distinctive characteristic of scientific knowledge was the chance it gave to learn from experience. According to Settle, therefore the rationale of Popper's criterion of falsifiability is not logicism, (NR) but rather, the principle of empiricism.

Robert Nola [1987] also considers that the cornerstone of Popper's methodological falsification is the role that experience plays in the rejection of theories or their tentative acceptance. First Nola highlights several ambiguities in Popper's metamethodological concepts, which seem to be responsible for the controversy between Swann and Settle. (This problem surfaces again in Popper's position on the probability of theories[1]). Nola distinguishes between Level I of scientific theories, Level II of methodology and Level III of metamethodology. At Level III Popper rejects naturalism, a priorism, empiricism, logicism[2] and transcendalism[3] as metamethodological criteria. Instead he offers his falsificationist methodology as a proposal or

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1 The problem concerns the assignment of initial probabilities. Popper's own assignment assumes probabilistic independence between properties, which leads to universal generalisations having a probability, Howson and others have questioned this. Howson maintains that the assumption is not logically transparent; but betrays an epistemic attitude to which logical alternatives are viable.

2 `logicism': the contention that rules of method are like rules of logic.

3 `transcendalism': the view that metamethodology could be justified from the bare possibility of science.

as a convention, thus adopting conventionalism at this level. But at Level II of methodology, Popper rejects induction because it is logically invalid; (i.e. on grounds of Narrow Rationalism) and he rejects conventionalism because it fails to satisfy the principle of empiricism. Popper does not explain why induction and conventionalism as methodologies, might not be appraised as proposals or conventions, on par with his own falsificationist methodology. Nola thinks the reason is an ambiguity in Popper's metamethodological concepts. Nola points out that at places, Popper emphasizes the logico-epistemological properties of Level III statements, i.e. of scientific theories as the criterial properties to be specified by a theory of method. This would lead to Narrow Rationalism which is what Swann accuses Popper of. But at other places Popper maintains that what demarcates myth (or presumably metaphysics) from science, is the absence/presence of an accompanying second-order tradition of rational criticism. Nola considers the two metamethodological critera i.e. (i) that of method as specifying the logico-epistemological properties of scientific theories and (ii) of method as prescribing a second order critical tradition - as independent. But it must be noted that Popper defines rational criticism as criticism of the *claim of a theory to be true and to solve the problems it is designed to solve.* The former claim involves the logico-epistemological properties of scientific theories; for owing to the logical asymmetry emphasized by Popper, the claim to truth of a theory cannot be verified but can only perhaps, be falsified. Popper therefore would appear to adopt the criticism of falsificationism primarily because it satisfies the principle of empiricism.

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(criteria?)

If we now appraise the three methodologies which concern Popper - i.e. naive induction, conventionalism and falsificationism - in the light of their aims or goals, then the difference in epistemic orientation becomes very clear. Justificationism seeks certain or probable knowledge, and a principle of induction, considered as a proposal or a convention on par with the method of falsificationism, would deliver this goal. Conventionalists require of their theories only that these be pragmatic instruments of explanation and prediction. They repudiate[4] (atleast at the level of individual laws) empiricism. Finally, falsificationists seek ***testable*** knowledge, knowledge which can be tested against the results of observation and experiment. The rationale of Popper's falsificationist methodology therefore, in the light of his cognitive aims is the principle of empiricism. If ***this*** principle is abandoned, then Popper's philosophical position collapses. Nola considers a final metamethodological point: how are

4 The detailed analysis of conventionalist methodology is found in Ch. II.

goals or aims to be appraised? Popper ([1972] p. 49) regards the choice of goals as a decision which depends `..... upon the *aims* which we choose from among a number of possible aims'. (for science) As for aims themselves, Popperr ([1972] p. 38) declares, `Thus I freely addmit that in arriving at my proposals, I have been guided, in the last analysis, by value judgements and predilections'. Value judgements and predilections are not the sort of things one can quarrel over; but perhaps arguments for and against such positions might be proffered. This would certainly constitute a tradition of rational criticism in the Popperian mould, and perhaps this is the best way of construing Popper's criticism of inductivist and conventionalist methodologies.

1.2 Abandonment of the Principle of Empiricism

It has been established that the cornerstone of Popper's falsificationist methodology is the principle of empiricism. It will now be argued that Popper's thesis of theory-laden observation leads to the abandonment of this principle and the undermining of the possibility of any application of theoretical systems to reality.

First certain objections to Popper's criterion are considered, which lead directly to the main theme. The criterion invokes the logico-epistemological property of falsifiability because according to Popper, falsifiability

Imp

Kneale (1974)'s point is questionable since the ~~thesis of~~ thesis of asymmetry between verification and falsification is a thesis about universal statements. The fact that a falsification of a univ. statement leads to verification of an exist statement in no way affects the thesis.

Also Popper answer is also questionable. The fact that univ. statements are stronger in no way makes the rel^n ~~asym~~ asymmetrical. ~~If it~~ even if it ~~does~~ does, that asymmetry can not be carried to the asymmetry about which Popper spoke earlier.

alone satisfies the demand of empiricism that scientific theories be testable against experience. This is on account of the logical asymmetry between verification and falsification. But the asymmetry has been challenged. William Kneale [1974] points out that the refutation of a universal hypothesis is at the same time the establishment of the unrestricted existential proposition which is its contradictory; and if the procedure involves appeal to experience under the first description it must invoke the same under the second description also. Kneale's point is that both the falsification of a universal statement and the verification of the corresponding unrestricted existential proposition (which can be derived from Popper's basic statements by dropping the space time co-ordinates) satisfy the principle of empiricism. There is therefore no asymmetry between verification and falsification as far as the appeal to experience is concerned. Popper [1983] responds by asserting that the asymmetry is *logical*. It is also methodological and heuristic. The asymmetry is logical because universal statements are logically stronger than existential statements; for whereas, from a universal statement in conjunction with certain auxiliary assumptions, singular existential (and therefore pure existential) statements might be derived, the converse is not true. From existential statements one cannot infer universal statements; indeed from pure existential statements not even

singular existential statements are derivable. This, according to Popper, is the source of the logical asymmetry between universal statements and pure existential statements, and accordingly between falsification and verification. Owing to this logical power, universal statments are of interest to science as *explanatory hypotheses* which may explain singular events or statements. Pure existential statements on the other hand, are too weak logically to explain anything. Kneale might contest this. He might point out that in the context of testing universal hypotheses, if a conflict arises regarding which basic statements to accept: the conflict is to be mediated, on Popper's [1972] own methodology, by invoking the theory/theories with which the basic statements are impregnated; and then proceeding to draw further test implications from these. In other words, singular existential statements (and therefore the pure existential statements which are derivable from these) owing to their theory-ladenness, permit inference of singular events and statements in much the same way as overtly universal statments do; and presumably explain the events/statements spelling
they imply. Kneale therefore fails to preceive any distinction, in Popper's methodology, between universal statements and singular statements. But since the concept of logical asymmetry depends crucially on this distinction (singular statements can falsify, but not verify universal

statements) Popper's unwitting conflation of the distinction would lead to a collapse of the case for falsificationism.

Deeper issues underlie the varying perceptions of Popper and Kneale on the crucial question of logical asymmetry. Popper [(1974) p. 989] grants Kneale `..... we test singular statements always in connection with universal theories. It is also true that I have said that our language is theory-impregnated ....' Nevertheless he continues: `But although these arguments tend to put singular statements nearer to universal statments than is usually assumed; they are far from suggesting that only universal statements are testable and thus empirical ...'. Yet it is precisely the case that in Popper's methodology only universal statements are falsifiable. To see how this is so, we first note that Hilary Putnam [(1974) p. 222] points to `the remarkable fact' that the *Logic of Scientific Discovery* `contains but a half-dozen brief references to the *application* of scientific theories'. Elsewhere Kuhn [197~~4~~0] expresses his dissatisfaction with the falsifiability criterion, which he says is purely syntactic in character. It covers a relation between statements and statements, not between statements and experience/observation/experiment. All this would appear to belabour the obvious; for Popper [(1983) p. xxii] himself has always emphasized that his criterion is a purely logical or syntactical one, based on the relation of logical asymmetry between universal

So?

statements and singular statements. The *application* of the criterion and the difficulties thereof, are not according to Popper, the business of the methodologist. Popper therefore, makes it a point to distinguish between `falsifiability' and `falsification'. Whereas falsifiability is a logical or syntactical relation between statments; actual falsifications belong to the praxis of science. Falsification and the difficulties and uncertainties thereof do not concern the theory of science. Obviously Popper considers the difficulties to be of a purely practical nature. But in point of fact, the application of the criterion involves a major *theoretical* difficulty which Popper has completely overlooked, and which is responsible for his misunderstanding of Kneale's criticism. The point in brief, is this: Relative to a set of *accepted* basic statements, logical asymmetry between universal statements and the accepted basic statements prevails. But if any controversy arises regarding which basic statements to accept, then the test-procedure laid down by Popper to resolve such conflicts, involves exploiting the theory-ladenness of the basic statements for inferential purposes[5].

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5 The concept of a `test-procedure' for resolving conflicts regarding basic statements is in any case, problematic. For example if A and B disagree over `a is a white swan'; the disagreement could concern the application of (i) `white' and/or (ii) `swan'. In either case Popper advocates invoking of the theory which constitutes the principle of application of the terms. But disagreement could arise over this. In fact, if the

Contd.

Such an exploitation emphasizes the universal aspect inherent in singular statements; and *in the context* obliterates the distinction between universal and singular statements. Logical asymmetry then ceases to prevail. Since the acceptance/non-acceptance of basic statements belongs to the domain of the praxis of science, such a contingency can arise only within the context of an application of the criterion of falsifiability, Popper [1972]. The criterion would then cease to be applicable. But this means that Popper's criterion is applicable only relative to a *consensus* regarding basic statements.

Popper [1972] certainly considers the acceptance of basic statements a matter of consensus or convention. This is precisely what Ayer [1974] accuses Popper of; to which Popper responds that convention need not be arbitrary. But Popper totally misses the point that whilst convention need not be arbitrary, its rationale is certainly non-empirical. That is the whole point about terming it a convention. What the argument leads to is that consensus regarding basic statments is *logically necessary* for the application of the criterion of falsifiability. This simply means that basic

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Contd. 5...

causal mechanisms between experience and perception are assumed to be functioning, disagreement could arise over the semantic theory with which the terms are laden and it is not obvious that such a controversy could be resolved empirically.

But Popper might answer that we do not take metaphysical theories to be enjoying consensus (even contextually).

statements accepted in the context of testing a universal hypotheses, are ***in that context*** non-falsifiable, and therefore by Popper's criterion, metaphysical. Falsifiability then, is relative to a set of metaphysical assumptions. This would seem to undermine the criterion as a criterion of demarcation between science and metaphysics.

Popper's methodology at this stage, begins to bear a ghost-like resemblance to the foundationalist theories against which he was reacting[6]. But the dilemma appears to be insoluble: if logical asymmetry is to prevail and universal hypotheses be falsifiable, then basic statments accepted in that context must be regarded as untestable; if on the other hand, basic statments come under a cloud, then logical asymmetry no longer holds, and in the changed context universal statements cease to be testable. It seems therefore, that Popper cannot after all, make the transition from a position of naive falsificationism which considers the `empirical basis' as incorrigible; to that of `sophisticated falsificationism' wherein both basis and hypothesis are considered fallible. Perhaps it is this internal contradiction in Popper's methodology which provoked Kuhn ([1974] p. 808) to remark that although Popper

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6 Nola points out that a ..... of Popper's criticism of justificationist methodology, is that it lacks the firm foundation of an incorrigible empirical basis from which to carry out its `inductions'. But the argument seems to cut both ways for it is now apparent that falsificationism cannot do without such a basis either.

was not (consciously) a naive falsificationist, yet Kuhn considered that Popper might `legitimately' be treated as one'.

`Sophisticated falsificationism' which is the methodological position Popper formally espouses, is not merely riddled with internal contradiction. In fact, the rationale of theory-ladenness which underlies this position, leads to the abandonment of the principle of empiricism, and with this the raison d'etre of Popper's falsificationist methodology collapses. In this context Susan Haack [1991] presents a cogent argument[7]: Popper ([1972] p.105) characterises basic statements as observational in content; yet he insists that ***basic statements cannot be justified or supported by experience***. This startlingly negative thesis is stated quite unambiguously:

".... the decision to accept a basic statement ... is causally connected with our experiences..... But we do not attempt to justify basic statements by these experiences. Experiences can ***motivate a decision***, and hence an acceptance or rejection of a statement, but a basic statement cannot be justified by them - no more than by thumping the table'.

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7 Haack's position is so closely argued that it is reproduced here practically ad verbatim.

Haack distinguishes two arguments at work here. The first goes something like this: Basic statements are theory impregnated. The content of a statement like `Here is a glass of water' goes beyond what is immediately observable; for the use of general terms like `glass' and `water' implies that the container and the contained substance would behave thus and so in these or those hypothetical circumstances. So basic statements could be justified by experience only if some kind of ampliative inference from a thing's observable character -istics to its future and hypothetical behaviour, could support them. But since only evidence which is deductively conclusive can support a statement, it follows that basic statements cannot be justified by experience. Since its crucial premise is that there is no supportive evidence which is not deductively conclusive, Haack refers to this as the `anti-inductivist' argument.

The second argument goes something like this: there can be causal relations between a person's experiences and his acceptance or rejection of a basic statement. A's seeing a black swan for instance, may cause him to reject the statement `All swans are white'. But there cannot be logical relations between experiences and statements. `Here is a black swan' logically implies `There is at least one black swan' and is logically incompatible with `all swans are white'; but it makes no sense to speak of A's seeing a black swan as implying `There is at least one black swan' or incompatible with `All swans are white'. (To speak this way would be a sort of category mistake). So basic statements could be supported by experience only if justification were a causal, psychological concept. But since justification is not a causal but a logical notion, it follows that basic statements cannot be justified by experience. Since its crucial premise is that justification is a logical rather than a psychological concept, Haack refers to this as the anti-psychologistic argument.

Both arguments are valid, but their conclusion, Haack points out, is simply incredible. For what is being claimed is that scientist's perceptual experiences are epistemologically, wholly irrelevant. A scientific theory is said to be `refuted' or `falsified' if it is incompatible with an accepted basic statement. But since the acceptance of basic statements is in no epistemologically relevant way supported or justified by experience, it seems we have no reason to suppose that accepted basic statements are true; nor consequently that a `refuted' or `falsified' theory is false. Sciencce is not after all, even negatively under the control of experience.

Popper's position then, is that basic statements cannot *for logical reasons*, be supported by experience. Instead basic statements are accepted by convention. Of course, Popper denies, as has been noted, that convention is arbitrary. He maintains that basic statements, if disputed can be tested against other basic statements; with the process resting, temporarily and provisionally of course, with basic statements ***which are readily testable***. But as Watkins ([1984] p. 53) points out, having arrived at some basic satements which is especially easy to test, scientists surely ought, before they accept it, `to make one last effort and actually test it'. What Watkins, Haack, Ayer et.al. are driving at, is that testing must, ultimately at some point, be testing against ***experience***. Otherwise the principle of empiricism is abandoned. But this path of testing against experience is closed to Popper because it militates against assumptions that are fundamental to his epistomology. That "justification" is logiccal rather than psychological is the fundamental idea behind Popper's ([1972 b] p.106-52) `epistemology without a knowing subject'; and that since induction is invalid, scientific method must involve support relations which are exclusively deductive in character is the fundamental idea behind Popper's falsificationist methodology. But together these assumptions militate against the principle of empiricism. That is why Quinton [1966] has pointed out that Popper's conventionalism about basic statements, which in turn stems from his thesis of theory-laden observation, undermines his whole theory of empirical knowledge.

The contradictions which riddle sophisticated falsificationism stem from the pervasive problem of theory laden observation, which bedevills all philosophy of science especially in the second half of this century. in this century. The ramifications and implications of this problem will be analysed at a later stage. But first we note what remains of Popper's position. The rationale of Popper's falsificationist methodology is avowedly the principle of empiricism. This rationale grounds itself on logical asymmetry. But owing to theory-ladenness, logical asymmetry prevails only if acccepted basic statements are treated as incorrigible. Accepted basic statements are not only untestable, i.e. non-falsifiable in context; but basic statements cannot ever, *in any context* be justified or supported by experiencce. This leads to the abandonment of the principle of empiricism. So the rationale of falsificationist methodology is not after all, the principle of empiricism. Popper's methodology therefore, cannot claim an edge, in this regard over justificationist (i.e. inductivist) or conventionalist methodologies. They remain viable alternatives. But it is true that relative to a basis of conventionally accepted basic statements, induction i.e. verifiability is logically invalid, and falsifiability is logically valid. So *within a context of strictly metaphysical assumptions*, falsifiability is logically preferable to verification. A position of naive falsificationism might therefore appear to be tenable for

Popper; but its rationale after all would only be Narrow Rationalism. Swann would apear to be right after all!

Yet when we further explore the nature of the metaphysical assumptions which are implicit in the accceptance of basic satements, even this logical asymmetry vanishes. What remains is simply, a contrast in epistemic attitudes. To see how this is so it might first be noted that the form of Popper's basic statements is that of singular existential statements. Acceptance of such statements involves (i) the acceptance of existential claims. Commonsensically, such claims are decided by an appeal to experience. but Popper's anti-psychologism precludes this. Hence even existential claims must be decided by agreement or consensus (ii) acceptance of a theory of semantic classification of objects into kinds. This is because the universal terms in singular statements are `theory laden' or `theory soaked' ([1969] p. 118f, 279, 388) which means that their principle of application is a law or a theory. but since no law/theory can be justified as true, the acceptance of universal laws for purposes of semantic classification amounts to pure convention ***at the theoretical level***. This leads to the assimilation of falsificationism to the position of conventionalism. Popper ([1983] p. xxi) himself seems to dimly realise this when he considers objections to his falsificationist criterion. He maintains that the statement `All swans are white', is by his criterion falsifiable. But then he goes on to concede: `Suppose, however that there is someone who, when a non-white swan is shown to him, takes the position that it cannot be a swan, since it is "essential" for a swan to be white.... such a position amounts to holding non-white swans as logically imposssible structures (and thus also as unobservable). It excludes them from the class of potential falsifiers. Relative to this *altered* class of potential falsifiers the statemet `All swans are white' is of course unfalsifiable. In order to avoid such a move, we can demand that anyone who advocates the empirical - scientific character of a theory must be able to specify under what conditions he would be prepared to regard it as falsified....'. What does this argument amount to? Popper is simply demanding that anyone who wishes to consider a theory as `empirical-scientific' must accept a semantic classification of the universal term which does not include the property under test as a defining propety of the kind. If `All swans are white' is to be falsifiable, then whiteness must not be considered a defining property of swans. But if we remember that Popper's thesis of theory-laden observation leads to the abandonment of the principle of empiricism, then `falsifiable' does not mean capable of being proved untrue; hence there would appear to be no premium on seeking `falsifiable' theories in science. So if `falsifiable' is relative to a semantic specification which

This is not convincing. However, it is not necessary to show that the thesis of asymmetry is undermined by the thesis of theory-ladenness of observation so far as the major thrust of your attack on Popper is concerned.

Did inductivists maintain the theory-ladenness of observations?

considers the property under test to be a non-defining property; then `verifiable' would be logically tenable relative to a semantic specification whcih considers the property to be a defining property of the kind. Thus if `whiteness' is considered a defining property of swans, then `All swans are white' is obvisously true by definition. But then `All swans are white' is also `falsifiable' only by virtue of definition. Logical asymmetry now vanishes, for accepted basic statements can now both `verify' and `falsify' a theory; provided only that the choice of background semantic theory is made accordingly. And such a choice can reflect only epistemic preferences.

Can we call it verifica-tion.

how?

There would therefore appear to be nothing in logic or in experience to choose between the inductivist and Popper's falsificationist methodology. In fact, granted the theory-ladenness of observation, both methodologies converge upon the conventionalist position; and relative to such a conventionalist basis, only epistemic preferences, which in their semantic form amount to cultural predilections, would appear to adjudicate between competing methodologies of science. This is also the conclusion to which one is lead, in considering Popper's criticism of neo-justificationism (or the attempt to establish scientific theories as probable in the sense of the proability colculus).

1.3 The Popperian Concept of Evidence

First we note that the inferential structure generated by methodological falsificationism evades, as Quine ([1974] p. 218-220] has remarked, Hempel's `raven paradoxes of confirmation'. This can be understood in the following way: The symbolic form of a universal generalisation is a hypothetical conditional of the form (x) (S x --> W x). By the rules of material implication, if the antecedent is false, the statement as a whole, is true. In the context of a generalisation like `All swans are white' this means that in a Universe practically devoid of swans in most parts, the generalisation would be `cheaply' or `vacuously' verified almost all the time. To preempt this, Popper incorporates into his structure the requirement of initial conditions in the form of singular existential statements. This composite structure evades the `paradoxes of confirmation'.

