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 Popper, 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 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

3
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. 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 demarcation 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'! This lends itself to the stoic interpretation: 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
Swann's Stand:

1) Popham attacks
2) But he uses it in his attack on Indochina
3) he uses it to defend his own position in critical rationalism.
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 unacceptable, 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 falsificationism (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 narrow rationalist argument against induction. Moreover Popper defends the method of falsificationism 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 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 employ 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). 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 and transcendentalism as metamethodological criteria. Instead he offers his falsificationist methodology as a proposal or

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 'transcendentalism': 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 criteria 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.

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 (at least 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, Popper ([1972] p. 38) declares, 'Thus I freely admit 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
Knaeble (1974)'s point is questionable since the thesis of asymmetry between verification and falsification of a thesis about universal statements. The fact that a falsification of a universal statement leads to verifying another exist ent in no way affects the thesis.

Also Popper's answer is also questionable. The fact that universal statements are stronger in no way makes one reverify anything. If it even if it does does, what asymmetry can not be caused by the asymmetry about such Popper stroke 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
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 statements 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 statements do; and presumably explain the events/statements they imply. Kneale therefore fails to perceive 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
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 statements 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 [1974] 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
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 statements; 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.

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
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 statements is logically necessary for the application of the criterion of falsifiability. This simply means that basic 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 don't take metaphysical
realism to be enjoyable, concerning (even counterfactually)
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. But the dilemma appears to be insoluble: if logical asymmetry is to prevail and universal hypotheses be falsifiable, then basic statements accepted in that context must be regarded as untestable; if on the other hand, basic statements 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

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 argument⁷: 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'.

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 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. Science 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 statements 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 epistemology. That justification is logical 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 bedevils all philosophy of science especially in the second half of 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 accepted 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 experience. 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 appear to be right after all!

Yet when we further explore the nature of the metaphysical assumptions which are implicit in the acceptance of basic statements, 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 impossible structures (and thus also as unobservable). It excludes them from the class of potential falsifiers. Relative to this altered class of potential falsifiers the statement '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 property 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 theory of asymmetry is undermined by these q weak Laden." Is observation, so far as the major theme of your attack on puppet is concerned.

Did inductive maintain the weak Laden's observation?
considers the property under test to be a non-defining property; then 'verifiable' would be logically tenable relative to a semantic specification which 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 obviously 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.

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 probability calculus).
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 $(\forall x) (S x \rightarrow W x)$. 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'.

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 at least 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 difference 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. 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 semantical 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 processes, 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 protagonists 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 preempt 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 \( g \equiv g' \), and \( h \equiv h' \),
   then if \( g \) confirms \( h \), \( g' \) confirms \( h' \).

(ii) Entailment: Any entailment of a proposition \( h \) must be confirmed by \( h \).
   i.e. If \( h \rightarrow g \), then \( h \) confirms \( g \).

(iii) Converse entailment: If \( h \rightarrow g \), then \( g \) confirms \( h \).

(iv) Special consequence: If \( f \) confirms \( h \), and \( h \rightarrow g \), 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 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 Hesse [(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 rigorous formalisation. In the process the original intuative concept might be modified to sort out ambiguities and contradictions.
initial probability. Thus 

\[
e \text{ confirms } h, \text{ iff } p(h/e) > p(h).
\]

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 \( \overline{Q} \) are \( \overline{P} \).'
   and to 'Everything which is \( P \) or \( \overline{P} \) is either \( \overline{P} \) 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 \( \overline{P} \) and \( \overline{Q} \) and (iii) a \( \overline{P} \) 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. Moreover, whilst Popper's model involves existential assumptions regarding only the one kind of object mentioned in the initial conditions (say ravens); the 'proportionality' 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 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 instances 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 assumption 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
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 instances of 'All P are Q' than that there will be any other proportion of Q's. The clustering postulate would appear to be utterly gratuitous 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 independent, 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 $h = \text{all } p_1 \text{ are } p_2$, on the evidence $e$ of $s$ positive and no negative instances, confirmation $C_0(h)$ is zero; and hence $C_0(h/e)$ is zero for any $e$ whatever. This result holds in general for the $C_0$ value of any universally quantified hypothesis in an infinite
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 \( C_0 \) function yields values of instance confirmation as \( \frac{1}{4} \) and of qualified instance confirmation as \( \frac{1}{2} \). 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 DeFinetti'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 can 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 \( 2^n \) 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 \( (h \lor e) \) is deductively implied by \( e \); and the other factor \( (h \leftarrow e) \) 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. \( (h \lor e) \) which is deductively implied by \( e \). What is more, \( e \) counter supports all of \( h \) i.e. \( (h \leftarrow e) \) 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 confirms 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

\[ s(h/e) = s(hve/e) + s(hv-e/e) \]

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 \( (h \lor e) \) 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 \( (h \lor e) \) is equivalent to h. They call \( (hv-e) \) 'all of h that goes beyond e'. e supports \( (h \lor e) \) 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 probabilistic 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 \( X \rightarrow Y \), \( S(Y/X) \) 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 $(hve)$ on $e$. The fact that the posterior probability is 1, is a consequence of the fact that $e$ deductively implies $(hve)$; 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 $(hve)$. On the other hand, the particular magnitude of $s(hve/e)$ being a degree, and a function partly of $p(hve)$ [i.e. the initial probability $(hve)$], clearly 'goes beyond' the deductive relations between $e$ and $(hve)$. This aspect of $s(hve/e)$ could be correctly described as representing inductive support of $(hve)$ by $e$. Thus:

$$s(hve/e) = p(hve/e) - p(hve)$$

$$= 1 - p(hve)$$

.. if $p(hve)$ i.e. $p(e) \ll 1/2$, $s(hve/e)$ 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 $p(h)=p(h/e)$, 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
GU/Exam/Ph.D/97/

To
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. Nilan 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:


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).

Yours faithfully,

( A.G. Khanolkar )
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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 $h \rightarrow e$, $p (e, hb) = 1$; 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 'acceptance\textsubscript{1}' and 'acceptance\textsubscript{2}' i.e. acceptance for testing, and acceptance on testing, marks no real distinction. The distinction can
only be drawn between the concepts 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 calculus.

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 counterfactual 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 not because they are complementary but because they are models for entirely different things. H-D model is a model for me as a scientist, whereas D-N model is a model for me as a structure and explanation for Popper's acceptance and logic. In fact, he claims that in his first paper it...
Probabilistic independence therefore, is the appropriate assumption here. But surely 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 reinterpretation of this problem is therefore undertaken.
(i) and (ii) constitute the grounds for the say-latitude of observation. The statement "Moreover..." which is implied by (i) and (ii) underlines the principle of...