Popper, Refutation and 'Avoidance' of Refutation*

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Gregory Stuart Bamford B Arch (hons.)
Department of Philosophy
The University of Queensland
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*NOTES

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The content of this thesis is, to the best of my knowledge and belief, my own work except where otherwise acknowledged. The content of this thesis has not been submitted, either in whole or in part, for a degree at this or any other University.

Abstract

Popper's account of refutation is the linchpin of his famous view that the method of science is the method of conjecture and refutation. This thesis critically examines his account of refutation, and in particular the practice he deprecates as avoiding a refutation. I try to explain how he comes to hold the views that he does about these matters; how he seeks to make them plausible; how he has influenced others to accept his mistakes, and how some of the ideas or responses to Popper of such people are thus similarly mistaken. I draw some distinctions necessary to the provision of an adequate account of the so-called practice of avoiding a refutation, and try to rid the debate about this practice of at least one red herring. I analyse one case of 'avoiding' a refutation in detail to show how the rationality of scientific practice eludes both Popper and many of his commentators.

Popper's skepticism about contingent knowledge prevents him from providing an acceptable account of contingent refutation, and so his method is really the method of conjecture and conjecture. He cannot do without the concepts of knowledge and refutation, however, if his account of science is to be plausible or persuasive, and so he equivocates between, amongst other things, refutation as disproof and refutation as the weaker notion of discorroboration. I criticise David Stove's account of this matter, in particular to show how he misses this point. An additional advantage Popper would secure from this equivocation is that if refutations were mere discorroborations they would be easier to achieve, and hence more common in science, than is the case. On Popper's weak notion of refutation, it would be possible to refute true theories since corroboration does not entail truth.

There are two other related doctrines Popper holds about refutation which, if accepted, make some refutations seem easier to obtain than is the case. I call these doctrines 'Strong Popperian Falsificationism' (SPF) and 'Weak Popperian Falsificationism' (WPF). SPF is the false doctrine that if a prediction from some theory is refuted then that theory is refuted. Popper does not always endorse SPF. In particular, when confronted with a counter-example to it, he retreats to WPF, which is the false doctrine that if a prediction from some theory is refuted then that theory is *prima facie* refuted. WPF , or even SPF, can seem plausible if one has in mind predictions derived from theories in strong or conclusive tests of those theories, which I suggest Popper characteristically does.

Popper is disposed to describe any such case of predictive failure which does *not* lead to the refutation of the theory concerned as one in which that refutation has been avoided. To reinforce his portrayal of the refutation, or the attempted refutation, of major scientific theories as the rational core of scientific practice, Popper treats the so-called practice of avoiding a refutation as untypical of science, and much so-called avoidance he dismisses as unscientific or pseudo-scientific. I argue that his notion of avoiding a refutation is incoherent. Popper is further driven to believe that such avoidance is possible, however, because he conflates sentences with propositions and propositions with propositional beliefs. Also, he wishes to avoid being saddled with the relativisim that is a consequence of his weak account of refutation as discorroboration.

Popper believes that *ad hoc* hypotheses are the most important of the unscientific means of avoiding a refutation. I argue that his account of such hypotheses is also incoherent, and that several hypotheses thought to be *ad hoc* in his sense are not. Such hypotheses appear to be so largely because of Popper's use of rhetoric and partly because these hypotheses are unacceptable for other reasons. I conclude that to know that a hypothesis is *ad hoc* in Popper's sense does not illuminate scientific practice. Popper has also attempted to explicate *ad hoc*ness in terms of some undesirable, or allegedly undesirable, properties of hypotheses or the explanations they would provide. The first such property is circularity, which is undesirable; the second such property is reduction in empirical content, which is not. In the former case I argue that non-circularity is clearly preferable to non-*ad hoc*ness as a criterion for a satisfactory explanation or *explanans*, as the case may be, and in the latter case that Popper is barking up the wrong tree.

Some cases of so-called avoidance are obviously not unscientific. The discovery of Neptune from a prediction based on the reasonable belief that there were residual perturbations in the motion of Uranus is an important case in point, and one that is much discussed in the literature. The manifest failure of astronomers to account for Uranus's motion did not lead to the refutation of Newton's law of gravitiation, yet significant scientific progress obviously did result. Retreating to WPF, Popper claims that Newton's law was *prima facie* refuted. In general, astronomers have never shared this view, and they are correct in not doing so. I argue that the law of gravitation would have been *prima facie* refuted only if there had been good reason at the time to believe as false what is true, namely, that an unknown trans-Uranian planet was the cause of those Uranian residuals. Knowledge of the trans-Uranian region was then so slight that it was merely a convenient assumption, one which there was little reason to believe was false, that the known influences on Uranus's motion were the only such influences. I conclude that in believing or supposing that it was *this* assumption that was false, rather than the law of gravitation, Leverrier and Adams, the co-predictors of Neptune, were acting rationally and intelligently.