$(x)\ \varphi x \supset \psi$

Popper's [(1974) p. 990-993] own argument is presented thus: Firstly, from a universal generalisation alone, without initial conditions, ***nothing observable follows*** `All swans are white' and `All swans are black' contradict each other only on the assumption that atleast one swan ***exists***. Together therefore, they entail `No swans exist'. ***This statement*** cannot be `confirmed' or `verified' by any experience, it can only be refuted, by finding a swan. Thus no empirically verifiable statement) follows from a purely universal theory. In particular, the so-called `positive instances' of a law of the form `within the spatio-temporal region k there is a white swan' cannot be deduced without existential assumptions. `Instantial' statements which *can* be deduced without initial conditions have the form `within the spatio-temporal region k there is either no swan, or else a swan that is white'. These type of instantial statements Popper considers completely ***valueless*** and ***uninteresting***, because they permit vacuous verification. They betray an `inductivist prejudice'. Such inference however, is not ***logically*** invalid. Hence Popper ([1983] p. 234-235) distinguishes, in this context, between ***attitudes***:

(a) The uncritical or verificatinist attitude: one looks out for `verification' or `confirmation' or `instantiation', and one finds it as a rule. Every observed `instance' of the theory is thought to `confirm' the theory.

(b) The critical attitude, or falsificationist attitude: one looks for falsification, or for counter-instances. Only if the most conscientious search for counter-instances does not succeed may we speak of a corroboration of the theory.

Hence for Popper ([1974] p. 990-993] `positive instance' is not `positive evidence'. Only the absence of a counter example may constitute such evidence. Popper thus emphasizes that it is a difference in epistemic attitudes which underlies the diference in the concept of `supporting evidence'. But Popper goes on to argue that whilst the justificationists' concept of `positive instance' as `positive evidence' is not strictly speaking, logically invalid, yet it is nevertheless, counter-intuitive. In this context, he discusses the so-called `inductive syllogism':

Socrates is a man and a mortal

Plato is a man and a mortal

Crito is a man and a mortal

....

....

Conclusion: All men are mortal.

But, Popper argues, if on the evidence of these positive instances `All men are mortal' is valid, then by the symmetry of `and' the same evidence should render `All men are mortal' valid as well. According to Popper, the reason why it doesn't is because of the availability of counter-examples[8].

But when in spite of assiduous efforts, no counter-examples are available yet, then which of competing hypotheses might be held? Nelson Goodman ([1965] ch.3) has argued that it is the `projectibility' of `entrenched' concepts which decides which of competing generalisations

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8 Popper presents one: `My neighbour's bulldog socrates died two years ago; it was mortal but no man'.

are actually accepted. Goodman maintains that we prefer `All emeralds are green' to `All emeralds are grue' (where `grue' applies to all things green before time t and to blue things thereafter) because the predicate `green' is historically entrenched in our language, whilst `grue' is not. Only entrenchment can explain this, according to Goodman, because otherwise the positive evidence for both hypotheses is the same before time t; also before time t no counter-example is available to either hypothesis, so Popper's criterion does not apply. Goodman's solution makes the acceptability of scientific theories relative to facts about language; in particular to the conceptual framework entrenched in a language. Quine ([1974] p. 218-220) `...equates projectibility of predicates to the naturalness of kinds'. Popper ([1974] p. 993) maintains that his way of looking at these problems is somewhat different from Goodman's way. He says `.... in my view, predicates or concepts, are the result of the formation of expectations and theories rather than the other way round...!

What is the upshot of this discussion? The controversy can be clarified in the following way: (1) According to Goodman, subject to the condition that positive instances are available, and no counterexample is known, theory preference is made on the basis of semantic facts What is the nature of these facts? Possibly whilst learning to apply the predicate `green' we are typically both shown green things and also taught `All emeralds are green', so `green' enters into the meaning of the natural kind `emerald' as one of its defining properties. Relative to this semmantic classification of objects into kinds, the generalisation `All emeralds are green' is projectible. Since `grue' is not a predicate of the language, `All emeralds are grue' is not similarly projectible. Thus meaning comes first, theories follow. (2) Popper stands this analysis on the head. He grants a Kantian conceptual framework; but concept-formation according to him, is the *result* of the formation of theories. We first conjecture `All emeralds are green', test it for counter-examples, and finding none, accept this `corrobarated' theory as determining the meaning (though Popper doesn't like the term) of the concept. This is what Popper means by the theory-ladenness of universal terms. But how does this explain why we don't similarly conjecture `All emeralds are grue' etc.? Here Popper ([1974] p. 993) invokes like Quine, `our native primitive intuition of natural kinds' which can be accounted for by `Darwinian natural selection'. What Popper means is that like all Darwinian processes, conjecturing is also a random mutation process, (we can also call it a `creative' process). We are therefore free to conjecture `All emeralds are grue' etc. But as in the case of all natural process, Nature `selects' some conjectures by eliminating others as falsified. Our semantic conceptual framework therefore, is the result of a process of Darwinian selection, and this constitutes its objective rationale. Moreover, pending refutation, theory preference can be made with reference to `simplicity', thus `All emeralds are grue' predicts change (i.e. emeralds after time t will turn blue) where `All emeralds are green' predicts none. But Mary Hesse ([1974] p. 75-82) points out that change can only be predicted relative to the acceptance of a common theory which defines the change. Protagonists of the `green' hypothesis and protogonists of the `grue' hypothesis can agree to what constitutes a colour change only if colours are defined, not circularly in terms of `green' and `grue'; but in terms of a commonly accepted scientific theory. (e.g. the electromagnetic theory of light which defines colour in terms of wave-length). But then according to Popper, our concepts *are* defined in terms of scientific theories; and it is rational to accept these theories for semantic purposes, because the conceptual framework which incorporates these theories, is the ~~objective~~ result of an objective process of Darwinian selection. This would seem to constitute a perfectly satisfactory solution to the `grue' paradox, ***but for the crucial point***: `Refutation' like `verification' is a logical concept; therefore it is a relation which can hold only between statements, not between statements and experience. `Nature' therefore cannot falsify or refute any theory; and with this, Popper's entire attempt to explicate a semantic framework of classification in terms of scientific theories, whose rationale is an objective process of Darwinian selection, falls to the ground. With it, so does his attempted resolution of the `grue' paradox.

1.4 The Neo-justificationist Position

The inferential structure generated by methodological falsificationism does however, manage to evade Hempel's `raven paradoxes of confirmation'. This is because Popper in order to prexmpt vacuous verification, incorporates into his structure the requirement of initial conditions, in the form of singular existential statements. Hempel [1965] on the other hand, denies that the statement regarding initial conditions, though part of an explanatory structure, is part of the theory under test. The grounds of denial are: (a) Logical equivalences (of universal statements) are accepted as permissible in general usage, as for example in `All sodium salts burn yellow' which is treated as logically equivalent to `Whatever does not burn yellow is no sodium salt' (b) Customary formulations in science do not contain an existential clause (c) Many universal hypotheses cannot be said to imply an existential clause at all. One notes that (a) underscores the nomic character of laws in Hempel's deductive-nomological model. The invoking of logical equivalences is tantamount to considering the property under test as a defining property; which in a context of confirmation has a somewhat paradoxical air, (b) is challenged by Kuhn [1970] who maintains that theories are always accompanied by exemplars which are striking applications of the law. Apart from the stated reasons, the deeper motives for rejecting the statement regarding initial conditions would appear to be the need to approximate test - conditions to the random sampling conditions required for the application of the probability calculus to the context of confirmation. In considering this neo-justificationist position which seeks to explicate confirmed hypotheses as probable in the sense of the probability calculus, we first note some general conditions for any theory of confirmation, first laid down by Hempel. Mary Hesse [1974] reformulates some of these conditions as:

(i) Equivalence: Logically equivalent expressions should have identical effects in confirming logically equivalent expressions.

i.e. If $g \equiv g^1$, and $h \equiv h^1$,

then if g confirms h, g^1 confirms h^1.

(ii) Entailment: Any entailment of a proposition h must be confirmed by h.

i.e. If h --> g, then h confirms g.

(iii) Converse entailment: If h --> g, then g confirms h

(iv) Special consequence: If f confirms h, and h --> g, then f confirms g.

Hesse points out that the equivalence condition together with converse entailment and Nicod's criteria, generates the raven paradoxes; whereas converse entailment and special consequence jointly lead to the transitivity paradox. Furthermore, the raven paradoxes affect directly the confirmation of universal generalisations; whereas the transitivity paradox afflicts `next instance' confirmation. But since the confirmation of hypotheses is required for explanation in science, and `next instance' confirmation explicates predictive inference; the resolution of these paradoxes assumes importance for any theory of confirmation.

Hesse believes with others, that if the logic of confirmation is explicated[9] in terms of probability logic, augmented by a principle of clustering; then the resolution of both paradoxes is possible. Towards this end, Hesse ([1974] p. 133-134) first considers two ways of interpreting `confirmation' as a probability function. One is the `k' criterion whereby we regard a hypothesis h as confirmed by evidence e if and only if the probability of h on e attains atleast some fixed value k such that 1 > k > 1/2. The other is Carnap's `positive relevance criterion' which requires the posterior probability of h on e to be greater than its

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9 Heese [(1974) p. 97] uses the term `explication' in Carnap's sense whereby the rules of use implicitly embedded in a vaguely formulated concept are sought to be made explicit by rigourous formalisation. In the process the original intrutive concept might be modified to sort out ambiguities and contradictions.

initial probability. Thus

e confirms h, iff $p(h/e) > p(h)$.

Hesse opts for the latter criterion as more suitable for explicating the logic of confirmation in terms of a Bayesian confirmation theory.

Hesse ([1974] p. 155-158) formulates the raven paradoxes as following from three apparently innocuous assumptions:

1. The equivalence condition for confirmation.
2. \`All Pare Q' is logically equivalent to \`all $\bar{Q}$ are $\bar{P}$. and to \`Everything which is P or $\bar{P}$ is either $\bar{P}$ or Q.
3. Nicod's criteria: For any P and any Q (i) an object that is P and Q confirms h = \`All P are Q'; and (ii) a $\bar{P}$ and $\bar{Q}$ and (iii) a $\bar{P}$ and Q are respectively irrelevant to h.

Consider h = All ravens are black', or for short \`all R are B'. By (3) this is confirmed by a black raven, and a non-black non-raven is irrelevant to it. But by (2) h is equivalent to $h^1$ = \`All non-black things are non-ravens', which (3 i) is confirmed by non-black non-ravens. By (i) anything which confirms h^1 confirms h, hence non-black non-ravens after all confirm h contrary to [3(ii)]. This constitutes the first paradox. By a similar argument a second paradox follows from (1) and (2) namely that anything that is a black non-raven confirms h, contrary to [3(iii)].

Hesse maintains that the second paradox can be disposed of easily. From (1) and (2) and from converse entailment, it follows that if anything that is R or $\overline{R}$ (that is anything at all) is also either R or B, that thing confirms h. But it does not follow from this that anything which is both R and B confirms h. To know that an object is either not a raven or is black is to have quite different data from the knowledge that it is a black non-raven; and no paradox arises from supposing that something which is only known to be either a non-raven or black confirms `All ravens are black'.

The solution of the second paradox involves accepting (1) and (2) and concentrates on breaking (3). In this context Swinburne [Hesse, 1974] has shown that there are some circumstances of background information under which each of Nicod's criteria are intuitively incorrect. Instead, when we consider further background information regarding the proportion of ravens and non-ravens in the general population, then what we find is not that a R. B and a R.B are respectively irrelevant to `All R are B' as Nicod claims; instead, considering the fact that the universe contains far more non-ravens than ravens, the probabilities computed by Bayesian transformations, indicate that an object which is R.B confirms `All R are B' ***more*** than an object which is R.B. i.e. `positive instances' confer greater confirmation than do `vacuous instances'. This has been held to be a sufficient resolution of this paradox.

Features of this type of `proportionality' solution might be noted: Firstly, it certainly involves existential assumptions. If the evidence e is to include background information regarding the proportion of say, ravens in the population, then it must certainly assume that objects of this kind exist. Moreoever, whilst Popper's model involves existential assumptions regarding only the one kind of object mentioned in the initial conditions (say ravens); the `proportinality' solution invokes assumptions regarding *all kinds* of objects in the Universe (ravens, non-ravens, black, non-black, red herrings, white slippers et.al). Indeed, the proportionality solution presupposes an entire network of semantic classification of objects into kinds.

In fact, it is precisely such a theoretical network of the semantic classification of objects into kinds, which Hesse invokes in order to surmount the difficulties associated with `next instance' confirmation, which is required for predictive inference in science. In this connection we note that the raven paradoxes afflict primarily the confirmation of universal hypotheses. Nevertheless the resolution of these paradoxes might have been relevant for next instance confirmation, if confirmation of h entailed confirmation of next instances. But this is not so, on account of the `transitivity paradox' pointed out by Hesse ([1974] p. 141-150): Consider h = f.g.

Then f confirms h by converse entailment. Also h entails g, hence f confirms g by special consequence. But f and g may be any propositions whatever; and the example shows that the two conditions together entail that f confirms g. A relation of confirmation which allows any proposition to confirm any proposition whatsoever, is obviously trivial and unacceptable. Thus a paradox arises by taking together a set of adequacy conditions, all of which seem to be intuitively required for predictive inference. Hesse terms this the transitivity paradox.

Expressed in Bayesian language, the paradox can be represented as follows: We are interested in the value of p (e_2/e_1) where e_1 is an observed or otherwise given consequence of a theory h; and e_2 is an as yet unobserved or otherwise problematic further consequence of h, that is, an untested prediction made by h. Now p (e_2/e_1) is a single-valued probability function of its arguments alone; and its value cannot depend on whether or not we are interested in some particular h from which e_2 and e_1 are deducible. For successful prediction we require p $(e_2/e_1) >$ p (e_2) which is the condition that e_1 and e_2 are not probabilistically independent, but are related to h by positive relevance. But the transitivity paradox highlights the irrelevance of the hypothesis h to this dependence. Therefore Hesse [1974] concludes that hypothesis confirmation is quite irrelevant and vacuous with respect to prediction.

Resolution of the raven paradoxes therefore, do not help to solve the problems related to next instance confirmation. What is worse, the assumptions underlying the proportionality solution, militate against the assumptions required for successful prediction. This can be understood in the following way: Predictive inference demands that $p(e_2/e_1) > p(e_2)$ i.e. p(bRB/aRB) > p(bRB) or p(bRB/aRB & bR) > p(bRB). This is the condition that two instance of a hypothesis be positively relevant to each other. But the proportionality solution. Hesse [1974] involves exactly the opposite assumption. Its assignments of initial probabilities based on the proportions of objects in the population, assumes probabilistic independence between properties of objects involved (and hence between instances of the hypothesis). Thus, for example, R or `being a raven' is assumed to be probabilistically independent of B `or being black' i.e. p(B/R) = p(B) or p(R.B) = p(R)p(B). Since by the multiplication thereon the probability of an object being R.B. is a function of the initial probability distribution; once this distribution has been fixed on the asumption of stochastic independence, evidence to the effect that all observed R's are B, can alter it only on pain of altering the conditions of the experiment. From this perspective it is obvious that the proportionality solution to the raven paradoxes is riddled with contradiction; also that its basic assumptions militate against the requirements of next instance confirmation. Next-instance confirmation requires, as has already been noted, positive relevance amongst instances; i.e. given that an object a is RB and b is R, it is initially more probable that bB than that bB. This is the requirement that the properties R and B are not probabilistically independent, but that p (B/R) > p(B). Hesse satisfies this requirement by adopting the postulates of `exchangeability' and `clustering'.

The exchangeability condition is the condition of randomness of selection of individuals. Carnap [1962] calls it symmetry of individuals. DeFinnetti, Hesse ([1974] p. 153) elucidates exchangeability by the example of a sequences of coin-tosses. He says: `..... It is particularly interesting to study the case where the probability does not depend on the order of trials. In this case every result having the same frequency r/n on n trials has the same probability ... if this condition is satisfied, we will say that the events of the class being considered, for e.g. the different tosses in the example of tossing coins, are exchangeable (in relation to our judgement of probability).

DeFinnetti goes on to maintain that events are considered exchangeable i.e. of the same type when they have *analogous* characteristics, but considers the judgement of analogy to be arbitrary. Hesse, however upholds a resemblance theory of universals whereby objects are classified into natural kinds, based on an inter-subjectively valid pattern of similarities and differences. On the basis of this classification, objects which belong to the same natural kind are analogous, and therefore exchangeable. Since only exchangeable events constitute a random sequence to which the probability calculus is applicable; only objects which belong to the same natural kind (on the basis of resemblance) constitute the reference class for probabilistic confirmation. But this means that only objects which are ravens are relevant for testing `All ravens are black'. This simply brings us back to Popper's position!

Since exchangeability is insufficient for instance confirmation, Hesse adopts a clustering postulate: Given r instances of p's it is initially more probable that none or all will be positive insances of `All P are Q' than that there will be any other proportion of Q's. The clustering postulate would appear to be utterly gratuitious and ad hoc unless combined with the intuition underlying the exchangeability condition: The judgement of exchangeability is based on inter-subjective analogy. This means that objects are classified into natural kinds based on similarities in salient respects. Now the clustering postulate expresses the intuition that since objects which belong to the same natural kind are similar in many important respects, they probably resemble each other in some further respect as well. Thus, since ravens constitute a natural kind on the basis of resemblance in a large number of properties; then if a large number of ravens are observed to share a further property, say of blackness, then by exchangeability and clustering, we might infer that the next raven is probably black as well. Thus exchangeability and clustering explicate according to Hesse, next instance confirmation, which permits predictive inference in science. According to Hesse, Carnap also adopts a version of the clustering postulate.

1.5 The Issue of Stochastic Independence

What is the upshot of this discussion of the neo-justificationist position, which seeks to explicate confirmation as a probability in the sense of the probability calculus? Two major conclusions emerge: (1) Firstly if it is assumed that properties are probabilistically independdent, then the probability of hypotheses which are universal generalisations is zero i.e. $p(h) = 0 = p(h/e)$. This result is independent of the evidence, and is therefore, devastating for any theory of probabilistic confirmation. Also, where confirmation of 'next instance' is concerned, the assumption of probabilistic independence between properties yields undesirably low probabilities for prediction. (2) On the other hand, if we assume probabilistic dependence between properties, this dependence invokes the semantic theory of classification of objects into kinds. This is because the dependence involves analogical inference from the criterial properties of kinds to the property under test; or simply considers the property as an essential property of the kind. This type of natural kind inference captures precisely the sense of nomic necessity which permits predictive and counterfactual inference in Hempel's nomological-deductive model. Evidence has a role to play here, but only in Popper's sense of the absence of a counter example. The implications of both positions are further analysed.

Position I which assumes probabilistic independence between properties is the position that Popper espouses. It is implicit in his criterion of falsifiability. Thus `All swans are white' is falsifiable only if whiteness is not considered an essential property of the natural kind `swan'; and it is falsifiable to a greater degree if there is no analogical inference from the properties of swans to the property of whiteness. Now, on the assumption of probabilistic independence Carnap [1962] shows that for a universal generalisation of the type h = all p_1 are p_2, on the evidencce e of s positive and no negative instances, confirmation C_o (h) is zero; and hence C_o (h/e) is zero for any e whatever. This result holds in general for the C_o value of any universally quantified hypothesis in an inifite domain, and has been considered the death-blow to any confirmation theory of Carnap's type, since it is generally assumed that universality in infinite domains is an essential characteristic of scientific laws and theories. Hesse comments that it is easy to see that the same result must follow for any method of calculating initial probabilities that depends on indifference among structure descriptions i.e. on an assumption of probabilistic independence, since the number of structure descriptions is infinite for infinite n. Popper ([1972] p.257) emphasizes the same result: `One might ascribe to a hypothesis .... a probability, calculated, say, by estimating the ratio between all the tests passed by it to all those (conceivable) tests which have not (yet) been attempted. But this way, too, leads nowhere; for this estimate can be computed with precision, and the result is always that the probability is zero'. It is clear, therefore that on the assumption of probabilistic independence, the probability of a universal generalisation is always zero. What is worse, this result holds irrespective of any evidence whatsoever. It holds for any amount of favourable evidence and also for any amount of unfavourable evidence in the form of negative instances. This is because the probability of a refuted generalisation is also zero. But this conclusion is absolutely damning for any theory of probabilistic confirmation which seeks to explicate the relation between hypothesis h and evidence e, as a probability such that p(h) increases with increasing favourable evidence.

