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CAN THEORIES BE REFUTED?

Essays on the Duhem–Quine Thesis

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To Dorian and Emily

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INTRODUCTION

According to a view assumed by many scientists and philosophers of science and standardly found in science textbooks, it is controlled experience which provides the basis for distinguishing between acceptable and unacceptable theories in science: acceptable theories are those which can pass empirical tests. It has often been thought that a certain sort of test is particularly significant: 'crucial experiments' provide supporting empirical evidence for one theory while providing conclusive evidence against another. However, in 1906 Pierre Duhem argued that the falsification of a theory is necessarily ambiguous and therefore that there are no crucial experiments; one can never be sure that it is a given theory rather than auxiliary or background hypotheses which experiment has falsified. W. V. Quine has concurred in this judgment, arguing that "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body".

Some philosophers have thought that the Duhem–Quine thesis gratuitously raises perplexities. Others see it as doubly significant; these philosophers think that it provides a base for criticism of the foundational view of knowledge which has dominated much of western thought since Descartes, and they think that it opens the door to a new and fruitful way to conceive of scientific progress in particular and of the nature and growth of knowledge in general.

In this introductory essay, I shall indicate what considerations led Duhem and Quine to their views, and how some other leading philosophers and historians of science independently have arrived at similar conclusions. Then the major criticisms of the Duhem–Quine thesis will be presented. Finally I shall sketch the outlines of the rich and wide-ranging discussion of the implications of the Duhem–Quine thesis – a discussion which has occurred mainly in the last decade.

In his great book, *The Aim and Structure of Physical Theory*, Duhem was concerned with the way scientific theories were discussed by most scientists and philosophers of science of the late nineteenth century.

While these scientists and philosophers recognized that theories about nature could not be proved true, they did believe that by eliminating rival hypotheses through prescribed methods, science could finally reveal the residual, single, true description of nature. One kind of experiment was thought to be ideal for the purpose: 'crucial experiments' simultaneously refuted one hypothesis while verifying another hypothesis which was presumed to be the only logical alternative to the target hypothesis. Crucial experiments were thus thought to play a central role in science's project of searching for the truth.

In the selection presented here, Chapter VI of his book, Duhem argues against this view. He shows that two conditions must be satisfied if simultaneous falsification and verification are to take place, and that neither of these conditions can, as a matter of fact, be fulfilled. In the first place, an unambiguous falsification procedure must exist. *Modus tollens* arguments are usually taken to represent the appropriate falsification procedure¹, but Duhem argues that *modus tollens* is rarely, if ever, the structure of argument in the sciences since a scientist's predictions are in fact based not on any single hypothesis but, instead, on at least several assumptions and rules of inference, some of which are often only tacitly held. It is the target hypothesis plus a set of auxiliary hypotheses from which predictions are deduced. "The physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses". Thus there is no reason to single out any particular hypothesis as the guilty one for isolated hypotheses are immune from refutation: Duhem denies that unambiguous falsification procedures do exist in science.

Secondly, even if it were possible to refute a particular hypothesis, one would not be justified in presuming that one had thereby shown any alternative hypothesis to be true, or as the claim has more recently been stated, shown any alternative hypothesis to be closer to the truth. In order to make this stronger truth claim – or truth-like claim – one must be able to show that *reductio ad absurdum* methods are applicable to scientific inference. First of all, Duhem points out that if it were possible to falsify any single hypothesis, then it might prove possible in the future to falsify any hypothesis to date unrefuted. But furthermore, in the future it might also be the case that some alternative explanation more satisfactory than any now known might be produced or discovered. We can see that

this second problem arises because the concepts involved in our hypotheses change as knowledge grows; the plausibility of a description of some characteristic of nature is as much a consequence of the adequacy of one's concepts as it is due to the truth of one's claims. Thus, even if one could falsify a given hypothesis, the only truth established by such a falsification would be the denial of the hypothesis. But the denial of the hypothesis is not itself a single hypothesis but, given conceptual creativity, a potentially infinite disjunction of hypotheses. Because the physicist – unlike the Greek geometer – cannot enumerate all the possible alternative hypotheses which would explain an event, *reductio* methods are not applicable to scientific inference: we can't "assimilate experimental contradiction to reduction to absurdity", as Duhem says. So neither condition required for experiments to be crucial can, in fact, be satisfied, according to Duhem.