Carnap's [1962] own response to the zero confirmation of universal laws is to argue that the application of inductive logic never involves more than finite sets of instances; and so he is content to allow non-zero confirmation values only to what he calls `instance confirmation' (the probability that the next individual will be a positive instance of a law) and to `qualified instance confirmation' (the probability that the next instance satisfying the antecedent of the law will satisfy its consequent). But on the assumption of indifference over state descriptions, Carnap's C_o function yields values of instance confirmation as 1/4 and of qualified instance confimation as 1/2. What is worse, in such a loose and separate world i.e. a Humean world or the world of Wittgenstein's Tractatus, there is, according to Carnap ([1972] p. 562, 565) `no learning from experience'; This is because although the evidence of favourable observations might indicate probabilistic dependence between properties, this evidence is not reflected in the distribution of initial probabilities which continues to assume on indifference principle of equi-probabilities over all state-descriptions. According to Popper ([1983] p. 316-319), it is inevitable that the probability-distribution should not change in response to evidence consisting of past repetitions; for otherwise this leads to the `paradox of inductive learning'. The paradox consists in this: the condition of randomness or of DeFinnetti's exchangeability requires that the probability of an event remain unaffected by the results of past repetitions. Otherwise, these would not constitute repetitions of the same experiments and the probability calculus would not be applicable to the sequence. Inductivists, on the other hand, demand that the results of past repetitions increase the probability of an event. This leads to the paradox which Popper puts thus: `Assume that our knowledge grows, in accordance with the subjective (inductivist) theory, if and only if we observe a repetition of an experiment. Then it cannot grow; for since its growth would alter the known conditions of any experiment, no experiment an ever be repeated. In other words, the assumption that the new experiment is a repetition of the old one is contradictory, from the subjective (inductive) point of view. For if it is a repetition, then the simple inductive rule applies which makes all past instances highly relevant conditions, so that it must be a case essentially different from the previous cases. Thus, no experiment can ever be repeated?

The solution to Popper's paradox of inductive learning consists (1) in granting that evidence in the form of past observations of favourable instances must not be invoked for increasing the probability (and hence the confirmation) of either hypothesis or next instances. (2) Instead the function of such evidence is to refute the earlier distribution made on the assumption of probabilistic independence, and to suggest a *fresh* distribution which reflects probabilistic dependence between properties. Colin Howson [1987] emphasizes this view: He says that Popper is celebrated for his view that if h is any non-tautologous universal hypothesis interpreted over an infinite domain D, and e is any statement describing the properties of a finite set of individuals in D, then

$$p\ (h/e) = 0$$

According to Howson, a majority of Popper's arguments for this conclusion are based on his use either of so-called `classical' measures on certain types of probability-space or on close relations of these, namely independence measures on product-space. But Howson maintains that such measures have no privileged status in supplying the foundation for a theory of inductive inference; but then also, *no* a priori measure has. Popper's insistence on a favoured a priori distribution, even if it has anti-inductivist implications, is therefore at odds with his generally fallibist philosophy. Apart from technical objections, Howson argues that the heart of the matter is the question of why prior probabilities should be assigned the way Popper urges. There is certainly nothing in logic that tells us this must be so. Now suppose, Howson continues, that for any given method of assigning probabilities a priori, we think of the sort of physical probability model or models which yield the same values (i.e. p(h) = 0], assuming that we are permitted to think of types of world as outcomes of some stochastic trial. We do not have to look very far to see what sort of model gives the values obtained via that classical method which assigns equal probabilities to the $2^n 2$-Q predicate state description of length n. It is just that which the elementary possibilities, state-descriptions or points in a continuum are completely randomly generated and in which therefore, very strong conditions of independence hold. This is why, according to Howson, Carnap rejects his earlier confirmation measure.

It is certainly legitimate, Howson further continues, to question why the probability function characterizing such a random model, or more generally any model generating independence in a product-space should be thought the only one appropriate for assigning probabilities a priori. Why should we assume that the correct evaluation a priori of say, the probability that the (i + 1)st individual examined will be A conditional on the first i being A, is that which characterizes the picking of balls at random from a randomly structured urn? .... Why indeed? For random models are maximally disorderly, and an a priori assumption of randomness here is equivalent to an assumption that generalisations i.e. very low entropy states are extremely unlikely to hold in the limit as the universe becomes very large without bound. Howson says Popper assumes an extreme bias against generalisations; which does *not* correspond to an attitude of epistemic neutrality. ***It all depends on how you characterize the possibilities.*** The choice of ultimate partition is not a logical matter; indeed it is ***determined entirely by what you think is an appropriate system of categories.*** To assume a priori that the possibilities are equally weighted, given the evidence which strongly suggests a highly structured universe is, to say the least perverse.

Interestingly enough, Howson says, Popper's approved prior distribution over state-descriptions with two `observable' Q-predicates is, if formulated as a statistical hypothesis, testable by the usual method for testing statistical hypotheses. Moreover it (i.e. on an a priori distribution which assumes independence) would be rejected in a considerable number of cases where there is a highly confirmed hypothesis that a particular effect is invariably forthcoming. Thus were Popper to take his own prior distributions as hypotheses, he would actually not only find them as sometimes falsified; but would also find that in these cases the true distributions are those which seem to assign a probability in the neighbourhood of 1 to a hypothesis approaching the strength of a universal generalisation. Howson concludes that Popper's attempted disproof [via the thesis that p (h/e) = 0] of the possibility of probabilistic inductive confirmation of laws is vitiated.

Ken Gemes ([1989] p. 183j) also considers the assumption of stochastic independence to be `the heart of Popperian inductive scepticism'. He claims to derive a contradiction from four statements of probabilistic independence; which he thinks are entailed by Popper's position and this, he thinks refutes Popperian inductive scepticism. But David Miller ([1990] p. 137-139) points out that Gemes' so-called `proof' is flawed; and that this restores Popperian inductive scepticism. Furthermore, Miller goes on to argue that the principle of instantial irrelevance P (Fa/Fb) = P (Fa) is far from the `heart of Popper's inductive scepticism'. According to Miller it is Hume's argument viz. the invalidity of inductive inference, which is the heart of inductive scepticism. In Popper, the principle (of independence) is introduced not for its own sake at all; but as one of the assumptions of an argument that universal hypothesis should receive probability 0. Yet the crucial argument here is now the argument of Popper and Miller ([1983] p. 687f; [1984] p. 434) that all positive probabilistic relevance has its origin in purely deductive relations. Popper and Miller offer the following proof:

Let h be any hypothesis, and e (possible) evidence in favour of it. In a simple case e is deducible from h in the presence of background knowledge b (b includes the initial conditions needed to derive the prediction e from h, and it may for the time being be regarded as unproblematic). Then it can be shown that:

$$P(h, eb) > p(h,b) \text{ provided } p(h,b) > 0.$$

This would seem to justify the belief in induction. But h can be split up into two factors, one of which (h v e) is deductively implied by e; and the other factor (h <-- e) contains all of h that goes beyond e. Popper and Miller then go on to prove that e probabilistically supports only that part of h i.e. (h v e) which is deductively implied by e. What is more, e *counter supports* all of h i.e. (h <-- e) that goes beyond e; and this counter support is the greater, the greater the content of e. Indeed the counter support increases with the content of e, whether e supports h or not. Popper and Miller conclude: `This result is completely devastating to the inductive interpretation of the calculus of probability. All probabilistic support is purely deductive: the part of a hypothesis that is not deductively entailed by the evidence is always strongly counter-supported by the evidence - the more strongly the more the evidence asserts. This is completely general; it holds for every hypothesis h; and it holds for every evidence e, whether it supports h, is independent of h, or counter-supports h. There is such a thing as probabilistic support; there might even be such a thing as inductive support (though we hardly think so). But the calculus of probability reveals that probabilistic support cannot be inductive support'.

Ellery Ellis ([1988] p. 111-116) argues that if we are careful in distinguishing between the ideas of `*support that is purely deductive in character*' and `*support of a deductively implied hypothesis*'; it is easy to see that Popper and Miller's argument fails to establish the conclusion that all probabilistic support is purely deductive in nature. Ellis's argument is as follows: According to the Bayesian theory of probabilistic inductive support, the degree to which evidence e supports a hypothesis h is given by the measure: s(h/e) = p(h/e)-p(h). Where p is an appropriate probability measure, p (h) is the prior probability of h and p(h/e) its posterior probability. If s(h/e) is positive then e cnfms h; disconfirmation (counter-support) and its degree are indicated by a negative s(h/e), and evidential neutrality by s(h/e) = 0.

Ellis gives the gist of the Popper-Miller argument thus: For any hypothesis h and evidence e, h is logically equivalent to the conjunction (h v e) and (hv-e) (or h <-- e). For simplicity, assume in what follows a probability p that assigns only non-extreme (not 0 and not 1) values to h, to e and to non-tautologous truth - functional compounds of h and e. Then it is easy to see that

$$s\ (h/e) = s\ (hve/e) + s\ (hv\text{-}e/e)$$

It is also easy to see that s (hve/e) is necessarily positive and that s (hv-e/e) is necessarily negative. Popper and Miller point out that the disjunction (h v e) deductively follows from e and that it is the strongest part of h. (hv-e) is the weakest part of h that in conjunction with (h v e) is equivalent to h. They call (hv-e) \`all of h that goes beyond e'. e supports (h v e) and counter supports (hv-e).

Ellis contests the inference from \`e probabilistically supports only that part of h which is deductively entailed by e' to \`all probablistic support (as opposed to counter-support) is purely deductive'. He maintains that on a proper understanding of inductive support; even if an item X deductively entails an item Y, some aspects of X's support to Y may be purely inductive in nature. Given that X --> Y, S (Y/X) is a function wholly of P(Y). The function is 1-P(Y) where p(Y) is completely independent of deductive relations between X and Y.

By means of numerical examples Ellis establishes that the over-all probabilistic support cannot be a function of just the evidence's deductive support and counter support.

This is further substantiated by Popper and Miller's associateing *a degree* with the component of an evidence's support of a hypothesis that they call purely deductive in nature. But support that is purely deductive in nature is an `all or nothing' affair; either the evidence fully guarantees the truth of the hypothesis (deductively implies it) or not (does not deductively imply it). Purely deductive support does not come in degrees.

Ellis therefore argues that the significance of s(hve/e) is the *difference* between the posterior and the prior probabilities of (h v e) on e. The fact that the posterior probability is 1, is a consequence of the fact that e deductively implies (h v e); but it is this fact alone about s(hve/e), along with the consequences of this fact such as the measures necessarily being non-negative-that has anything to do with e's purely deductive support of (h v e). On the other hand, the particular magnitude of s (hve/e) being a degree, and a function partly of p(h v e) [i.e. the initial probability (h v e)], clearly `goes beyond' the deductive relations between e and (h v e). This aspect of s (hve/e) could be correctly described as representing *inductive* support of (h v e) by e. Thus:

$$s\ (hve/e) = p\ (hve/e) - p\ (h \vee e)$$

$$= 1 - p\ (h \vee e)$$

.. if p (h v e) i.e. p (e) << 1/2, s (hve/e) increases.

Ellis concludes that Popper and Miller *have* shown that the evidence only probabilistically supports that part of the hypothesis which the evidence deductively implies and probabilistically counter supports the rest. But their argument fails to establish the conclusion that all probabilistic support is purely deductive in nature.

1.6 Corroboration or Probability?

The threads of this discussion can be wound up in the following way: Howson queries why initial probabilities must assume probabilistic independence between properties. According to him there is nothing in logic to prejudge this. According to Popper and Miller, it is precisely logic which dictates on a priori distribution that reflects stochastic independence. The logic is Hume's argument to the effect that `inductive inference' is invalid. Hence any inference to the validity of universal generalisations ***must*** be invalid; as well as predictive inference to future instances; for both involve ampliative inference which is non-deductive. Furthermore, since all probabilistic support is also purely deductive (they claim); the probability calculus in its logical interpretation (as a relation between statements) ***must*** yield p(h)=0=p (h/e), and also yield values for next instance confirmation as much less than the required levels for prediction. Ellis maintains that aspects of e's support of h are inductive, but he grants Popper and Miller their main contention viz. that the evidence only probabilistically supports that part of the hypothesis which it deductively entails, and counter supports the rest. This means that for a hypothesis of unrestricted generality, evidence which can only be finite, tautologously yields $p(h) = 0 = p(h/e)$. The appropriate distribution of prior probabilities is therefore precisely the distribution which yields this result; and this just happens to be the one which assumes stochastic independence between properties. A loose Humean world therefore, with no metaphysical cement between properties, would appear to be forced upon us by logic.

The argument from logic is buttressed by Popper ([1969] p.286-287; [1983] p. 224) with an appeal to the principle of empiricism. Since science aims at ***growth*** which is also ***empirically testable***, it aims at theories with high empirical content. But by the probabilistic axiom of monotony or the `rule of content', probability is inversely proportional to content. Hence science must aim at theories which have low probability.

Popper's [1983] final argument in favour of a probabilistic distribution which assigns $p(h) = 0$, is to formulate his own conception of the relation between hypothesis and evidence, which satisfies the following intuitive requirements: (1) Corroboration of a theory (or

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The Head
Department of Philosophy,
Goa University
Taleigao Plateau, Goa.

Sub: Ph.D. viva-voce examination.

Sir,

This is to inform you that the viva voce examination of Smt. Milan B. Desai, Ph.D. student in Philosophy of this University is fixed on Saturday, 8th March, 1997 at 12.00 a.m. in the Department of Philosophy, Goa University, Taleigao Plateau, Goa. the other details are as under :

Title of the thesis	:	"A Critical Study of Karl Popper's Theory of Falsification: A Semantic Approach.
Name of the Guiding Teacher	:	Dr. A.V. Afonso Professor and Head Department of Philosophy Goa University.

You are requested to take note of the Ordinance 19.9 (xi). One copy of the thesis has been sent to our Library as per provision of our Ordinance 19.9(xii).

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the support of h (y e) must be an evaluation of the results of the empirical tests it has undergone. (2) There are two attitudes or ways of looking at the relations between theory and experience: one may look for confirmation or for refutation. Scientific tests are always attempted refutations. (3) The difference between attempted confirmation and attempted refutation is largely amenable to logical analysis. (4) Degree of corroboration increases with the severity of tests it has passed. (5) A test is the more severe the greater the probability of failing it. (6) Thus every genuine test is an attempt to `catch' the theory. (7) Assuming sincerity, degree of corroboration increases with the improbability (in the light of background knowledge) of the predicted test statements. Taking into account these intuitive requirements, Popper arrives at the following formula:

$$C\ (h,e,b) = \frac{p\ (e,\ h\ b) - p\ (e,\ b)}{p\ (e,\ hb) - p\ (eh,b) + p\ (e,h)}$$

Since the denominator represents only a normalisation factor, the formula indicates that corroboration is a function of Fisher's likelihood measure i.e. of p (e, h b) as well as of p (e, b). Furthermore, since h --> e, p (e, hb) = 1; hence the function assumes the form of 1 - p (e, b) which is the initial improbability of e with respect to the background theory. The degree of corroboration therefore, increases as e becomes more and more improbable with respect to b. Only on e which is highly improbable therefore, constitutes significant support for h. Donald Gillies ([1990] p. 143-146) calls that `the principle of severe testing' and perceives it as the cornerstone of Popper's theory of corroboration. Gillies compares Popper's support function with the Turing - Good `weight of evidence' function; and maintains that the two are very closely related, though Popper's function is the superior of the two.

It is also interesting to note that since e is equivalent to (h v e) i.e. to the part of h which e deductively implies, the improbability of e is equivalent to the improbability of (h v e); so that corroboration can also be interpreted as a function of the improbability of (h v e) with respect to the background theory. This brings it close to Ellis's Bayesian support function which construes s(hve/e) as a function of 1-p(h v e). Finally, in view of Popper's and Miller's result that e probabilistically supports only the part of h i.e. (h v e) which it deductively implies, and counter supports the rest i.e. (h <-- e), it follows that the corroborated theory remains as improbable on the evidence, as the corroborable theory. In other words p(h)=0=p(h/e). In this respect therefore, Bar-Hillel's ([1974] p. 332-348) distinction between `$\text{acceptance}_1$' and `acceptance_2' i.e. acceptance *for* testing, and acceptance *on* testing, marks no real distinction. The distinction can only be drawn between the conepts of corroboration and of confirmation. Corroborated theories remain unrepentantly *improbable*; whereas confirmation is supposed to render theories as highly probable in the sense of the probability calculuss.

The foregoing analysis yields the conclusion that Popper's arguments against probabilistic induction (or neo-justificationism) are precisely the very same as his arguments against induction. They are based on logic and on intuitive considerations, which in turn rest on the appeal to the principle of empiricism. But it has already been noted that in Popper's philosophy, the thesis of theory-laden observation undermines the appeal to experience. Testability therefore, is not with respect to experience; but only relative to the background semantic theory which classifies objects into kinds. If this result is brought to bear upon the context of confirmation/corroboration; then once again there would appear to be no premium on adopting a falsificationist stance. The verificationist attitude, on the evidence of a highly structured universe (i.e. on the evidence that all observed P's are Q's without exception) assumes a modified probability distribution which reflects probabilistic dependence between properties. This is a Natural Kind inference in which the property after testing is assumed to be an essential property of the kind, i.e. to co-occur invariantly with the other properties which define the kind. This reflects the closest form of probabilistic dependence. On the basis of this, universal generalisations which articulate the semantic dependence can be assigned a probability approaching, in the limit, 1. This captures precisely the sense of `nomic necessity' which permits predictive, subjective and counter factual inference in Hempel's nomological-deductive model of explanation. From a historicist point of view, Kuhn [1974] and Hilary Putnam [1974] amongst others, have pointed out, that this marks the stage of `normal science' when a hypothesis is no longer up for test; but is `accepted as background knowledge which is paradigmatically true. This change in epistemic attitudes distinguishes the content of explanation, from the context of testing.

Popper's model which emphasizes growth, on the other hand, is the appropriate one for the stage of testing which precedes acceptance. This testing of course, is against the accepted background semantic theory of natural kinds. At this stage, nothing can be taken for granted. As Joseph Agassi ([1990] p. 141-142) puts it: Is the law `All swans have the same colour' true? No. Is it possibly true? Yes. How can we learn from experience whether such a law is true or false? By taking no dependence between any two items as a priori given, but rather as something to investigate'.

It is true that H-D model and D-N model are NOT rivals. But this is so, not because they are complementary but because they are models for entirely different things. H-D model is a model for the method of science, whereas D-N model for the structure of explanation. ~~In~~ Popper accepts, by and large, D-N model. In fact, he claims that it is he first proposed it.

Probabilistic independence therefore, is the appropriate assumption here. But suely after testing, when the evidence is in, probability-distributions need revising, to conform to the changed epistemic attitudes? In this context it is noteworthy that the deductive-nomological model and Popper's hypothico-deductive model do not differ in their logical structure. From `hypothesis' to `law' marks only a change in epistemic value. From this perspective the two models are not rivals; but complementary stages in the same evolutionary process of the growth of knowledge.

But unless the principle of empiricism is restored, the growth of scientific knowledge can represent only the evolution by trial and error of a conventionally accepted, underlying semantic theory. The real challenge to both verificationism and falsificationism is posed by the problem of theory-laden observation. This pervasive problem undercuts the empirical basis of science; and renders impotent the appeal to experience. An analysis and re-interpretation of this problem is therefore undertaken.

(i) and (ii) constitute the grounds for theory-ladenness of observation, i.e. the statement "moreover........" ~~in the~~ which is implied by (i) and (ii) undermines the principle of empiricism.

CHAPTER II

THE THESIS OF THEORY-LADENNESS

2.1 POPPER'S THEORY OF UNIVERSALS

Popper's thesis of the theory-ladenness of observation can be formulated as consisting of two major assetions: (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 itensionally defined by laws or theories, these concepts cannot be correlated with anything given in experiences; moreoever 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 occuring in it have been given their meaning[1]. 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[2].

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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'[3]. Again, Popper [1983] counters Berkeley's contention that scientific theories are only instruments (for calcutation) 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.

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contd..

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 empircal 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-verifieable 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 satisfed 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 enthymematic 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 exemplity 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.

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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 vertification constitute justification (of either basic statements or of theories). It follows that basic statements cannot be either justifiedd or refuted (tested) by experience; and science is therefore not even negatively uner 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 (and Kuhn'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 arbitiary. Again since the truth-value of basic statement, cannot be established indubitably, (because the relationship between experience and statements is only cuasal not logical) basic statements must be accepted by consensus.

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 definied' (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'.

'Theories are true because they are accepted, It is not that ~~that~~ they are accepted because they are true'

In fact, Lakatos considers Instrumentalism to be a degenerate version of conventionalism. (Cf Falsification 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 tautologously 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 suggeted 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 Duhem - Quine Thesis of Holism

The foregoing analysis elucidates a controversy which has dogged methodological faslsificationism 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 fulness. Quine's thesis and its criticism (especially by Strawson) are too well-known to require ~~representation~~ repetition. Instead certain points relevant to the argument being developed in this thesis can be made: If, Quine is looking for a naturlistic 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 non-existent.

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-->p.p) --> 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 auxilary 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 fallibist 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~~ very 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 ***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 auxilary hypotheses from which, together with the target hypotheses in question, the findings could be deduced. But this Duhem cannot guarantee. Thus Duhem committed a logiccal 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.

reference?

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[5]. 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 auxilary 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

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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 auxilary hypotheses that they be well corroborated. In fact Duhem emphasises that auxilary 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 dischotomy 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 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 experience as a whole.

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 auxilary 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].

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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 physiccs to be solved practically; in order to produce a certain effect you wish to make use of knowledge acquired by phycists; you wish to light an incandescent bulls; Spelling 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 legitimize; 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

where does the quote end?

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 auxilary 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***

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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 auxilary hypotheses are granted the derivation does not proceed and the inference does not go through. `Accceptance' 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 instancce 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. $H \wedge A \longrightarrow A \wedge P$ (where H is hypothesis,
2. $A \wedge \overline{P}$ A the auxilary assumptions,
3. $A \wedge \overline{P} \longrightarrow \overline{H}$. and P the prediction).

This formulation emphasizes that falsification goes through only relative to epistemological assumptions, whose semantic function renders them tautologously (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] suggets 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 --> P' being metalogically equivalent to `A, $\bar{P}$ --> $\bar{H}$'. 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 Sydrome

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 stresed that the growth of knowledge takes place only against a theoretical backgroundd 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 (a 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 conceputal 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 jettissoning 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 interepret 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[8]) only it is at variance

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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 vesitiges of empiricism have met with criticism, especially by Dudley Shapere [1976].

with his own conception of science. Popper's position of methodological falsificationsim 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 jettissoning 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

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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 acccount 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'.

jettissoning) 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 auxilary 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 auxilary 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 auxilary 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

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10 In order to substantiate this claim, Andersson analyses some of Kuhn's and Feyerbends 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 Feyerabendd have offered.. in what way?

experimental results have different meanings even when the words used are the same. An example would be the term `mass' which in Newtonion 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 Einsteinian 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 confiramtion 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[11]. 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 Einstenian 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 maitain that theories cannot be tested against *other theories*[12]

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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 Feryerabend disparagingly characterizes as of the `All ravens are black' variety). ~~He~~ 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 Weltanshauung philosophers; it is the contretemps ~~to~~ of 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'[13]. Finally

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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 tansfer 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 *premisses* 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 commmon 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 betwen 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[14] 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 predecesors; 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-a-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

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14 Versimilitude is a measure-theoretic comparative relation between two theories t_2 and t_1 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?
~~Justify your~~ Justification of your position needed.

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,[15] 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.[16] 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, th~~a~~en they are ~~scientific~~ semantic *taxonomies*. In this context Ernest W. Adams and Williams. Admas ([1987] p. 419) maintain that the

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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 posses 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 rebutt 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.

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CHAPTER III

UNIVERSALS REVISITED: THE LOGIC OF IDENTITY

The foregoing analysis reveals that the thesis of theory-dependence of observation leads to context-dependence and incommensurability. More importantly, it involves the abandonment of the principle of empiricism. An attempt is therefore made at the re-interpretation of this thesis in the light of results drawn from the new theory of reference developed by Kripke, Putnam, Donellan et al; as well as from developments in cognitive science. The implications of this reinterpretation for the problem of theoretical growth, theory change, and empirical constraints on scientific systems, will be subsequently analysed.

3.1 The New Theory of Reference

The new theory of reference developed by Saul Kripke, Hilary Putnam, Keith Donnellan and others is partly a reaction to the semantic tradition of Russel and Frege. This tradition construed names and general terms as attenuated description; and considered the reference of these terms to be a function of their Fregean sense. In contrast, amongst the views of the modern semanticists is that names have no intension in the traditional sense; and that at least general terms which designate natural kinds (naturally occurring species) are like names and thus do not C.

have their extensions determined by concepts. Instead reference is achieved by something like a causal chain rather than by associated descriptions. This new theory of reference poses, according to Stephen Shwartz [1977] the most serious challenge ever to traditional theories of meaning; and has important implications for the philosophy of science.

Shwartz says: According to traditional theories of meaning there is an intension/extension distinction i.e. concepts or meanings associated with general terms and names determine the set of things to which they apply or refer. The heart of the traditional theory of meaning is described by Hilary Putnam ([1975] p. 140) in the following way: `On the traditional view, the meaning of say "lemon" is given by speccifying a conjunction of properties. For each of these properties the statement "lemons have the property P" is an analytic truth; and if P_1, P_2 .....P_n are all the properties in the conjunction, then "anything with all of the properties P_1 .... P_n is a lemon" is likewise an analytic truth. The conjunction of properties associated with a term such as `lemon' is often called the intension of the term `lemon'. This intension determines what it is to be a lemon. Thus according to traditional theories intension determines extension. Putnam ([1977] p.119-120) says the ancient and medieval traditions also maintained `that the cconcept corresponding to a term was just a conjunction of predicates, and hence that the concept corresponding to a term must ***always*** provide a necessary and sufficient condition for falling into the extension of the term. According to Putnam ([1977] p. 120) Carnap also espoused a version of the traditional theory because for him, `the concept corresponding to a term provided (in the ideal case, where the term had "complete meaning") a *criterion* for belonging to the extension (not just in the sense of `necessary and sufficient condition' but in the strong sense of ***way of recognizing*** whether a given thing falls into the extension or not.)

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More recently Irving Copi ([1972] p. 125) endorses the intension/extension dichotomy. He says" `To understand a term is to know how to apply it correctly, but for this it is not necessary to know all of the objects to which it may be correctly applied. It is required only that we have a criterion for deciding of any given object whether it falls within the extension of that term or not. All objects in the extension of a given term have some common properties or characteristics which lead us to use the same term to denote them .... The collection of properties shared by all and only those objects in a term's extension is called the `intension or connotation of that term'. Copi adds that `extension is determined by intension but not the other way round'.

A version of the traditional theory is the `cluster theory' espoused by Wittgenstein wherein it is not a conjunction of properties, but a cluster of properties which is associated with a term. An object need not possess all the properties to be classified under the term; but must nevertheless possess a goodly amount of these. Cluster theorists (including Searle) deny essences i.e. essential properties; objects in the extension of the term are related only by ``family resemblances'; but clusters or criterial properties are nevertheless associated with a term and determine its extension.

The central features, therefore, of the traditional theory are: (i) Each meaningful term has a concept or intension associated with it. This meaning is known or present to the mind when the term is understood. (ii) Intension determines extension and (iii) Analytic truths are based on the meanings of terms (a cluster thorist would deny this). ?

The new theory challenges all these tenets. Saul Kripke [1980] concentrates first on the analysis of proper names, and then extends the analysis to general terms which signify natural kinds. According to Kripke's thesis of a rigid designation, a rigid designation is a term which refers to the same individual in all possible worlds in which the individual exists. Names are rigid designators which refer to the same individual, even in counterfactual situations, where associated descriptions might be false. This means that names refer to the same (not similar) individual whether or not he satisfies some list of commonly associated descriptions. So also general terms which signify natural kinds are rigid designators of kinds. They refer to the same substance in `all possible worlds'; associated properties therefore are not criterial, and the statements attributing such properties to individuals in the extension, are not analytic. Furthermore if the terms of an identity statement are rigid designators, the identity statement if true at all, is necessarily true. But it is neither analytic nor a priori. Thus Kripke distinguishes between the metaphysical notions of necessity/contingency, the epistemological notions of a priori/a posteriori, and the linguistic notions of analytic/synthetic. This analysis, when applied to statements of theoretical identity like `Water is $H_2O$' or `Gold is the substance with atomic number ---' implies that (i) the statements if true are necessarily true (ii) they are not known a priori for they are matters of scientific discovery and (iii) the statements are not analytic, for the scientific theory might be falsified; and in any case, if true, is true on account of the way the world is, and not on account of any fact about language.

Hilary Putnam 1977] has been the most important influence in the application of the new ideas on reference

This sentence is misleading. You have rightly contradicted on 118-119. For, if

~~It~~ that theory is taken to be a semantic taxonomy, does it not follow that extension is determined by intension (though here we are extending the concept of 'intension')?

to natural kind terms. He holds that water, for example is H_2O in all possible worlds. Thus water is necessarily water. This means that anything that is not H_2O is not water, even if it satisfies some list of superficial features that we think characterize water. These features or properties traditionally associated with a natural kind term merely provide, according to Putnam, a `stereotype' which helps to fix a reference. It is the true, scientific theory associated with a term which determines its extension. Thus water is H_2O in all possible worlds because nothing would count as a possible world in which some stuff that was not H_2O was water. Thus, it is not the satisfying of some descriptions analytically associated with the terms, but the having of a particular scientific nature or structure, that determines the natural kind to which objects belong.

Keith Donnellan [1977] distinguishes between the referential and the attributive uses of descriptions /properties. In the case of the referential use, the reference might succeed even where associated descriptions/ properties are false.

The general position of the new theory of reference can be further analysed as follows: Firstly, the properties associated with a term, in particular the `observable' properties do not determine its extension. They do not constitute its meaning in the traditional sense. What then, is the role of the associated properties? In this context Kripke [1980] distinguishes between fixing the reference of a term and giving its definition. When we fix the reference of a term, we give a description that helps the hearer pick out what we have in mind. Thus the description fixes the referent of the term, not its meaning in the traditional sense. One has a definite kind of thing in mind when applying the term, and one wants to help the audience pick it out. It is in this way that the descriptions associated with natural kind terms function. Of particular relevance is Kripke's ([1980] p. 54-56) extension of the notion of `fixing a reference' to encompass `operational definitions' in science. According to Kripke, the operation of measurement does not define the term, it only fixes its reference; and this holds even for operations of fundamental measurement where standards of reference are involved. Thus, for example, the operation of marking off a certain length (of stick or rod) as `one metre' does not define the term `one metre'; for under certain easily conceivable counterfactual conditions (of stresses and strains, of heating and cooling or whatever) it is quite possible to assert that the length in question would not be what in fact it is. `Operational definitions' therefore do not express synonyms, they merely fix the reference; these statements whilst epistemologically a priori, are nevertheless metaphyically contingent'[1].

Hilary Putnam ([1975] p. 52-54) can be interpreted as making the same point (albeit in a different context) with his distinction between `law-cluster' concepts and general terms which express nominalistic essences. Law-cluster concepts are constituted not by a bundle of properties as are typical general terms like `bachelor' and `man' (where `man' is used conventionally as in Aristotle's definition viz. `Man is a rational being') but by a cluster of laws which determine the identity of the concept. What distinguishes law-cluster concepts from nominalistic ones is that changes in the former, whilst not mediated by experiment, are nevertheless based on theoretical considerations; and do not amount to mere changes in linguistic conventions, as in the case of nominalistic concepts. In sum, the element of stipulation or convention in natural kind terms (i.e. associated properties or operational definition) only fix a reference, they do not constitute its meaning in the sense of determining the extension.

Prima facie it might seem that the scientific theory

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1 The important point that Kripke is making is that the stipulative or conventional element in scientific terms does not determine its meaning. This is in agreement with Popper's [1969, 1972] contention that universals are dispositional and cannot be operationally defined.

associated with a natural kind term determines its extension, if it is the true theory of the kind. But in the absence of this Utopion ideal of privileged access to ontological realities, what is the role of `current best theory'? Shwartz [1977] explains the position in the following manner. He says Kripke and Putnam hold, for example, that water is necessarily $H_2O$; $H_2O$ is the true nature of water. `Water' rigidly designates H_2O regardless of what superficial properties the H_2O might or might not have. Shwartz says that at this point it is very easy to confuse the new theory with a possible refinement of traditional theories. Such a confusion would ensue if we thought that `Water is H_2O' is analytic. It might be thought that Kripke and Putnam are merely trying to replace ordinary definitions with scientific ones so that instead of defining water as a clear, colourless liquid, we define it as H_2O. This is not the view of the new theory. One would come closer to the position of Kripke and Putnam if one simply said that `water' has no definition at all, at least in the traditional sense; and is a proper name of a specific substance. The reason why the `current best theory' cannot constitute the intension, cannot generate analytic truths is that firstly, it could turn out to be false, and secondly that scientific theories are a matter of scientific discovery and not of conventional definition. Hence the current best theory generates if anything, only an epistemic certainty or a priority; the certainty of a well-established empirical theory, not the necessity that ensues from access to metaphysical or ontological realities. Nevertheless, since theories are a matter of scientific discovery, not of definition, it would appear that scientists are at least approximating to metaphysical i.e. necessary truths. A scientific investigation into the atomic, chemical or biological structure of some kind of thing is an investigation into the essence of that kind. Irving Copi [1972] and Quine [1977] sympathize with this view.

If the reference of natural kind terms is not determined by the current best theory; what then *does* determine their reference? Here Kripke, Putnam, Donellan et al invoke causal chains emanating from original acts of `ceremonial baptism' (of objects/persons in the case of proper names, and of `paradigm -atic instances or samples in the case of natural kind terms). Putnam's view is that we `baptise' what we take to be good examples or paradigms of some substance such as water; and then use `water' to refer to whatever has the same nature as the paradigms. When we introduce the term it is not necessary that we know the nature of the stuff we are naming, but hope that such knowledge will come with empirical scientific investigations. The term, once introduced, can be handed on from person to person in the referential chain, maintaining its original reference at each link. Putnam calls such a term `indexical', from which it follows that the term is rigid in Kripke's sense.

According to Shwartz [1977] the extension of the causal theory to natural kind terms is admitted to be quite rough. Thus Kripke ([1980] p. 353) says, after an account bearing many similarities to Putnam's: `Obviously, there are also artificialities in this whole account. For example, it may be hard to say which items constitute the original sample. Gold may have been discovered independently by various people at various times. I do not feel that any such complications will radically alter the picture'. Shwartz ([1977] p.34) comments: `What Kripke is trying to do is present a better picture of how reference takes place than the traditional one, and this can be done without supplying complicated and complete analyses'.

The central thesis of the new theory of reference then, is that universal terms are names which designate the same substance or kind in all possible worlds; and that reference is achieved indexically through causal chains. From this it follows that the two fundamental intuitions underlying the new theory are (i) existents (natural kinds) come first, associated properties and (scientific) theories follow; and (ii) the fundamental logic of universals is based on identity (Universals as names designate the same substance or kind in all possible worlds). Claim (i) challenges the fundamental tenet of theory-laden observation viz. that all observation (of natural kinds) is in the light of theory (i.e. universals are intensionally defined); and claim (ii) is in contrast to the traditional theory of universals which stresses resemblance as the logic of classification. (Properties which constitute the intension are the properties in respect of which objects in the extension are similar). The merits of the first claim are assessed in the light of current research in congnitive science. This is undertaken in 3.2. Subsequently, in 3.3, a comparative analysis is presented of the logics of similarity and of identity in relation to universals.

3.2 The Psychology of Perception

In this section two models are presented from current perception theory viz. the computational model developed by Fodor, Marr, Gilson et al; and Gibson's ecological optics. Both seek to establish that basic perception (observation) though theoretically structured, involves principles of organization which are endogenously specified and not exogenously imposed. This is in contrast to the position of certain philosophers of science (including Hanson and Kuhn) who often cite empirical evidence (from the psycology of perception) to suggest that observation, even at its most fundamental level, is influencced by socio-cultural and linguistic factors. The computational model is introduced by way of a critical discussion of the Kuhnian position by Daniel Gilman [1992]; whilst Rom Harre' [1986] sets forth the main ideas of Gibsonian ecological optics as a solution to the inadequacies of the computational model. Finally, implications are drawn from this analysis for the claims regarding universals of the new reference theorists.

The analysis can be elaborated as follows: Daniel Gilman [1992] maintains that criticism of the observation/theory distinction generally supposes it to be an empirical fact that even the most basic human perception is heavily theory-laden. Gilman offers a critical examination of the experimental evidence cited by Thomas Kuhn [1970] and Paul Churchland [1988] on behalf of this supposition. He argues that the empirical evidence cited constitutes inadequate support for the claims in question. He further argues that we have empirical grounds for claiming that the Kuhnian discussion of perception is developed within an inadequate conceptual framework and that a version of the observation/theory distinction is indeed tenable. Before presenting Gilman's argument in detail however, it might be instructive to juxtapose it with the views of N.R. Hanson [1958] who is considered as archetypically representative of the position Gilman claims to be attacking.

Hanson's ([1958] p. 71) discussion of observation has the twin goals of discrediting the `Received View's'[2] doctrine of a neutral observation language, and establishing the point that observation is `theory-laden'. The Received View postulates the existence of an intersubjective observation language which can be given a direct semantic interpretation, independently of any theories which employ it; as such the observation language is theory neutral. Since assertions in the observation languagge can be verified by direct observation, its intersubjective nature requires that all who employ the language see the same things when looking at the same objects. Hanson challenges this assumption: according to him, when two people with varying theoretical backgrounds view the same objects, they see different things. One might claim they do see the same thing since they have a common visual experience - but if by this is meant that their eyes receive similar stimuti or retinal impressions, it does not follow that they see the same thing; for receiving a retinal impression is to be in a physical state, whereas to see is to have a visual

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2 Frederick Suppe ([1977] p. 3) points out that in response to developments in physics in the 1920's it became commonplace for philosophers of science to construe scientific theories as axiomatic calculi which are given a partial observational interpretation in terms of a neutral observation language, by relating `theoretical' terms to `observational' terms via correspondence rules. Hilary Putnam [1962] dubbed this the `Received View'. Obviously the postulate of a neutral observation language is crucial to the Received View.

experience - and the two are not the same thing.

Hanson goes on to reject what Frederick Suppe ([1977] p.153) terms the `sensory core theory' viz. that persons with different theoretical backgrounds *do* have a common visual experience inasmuch as they perceive the same `sense datum' but only interpret it differently. This rejection proceeds by way of a Wittgensteinian consideration of various ambiguous figures such as duck-rabbits, perspex cubes etc. which are sometimes perceived as one thing and at other times as another thing by the same viewer. This is construed by sensory core theorists as interpreting the same sense-datum variously. But Hanson argues that: (1) If seeing the same figure differently is interpreting differently, then having a different interpretation just *is* to see something differently. (2) Interpretation is a kind of thinking, whereas seeing is an experiential state; therefore `interpreting' would appear to be an inappropriate concept for describing differences in perception. (3) The appropriate account would be in terms of a difference in conceptual organisation. (4) Conceptual organisation is a function of the context as well as person's background knowledge and theories. (5) Hence seeing is an epistemic achievement; it involves a logical component of `seeing that' whose nature is linguistic and inferential (i.e. involving ampliatuve inference to future and counterfactual behaviour). (6) Hanson concludes that all observation especially in science, necessarily incorporates the element of `seeing that' which renders it theory-laden.

Frederick Suppe [1977] considers Hanson's mode of argument which he says is patterned after that in Wittgenstein [1953] as persuasive but not conclusive; it might still be tenable to maintain that there is a kind of seeing which is not `seeing that'. However Suppe thinks Hanson's major point viz. that all observation in science involves ampliative inference, as well taken.

Hanson's views on basic perception are challenged by Daniel Gilman [1992] who offers a computational and modular theory of perception. Gilman grants that perception is a complex business which is inferential in character, and which exploits assumptions as well as received information; but holds that (owing to modularity) such assumptions have nothing to do with the sorts of beliefs and theories which differentiate members of the scientific community; or which divide cultures and languages in the present or over time.

Gilman's argument assumes the following form: First he interprets the thesis of modularity in Fodor's ([1983] p. 37) terms as `domain specific, innately specified, hardwired, autonomous, and not assembled'. The point about maintaining that a system is modular is that it is sealed off to some interesting extent, not that the computation it performs is purely autonomous. Next Gilmaan presents Marr's [1982] theory of perception as a computational process: According to Marr there are three basic levels at which one has to approach any complex information processing system. The first and highest level is that of `computational theory'. Here we develop an account of what we take the process to be for, of the abstract structure of the process, and of how the process is suited to its purpose. Second is the level of representation and algorithm which is concerned with how information is to be represented at both the beginning and the end of the process. Finally, the third level is that of `hardware implementation'. Marr's discussion of vision is primarily in terms of the first two levels. According to him, vision (Marr [1982] p. 3) is `the process of discovering from images what is present in the world, and where it is'. We may know by looking, what shapes things have, and how they are laid out in front of us. The source of this knowledge, is the light which strikes the eye, and which forms an image on the back of the retina. In studying vision we are studying how it is that the mind is able to extract and represent this information about the world from the original image.

Part of what Marr offers is a rigourous analysis of how the visual system can construct *reliable* models of the world it encounters by (computationally) operating on the stimulus presented to the retina. We ought to note that this construction depends upon a number of intermediate transformations producing, and operating upon, a number of intermediate representations. Finally, we must note that the system is generally successful despite computational and informational limitations, because it has evolved *`internalised assumptions'* about the typical structure of the world and about the relationships which typically hold between retinal images and that physical structure. Marr thus conceives of vision (perception) as a complex computational process which is theoretical and inferential in character; but which involves internalized assumptions that are not accessible to permeation by external theories.

Gilman holds that there is no significant empirical evidence against the modularity thesis (of the escapsulation) of perception. He questions the relevance of the experimental literature (from the psychology, biology and neurology of perception) cited by Kuhn [1970] and Churchland [1988]; which purports to establish that perception, even at the most fundamental level is influenced by exogenous theories. But Gilman points out that this literature concentrates on experimental studies of abnormal, damaged or impoverished (conditions of) vision; and apart from considering the conclusions to be often unwarranted, Gilman fails to see it's relevance to the central paradigm of *normal* vision.