In his well-known essay, 'Two Dogmas of Empiricism', Quine refers approvingly to Duhem when he argues that only science as a whole, including the laws of logic, is empirically testable. Many have seen this as a radical conventionalist thesis. A great deal of critical attention has been focused on Quine's attack on the analytic/synthetic distinction; but that may well turn out to be the less important claim Quine makes in this essay. In the first four sections of the essay, Quine criticizes several arguments which might be given in defense of the analytic/synthetic distinction. He then goes on to consider a way of defending the distinction which relies on the verification theory of meaning. Perhaps a statement can be taken to be analytic if it is confirmed by anything whatever that happens in the world. Surely, if we can take some particular statements to be verified by particular experiences, we can also take other statements to be verified 'come what may'. But to this line of argument Quine objects that in fact no individual statement can be verified. Well, one might think, perhaps a statement is analytic if it is disconfirmed by nothing whatever that happens in the world. But, Quine notes, "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system". And, by the same token, "no statement is immune to revision". He says, "the unit of empirical significance is the whole of science". If Quine could provide the philosophic underpinnings to defend this point, he would have shown not only that the analytic/synthetic distinction is untenable, but, more importantly, that the true/false distinction is not

defensible except as applied to science as a whole, and that we cannot defend on epistemological grounds the distinction between physics and logic. However, Quine does not really provide the philosophic underpinnings needed to support these broad claims.

Quine's thesis is stronger than Duhem's, for where Duhem claimed that the physicist can never be sure that no saving set of auxiliary assumptions exists which, together with the target hypothesis, would entail the actual observational results, Quine seems to hold that saving hypotheses always exist: "Any statement can be held true come what may". Quine's thesis is also more general than Duhem's, for Quine extends Duhem's claim for conventionalism in physics to include the truths of logic as well as *all* of the laws of science.

Starting from somewhat different problems, both Carl Hempel and Thomas S. Kuhn have arrived at conclusions similar to Duhem's and Quine's. Hempel began with the problem of defining theoretical terms. He argued that the positivists were wrong to think that the theoretical terms of science can be explicitly or operationally defined using only observation terms. Instead, the theoretical terms must be introduced into science by the theories themselves, and this means that the theoretical term is, in effect, implicitly defined not by observation terms but by the theory. However, because statements are deducible from the theory which do not contain the theoretical terms in question, the theory as a whole can be said to have empirical significance, and, Hempel thinks, can be confirmed or falsified. He reminds that

It is not correct to speak, as is often done, of 'the experiential meaning' of a term or a sentence in isolation. ... A single sentence in a scientific theory does not, as a rule, entail any observation sentences; consequences asserting the occurrence of certain observable phenomena can be derived from it only by conjoining it with a set of other, subsidiary, hypotheses. Of the latter, some will usually be observation sentences, others will be previously accepted theoretical statements.²

Thus for Hempel, the unit of empirical significance – the unit which is tested – must be only the theory as a whole, where this is evidently taken to include all the possible statements of any kind required for the derivation of observation sentences. Hempel has in effect given a defense of the Duhem–Quine thesis for that part of science which includes theories.

Kuhn's project was to give an account of the nature of the scientific enterprise and the reasons for its special success – an account which would

fit the history and practice of science better than what he claims are the obviously inadequate accounts standardly found in science textbooks and in many discussions in the philosophy of science. Kuhn proposed a fundamental change in our perception and evaluation of familiar episodes from the history and practice of science. He suggested that the everyday practice of science – ‘normal science’ – consists for the most part of a very important kind of puzzle solving in which the nature of the puzzle and the terms set for its solution are not themselves regarded critically or subjected to test by scientists in any significant way. Normal science takes place within holistic ‘paradigms’ – extremely broad theoretical, metaphysical and methodological models of nature and of how to discover her secrets. In Chapter X of *The Structure of Scientific Revolutions*, the selection included here, Kuhn argues that the theories which are part of scientific paradigms are not refutable by observations at all. This is because, on Kuhn’s view, sensory experience is not fixed and neutral and theories are not simply man-made interpretations of given data: “What occurs during a scientific revolution is not fully reducible to a re-interpretation of individual and stable data. ... Paradigms are not corrigible by normal science at all”. Instead, it seems that the paradigmatic theories in fact define what is to count as a relevant observation; they define the ‘world’ within which the scientist works. So, a theory is not refuted by experience but instead simply abandoned when there are a large number of problems with it and when a possibly more fruitful way of perceiving the world is at hand. Kuhn claims that his account of science directly challenges the Cartesian paradigm which has guided epistemology and accounts of science for three-hundred years.