Finally, Gilman ([1992] p. 303) thinks that Fodor is `quite tidy' on ambiguous figures as evidence for the theory penetrability of perception. Fodor ([1988] p. 190) says: `One doesn't get the duck-rabbit (or the necker cube) to flip by "changing one's assumptions"; one does it (for example) by changing one's fixation point'. Fodor thinks that (external) beliefs and theories play no role in deciding which forms are illusory, nor in how ambiguities get resolved. Cases such as the vase/face ambiguity might appear to favour the conceptual (permeation) theory. But even Richard Gregory [1970] reports that properly constructed abstract forms, which bring no general (kind of) object to mind, will appear likewise ambiguous. And Edgar Rubin (Gilman [1992]) describes certain contours as generating a sense of an object seen against a ground independently of our ability to recognize the object as anything other than a form, or what he calls the `thing-character' of an object. Gilman concludes that ambiguous figures indicate only that perception involves plasticities which are inherent in the computational process; but do not constitute evidence for theory-penetration.

Reviewing the argument Gilman ([1992] p.300) arrives at the general conclusion that (the experimental studies from) `New look psychology' is right in emphasizing that perception is a complex problem-solving process (as opposed to a simple stimulus registration process); which exploits theoretical assumptions as well as received information. But it fails to take note that both the assumptions and the available forms of inference may be endogenously specified. The computational approach, on the other hand, takes this into account, and conceives of a fundamental level of perception which is intersubjectively stable; and in which (computationally structured) objects and their properties are representationally `given'.

Rom Harre' [1986] considers the computational model of perception as totally inadequate for supplying the metaphysics (ontology) of experience, demanded by a realist (and referential) interpretation of science. To be a realist. According to Harre' (p.146) is to acknowledge an `aboutness' in one's discourse, a referential tie to something other than one's own states.[3]

But the computational theory is only the culmination of a (three-hundred year old) representationalist tradition which construes perception as a mere representation, rather than part of a world of actual human experience.

Harre' sketches the main features of this tradition in the following manner: Classical perception theory inserts

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3 This notion of a referential tie is fundamental to the thesis of indexicality emphasized by the new theory of reference. In general, realism (i.e. existents) is a presupposition of the theory.

two stages between world state and percept. In the first a causal relation is supposed to obtain between world state and sensation. In the second stage the sensation is reworked in some congnitive process to yield the percept. Reliabilism is the doctrine that scientific support can be found for confidence in the verisimilitude of the product of that causal relation; so that the sensation is, in some measure a correct representation of the state of things that produced it. The reliabilist's move is to try to find that justification in the results of a scientific investigation of the causal conditions of perception.

The reliabilist theory of perception has two main versions: According to naive reliabilism, at least some of the properties of the representation accurately reflect, corresponding properties of the real world. Harre considers `Locke's theory of primary qualities and Descartes' doctrine of `natural geometry to be naive reliabilist theories. In the theories of perception of Reid and Whewell (Harre [1986]), naive reliabilism is transcended. Sensations are not reliable representations of that which causes them; as for the reliability of the representational percepts produced by the cognitive reworking of sensations, both Reid and Whewell seem ready to accept an answer couched wholly in cognitive terms. In Reid's psychology the question of the verisimilitude of sensations is displaced in favour of the problem of the representational quality of percepts. From the point of view of (a defence of) scientific realism (and of reference), Harre' considers this shift in emphasis as deeply disturbing.

Harre' views the computational model of perception as a mere reworking of Reid's theory; and hence as involving the same disturbing implications. His criticism of this model is of Fodor's (Harre [1986]) version of it; and takes the following form: Harre' says Fodor's account relies heavily on two technical notions. First, it is an exercise in the formal science of mind. Mental processes are treated as computations which take account only of the structural or syntactic properties of the states in which representations of external states of affairs are realized. The computational model necessarily cannot take into account any semantic properties of representations such as their meaning or truth. This, says Fodor (Harre [1986] p. 152) is `tantamount to a sort of methodological solipsism. If mental processes are formal... they have no access to the semantic properties of such representations'. Hence no mental (cognitive) process can be used to tell whether a representation is true or false.

The second important notion is of `referential opacity'. This can be elucidated in the following manner: According to Fodor (Harre [1986] p. 152-153) perceptual statements like `Jim saw a bird on the bough' might be true, even if it is `objectively false', because from the point of view of the psychology of perception, it is what Jim thought he saw that matters. Referential opacity is actually a corollary of the computational model, which cannot make the semantic distinction between veridical/non-veridical perception.

Harre' considers these shortcomings as powerful reasons for rejecting the computational model. In fact he rejects the entire representationalist tradition because it runs counter to scientific ***referential*** realism. Harre' traces the root of the problem to the ubiquitous assumption underlying four centuries of perception theory viz. that perception is built out of sensations. It is just this unexamined foundation that is challenged by Gibson's [1979] ecological optics. Both clauses of the representationalist traditional are challenged: that percepts are cognitively transformed sensations; and that the basis of perception is an awareness of states of the brain that are the remote effects of physical causes.

Harre' presents the basic Gibsonian ideas as follows: *Information pick-up and non-cognitive perceiving*: According to Gibson ([1979] p. 242) physical objects and their properties are specified by information present in the `ambient array'. The ambient array is a flux of energy shaped by the presence of both the perceiver and that which is perceived. Sensations do not specify physical things and their states. They specify only the current state of the sensory organs. `Information' in the ambient array `specifies' the object which structured the array. An organism, in actively exploring that array for higher-order invariants `picks-up' that information. It is as the `pick-up' that perception occurs.

Gibsonian `information' is sharply distinguished from information in the sense of informational content: It is an optical structure not similar to its sources but specific to them. This structure lawfully and uniquely maps the structural properties of the object. It is on account of the specificity of the information that it is non-inferential. It is important to note that the structures recognized are in the ambient array, not in the pattern of events at the retina, or any sensory representation of them. Thus Gibson stresses that perceptual systems are active, exploratory, interconnected systems rather than passive receptor channels.

Whilst largely accepting the Gibsonian theory of perception Harre cautions that it offers a solution only to the problem of how generic information is possible. It explains how we can have experience of the physical world mediated by perception, yet unmodified by cognitive work. But what we perceive directly in the Gibsonian sense is coarse-grained with no more subtle categorization than those from which Kant deduced the system of the schematisms. Gibsonian theory shares with the Kantian account of perception, the principle that the organisation of experience as it is manifested in things, events and so on, is not extracted from the sensory flux. But it is non-Kantian in that what corresponds to the schematisms, higher-order invariants, are not a priori, but are found in the exploration of the ambient array. Accepting the broad outlines of Gibsonian psychology permits us to hand-over responsibility for the defence of the reality of perceived things and events, and of certain general types of relations, to the psychology of perception.

In attempting to draw conclusions from the foregoing discussion on the psychology of perception, it is perhaps needless to caution that theories in the field are highly speculative. But since no analysis of the methodology of the empirical sciences can do altogether without *some* assumptions regarding the nature of what is `given' in observation, we might draw some minimal conclusions as follows: Whether computationally processed or informationally `picked up', perception theorists stress that the objects of even fundamental perception are highly structured in constrast to the sense-datum theorists of yore. But they concur in rejecting the thesis that this structural or categorical organisation is the result of the permeability of higher-level (conceptual) theoretical systems. Hence it would appear that we have intersubjective perceptual access only to generically structured objects and their properties, which is relatively stable. Moreover the acceptance of Gibson's ecological optics provides not only the ontological foundation for scientific realism; but in its emphasis on active exploration (of the environment) introduces the conceptual possibility of experimental manipulation (of objects) as part of observation. This notion is of particular significance in view of current opinion in the philosophy of science that the appropriate distinction in science is not between observation and theory; but between experiment and theory.[4]

Secondly, although perception theorists concur in granting that (generic) categorial organisation is endogenously specified in perception; they resist the suggestion that the more subtle, (species) specific

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4 In this context of the distinction between experiment and theory, Frederick Suppe ([1977] p. 690) represents Dudley Shapere's position thus `His approach is to begin with an exammination of the scientific use of "observation" and "direct observation" in astrophysics; and he finds that astrophysicists regularly write, for example, of *detecting* neutrino fluxes as yielding *direct observation* of the centres of stars. Moreover the astrophysical use of "observation" or "direct observation" (as well as "detection" and "probe") is not used in opposition to "theoretical" but rather in opposition to "experimental" - experiment involving interfering with processes which will allow us to test our hypotheses at will and in the most convenient manner, whereas observation generally does not involve such intereference or manipulation.

organisation involved in the recognition of natural kinds is also thus specified.

3.3 The Thesis of `Primitive' Classification: Similarity or Identity?

In the light of these results from the psychology of perception, the reference theorists' contention that universals name kinds which are indexically indicated needs careful interpretation: (i) If the claim is that natural kinds are endogenously specified by perceptual mechanisms, then this thesis is not supported by current perception theory. Obviously the specification of kinds involves exogenous factors; and observation (of kinds) is in this sense, theory-laden. It is (culturally and linguistically) context-dependent. (ii) If however, the further question is whether the exogenous contexts are intensionally defined or extensionally exemplified; then not only the reference theorists, but also philosophers of science including Kuhn, Hesse, Quine et al, emphasize that the acquisition of classificatory structures (relating to natural kinds) takes place through exemplars. (cf. Putnam's paradigms, Kripke's samples, Kuhn's exemplars et al.). This is Hesse's ([1974] p.67) thesis of `primitive' classification viz. that the `intensive design' (Rom Harre' [1986] p. 104) of natural kinds is causally given in recognition, not explicity defined.

The thesis of primitive classification of natural kinds involves ontological and logical committments: If the observation of natural kinds is in the context of a causal or referential tie between the observer and the world then this presupposes existents. This constitutes the ontological presupposition. The logical constraint is on the `intensive design' or principle of classification of natural kinds. This principle must be in terms of a `primitive' relation i.e. a relation which can only be extensionally exemplified, not intensionally defined[5]. According to the referencce theorists this constraint is satisfied by the relation of identity.

However, the reference theorists' contention runs counter to the mainstream tradition in the philosophy of science, which emphasizes the relation of similarity as the principle of classification, as well as the mode of reasoning (i.e. by analogy and metaphor) in science. A defence of (the principle of) identity is therefore undertaken in three stages: (i) by a comparative analysis of the logics of (the relations of) similarity and of identity, in relation to universals (ii) by citing exerimental laws from the corpus of physical science as exemplifying the relation of mathematical identity in the form of laws of

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5 Hesse's concept of a primitive relation as a principle of classification of natural kinds will be shortly elaborated.

functional dependence, and (iii) by adducing examples of theoretical structures (from physical science) which indicate that thoretical growth employs as its fundamental principle of infernce, Leibniz's principle of the Identity of Indiscernibles. Whereas (ii) and (iii) are undertaken in the next chapter, stage (i) of the analysis of the logics of similarity and of identity as principles of classification, is presented as below.

The general features of the ***resemblance theory*** of universals are set forth by Hesse ([1974] p.45) under the rubric `A Network Model of Universals'. Hesse first contrasts the resemblance theory with the absolute theory: According to the ***absolute theory*** P is correctly predicated of an object `a' in virtue of its absolute quality of P-ness. According to the ***resemblance theory***, on the other hand we predicate P of objects a and b in virtue of a sufficient resemblance between a and b in a certain respect, which is the same for all pairs of objects in the extension. Wittgenstein's [1953] theory of family resemblance provides a twist to the classical theory in its suggestion that objects may form a (conceptual) class to the members of which a single descriptive predicate is ascribed in common language, even though it is not the case for every pair of members that they resemble each other in any respect which is the same for each pair. Hesse adopts Wittgenstein's theory which she thinks is the general case which accommodates both the absolute theory and the classical theory (of resemblance) as limiting cases.

The resemblance theory of universals exemplifies the relation of similarity; and powerful objections have been raised against its suitability as principle of classification. Thus Popper ([1972] p. 420-421) points out that one of the main characteristics of similarity is its *relativity*. Two things which are similar are always similar in *certain* respects, and therefore may always be similar in *different* respects. Moreover things which are similar in some respects, are always dissimilar in other respects, unless indeed, they are identical. Generally speaking, similarity presupposes the adoption of a `point of view' i.e. a theoretical stand-point. But if (the judgement of) similiarity presupposes the adoption of a point of view, or an interest, or an expectation; it is logically necessary that points of view, or interests, or expectations, i.e. theories, are logiccally prior, as well as temporally (or causally or psychologically) prior, to (the judgement of) similarity. Hence similarity cannot be a constitutive principle for classification.

Nelson Goodman (Hesse [1974] p. 66-70) offers criticism in a similar vein: Suggesting that theory `creates' or `governs' judgements of similarity, he says: `The fact that a term applies .... to certain objects may itself *constitute* rather than arise from a particular similarity among objects.' Again he maintains: `We cannot repeat an experiment and look for a covering theory; we must have at least a partial theory before we know whether we have a repetition of the experiment. More generally, Goodman lists `seven strictures on similarity'. (i) It does not distinguish between representation and description. (ii) It does not pick out `tokens of a common type' or *replicas*. (iii) It `does not provide the grounds for accounting two occurrences as performances of the *same* work, or repetitons of the *same* behaviour or experiment. (iv) It does not explain metaphor or metaphorical truth. (v) It does not account for our predictive, or more generally our inductive practice. (vi) (As a relation) between particulars it does not serve to define qualities. (vii) It cannot be equated with or measured in terms of possession of common characteristics.[6]

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6 Strictures (ii) and (iii) are particularly relevant to our analysis and can be explicated thus: The burden of (ii) and (iii) is that similarity is non-vacuous as a principle of classification only if it is construed as similarity in *relevant* respects. This is because (as Popper has pointed out) *all* objects are similar in *some* respects; hence if the relevant respects are left unspecified, then the relation is rendered vacuous for purposes of classification. On the other hand, if the relevant respects are specified, then similarity cannot explain reductively, classifications based on identity:

Contd.

Hesse ([1974] p. 167) defends similarity as a *primitive*, symmetrical and intransitive relation between objects. Primitive in the context means the similarity is recognized i.e. extensionally exemplified, not intensionally defined. Hesse says: `It is a relation given in the causal interaction of the perceiver and the world. It follows that it is not possible to *state* further conditions for the relation to hold'. Hesse thinks this answers Goodman's strictures because firstly if similarity is a primitive relation which is only extensionally exemplified and not intensionally defined; then we are under no onus either to explicate identity reductively in terms of similarity (i.e. to explicate in what respects a is similar to a); nor to explicate the metaphorical/non-metaphorical distinction. Again, because similarity (in the sense of family resemblance) is an intransitive relation; it is not, in any case possible to state the respects in which all objects in the class are similar. Therefore, Hesse believes the concept of similarity developed in her network model answers Goodman's objections.

Contd...

for example, when two objects or experiments are construed as repetitions of the *same* type, then they must be accounted as similar in *all* respects; whereas similarity is specified (for classification) only in relevant respects. Goodman concludes that similarity cannot account reductively for classifications based on identity.

Kuhn ([1977] p.475-482) also emphasizes that the classification of natural kinds is `primitive' in Hesse's sense of the term viz. that the principles of the classification are extensionally exemplified, and not intensionally defined. Kuhn agrees with Hesse that `primitive' does not mean that the classification is endogenously specified (by perceptual mechanisms); but only that it is extensionally exemplified.[7] This is Kuhn's thesis of learning by exemplars, which is the same as Hesse's thesis of primitive classification. Thus Kuhn asserts that the principles of classification are never explicitly articulated; instead in everyday contexts, one learns to apply terms, based on the implicit recognition of resemblances and dissimilarities between objects which are ostensively (indexically) indicated. Similarly, in the context of science, exemplars which are concrete problem solutions, as well as direct exposure in the course of laboratory work, teach the student the application of scientific terms. Presumably such application is also based

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7 Hesse ([1974] p. 48-54) makes it clear that by a primitive relation (or classification) she does not mean one that is purely endogenously specified by perceptual mechanisms. Thus, the primitive relation of similarity she defends has both a `correspondence component' and a `coherence component'. The correspondence assumption is that classification is in terms of similarities which are recognizable i.e. given causally in `the physics and the physiology'; whereas the coherence component allows for the aims of classification in modifying the initial classification. Nevertheless there are ambiguities in Hesse's account.

on the implicit recognition of similarities and differences. Quine ([1977] p. 157) whilst suggesting that `the notion of a kind and the notion of similarity or resemblance seem to be variants or adaptations of a single notion'; emphasizes `the dubious scientific standing' of both. He says: `The dubiousness of this notion is itself a remarkable fact. for surely there is nothing more basic to thought and language than our sense of similarity; our sorting of things into kinds. The usual general term, whether a common noun, or a verb, or an adjective, owes its generality to some resemblance among the things referred to'. But, Quine goes on to maintain: `...and yet, strangely, there is something logically repugnant about it. For we are baffled when we try to relate the general notion of similarity significantly to logical terms'. What Quine is emphasizing is that the logic of similarity in relation to kinds, is elusive.

Quine illustrates this in the following manner: First he criticises the attempt to define similarity in terms of kinds. Like Popper and Goodman, Quine points out that the notion of similarity is non-vacuous as a principle of classification (of kinds) only if the significant respects (or properties) of resemblance are specified. But if the significance (of properties) is referred to the principles of kinds (i.e. the significant respects are the ones in which members of the kind are similar); then since the notion of kinds is itself ambiguous, this is tantamount to accepting the notion of similarity as undefined.

Quine is equally critical of the converse project i.e. of attempting to define kinds in terms of similarity: Thus if we set up a `paradigm' case and specify the kind as consisting of objects similar to it (in a greater degree than to other objects); then this once again raises the problem of specifying the relevant respects (of similarity). Furthermore, Quine points out that the Carnapian version of this attempt (at defining kinds in terms of similarity) yields classes or sets which are counter-intuitive as kinds.

Quine concludes that the notions of similarity and of kinds is correlative; and that our sense of similarity, and therefore of kinds, is primitive; by which he means that it is innate.

The foregoing discussion makes it clear that Hesse, Quine and Kuhn defend similarity as the principle of classification for natural kinds, on the grounds of it being a primitive relation. By `primitive' Quine seems to indicate that the relation (and the classification based on it) is endogenously specified by perceptual mechanisms. This intuition, it has already been noted, is not supported by perception theory. Hesse and Kuhn, on the other hand, construe `primitive' to mean mainly, that the classification of natural kinds is only extensionally exemplified, not intensionally defined. But the logic of similiarity is primitive in this sense, only if similarity is interpreted in the Wittigenstenian sense of family resemblance, as an intransitive relation.

The logic of the relation of identity on the other hand, as conceptualised by Leibniz's principle of the Identity of Indiscernibles, is without qualification, a primitive relation i.e. incapable of intensional definition. It is therefore the appropriate principle for a system of `primitive' classification for natural kinds.

The case is argued as follows: Tarski ([1965] p. 54-64) says: `Among the logical concepts not belonging to sentential calculus, the concept of Identity or Equality is probably the one which has the greatest importance'. The relation is expressed in phrases such as `x is identical with y', `x is the same as y' and `x equals y'. All these forms are symbolically transcribed as `x=y' whose negation is `$x \neq y$'.

The fundamental form of the concept of identity is Leibniz's Law of the Identity of Indiscernibles which Tarski formulates as: `x=y if, and only if, x has every property which y has, and y has every property that x has'. Tarski points out that the law has the form of an equivalence, and enables us to replace the formula `x = y' which is the left side of the equivalence, by its right side, that is; by an expression no longer containing the symbol of identity. With respect to its form this law may therefore, be considered as the definition of the symbol `='.

From Leibniz's Law as the definitional law for identity, we can derive the laws of reflexivity, symmetry and transitivity for identity, which Tarski lists as follows:

(i) Law of Reflexivity i.e. $x = x$

(ii) Law of Symmetry i.e. If $x = y$ then $y = x$

(iii) Law of Transitivity i.e. If $x = y$ and $y = z$, then $x = z$

Further properties of identity which follow directly from the definition of this relation by Leibniz's Law can be stated as follows: Things or substances are identical only to themselves. This follows directly from the definition of identity because if two things or substances share all their properties in common, then they are the self-same substance. Hence identity is necessarily an *internal* relation which can hold only between a substance and itself; or else internally between properties of the same (not similar) object. This is brought out by Donald Rutherford ([1995] p. 133) in his explication of Leibniz's Law.[8] He says: `It follows from

8 Donald Rutherford's explication of Leibniz's Law as well as the following explication by Robert McRae are within the context of a discussion of Leibniz's metaphysics i.e. his theory of monads. But the general logic of the relation (of identity) holds, even outside this context.

the principle of the identity of indiscernibles not simply that there must be some difference among monads, but that "each monad must be different from every other. For there are no two things in nature that are perfectly alike, two beings in which it is not possible to discern an internal difference that is, one founded on intrinsic denomination".

Again, Robert McRae ([1995] p. 179) stresses the internal complexity of the relation: `The principle of the identity of indiscernibles or "that there is no perfect similarity anywhere" requires that these simple substances must be distinguished by their internal qualities and that there must then be a plurality of affections and relations within the unity of the simple substance. The only way in which this plurality in unity can be conceived is as we find it in our own experience, namely the plurality in unity which characterizes a perception'.