The two strongest attacks on the Duhem–Quine thesis have come from Adolf Grünbaum explicitly, and, mainly implicitly, from Karl Popper. In his extraordinarily influential *The Logic of Scientific Discovery*, published in German in 1935, but translated into English only 24 years later, Popper begins by identifying what he takes to be the fundamental problems for philosophy of science and for epistemology. Virtually all problems in epistemology come down to two, Popper says: the problem of induction (‘Hume’s problem’), and the problem of finding an acceptable criterion of demarcation between science and metaphysics (‘Kant’s problem’). With respect to Hume’s problem, Popper says that the inductivists have simply misconceived the nature of scientific method and of

human knowledge. Scientists do not try to prove universal statements true by demonstrating them to be reducible to true singular statements; what scientists do is deduce singular statements from a theory and compare these with the results of experiments. If the comparison is favorable, the theory has temporarily passed its test; if the conclusions of the deductions have been falsified, then their falsification also falsifies the theory from which they were deduced. Therefore, it is falsifiability, not verifiability, which is at the heart of scientific method. Popper claims to dissolve Hume's problem of 'inductivism' by substituting for it a theory of 'deductivism' – a theory of falsifiability.

And it is his theory of falsifiability which provides the demarcation line between science and metaphysics, Popper says, thereby solving Kant's problem. For the positivists at one time, both the universal laws of science and also metaphysical speculations were assigned the status of 'non-statements' – they were 'meaningless', unable to be reduced to elementary statements of experience. But for Popper, while the universal laws of science cannot be verified, they, unlike metaphysical speculations, can be falsified by experience. Thus a theory of the falsifiability of singular statements and of the universal laws from which they can be deduced lies at the heart of Popper's solutions to what he takes to be the two leading problems in epistemology.

In the sixth section of the first selection here – Chapter I of *The Logic of Scientific Discovery* – Popper considers the objection that no theoretical system is ever conclusively falsified for it can always be saved by the logically admissible procedures of "introducing *ad hoc* auxiliary hypotheses, by changing *ad hoc* a definition, or by simply refusing to acknowledge any falsifying experience whatever". Popper proposes that "the empirical method shall be characterized as a method that excludes precisely those ways of evading falsification which... are logically admissible". The method of science is precisely a method insuring that the singular statements deduced from theories can be falsified by experience, thereby transmitting their falsity back up to the theory from which they were deduced. Appended here to Chapter I of *The Logic of Scientific Discovery* are two brief selections from later pages of Popper's book which stress his reasons for arguing that empirical science should be characterized by its methods.

More recently, in the short excerpt from *Conjectures and Refutations*

which is reprinted here, Popper argues specifically that the Quine–Duhem thesis, “the holistic view of tests”, both “does not create a serious difficulty for the fallibilist and falsificationist” and also that “on the other hand... the holistic argument goes much too far”. Evidently he thinks it does not create a difficulty since while the falsificationist does indeed take for granted a vast amount of traditional knowledge,

He does not accept this background knowledge; neither as established nor as fairly certain, nor yet as probable. He knows that even its tentative acceptance is risky, and stresses that every bit of it is open to criticism even though only in a piecemeal way. We can never be certain that we shall challenge the right bit; but since our quest is not for certainty, this does not matter.... Now it has to be admitted that we can often test only a large chunk of a theoretical system, and sometimes perhaps only the whole system, and that, in these cases, it is sheer guesswork which of its ingredients should be held responsible for any falsification.³

However, the holistic argument goes too far since it is possible in “quite a few cases to find which hypothesis is responsible for the refutation; or in other words, which part or group of hypotheses was necessary for the derivation of the refuted prediction”. Does the fact that in some cases scientists reach intersubjective agreement, at least temporarily, as to which part of their theories to revise save Popper’s falsificationism from the Duhem–Quine thesis? The latter would appear to challenge not this uncontroversial sociological fact but the notion that it is tests which determine *which* part of our web of hypotheses and beliefs should be counted as refuted. We must still ask how Popper has succeeded in deflecting the challenge posed to his falsificationism by “the holistic view of tests”.

In several publications Adolf Grünbaum has challenged Duhem’s thesis that the falsifiability of an isolated empirical hypothesis as an explanans is unavoidably inconclusive. He regards the Duhemian thesis as a conventionalist ploy to be found also in Einstein, Poincaré, and Quine. In his well-known 1960 essay reprinted in this collection, Grünbaum argues that the Duhem–Quine thesis is both a logical non-sequitur and, furthermore, false. First he argues that conclusive falsifying experiments are possible. To deny this; Grünbaum says, Duhem would have to prove on general logical grounds that for any empirical finding whatever (e.g., ~ 0), there is a set of non-trivial auxiliary hypotheses from which, together with the target hypothesis in question, the findings could be deduced. But this Duhem cannot guarantee. Thus Duhem made a logical

error in taking the conventionalist thesis to follow from the occasional inconclusiveness of supposedly crucial experiments. But, furthermore, Grünbaum argues that as a matter of fact crucial falsifying experiments have actually occurred in physics, and he discusses what he thinks is an example of such a case. In his response, Quine himself suggests that he finds the Duhem–Quine thesis as challenged by Grünbaum tenable only if taken trivially. Grünbaum has since discussed this thesis in several places, most recently in the second of his essays reprinted here.

Recently a number of philosophers have thought further about the Duhem–Quine thesis, in large part due to Grünbaum’s arguments. Laurens Laudan, Carlo Giannoni, and Gary Wedeking all point out that there are two versions of the Duhem–Quine thesis. There is a stronger one held not by Duhem but probably by Quine; the weaker one, actually held by Duhem, is untouched by Grünbaum’s attack.

Grünbaum presumes, Laudan points out, that the burden of proof is on the scientist who refuses to call a refuted hypothesis false to show that his hypothesis can be saved by some suitable auxiliary hypothesis. But Duhem did not make this strong claim but only the weaker one that those who deny the target hypothesis must show that there does not exist an auxiliary hypothesis which would make the target hypothesis compatible with the unforeseen experimental results. Unless such a proof is forthcoming, Laudan points out, “a scientist is logically justified in seeking some rapprochement between his hypothesis and the uncooperative data”. Duhem does not make the purported logical blunder ascribed to him by Grünbaum. Laudan goes on to argue that Grünbaum’s counter-example purporting to make out a case for conclusive falsification does not hold up either. Grünbaum says that in the history of science one can find cases where a set of hypotheses has been falsified. But Duhem would not disagree with that holistic view, but only with a claim that isolated hypotheses are falsifiable. Furthermore, Grünbaum assumes that the fact that in a particular case the auxiliary hypotheses are highly probable forces a scientist to relinquish the target hypothesis. But ‘highly probable’ is not ‘known to be true’; “the demands of prudence do not carry logical weight”, Laudan notes.

After criticizing the Grünbaum objections to the Duhemian thesis, Giannoni goes on to defend that balance of the Quinean thesis which is both the same as the original Duhemian thesis and which is not defended

by Hempel. Giannoni defends the Duhem–Quine thesis for the part of science depending on the use of operational definitions which refer to measuring instruments, and, in particular, where the measurement is derivative rather than fundamental. In the last part of his essay, Giannoni points to some broader implications of the Duhemian thesis for our conception of scientific knowledge. The Duhemian thesis is not an epistemological thesis regarding our knowledge of the world, he says, but a semantical thesis regarding the meaning of scientific words and of scientific language. But to say this is not to trivialize the thesis, he thinks, for the thesis is required by the very notion of scientific discovery. This and other considerations lead Giannoni to suggest that the question about the nature of Duhemian conventionalism in physics comes down to a choice between a realistic or a nominalistic approach to the symbols of physics. And this in turn leads him to conclude that the Duhem–Quine thesis shows that the distinction between descriptive simplicity and inductive simplicity is not a logical distinction but an ontological one. Where a realist searches for inductive simplicity, a nominalist searches for descriptive simplicity.

Wedeking argues that Popper as well as Grünbaum are examples of the “empirically-minded philosophers” who think that there are empirical theories which are ‘falsifiable’ in the sense that each such theory unambiguously separates out those basic statements which it prohibits and those which it permits. After discussing Grünbaum’s position, Wedeking tries to formulate the Quinean version of the Duhem–Quine thesis in such a way that its validity can be tested. But, he can turn up only the pragmatic formulation that the new theoretical system in which the target hypothesis is held true in light of the unforeseen observations must be one which is ‘adequate’ to the facts of experience. Thus Wedeking doubts the truth of the Quinean thesis in any but its most trivial form. Does this mean, he asks, that Quine’s theory is worthless? No, for it leads us to observe that those cases in which Quine’s thesis holds only trivially and those in which we are confronted with a real and significant alternative as to which sentences we should reject as false and which we should retain as true “differ only in a matter of degree”.