McRae's analysis leads directly to the main thesis viz. that the logic of identity is that of a primitive relation which can only be indexically indicated (in perception) and not intensionally defined. This is because the concept of identity (as defined by Leibniz's principle) is identity in *all* respects; and if we consider that any object or substance has an indefinite number of properties (including relational properties), it follows that the respects in which identity holds cannot *in principle* be intensionally defined. The relation of identity therefore is without qualification, a primitive relation, and *a fortiori*, the classification of natural kinds which it generates, is likewise primitive i.e. only extensionally accessible and not intensionally defined.

In concluding this analysis we note its implications for the problem of theory-ladenness observation in science. The primary tenet of this thesis is that (universal) terms in science are intensionally defined by theories. This leads (as argued in Ch. II) to methodological conventionalism to meaning-variance and to the abandonment of the principle of empiricim. Reference theorists reject the thesis that universals relating to natural kinds are intensionally defined. Their counter-thesis of indexicality viz. that universals name kinds which are indexically indicated can be interpreted to imply: (i) that natural kinds are endogenously specified by perceptual mechanisms and/or (ii) that the logic of classification of natural kinds is 'primitive' i.e. in terms of a relation which can only be extensionally exemplified and not intensionally defined. Whilst (i) is not supported by research in cognitive science; (ii) is lent credence by the logic of identity. This tenability of the reference theorists' thesis in terms of the logic of identity dissolves the problems of meaning variance and of incommensurability;[9] it also indicates the empirical constraint for scientific taxonomies - the taxonomies must employ `primitive' logics i.e. structural principler which can only be extensionally exemplified in empirical contexts. Further constraints on scientific classification are analysed in the next Chapter.

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9. This should be clear from the following: If universals in science are not intensionally defined, then they have no meaning or concept attached to them. Scientific classifications therefore are not conceptual frameworks. Since scientific theories do not constitute the meaning of terms, nor determine the reference of these terms, changes in theory do not lead to meaning-variance or to incommensurability.

CHAPTER IV

IDENTITY AS THE LOGIC OF SCIENTIFIC DISCOVERY

4.1 Identity as Functional Dependence

The analysis (in the preceding chapter) identified the empirical constraint on scientific classification in terms of the concept of a primitive logic of classification. Furthermore it was argued that the logic of two relations viz. Wittgenstein's theory of Family Resemblance, and Leibniz's Law of Identity, qualify as primitive in the requisite sense. In addition (to this constraint) scientific classifications (like all taxonomies) need to satisfy constraints in terms of (specific) goals and purposes. This appears to be one question on which philosophers of science voice near unanimity viz. that the aim of science is explanation and prediction. It is now proposed to analyse the form of law generated by classifications based on the law of identity;[1] which at the same time, satisfies the constraint of predictive inference.

First we reiterate the initimate connection between systems of classification and forms of law. In this context Quine's ([1977] p. 168-170) discussion of the relevance of

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1 Although Wittgenstein's relation of family resemblance is primitive, it is not proposed to explore the form of law, if any, appropriate to this form of classification.

subjunctive

kinds for explicating the `dim' notions of cause, of dispositional terms and of subjective conditionals, is illuminating.[2] Using Carnap's example of `soluble' as a dispositional (law-like) term, Quine says: `To say of some individual object that it is soluble in water is not to say merely that it always dissolves when in water, because this would be true by default of any object, however insoluble, if it merely happened to be destined never to get into water. It is to say rather that it *would* dissolve if it were in water; but this account brings small comfort, since the device of a subjunctive conditional involves all the perplexities of dispositional terms and more. Thus far I simply repeat Carnap. But now I want to point out what could be done in this connection with the notion of kind. Intuitively what qualifies a thing as soluble though it never gets into water is that it is of the same kind as the things that actually did or will dissolve ....'. The point that Quine is making is that dispositional terms i.e. law-like terms are natural kind terms; and that subjunctive conditional (predictive) inference goes through only

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2 Quine ([1977] p.168-170) as already noted, whilst condemning similarity as `logically repugnant' nevertheless holds that similarity is the constitutive principle or at least correlative with kinds. This is in contrast to the position ~~The~~ developed in this thesis i.e. of identity as the logic of kinds. This difference does not however affect the point at issue viz. the close relation between laws, law-terms, and predictive inference on the one hand, and systems of classification on the other.

relative to (natural kind) classification.

Again Quine ([1977] p. 169) maintains: `Another dim notion, which has intimate connections with dispositions and subjunctive conditionals is the notion of cause; and we shall see that it too turns on the notion of kinds. Hume explained cause as invariable successsion; and this makes sense as long as the cause and effect are referred to by general terms. We can say that fire causes heat and we can mean thereby, as Hume would have it, that each event classifiable under the head of fire is followed by an event classifiable under the head of heat, or heating up. But this account, whatever its virtues for these general causal statements, leaves singular causal statements unexplained'. Quine clarifies this point in the following manner: If a singular event is succeeded by another event then the simple fact of succession does not explain the law-likeness of the connection. Furthermore, an arbitrary assignment of the preceding event to a class (set) and the succeeding event to yet another class does not yet solve the problem. (For sets could always be rigged up arbitrarily). Singular causal statements make (law-like) sense only when the events concerned are referred to natural kind sets. Therefore Quine remarks: `What I wanted to bring out is just the relevance of the notion of kinds, as the needed link between singular and general causal statements'. What Quine is now maintaining is that inferences to universal generalizations go through only relative to (a primitively accessible) classification of natural kinds. It follows that the only principle of classification of natural kinds cannot be universal generalisation.

Hesse[3] ([1974] p. 71-72) emphasizes much the same points as Quine. She says: `In the history of philosophy the problems of universals and of natural laws are closely connected. Aristotle's "stabilisation of the universal in the mind" as a result of reflection on experience, is his acccount both of how we come to predicate a new object correctly as "swan", and also of how we know "all swans are white', for "swanness" is a complex universal incorporating "whiteness" ...... The account of causality or law-like relations, which depends on regularities of co-presence, co-absence and covariance, may thus be seen as parallel to an account of qualities as classes defined by their similarities and differences. Directly experienced spatial and temporal relations between objects required for causality are then seen as parallel to directly experienced resemblances required for the definition of reference classes'. Hesse can be interpreted very simply as making the point that laws are framed in terms of properties

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3 Hesse like Quine upholds the resemblance view of universals. As in the case of Quine, this does not affect the point at issue viz. the dependence of law's upon universals.

relevant to natural kind classes; and that it is this feature which distinguishes natural laws from accidental generalisations. This is a reiteration of Quine's contention that claims to universal regularities go through, relative to natural kind classification; and that predictive and counterfactual inference rests therefore, not on universality per se, but on the underlying logic of kind classification.

Again Frederick Suppe ([1977] p. 628-629) points out that Hinkikka [1976] and his Finnish colleagues also employ natural kind inference to justify probabilistic induction to universal generalizations. They attempt to modify Carnap's basic approach to inductive logic so as to obtain probability measures which assign non-zero probabilities to generalisations. Whereas Carnap's state descriptions are descriptive of individuals and their attributes, their approach is to construe state descriptions as being about *kinds* of individuals Suppe remarks that this shifts the problem of justifying probabilistic induction to the question of justifying the classification (of kinds) which underlies probabilistic inductive inferences to universal generalisations.

More recently, John MacNamara [1991] claims to offer a better understanding of induction; one that assimilates it to induction based on essential properties rather than to statistical inference. This is made possible, according to MacNamara, by appealing to the logic of common nouns and applying it to the logic of natural kind-terms.

The extensive literature on natural-kind inference (exemplified above) indicates (i) the crucial relevance of classification to laws: on this view laws are explicit articulations of the underlying classificatory structure, and (ii) claims to or assumptions of universality go through only relative to natural kind classification. It follows that the constitutive principles of class organization cannot be that of universal generalisation.

This intuition seems to be supported by Popper ([1972] p. 422). Thus Popper in replying to the criticism of William Kneale (Popper [1972]) admits that there are structural theories in science (which include the atomic theory. Newton's laws of notion, and the law of universal gravitation) whose form is not really that of universal generalisation. Popper says that although these laws might be expressed as universal generalizations, yet the `all' form is comparatively unimportant in the case of these laws. `.... The difficulty with these structural theories is not so much to establish the universality of the law from repeated instances as to establish that it holds even for a single instance'. Yet, Popper does not offer any suggestion for the form of such laws, and maintains that William Kneale does not succeed in making clear what the difference is between a universal statement and a `principle of necessitation'.

The thesis is now put forth that in the case of classifications based on the principle of identity, the logic of identity (as exemplified by Leibniz's Law); in conjunction with the constraint of predictive inference (specified as the aim of scientific classification) indicates the appropriate form of law as that of functional dependence between properties of the same (not similar) object. This thesis reinterprets the controversial concept of nomic necessity both in terms of an intuitive notion of relational structure and more strongly, in the sense of a (functional) rule-bound correlation of properties.

This thesis can be defended in the followingg manner (i) Firstly, as has already been argued (in the preceding Chapter), the logic of the relation of identity is that of a primitive relation which in principle, can only be indexically indicated (extensionally exemplified) and not intensionally defined. From this it follows that properties and relations associated with a kind term (based on identity) do not constitute its intension; they only specify the structure of the kind. (ii) Furthermore since identity is a `totally reflexive' relation. (Copi [1986] p.387) it holds only between the internal properties of a substance, and (iii) finally since identity is a ***relation***, it classifies kinds only on the basis of (internal) relations that hold (between properties); and not on the basis of the properties of kinds.

The implications of the foregoing analysis can be elaborated as follows: Point (iii) rules out the absolute theory of universals and the associated form of law viz. universal generalisation as appropriate for empirical systems. This can be understood in the following way: To reiterate the formulation of Hesse, [1976] according to the absolute theory P is predicated of an object a in virtue of its (absolute) possession of P-ness i.e. of a conjunction of properties. The emphasis of the absolute theory is on the properties (of objects/substances) and not on the relation of co-presence. This seems to be because mere co-presence (of properties) satisfies no intuition of necessary structure; nor does co-presence in individual cases permit predictive inference to future, counterfactual or subjunctive conditional cases. In brief, co-presence in individual cases is not a law-like relation - it exemplifies a Humean or radical empiricist conception of the Universe; wherein as the early Wittgenstein would put it, there is no metaphysical cement structuring properties into wholes.

Furthermore, neither the gratuitous assumption of universality (which amounts to Popper's hypothetico-deductive model) nor the (illegitimate) inductive inference to generality (implicit in Hempel's deductive nomological model) is sustained by the logic of natural kind classifications based on identity. To appreciate this we need only note that since identity is a primitive relation, properties associated with the kind-term do not constitute necessary or essential properties. Hence the assumption of, or inference to universality is unsupported by any intuition of necessity regarding (the conjunction of) properties associated with natural kind terms. Hence when universality *is* assumed or inferred, it legitimizes mere co-presence (in individual cases) to a (universal) law-like relation which permits predictive inference; but this transition is mediated not by logic, but by the stratagem of convention. This reflects a change in epistemic attitudes, wherein properties which are (observed to be) merely typical of the kind are converted into nominalist essences which define the kind. This has the effect of transforming an empirical classification based on identity into a conceptual framework peculiar to a language. Both Popper's hypothetico-deductive model and Hempel's deductive-nomological inferential structure exemplify this form of law (i.e. universal generalisation) whose rationale is convention; with all the attendant difficulties (analysed in Ch. II).

The empirical constraint on scientific classification, on the other hand, demands that (associated) laws specify the structure and not define the kind. This leads directly to the conception of law as a *relation* between properties. Again, the constraint of prediction requires that the relation be *necessary* i.e. interdependent. It is suggested that both intuitions are satisfied by the relation of functional dependence between properties, the fundamental form of which is `$x \propto y$' where $\propto$ signifies the relation of proportionality.

Before presenting Tarski's formulation and discussion of relations of functional dependencce; we might consider certain reflections of Kant's which lend credence to the thesis that universal generalisations (from experience) do not exemplify causal necessity. Kant's views on material or causal necessity as set forth by William Harper [1986] are:

i. Cosmological or structuraal theories (such as the laws of motion or of universal gravitation) constitute mixed items of knowledge, which according to Kant's officiaal characterization of necessity in the *Postulates of Empirical Thought* count as necessary.

ii. Experience never confers strict universality. Therefore universal generalisation through induction confers *merely aassumed and comparative universality* which carries no necessity.

iii. Strict universality is derived not from experience but is valid absolutely a priori.

Characterization of Possibility and Necessity

4 (a) That which agrees with the formal conditions of experience is possible.

(b) That which is bound up with the material conditions of experience is actual.

(c) That which in its connection with the actual is determined in accordance with universal conditions of experience is necessary.

Thus there are two concepts of necessity (1) a judgement whose negation is not possible because it violates the formal conditions of experience and (2) material necessity which Kant identifies with causal necessity and whose form is:

If A^1 then, if A then B.

where A^1 specifies some actuality, \`A then B' specifies a causal necessity.

Obviously, universal generalisations (which are merely assumed and comparative) are not statements of causal necessity, on Kant's account. On William Harper's interpretation, Newton's inference to centripetal forces involves as actuality Kepler's law of areas and as the universal conditions of experience: Newton's laws of motion, the law of parallelogram of forces, Euclidean geometry, the calculus, the relativeity of inertial motion. The final inference is to centrepetal forces.

The important point that emerges is: the `actuality', the background assumptions, as well as the final inference to gravitational forces are laws of functional dependence which exemplify the relation of proportionality.

Tarski [1965] gives the most general form of a relation of functional dependence as x = R(y) or x = f(y) which is read as: x is that value of the function f which corresponds to (or is correlated with) the argument value y. According to Tarski a relation R is called a functional relation if to everything y, there corresponds at most one thing such that xRy, where the values of y are the argument values, and the values of x are the function values. Tarski emphasizes that functions are of particular significance as far as the application of mathematics to the empirical sciences is concerned. He says: `Whenever we inquire into the dependence between two kinds of quantities occurring in the external world, we strive to give this dependence the form of a mathematical formula, which wouuld permit us to determmine exactly the quantity of the one kind by the corresponding quantity of the other; such a formula always represents some functional relation between the quantities of two kinds. (reference?)

An objection to the conception of law as functional relationship in particular of the form `$x \propto y$', might be that it presupposes universality of precisely the form of empirical generalisation. it says that for all values y the function (in particular of proportionality) assigns an unique value x. But a (tentatively offered) symbolic form of this might be (y, x) [Py $\propto$ Qx] which may be read as: For all values y, x, if the quantity (property) P takes the value y, this implies (by the functional rule of proportionality) that the property Q takes the value x. (This formulation is very tentatively offered and may be non-standard, but conveys the spirit of the conception being developed). In contrast the form of empirical generalisation is: (x) [Px $\supset$ Qx] to be read as: For all objects x, if x exhibits property P it exhibits property Q. The former generalisation, and in general the notion of function, exemplifies a relational or structural view of the universe. From this perspective objects are not just bundles of properties, but are Knit together into systemic wholes, by possibly more than one functional relationship: The latter form of empirical generalisation on the other hand, is fundamentally a `property' view of the universe, wherein atomic properties are only co-present without any necessary relationship between them.

To summarize the foregoing discussion: relations of functional dependence (whose most general form is that of proportionality) satisfy both the constraints i.e. (i) of specifying structure, and (ii) of permitting predictive inference, imposed by classifications based on identity. At the same time, the logic of identity itself as a primitive relation, satisfies the empirical constraint on (scientific) systems of classification. In 4.2, examples of theoretical structure (from physics) are adduced, which illumine theoretical growth as a process of mathematical transformation, which employs as its fundamental principle of inference, Leibniz's Principle of Identity of Indiscernibles (and the attendant laws of identity which follow from it).

4.2 Representation and Reduction: The Changing Faces of Realism

The issue of theoretical growth trifurcates into: (i) a preliminary clarification of the distinction between the logic of mathematical derivation (based on Leibniz's Law) and the logic of propositional and quantification theory based deduction (which invokes rules of the propositional calculus and of quantification theory). This corresponds to Margaret Morrison's [1990] distinction between theory as (mathematical) Representation and theory as (truth-functional) Reduction (ii) A critical exposition of Nagel's [1979] development by reduction thesis in terms of Michael Redhead's [1990] defence of it; which largely assimilates the current literature on this position, and (iii) a critical analysis of the shortcomings of the reduction thesis by Margaret Morrison [1990] who advocates the representationalist view as better illuminating certain features of actual theoretical evolution in (physical) science[4]. The terms of Morrison's discussion relate it quite naturally to the realism-anti-realism debate in (the philosophy of) science. These points can be elaborated in the following manner.

Before presenting Tarski's [1965] and Copi's [1986] distinction between the rules of inference based on the propositional and quantification calculi on the one hand; and the principles of inference for the relational calculus of Identity, based on Leibniz's Law on the other hand; it would be instructive to consider the traditional schema for the deduction of events/laws. This will help us to understand Tarski's and Copi's distinction.

The traditional schematism for the explantion (deduction) of individual events is exemplified by Hempel's [1965] D-N model. Redhead ([1990] p. 137) presents it thus[5]: \`In the deductive-nomological (D-N) model of Hempel the explanans cites one or more scientific laws. In the

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4 Morrison's [1990] discussion focuses largely on the derivation of the Gas Laws from the Kinetic theory (of gases). But she also invokes other examples of theoretical growth to substantiate her points.

5 Redhead's [1990] notation is slightly modified in presentation.

usual schematic fashion adopted by philosophers of science, let us represent a typical scientific law in the universally quantified form (x) (Px ---> Qx) - succinctly all P's are Q's.

If a is a P i.e. Pa is true, then we seek to explain why a is a Q by deducing Qa from the premisses:

(x) (Px ---> Qx) (1)

Pa (2)

Thus, from (1) by Universal Instantiation

Pa ---> Qa (3)

whence, from (2) and (3) by ***modus ponens***

Qa - This is the traditional schematism for the deduction of individual events. The schematism for the deduction of a law from other laws is presented by Nagel ([1979] p. 35) as: `A schematic illustration .... is provided for an explanation of a law having the form "All A's are B's" when it is deduced from two laws having the forms, respectively "All A's are C's" and "All C's are B's".

Copi ([1986] p. 353-355) represents both schema succinctly thus[6]:

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6 Copi's [1986] notation is slightly altered in presentation.

Schematism I (Deduction of Individual events)			Schematism II (Duduction of laws)	
1.	(x) (Hx ---> Mx)		1. (x) (Hx ---> Mx)	
2.	Hs	/ .. Ms	2. (x)(Gx-->Hx)/..(x)(Gx->Mx)	
3.	Hs --> Ms	1, U.I.	3. Hy --> My	1, U.I.
4.	Ms	3,2.M.P.	4. Gy --> Hy	2, U.I.
			5. Gy --> My	4,3,H.S.
			6. (x) (Gx--> Mx)	5,U.G.

Notice that the above schema employ as rules of inference (1) rules from quantification logic viz. Universal Instantiation and Universal Generalisation, and (2) rules of the popositional calculus viz. ***Modus ponens*** and Hypotheticaal syllogism.

If we now try to interpret laws of functional dependence in terms of these schema, the derivation gets blocked at the very outset. This can be made clear by a single example. Thus Nagel ([1979] p. 77) cites as a law of functional dependence, the Boyle-Charles Law for ideal gases, which he formulates as `PV = aT where P is the pressure of the gas, V its volume, T its absolute temperature, and a, a constant that depends on the mass and the nature of the gas under consideration'. Now if we try to interpret this law in terms of the above schema, we obtain:

$$(x) \quad (PV = aT) \qquad --- \quad (1)$$

The very first step is clearly invalid. Hence next steps i.e. the dropping of the universal quantifier by invoking the rule of universal instantiation is blocked, because the formula (for Boyle's Law) contains no individual variable which might be replaced by an individual constant. All the symbols of the formula represent either properties (of the *same* individual/substance); or else numerical constants (which are experimentally determined). Also the further application of the rules from propositional calculus viz.modus ponens and hypothetical syllogism are blocked as well. Instead the derivation proceeds by *substitution* either by measured values of variables (properties) to obtain the value of the (functionally) related variable; or else by the substitution of a variable by an identical variable (in Leibniz's sense of identical properties). This pattern of substitution is based on the Rule of Replacement or Rule of Substitution for Identity; and not on the Rule of Replacement for Logical Equivalence. This is the fundamental distinction made by both Tarski [1965] and by Copi [1986] to which we now turn.

Tarski ([1965] p. 47) mentions as rules of proof for the propositional calculus, the rule of detachment (or the modus ponens rule) and the Rule of Substitution (for logical Equivalence). the Rule of Substitution defines logical equivalence, and its content is formulated by Tarski as follows: `If a sentence of a universal character, that has already been accepted as true, contains sentential variables, and if these variables are replaced by the sentential variables or by sentential functions or by sentences - always substituting equal expressions for equal variables throughout then the sentence obtained in this way may also be recognized as true', Parallel to but distinct from, the rule of substitution for the propositional calculus is the rule of substitution (or replacement) for identity. Tarski [p. 56] formulates this as follows: `As a consequence of Leibniz's Law we have the following rule which is of great practical importance: If in a certain context a formula having the form of an equation e.g.

x = y

has been assumed or proved, then it is permissible to replace, in any formula or sentence occurring in this context the left side of the equation by its right side e.g. "x" by "y" and conversely. It is understood that should "x" occur at several places in a formula, it may at some places be left unchanged and at others replaced by "y"; there is thus an essential difference between the rule discussed..... which does not permit such a partial replacement of one symbol by another'. Tarski thus emphasizes the difference in the rule of proof for logical equivalence and the rule of proof for logical identity.