The next two selections are extraordinarily rich and complex essays which, along with Feyerabend’s essay, set the Duhem–Quine thesis and its criticisms in the broadest philosophic context. Hesse shows how

Duhem introduces two important modifications into classical empiricism: a new theory of correspondence and a new theory of coherence. She argues that Quine has taken up both aspects of Duhem's new empiricism; and, after countering both Popper's and Grünbaum's objections to the Duhem–Quine thesis, she points to what she thinks are two residual and significant problems with Duhem's and Quine's 'network theory'. First, some theory of relative empirical confirmation must be given, and this must be done without identifying any statement of the system which expresses the evidence incorrigibly. Secondly, a way must be provided for analysing stability and change of meaning in the network. Hesse proposes solutions to these problems, and then marks five features which distinguish the new Duhem–Quine empiricism, as she has now developed it, from the older, classical empiricism. First, she points out how the difference between theoretical and observational aspects of science is not epistemological but pragmatic and causal. Second, on the Duhem–Quine theory, empirical applications of observation predicates are not incorrigible, and the empirical laws taken to hold between them are not infallible. Third, in order to avoid the meaning variance paradoxes, the majority of descriptive predicates must be intersubjectively stable. However, we cannot know which observation predicates will retain stability of meaning in subsequent theories, just as we cannot know ahead of time which observation statements will be retained as true in later theories. Fourth, she thinks that we must assume some prior principles of selection for picking well-confirmed theories, and some criteria for shifts of applicability of some observation predicates if we are to avoid complete arbitrariness in adoption of a new 'best' theory. Finally, while some aspects of the epistemological problem are systematically conflated with causal mechanisms in the new empiricism, this does not result in a fatal circularity for the network model. She concludes that while the new Duhem–Quine view of science can provide no guarantee that our knowledge of this world is firmly based, that demand of the old empiricists was evidently unreasonable anyway.

Lakatos thinks that Kuhn, in particular, faced with the problems with both the older empiricist views and with what he calls 'naive falsificationism', abandons efforts to give a rational explanation of the success of science. Kuhn settles, he says, for merely trying to explain changes in paradigms in terms of social psychology. Lakatos proposes, instead, to

follow Popper's lead and find a workable criterion of rational progress in the growth of knowledge; he presents a theory of 'progressive problem shifts'. On Lakatos' theory, the history of science is not the history of scientific theories but the history of series of theories – the history of 'research programmes'. A research programme consists of methodological rules: a negative heuristic tells us what paths of research to avoid, a positive heuristic tells us what paths to pursue. In particular, the negative heuristic forbids us to direct our *modus tollens* at the 'hard core' of a series of theories. The positive heuristic consists of a partially articulated set of suggestions or hints on how to develop the 'refutable variants' of the research programme, how to modify and sophisticate the refutable protective belt of auxiliary hypotheses. Lakatos claims that his demarcation between progressive and degenerating problem shifts is almost identical with Popper's celebrated demarcation criterion between science and metaphysics.

Like Popper, Lakatos does not think that the 'weak form' of the Duhem–Quine thesis conflicts with methodological falsificationism (though it does with 'naive falsificationism'). But a strong form, which he thinks Quine probably holds, is inconsistent with all forms of methodological falsificationism because, as he sees it, it excludes any rational selection rules among the alternative theories. Do Lakatos and Popper take the conventionalism of Duhem and Quine to involve a greater component of arbitrariness in the construction and selection of theories than would be espoused by either Duhem or Quine?

In the next section, Grünbaum in part modifies his original critique of the Duhem–Quine thesis. At least in some cases, he now holds, we can ascertain the falsity of a component hypothesis to all scientific intents and purposes, although we cannot falsify it beyond any and all possibility of subsequent rehabilitation. Grünbaum here replies to the discussions of his earlier position by Laudan, Giannoni, Hesse, Lakatos and others.

The essays above have shown how the Duhem–Quine thesis provides the cutting edge for an attack on the empiricist reliance on the authority of the senses and, more generally, for a criticism of Cartesian foundationalism, or authoritarianism, in epistemology. Many would take Feyerabend to be the most extreme anti-Cartesian today, and in the last essay in this collection, one can detect attacks on two forms of what might be called the 'methodological authoritarianism' which characterizes much of

epistemology since Descartes. In its historically extremely influential form, the 'naive inductivist' holds that there are indeed rational principles for the discovery of nature's regularities, and that these are identical with the rational principles for justifying the accepted theories of science at any particular time. The naive inductivist has the idea that there exist definite principles and rules of a quasi-logical sort for inducing theories from phenomena. But recent philosophers of science as different as Hempel and Popper have retreated to a second position: science definitely can be reconstructed as a rational enterprise, but there may well be no principles for a prospective methodology of science. However, Feyerabend holds that there are no rationally defensible rules for either a prospective methodology of science or for a retrospective assessment of science.⁴ In the selection presented here, the last five sections of his long (110 pp) essay 'Against Method', Feyerabend presents four objections to the first two positions, and suggests that the persistent attempt of epistemologists to identify a rational 'authority' – a firm source of knowledge or a sure method for discriminating between reliable and unreliable hypotheses – is not entirely separable from authoritarianism in values.

Against the naive inductivist, Feyerabend argues that since the history of science reveals no rationally justifiable rules for a prospective methodology, it should be granted that there are no rationally justifiable rules for retrospective assessments of science either. Second, Feyerabend points out that any proposed reconstructions of science which seem to us to be reasonably rational actually end up being useless, for they only succeed in eliminating all of science as irrational. Third, Feyerabend repeats his well-known criticisms of the purported asymmetry between observation statements and theoretical statements. It is not the case that observations are irrefutable while theories are refutable. While we often do test our theories by experience, we equally often assess experience in light of new or newer theories, changing our assessment of experience accordingly. Thus, for Feyerabend, research is an interaction between new theories explicitly stated and older beliefs which lie deep in the observation language. Finally, Feyerabend presents his well-known arguments for the incommensurability of theories.

Throughout Feyerabend's writing, and here in particular, can be found a conscious and conscientious attempt to push philosophy of science into an ethical dimension. Feyerabend sees epistemological and ethical prefer-

ences as tightly linked. For one thing, someone, or a culture, which prefers or is led to prefer a secure, routine, entirely predictable way of life will naturally be led to accept an epistemology characterized by unchanging, 'objective', guaranteed facts and by a theory of how, using just the right methods, we can apprehend and collect them. Ways of life and theories of knowledge are mutually supporting, Feyerabend suggests. And, the choice between alternative theories of knowledge becomes a normative act, the conscious selection of a certain way of understanding the world.

What exactly are the ideals of rationality which Feyerabend is here attacking? Is he in fact appealing to other, hidden ideals of rationality in his attack on Cartesianism? Is he proposing that there is an ideal of rationality which lies wholly or in part outside the forces which have shaped the history of science, outside the forces which produce maximum 'progress' in science? What account can be given of *this* notion of rationality?

With this concluding essay, the consequences of the Duhem-Quine thesis have been extended not only to challenge Cartesian epistemology, but also to open the possibility of a reconsideration of the link between epistemology, on the one hand, and ethics and political theory, on the other hand. The Duhem-Quine thesis may well take its place in the history of ideas as signaling a radical change in our understanding of the nature of both human knowledge and human knowers.

SANDRA G. HARDING

NOTES

¹ *Modus tollens* is represented by the schema $[(H \rightarrow O) \cdot \sim O] \rightarrow \sim H$. If, from a given hypothesis, H, we predict a certain observation, O, then, if the prediction turns out to be false, $\sim O$, that would serve to refute the hypothesis from which the prediction was deduced, $\sim H$.

² Carl G. Hempel, 'Empiricist Criteria of Cognitive Significance: Problems and Changes', *below*, p. 75.

³ Karl R. Popper, 'Truth, Rationality, and the Growth of Scientific Knowledge', *Conjectures and Refutations*, Routledge and Kegan Paul, London, 1963, p. 238-239, *below* p. 114.

⁴ C. A. Hooker has also set Feyerabend's philosophy of science in this context. Cf. his interesting review of 'Against Method': 'Critical Notice of Minnesota Studies in the Philosophy of Science', Part II, *Canadian Journal of Philosophy* 1 (1972).