Copi ([1986] p. 319) articulates the same distinction in greater detail. He formulates the Rule of Replacement for logical equivalence thus: `In any truth-functional compound statemnt, if a component in it is replaced by another statement having the same truth-value, the truth-value fo the compound sentence will remain unchanged. But the only compound statements that concern us are truth-functional compound statements. We may accept, therefore as an (additional) principle of inference, the Rule of Replacement, which permits us to infer from any statement the result of replacing any component of that statement by any other statement ***logically equivalent*** to the component replaced.[7] Thus logical equivalence is a truth functional concept. But the corresponding rule of inference for identity mentioned by Copi (p. 387) is based not on truth-functionality but on Leibniz's definition of identity. Copi, formulates this principle thus: \`x=y iff every attribute of x is an attribute of y, and conversely. This principle permits us to infer from the premises r = u and any formula containing an occurrence of r, as conclusion any formula that results from replacing any number of occurrence of r in the second premiss by the symbol u'.

The foregoing formulations of inference rules make it clear that (1) the rules for logical equivalence are truth

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7 Copi [1986] lists a number of forms for the Rule of Replacement (for logical equivalence) which include Commutation, Association, Distributivity, Double Negation, Transposition, Material Implication, Material Equivalence, Exportation, Tautology, De Morgan's Theorem et al.

Logical identity on the other hand, invokes as rules of inference the Rule of substitutivity for identity, and the laws of identity viz. reflexivity, tansitivity and symmetry.

functional whereas the rules for logical identity are relational (ii) these concepts i.e. of equivalence and of identity lead to distinctive inferential structures. In the light of this, the thesis is put-forth that (explanatory) theoretical structures (in physical science) which use laws of functional dependence (to specify kinds) exemplify the (mathematical) calculus of (the relation of) identity; and not the truth-functional calculus of logical equivalence. This leads directly to the view of sicentific theories as (mathematical) representation rather than as (truth-functional) deduction.[8]

The latter position which is implicit in Nagel's [1979] thesis of (theoretical)development by reduction' is defended by Michael Redhead [1990]. Redhead's discussion is in the context of the criteria for (good) explanation in science. The necessary criterion is deducibility (from universal laws and initial conditions) in the sense of Hempel's [1965] covering law model (exemplified in schematism I above). But this leads at once to what Redhead (p. 1137-1138) termms the `circularity objection'.

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8 It is important to guard against a misunderstanding here. It is not being maintained that laws of functional dependence are not statements or statement functions; What is being claimed is that the inferential structure generated by laws of functional dependence (using Leibniz's Principle and the laws basd on it, as rules of proof) exemplifies a relational structure, and not a (truth-functional) deductive structure.

The circularity objection is formulated by Redhead in the following way:[9] `In (1) the implication as we have written it is material implication. On a Humean (regularity) view of laws that is all there is to (1) it is true in virtue of all its instances being true. But if (1) depends for its truth on the truth of (3), and this, given the premiss Pa, must turn on the truth Qa. So is not the argument completely circular? The truth of Qa, given Pa, is grounded in the truth of a universal statement, whose truth is grounded in the truth of Qa, the very fact we are trying to explain. What this amounts to is that (1) is nothing more or less, on the Humean account, than a compendium of all the instances (3) (In the case of a finite variety of instances the universal law is indeed nothing else than the conjunction of its instances). On the Humean account the instances are "loose" (there is no cement!) so effectively the Hempelian model under this interpretation of law, amounts to the assertion that facts only explain themselves'.

In an attempt to circumvent the circularity objection Redhead points out that the whole argument hinges on the

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9 Redhead's [1990] schematism is abbreviated as follows to facilitate reading of the text:

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(x) (Px --> Qx)          --- (1)
Pa                       --- (2)
Pa --> Qa                --- (3)
Qa.
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assumption that universal laws are only deductively supported by the evidence. If we could construe evidence as in some sense, *inductively* supporting universal laws, the charge of circularity might yet be deflected. But here, says Redhead (p. 138) `we are backing ourselves straight into the problem of induction'. Hence (to avoid circularity) it must be acknolwedged that the explanans (i.e. universal laws) is never known definitely to be true. This converts the D-N model into Popper's hypothetico-deductive model; and Redhead considers this as `an obvious, but unavoidable defect in scientific explanations'. This is because our puzzlement over individual events (i.e. over the problem-situation which demands explanation) can hardly be mitigated by (deduction from) conjectural laws which are not merely not known to be true, but (according to Popper) more strongly, *cannot* be true'. Therefore Redhead says: `So a Popperian expects, insofar as he allows himmself any expectations, that an essential part of the explanans, in any scientific explanation, is definitely false (although not currently known to be false). What Redhead is emphasizing is that the hypothetico-deductive model, whatever its merits for the testing of laws, hardly seems to fulfil the (scientific) requirement for the explanation of events (or laws). Nevertheless Redhead admits that the logic of explanation as deduction thesis forces upon us the hypothetico-deductive model.

It ensues from the foregoing discussion that the problem of explanation in science reduces to that of the (non) confirmation of universal laws required for explanation (deduction). Redhead's (attempted) solution to this invokes the concept of unification which is intended to both provide increased confirmation for laws and to, circumvent the circularity objection. This can be clarified in the following way: Redhead (p. 140) following Nagel [1979] maintains that `In practice good explanations ... arise at the intersection of several universal laws, all of which are necessary to deduce the explanation'. This formulation points to the crucial `unification' aspect of explanation Redhead (p.140) says: `The world at the surface level of immediate experience appears very complicated, very rich in diverse phenomena with no apparent connection. But at a "deeper" theoretical level, can all this diversity get reduced to a few interlocking explanatory principles? This has always provided an ideal of theoretical progress in science, the ideal of unification'.

Redhead acknowledges that the logical structure for unification involves several problems. Most importantly, since unification is intended at both a logical systematization (of diverse domains) and at an increase in confirmation (of experimental laws) the reducing theory (which consists of a set of axioms) must itself be empirically confirmed. This brings in the element of novel prediction which imposes constraints on the logical structure for unification. Thus, for example, the simple conjunction of theories is precluded because this approach yields no significant predictions.[10] What is required by the idea of a unified explanation is a certain `interlocking working together of axioms', which results in both novel prediction and `depth'. Redhead (and reduction theorists generally) however, are unable to provide the logical schematism for this concept of `an interlocked working together of axioms'. He admits that there are many complications associated with the idea.

Primary among these (complications) is that the unifying (reducing) theory might *correct* the reduced theories (or laws); and ***then*** says Redhead `the idea of increased empirical content becomes formally problematic?' This contretemps (exemplied by the correction of Kepler's Laws, ~~and Ga~~lileo's law for free fall by Newton's theory of gravitation) certainly creates formal (logical) problemms fo the thesis of growth by reduction which Redhead is concerned

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10 In this context Redhead ([1990] p. 140) says: `Suppose we have two sorts of phenomena, P_1 and P_2 which stand for the sets of law-like regularity ... and suppose that P_1 andd P_2 are explained by theories T_1 and T_2. Then $P_1 \cup P_2$ is certainly explained by $T_1 . T_2$ ... In a trivial sense there are new predictions that can be deduced from $T_1 . T_2$ but not from either theory separately .... But there are no interesting novel predictions....'.

to defend. For logically speaking, a theory can hardly be allowed to correct (i.e. falsify) its own (deductive) consequences. This problem (and the associated example) are exploited by Popper [1972, 1983] to argue his own case for (theoretical) growth by conjecture and refutation.[11]

Redhead considers, however, that the fundamental intuition underlying the concept of unification is not the (increased) confirmation of experimental laws; nor even the display (ing) of phenomena as (logically) interconnected, but rather an intuitive notion of simplicity. This latter

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11 Popper [1969, 1983] rejects inter-theoretic reduction in the same domain whilst considering (Popper [1972b]) reduction across domains as an accepteable thesis. For the former case Popper [1969, 1983] cites counterexamples of new theories correcting i.e. falsifying previously held theoreis (in the domain) to both (1) reject induction as a method of discoverying new theories and (2) to reject the thesis of growth by reduction. Instead Popper maintains that growth takes place by conjecture and refutation. The competing theories are related by (the sharing of) a common problem-solution; and are (comparatively) evaluated in terms of the falsifiability criterion (which assimilates the criteria of unity, simplicity, depth, verisimilitate etc.). The logical schematism, therefore for Popper's model of growth by conjucture and refutation remains that of his basic schematism for explaning individual events. (i.e. universal law + initial conditions ---> prediction). This schema, when taken conjointly with his thesis of theory-ladenness leads (as analysed in Ch. II) to methodological conventioalism and to meaning - incommensurability.

As for the case of inter domain reduction, (e.g. the reduction of laws of chemistry from those of physics) Popper accepts this, but does not provide any logical schematism for it. Furthermore where no reduction has been effected, Popper advances the thesis of emergence.

notion, he further interprets as a reduction in the total number of laws which we have to accept without explanation.

There are several points about Redhead's exegesis of the reductionist position which are problematic: The most important of these is that reduction bases itself on what Rom Harre [1986] terms the bivalence principle i.e. of truth-falsity of statements, including theoretical statements. This is because laws are conceived as universal statements and theories as (axiomatic) sets of statements, and also because reduction is interpreted in terms of (truth-functional) deduction. This makes the confirmation of (explanatory) laws and theories a crucial issue for reductionists. But inasmuch as confirmation is implicated in the unificationist thesis, the inability (of reduction theorists) to provide a logical schematism for unification leads to problemms for confirmation (of laws and theories). The interpretation of unification in terms of a simplicity criterion merely shifts the problem. We must conclude that without an appropriate logical schematism for unification (which can account for actual theoretical growth in science) reductionism is a vacuous model for theoretical growth in science.

Margaret Morrison [1990] offers a more detailed and penetrating criticism of the reductionist programme. Focusing on Friedman's [1983] model she emphasizes that (i) unification as conjunction (of theories) presupposes realism instead of justifying it. Hence the role of unification in the confirmation of theories is circular. (ii) Theoretical evolution in actual (scientific) practice does not support the unification as conjunction thesis, nor does it support scientific realism in general and (iii) The réductionists' unification concept does not correspond to Whewell's consilience of inductions; and that neither concept provides adequate support for scientific realism. Finally, Morrison makes a case for theory as mathemamtical representation. These points can be elaborated in the following manner.

Morrison presents the Friedman [1983] model thus: We postulate a theoretical structure A (possessing certain mathematical properties) and an observational structure B. A explains or reduces the properties of B. Using the kinetic theory we can explain the observable properties of gases characterized by B by embedding them in A, Where A is literally construed as the world of molecular theory. This enables us to account for the behaviour of gases by identifying them with large configurations of molecules that interact according to the laws of Newtonian mechanics. Due to the properties and relations provided by the theoretical structure we can derive laws that govern the behaviour of observable objects. Friedman sees the relation between A and B as that of model to submodel; which permits literal identification of elements in A and B. On the representationalist acccount on the other hand, B is only `embedded' in A (p. 308).

Friedman prefers the literal construal because it yields greater unifying power and (hence) increased confirmation for both the unifying theory and the phenomenological laws reduced by it.[12] Friedman claims two virtues for his reductivist programme: First there is a type of inference i.e. conjunctive inference that is valid on the hypothesis of a genuine reduction, but not in the case of a representation. Secondly there is the utility of conjunctive inference for confirmation.

But Morrison points to Putnam's [1975] conjunction objection to the effect that the conjunction of theories

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12 Morrison ([1990] p. 308) exemplies Friedman's point thus: She says that: `for example, we can *conjoin* molecular theory with atomic theory to explain chemical bonding, atomic energy and many other phenomena. Consequently, the molecular hypothesis will pick up confirmation in all the areas in which it is applied. The theoretical description then receives confirmation from indirect evidence (chemical, thermal and electrical phenomena) which it `transfers' to the phenomenological description. Without this transfer of confirmation the phenomenological description receives confirmation only from the behaviour of gases. So in cases where the confirmation of the theoretical description exceeds the prior probability of the phenomenological description the latter receives the appropriate boost in confirmation as well. Hence the phenomenological description is better confirmed in the context of a total theory that includes theoretical description than in the context of a theory that excludes such description.

presupposes belief (in the truth of) these theories; hence unification (by conjunction) *presupposes* a reductivist approach that construes theoretical structure as literally true. Therefore unification cannot be invoked for justifying conjunction. Apart from the logical issue, Morrison does not think that actual theoretical evolution in science supports conjunctive inference.

In fact, even apart fromm the special case of the evolution by conjunction thesis, Morrison does not consider reduction a viable approach because of (i) the idealized nature of theoretical assumptions involved. She cites the example of the reduction of thermodynamics by statistical mechanics and emmphasizes that large parts of theoretical structure consists of mathematical representation which lacks physical significance. (ii) the problem of many models: Here Morrison invokes the example of the reduction of gas (the laws by the kinetic theory and points out that the Boyle-Charles law and the van der Waals law require different and incompatible models (of the kinetic theory). She concludes that the literal identification of observational with theoretical structure is precluded.

Furthermore Morrison (p. 326) thinks that the reductionists' notion of unification does not quite capture Whewell's concept of the consilience of inductions because it fails to explicate the `conceptual reshuffling of the phenomena' which takes place in a genuine consilience.[13] Finally, Morrison emphasizes that neither unification nor consilience warrant realism, on account of the factor of contextuality and historical relativism involved.

The general conclusion that Morrison draws from her analysis of the reductionist position is that it does not constitute a viable approach to the problem of theoretical growth in science. This is so partly because reductionists are currently unable to offer a coherent logical schematism or mode of inference for unification (which is the cornerstone of reductionist strategy). But the thesis holds also because certain features of theory structure/evolution in science indicate that theoretical statements cannot be construed literally (as true-false) and that (truth-functional) deduction is not the appropriate form of inference in science. Morrison thinks that these features support the thesis of theory as (mathematical) representation. However she does not provide a detailed schematism or positive arguments in favour of her position.

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13 Here Morrison cites as example the case of Newton's mechanics and Kepler's third law. The latter states that a^3/T^2 = constant. The Newtonian version of the law states that $a^3/T^2 = m + m_1$ where m is the mass of the sun and m_1 is the mass of the planet in question. By ignoring m_1 on the grounds that it is much smaller than m we can assume that the two laws are roughly the same. In what sense is the Keplerian formulation true? Only by leaving out the fundamental *qualitative* aspects of Newton's theory. But if we consistently ignore m_1 it becomes impossible to apply Newton's theory because there is no gravitational force on a body with zero rest mass.

4.3 The Creativity of Identity

In this Section we extend the notion of theory as mathematical representation to encompass that of theory as mathematical transformation. This interpretation reveals the underlying logic of scientific discovery (in the mathematical sciences) to be that of identity as defined by Leibniz's Law. It is argued furthermore that this form of inference is characterized by (a) its intrinsic creativity (b) the hypothetical or conjectural character of its conclusions, and that (c) it sustains a position of referential realism. This analysis can be elaborated in the following way:

First, we note that laws of functional dependence specifically of the form of mathematical proportionality are expressed as statements of mathematical identity (i.e. as mathematical equations) by adducing the constant of proportionality.[14] What is also important to note is that when the constant of proportionality is not numerically specified, then the laws (of functional dependence) assume a purely symbolic form. This is emphasized by both Kuhn [1977] and Duhem [1976].

Kuhn ([1977] p. 464-467) says: `In the Sciences,

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14 As already noted in Ch. III, Tarski gives the general form of functional law as $x = f(y)$. When the relation of mathematical proportionality i.e. $x \propto y$ is expressed in Tarski's formulation it assumes the form $x = ky$ where k is the constant of proportionality.

particularly in physics, generalizations are often found in symbolic form: f = ma, I = V/R, or ... others are ordinarily expressed in words: "action equals reaction", "chemical composition is in fixed proportions by weight..." Kuhn goes on to say `a shared committment to a set of generalizations justifies logical and mathematical manipulation and induces committment to the result. It need not however imply agreement about the manner in which the symbols, individually and collectively are to be correlated with the results of experiment and observation. To this extent the shared symbolic generalizations function as yet like expressions in a pure mathematical system, Kuhn however goes on to distinguish between a *pure* mathematical system and a scientific theory (consisting of mathematical equations). According to him, whilst the pure system is expressed as only one formulation (e.g. f = ma) scientific theories are more like schematic forms, which can express the same law variously. This introduces, he thinks, an empirical element into scientific theories even when they are expressed symbolically. However, Kuhn agrees that scientific theories are symbolically expressed, and are mathematically and logically operated by syntactic devices including the *substitutivity of identities* (p. 465). Moreover, Kuhn does not say that all the schematic forms of scientific theories have empirical significance, but only those that `attach to nature'.

Duhem [1976] anticipates both the points that Kuhn makes viz. that in the context of theory, experimental laws are symbolically expressed, and that only some of these symbolic forms are physically interpreted; (i.e. attach to nature) whilst large parts of theoretical structure consists of pure mathematical representation/manipulation which lacks physical significance. Thus Duhem ([1976] p. 17) says: `The facts of experience taken in their primitive rawness cannot serve mathematical reasoning; in order to feed this reasoning they have to be transformed and put into a symbolic form'. Again, he (p.20) maintains: `In the first place, no experimental law can serve the theorist before it has undergone an interpretation transforming it into a symbolic law. Finally, Duhem says: `The materials with which (this) theory is constructed are, on the one hand, the mathematical symbols serving to represent the various quantities and qualities of the physical world and on the other hand, the general postulates serving as symbols'. Duhem (p.28) goes on to stress that it is an error to insist that all the opertions performed by the mathematician connecting postulates with conclusions should have a physical meaning. According to him such a requirement is legitimate only when it comes to the final formulas of the theory, but has no justification if applied to the intermediary formulas or to the loico-mathematical operations establishing the transition from postulates to conclusion.

More recently, Peter Clark [1990] also emphasizes the pivotal role of mathematics in the articulation and testing of physical theory. He insists that no divide is possible between the purely mathematical context and the physical content of theory in mathematical physics; by which he means that the core notions and concepts of physics cannot be formulated without pre-supposing a very definite mathematical structure.

The larger point that all these philosophers can be interpreted as making is that *all* of physical theory consists of symbolic representation (only some of which need have physical significance).

The implications of these views for the thesis of identity as creative can be drawn as follows: First we make a preliminary clarification viz. that the philosophers whose views are exemplified above work from within very different and varied frameworks of philosophical assumptions regarding theoretical structure.[15] Yet they concur in emphasizing the symbolic form that *all* statements, including experimental laws assume in the context of theoretical structure/ evolution. The significance of this for our thesis consists in this: We have already argued (in Section 4.1) that

15 Thus Kuhn operates with the concept of paradigm, Duhem with the holistic thesis, whilst Clark seems to accept Nagel's development by reduction thesis.

identity is not a concept of the propositional calculus, and that therefore, transformations effected in accordance with Leibniz's law are not based on truth-functionality. This seems to indicate that the chain of inferences based on identity can operate only on symbolic formulatios (algebraic expressions or sentential functions) which are not (true-false) propositions. It is therefore necessary that laws (in the context of theory) be symbolically expressed to facilitate logical manipulation in accordance with Leibniz's law. This leads immediately to the `creative' aspect of identity - since assumptions (premises) are only symbolically formulated, there is no constraint on the free creation of premisses in terms of adherence to experimental facts. This enables a proliferation of theoretical assumptions which emphasizes the fecundity of identity.

The second aspect of the creativity of identity has to do specifically with the substitutivity of identity (i.e. the Rule of Replacement for Identity). An example of the `conceptual reshuffling of the phenomena' (Morrison [1990]) achieved by this rule is presented from Redhead ([1990] p. 146). Redhead, in the context of discussing the notion of cause in modern physics says: `The surprising thing is that physicists long ago gave up the notion of cause as being of any particular interest. In physics the explanatory laws are laws of functional dependence, how one physical magnitude is related in a regular (and law-like on the necessitarian account) fashion with another physical magnitude .... What we actually have in physics is a force law such as the inverse square law of gravitational attraction, which relates via Newton's second law, the acceleration of the body to the relative location of the bodies such as the earth. Instead of $S = 1/2\ gt^2$ (Galileo's law), we have in idealized approximation $S = 1/2\ (GM_2/R^2)\ t^2$, where M is the mass of the earth, R its radius and G is a new gravitational constant. So we are back with a regularity connecting S with t, but also now with M and R. But the force of gravity has been eliminated between the force law and Newton's second law ...'. This `cconceptual reshuffling of the phenomena' is achieved by inference according to the substitutivity of identity. This can be shown in the following way:

According to Copi if x = y and x = z then by Leibniz's Law y = z. Thus if we have:

$$Fg = \frac{GM_1 M_2}{R^2} \text{ (Law of gravitation)}$$

and F = m,a (Newton's second law)

then by Leibniz's law we derive $\frac{GM_1 M_2}{R^2} = m,a$

(assuming Mach's principle viz. gravitational mass M_1 = inertial mass m_1)

Thereupon (by dividing m_1) we get $\frac{GM_2}{R^2} = a$

Again since a = g

we get, by the transitivity of identity $\frac{GM_2}{R^2} = g$

Finally, by the substitivity of identity,
we transfrom $S = 1/2\ gt^2$ into $S = 1/2\ (GM_2/R^2)\ t^2$

Thus the relation betwe Newton's gravitational law and Galibeo's law is not that of truth-functional entailment or conjunction (i.e. conjuction of Galileo's law and Kepler's law to obtain Newton's law); but that of mathematical transformation in accordance with the laws of identity. This process (of derivation) is transformational not only because it leads to the `conceptual reshuffling' of the phenomena; but also because being non-truth-functional, it presupposes neither the truth of the old conceptual (classificatory structures) nor implies the truth of the new ones. Peter Clark ([1979] p.158) presents another example of conceptual reshuffling, this time invoking the `transitivity of identity'.

Several other examples (e.g. the transformational derivation of Kepler's law from Newton's law; or of the gas laws from the Kinetic theory) could be presented from the corpus of physics. They point to the `conceptual' creativity of the relation of identity as a mode of inference which permits transformational derivations in accordance with its laws.

That scientific discovery is a creative process is also emphasized by Popper [1972]. From this Popper draws the conclusion that it is a process which is not amenable to logical analysis. But Popper construes logic only in the sense of truth-functional deduction (in accordance with the rules of the propositional and quantification - logic calculus). However in the calculus of identity, which is not a concept of the propositional calculus, we have a mode of inference which is valid, but nevertheless non-deductive (i.e. non truth-functional). It would therefore appear to be the appropriate form of reasoning for the logic of scientific discovery.

The fact that identity is not a concept of the sentential calculus and that therefore transformations in acccordance with this principle are not truth-functional, also explicates the hypothetical character of (theoretical) formulations arrived at by this mode of reasoning. This thesis however needs careful interpretation.[16]

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16 It must be strongly emphasized that the analysis at this stage is concerned with areas in philosophical logic, which are currently very fluid and controversial. Hence conclusions are tentative, and an attempt is made to substantiate them with views/ arguments from various sources/philosophers and with examples from science. However, the main argument is independently developed.

In this context we note that both Duhem and Popper emphasize that the truth of a theoretical formulation like Newton's law of gravitation is not entailed by the truth or falsity of Kepler's law (or vice-versa). Duhem's and Popper's emphasis (on the mutual inconsistency of theoretical formulation and experimental law) is in the context of a rejection of induction as the method of scientific discovery. But both interpret induction in the sense of (in)valid (truth-functional) duduction, and in this sense it is true that induction cannot account for the discovery of laws/theories. But transformations according to the principle of identity permit formulations which are only hypothetical relative to their inferential basis. This aspect of identity has to do with the peculiar character of the relation as formulated by Leibniz. Since x and y are identitical properties iff they share all their properties in common, it follows that if x = y they are properties of the same object/substance/system (or else x and y are the self-same entity). This is because only properties of the same system share all their other attributes in common. From this it follows that identity holds iff the properties are indeed properties of the same system. But it is just this assumption that cannot be substantiated in the case of statements of theoretical identity (e.g. Temperature = mean kinetic energy of molecules) which are mediated by a process of inference according to Leibniz's Law. This is because the calculus of identity is not truth-functional.

The foregoing analysis has significant implications for the realism, anti-realism debate in the philosophy of science. These can be interpreted in the following way. We have emphasized that the concept of logical identity unlike that of logical equivalence, is not a concept of the propositional calculus. Creative `transformational' inference based on the laws of identity, therefore, operates only on symbolic forms which are not propositions/ statements. From this it follows that the conception of the growth (structure of a theory as à creative process in accordance with Leibniz's Law, cannot sustain a realism based on the bivalence principle (of truth/falsity). On the other hand, the relation of identity, as formulated by the principle of the Idenity of Indiscernibles, purports to hold between entities or properties (of entities); (i.e. x is identical with y iff all properties of x are properties of y). Therefore theoretical structure, as a system of identies (mathematical equations), might be interpreted as supporting a position of referential realism (which presupposes existents). The concepts of `bivalence realism' and referential realism can be clarified in the following way.

First we make a distinction (which is often conflated) between scientific realism based on the strict bivalence principle (i.e. the truth falsity principle) and referential realism. Scientific realism in the first sense is largely defined in terms of the truth and falsity of statements. Newton-Smith [1981] calls this the minimal form of realism. But Rom Harre ([1986] p. 35) prefers to call this position one of *`maximal realism'* since accepting it would commit one to an epistemological ideal that incorporates the strongest possible relationship between scientific discourse and the world, namely truth and falsity. Hence the principle of bivalence can be interpreted as maintaining that the theoretical statements of a science are true or false by virtue of the way the world is. Obviously, our position cannot be interpreted in terms of this form of realism.

Referential realism, by contrast is concerned with the existence/non-existence of theoretical entities. The classical statement of the referential position is due to Sellars (Harre [1986]) who expresses it thus: `To have good reasons for holding a theory is ei ipso to have good reasons for holding that the entities postulated by the theory exist'. Thus the question of the truth or falsity of statements is replacced by that of the existence, non-existence of entities at the heart of a specification of scientific realism.

In interpreting our position in terms of this form of scientific realism however, we should like to make a delicate distinction: Since our argument is based on identity, both as a principle of scientific classification (of natural kinds), and as a creative mode of transformational inference for theory structure/evolution; the conclusions of this evolution are also expressed as statements of theoretical identities (e.g. Temperature = mean kinetic energy of molecules or water is H_2O etc.). The question now arises: within the context of referential realism, how are these statements of theoretical identity to be interpreted. The answer lies in Leibniz's concept of the identity of indiscernibles. From this principle it follows that a statement of theoretical identity hold iff the entities/properties related by identity are properties of the same substance/system (or else if they are the self-same entity). Just in case this condition is satisfied, the statement of theoretical identity holds necessarily true. From this it follows that statements of theoretical identity are true/false by virtue of (i) the *internal structure* of objects/substances i.e. of existents; and (ii) the statements if true are tautologously true. Thus the truth/falsity of statements of theoretical identity presupposes *existents*. This conception of the truth/falsity of statements of theoretical identity obviously differs from the concept of truth/falsity in the correspondence theory (of truth). Whereas the former presupposes existents, the latter presupposes facts. Our argument from identity permits us to make this delicate distinction, and so interprets the thesis of referential realism in its own terms. We can therefore agree with Van Frassen's [1980] formulation of scientific realism according to which a realist holds (with respect to a theory) that sentences are true/false, and that what makes them true/false is something external. But this agreement is subject to the proviso that the statements are statements of theoretical identity, and provided `external' is interpreted to imply existents.

The notions of `existents' and of reference can be further clarified as follows: Reference according to Harre ([1986] p. 68) `consists in achieving a ***physical tie*** between embodied scientist and the being in question. Its existence is thus tied to that of the scientist'. On this view referring is a `material' practice which encompasses both ***indexicality and manipulation of existents.*** It therefore presupposes ontological realism. As Harre (p.67) says: `Referential realism requires that some of the substantive terms in a discouse denote or ***purport*** to denote beings of various metaphysical categories such as substance, quality and relation, that exist independently of that discourse'. Hacking [1983] can also be interpreted as supporting referential realism. How?

In the light of these contrasting formulations of scientific realism, the argument based on identity, developed in this thesis can be interpreted as supporting a position of referential realism. This follows from: (i) The conception of theory as symbolic representation (of mthematical identities) large parts of which lack physical significance, and hence are not true/false. (ii) The concept of theoretical growth by mathematical transformations, according to Leibniz's Law of Identity, which is not truth-functional. (iii) The `primitive' nature of identity as a principle of classification (for natural kinds) which can only be extensionally exemplified and not intensionally defined. This thesis is correlative with the thesis of indexicality and hence presupposes ontological realism.

REFERENCES*

Adams, W.E. and Adams, W. [1987], "Purpose and Scientific Concept Formation?" *BJPS* 38, 419-440.

Agassi [1990], "Induction and Stochastic Independence". *BJPS*, 41, 141-142.

Andersson, G. reviewed in Rednitsky, G. [1991], "Refined Falsificationism Meets the Challenge from the Relativist Philosophy of Science" *BJPS* 42, 273-284.

Ayer, A.J. [1974], "Truth Verification and Verisimilitude" in *The Philosophy of Karl Popper*, P.A. Shillp (ed.), La Salle III, The Open Court Publishing Co., Chicago, 684-692.

Bar - Hillal Y. [1974], "Popper's Theory of Corroboration" in *`The Philosophy of Karl Popper'*, P.A. Shillp (ed.), La Salle III, The Open Court Publishing Co., Chicago, 332-348.

Carnap, R. [1950], "Logical Foundations of Probability", University of Chicago Press, Chicago.

Carnap, R. [1962], "Logical Foundations of Probability", University of Chicago Press, Chicago.

Churchland, Paul [1988], "Perceptual Plasticity and Theoretical Neutrality: A Reply to Jerry Fodor", *Philosophy of Science* 51, 23-43.

---

* This list includes only the books and articles directly referred to in the thesis; material used extensively in the preparation of the text has not been included.

Clark, P. [1990], "Explanation in Physical Theory" in *Explanation and Its Limits*, Knowles, D. (ed.), Cambridge University Press.

Cohen, L.J. [1970], "The Implications of Induction", Mcthuem, London.

Copi Irwing, [1972], *Introduction to Logic*, McMillan, New York.

DeFinnetti, cited in Hesse [1974], "The Structure of Scientific Inference", MacMillan and Co., London.

Donnellan Keith, [1977], "Reference and Definite Descriptions" in *Naming, Necessity and Natural Kinds*. Stephen Shwartz (ed) Cornell Uuniversity Press, Ithaca and Londan, 42-65.

Duhem, P. [1976], "Physical Theory and Experiment" in *Can Theories be Refuted?*, Harding, S. (ed.), 1-40. Dordrecht Reidel Publishing Company, Holland.

Ellis Ellery [1988], "On the Alleged Impossibility of Inductive Probability", *BJPS*, 39, 111-116.

Feyerabend, P.K. [1976], "The Rationality of Science (from `Against Method') in *Can Theories be Refuted?*, Harding, S. (ed.), 289-315.

Fodor, J. [1983], *The Modularity of Mind*, MIT.

Fodor, J. cited in Harre, R. [1986], *Varieties of Realism*. Basil Blackwell Ltd., Oxford.

Fodor, J. [1988], "A Reply to Churchland's `Perceptual Plasticity and Theoretical Neutrality" *Philosophy of*

Science, 55, 188-198.

Franklin, A. [1984], "Are Paradigms Incommensurable" *BJPS*, 35, 57-60.

Franklin, A. and Howson, C. [1984], "Why Do Scientists Prefer to Vary Their Experiments", *Studies in History and Philosophy of Science*, 15, 51-62.

Franklin, A., Anderson, M. and Brock, P. [1989], "Can a Theory-Laden Observation Test The Theory", *BJPS*, 40, 229-231.

Frassen, B.C. Van [1980], *The Scientific Image*, Clarendon Press, Oxford.

Freeman, E. [1974], "The Search For Objectivity in Pierce and Popper" in *The Philosophy of Karl Popper*, Schilpp, P.A. (ed.), La Salle III, The Open Court Publishing Co., Chicago.

Gemes Ken, [1989], "A refutation of Popperian Inductive Sceptism", *BJPS*, 40, 183.

Giannoni, C. [1976], "Quine, Grunbaum, and the Duhemian Thesis" in *Can Theories be Refuted?*, Harding, S. (ed.), 162-175. Dordrecht Reidel Publishing Company, Holland.

Gibson, J. [1979], "The ecological approach to visual perception", Boston: Howard Miflin.

Gillies, O. [1972], "Operationalism", *Synthese*, 25, 1-24.

Gillies Donald [1990], "The Turing-good weight of Evidence function...". *BJPS*, 41, 143-146.

Gilman Daniel, [1992], "What's a Theory to do .... with Seeing..?" in *BJPS* 43, 287-310. Harre Rom, [1986], *Varieties of Realism*. Basil Blackwell Ltd., Oxford..

Good, I.J. [1960], "Paradox of Confirmation". *BJPS*, 9, 145-148.

Goodman, N. [1955], "Fact, Fiction and Forecast", Harvard University Press, Cambridge Mass.

~~Goodman, N. [1965], "Fact, Fiction and Forecast".~~

Goodman, N. cited in Hesse, M. [1974], *The Structure of Scientific Inference*. MacMillan.

Grunbaum, A. [1976], "Is it Never Possible to Falsify a Hypothesis Irrevocably?" in *Can Theories be Refuted?*, Harding, S. (ed.), 260-288.

Grunbaum, A. [1976], "The Duhemian Argument" in *Can Theories be Refuted?*, Harding, S. (ed.), 116-131. Dordrecht Reidel Publishing Company, Holland.

Haack Susan, [1991], "What is the problem of the empirical basis and does Johnny Wideawake solve it ? ", *BJPS*, 42, 369-390.

Hacking, I. [1983], "Representing and Intervening" Cambridge University press.

Hanson, N.R. [1958], *Patterns of Discovery*. Cambridge University Press, Cambridge.

Harding, S. [1976], "Introduction" in *Can Theories be Refuted?*, Harding, S. (ed.), IX-XXI. Dordrecht Reidel Publishing Company, Holland.

Harper, W. [1986], "Kant on the A Priori and Material Necessity" in *Kant's Philosophy of Physical Science*, Butts, R.E. (ed.), Dordrecht Reidel Publishing Company, Holland.

Harré, R [1986], "Varieties of Realism", Basil Blackwell Ltd., Oxford.

Hempel, C. [1976], "Empiricist Criteria of Cognitive Significance" in *Can Theories be Refuted?*, Harding, S. (ed.), 65-88. Dordrecht Reidel Publishing Company, Holland. ?

Hempel Karl [1965], "Aspects of Scientific Explanation", New York Free Press, U.S.A.

Hesse Mary [1974], "The Structure of Scientific Inference", MacMillan and Co., London.

Howson, Colin, [1987], "Popper, Prior Probabilities, and Inductive Inference", *BJPS*, 38, 207-224.

Kneale William [1974], "The Demarcation of Science" in *The Philosophy of Karl Popper*, P.A. Shillp, (ed.), La Salle III. The Open Court Publishing Co., Chicago.

Kripke Saul, [1980], "Naming and Necessity", Basil Blackwell, Oxford.

Kuhn, T. [1970], "Logic of Discovery or Psychology of Research" in *Criticism and the Growth of Knowledge*, Lakatos, I. and Musgrave, A. (ed.), 1-24. Cambridge University Press.

Kuhn, T. [1970], *The Structure of Scientific Revolutions*.

University of Chicago Press, Chicago.

~~Kuhn, T. [1974], "Logic of Discovery or Psychology of Research" in *The Philosophy of Karl Popper*, P.A. Schillp (ed.), La Salle III, The Open Court Publishing Co., Chicago, 798-819.~~

~~Kuhn, T. [1977], "Second Thoughts on Paradigms" in *The Structure of Scientific Theories*, Frederick Suppe (ed.), University of Illinois Press, Chicago.~~

Kuhn, T. [1977], "Second Thoughts on Paradigms" in *The Structure of Scientific Theories*, Frederick Suppe (ed.), 459-482. University of Illinois Press, Chicago.

Lakatos, I. [1976], "Falsification and the Methodology of Scientific Researcch Programmes" in *Can Theories be Refuted?*, Harding, S. (ed.), 205-260. Dordrecht Reidel Publishing Company, Holland.

MacNamara, J. [1991], "Understanding Induction", *BJPS*, 21-48.

Marr, D. [1982], *Vision: A Computational Approach*, Freeman.

McRae, R. [1995], "The Theory of Knowledge" in *The Cambridge Companion to Leibniz*, Jolley, N. (ed.), Cambridge University Press, Cambridge.

Miller David [1990], "Restoration of Popperian Inductive Sceptism", *BJPS*, 41, 137-139.

Morrison, M. [1990], "Unification, Realism and Inference", *BJPS*, 41, 305-332.

Nagel, E. [1979], "The Structure of Science", Routledge and

Kegan Paul Ltd. (originally 1961)

Newton-Smith, W. [1981], "The Rationality of Science", Routledge and Kegan Paul, London.

Nola Robert, [1987], "The status of Popper's Theory of Scientific Method", *BJPS*, 38, 441-480.

Pioncaré, H. cited in Duhem, P. [1976], "Physical Theory and Experiment" in *Can Theories be Refuted?*, Harding, S. (ed.), 1-40. Dordrecht Reidel Publishing Company, Holland.

Popper, K.R. [1963], "Synthese", 15, 67-86. (Name of the ~~article~~ article?

Popper, K.R. [1969], *Conjuctures and Refutations*, Routledge and Kegan Paul Ltd., U.K.

Popper, K.R. [1970], "Normal Science and Its Dangers" in *Criticism and the Growth of Knowledge*, Lakatos, I. and Musgrave, A. (ed.), 51-58. Cambridge University Press.

Popper, K.R. [1972], "The Logic of Scientific Discovery", Hutchinson and Co. Ltd., London.

Popper, K.R. [1974], in *The Philosophy of Karl Popper*, P.A. Schillp (ed.), La Salle, III The Open Court Publishing Co., Chicago.

Popper, K.R. [1983], *Realism and the Aim of Science*, Hutchinson and Co. Ltd., London.

Popper, K.R. and Miller, D.W. [1983], "A Proof of the Impossibility of Inductive Probability". *Nature*, 302.

Popper, K.R. and Miller, D.W. [1984], "The Impossibility of Inductive Probability". *Nature*, 310, 434.

name of the article?

Popper, K.R. and Miller, D.W. [1987], *Nature*, 321, 569-596.

Putnam Hilary, [1974], "The Corroboration of Theories" in *The Philosophy of Karl Popper*, P.A. Schillp, (ed.), La Salle III, The Open Court Publishing Co., Chicago. 221-240.

Putnam Hilary, [1975], *Mind Language and Reality*, Cambridge University Press, Cambridge.

Putnam Hilary, [1977], "Meaning and Reference" in *Naming, Necessity and Natural Kinds*. Stephen Shwartz (ed.), Cornell University Press, Ithaca and London, 119-132.

Quine, W.O. [1962], "Word and Object". Cambridge Mass Harvard University Press.

Quine, W.O. [1974], "On Popper's negative Methodology" in *The Philosophy of Karl Popper*, P.A. Shillp (ed.), La Salle III, The Open Court Publishing Co., Chicago, 218-220.

Quine, W.O. [1976], "Two Dogmas of Empiricism", in *Can Theories be Refuted?*, Harding, S. (ed.), 41-64. Dordrecht Reidel Publishing Company, Holland.

Quine, W.O. [1977], "Natural Kinds" in *Naming, Necessity and Natural Kinds*. Stephen Shwartz (ed.), Cornell University Press, Ithaca and London, 155-175.

Radnitsky, G. [1991], "Refined Falsificationism Meets the Challenge from the Relativist Philosophy of Science", *BJPS*, 42, 273-284.

Redhead, M. [1990], "Explanation in Physics" in *Explanation* *and Its Limits*, Knowles, D. (ed.), Cambridge University Press.

Reid, T. cited in Harre, R. [1986], *Varieties of Realism* Basil Blackwell Ltd., Oxford

Rubin, E. cited in Gilman, D. [1992], "What's a Theory to do ... with Seeing ..?, *BJPS* 43, 287-310.

Rutherford, D. [1995], "Metaphysics: The late period" in *The Cambridge Companion to Leibniz*, Jolley, N. (ed.), Cambridge University Press, Cambridge.

Settle Tom, [1990], "Swann Vs. Popper on Induction on Arbitration". *BJPS*, 41, 401-405 and the references cited therein.

Shapere, D. [1982], "The Concept of Observation in Science and Philosophy' in *Philosophy of Science*, 49, 485-525.

Sheffler, I. [1967], "Science and Subjectivity". Indianapolis, Ind: Bobbs Merrill.

Shwartz Stephen [1977], in *Naming, Necessity and Natural Kinds*, Stephen Shwartz (ed.), Cornell University Press, Ithaca and London, 13-41.

Swann, A.J. [1988], "Popper on Induction". *BJPS*, 39, 367-373 and the references cited therein.

Swinburne, cited in Hesse [1974], "The Structure of Scientific Inference", MacMillan and Co., London.

Tarski, A. [1965], *Introduction to Logic*, Oxford University Press.

Watkins, J.W. [1970], Against "Normal Science" in *Criticism* *and the Growth of Knowledge*, Lakatos. I., and Musgrave, A. (ed.), 25-38. Cambridge University Press.

Watkins [1984], "Science and Sceptism". full detail

Wharf, B.L. [1956], "Language, Thought and Reality", 〃

Whewell, W. cited in Harre, R. [1986], *Varieties of Realsim*, Basil Blackwell, U.K.

Wittgenstein, L. [1953], *Philosophical Investigations*, Basil Blackwell, Oxford.