Popper's commentators offer a variety of accounts of the alleged practice of avoiding a refutation, and of this case in particular. I analyse a sample of their accounts to show how common is the acceptance of some of Popper's basic mistakes, even amongst those who claim to reject his falsificationism, and to display the effects on their accounts of this acceptance of his mistakes. Many commentators recognize that anomalies are typically dealt with by changes in the boundary conditions or in other of the auxiliary propositions employed. Where many still go wrong, however, is in retaining the presupposition of WPF which encouraged Popper to hold the contradictory view about anomalies in the first place. Thus Imre Lakatos and others, for example, have developed a 'siege mentality' about major scientific theories; they see them as under continual threat of refutation from anomalies, and so come to believe that dogmatism is essential in science if such theories are to survive as they do. I examine various such doomed attempts to reconcile Popper with the history of science. It is a common failure in this literature to conflate or to fail to see the need to distinguish a belief from a supposition, and an epistemic reason from a pragmatic reason. I argue that only if one does draw these distinctions can one give an adequate account of how anomalies are rationally dealt with in science.

The other important strand in Popper's thinking about 'avoidance' of refutation which has seriously misled some of his commentators is his unfounded belief in the dangers of *ad hoc* hypotheses. I examine the accounts that a sample of such commentators provide of the trans-Uranian planet hypotheses of Leverrier and Adams. These commentators imply or assert what Popper only hints at, namely, that there *is* something fishy about this hypothesis. I provide a further defence of the rationality of entertaining this hypothesis at the time.

I conclude with a few remarks about Popper's dilemma in respect of scientific practice and his long standing emphasis on refutations.

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Introduction

What, if anything, characterises scientific method? Karl Popper believes that scientific method is characterised by the refutation of the major or fundamental theories in science, and by their replacement with measurably better theories. This belief underpins his famous account of scientific method as the method of conjecture and refutation:

> Assume that we have deliberately made it our task to live in this unknown world of ours; to adjust ourselves to it as well as we can; to take advantage of the opportunities we can find in it; and to explain it, . . . as far as possible, with the help of laws and explanatory theories. If we have made this our task, then there is no more rational procedure than the method of trial and error - of conjecture and refutation: of boldly proposing theories; of trying our best to show that these are erroneous; and of accepting them tentatively if our critical efforts are unsuccessful.[1]

Popper's account of method has been influential though not widely accepted in philosophy of science. To the extent that scientists are influenced by philosophy of science, however, his views on method appear to have had more currency amongst them than those of most other philosophers working in the area.[2] Popper's views on method have been influential outside science too. As a young architecture graduate I found them exciting; and his emphasis on problems, conjectures, and criticism altogether refreshing. I became a Popperian. Given the debate in design circles about design method at the time - the early 70s - it is not hard to see why this would be likely, if one were to encounter his views. It was often said or implied at the time that design would be rational, or at least much more likely to succeed, only if designers would do away with their preconceptions and begin with the facts, whatever 'the facts' might be. A *scientific* theory, it was assumed, was justified because it was logically derived from observation or experiment. A design, it was believed, should be derived in a similar, quasi-logical fashion from the requirements and constraints of a supposedly given design task.[3] Just how this was to be done was never made clear and the movement to 'scientise' design in this fashion has long since collapsed.[4] Its techniques or methods for refashioning design were often, at best, unhelpful; at worst, they were counter-productive and debilitating, leaving designers with so little time to design that their efforts could not but be the product of many of the very preconceptions the use of those techniques was supposed to displace. Moreover, the techniques themselves narrowed solution fields in ways their authors or disciples often did not understand, further reinforcing preconceptions about solution possibilities. To be told at this time that *science* really worked by, of all things, trial and error; and that its intellectual products, its theories, were justified not by where they had come from or by the process they were arrived at but by how well they stood up to criticism, was like a breath of fresh air.

So this research began with the ultimate aim of constructing a Popperian model of design method. After looking more closely at Popper, however, it has ended as an exercise in, amongst other things, dismantling and rejecting much of his model of scientific method, especially the role he attributes to refutation. This thesis, then, is firstly an attack on Popper's account of refutation and, in particular, on his account of the practice he deprecates as avoiding a refutation. Secondly, it is an attempt to retrieve the possibility that scientific practice is rational, which is lost on this account. I try to demonstrate how we should account for the so-called practice of avoiding a refutation - which is very common in science - and examine the case of the discovery of Neptune in detail to illustrate this account. Several of Popper's commentators or those he has influenced have attempted this same exercise or parts thereof and I examine a sample of their accounts, chiefly to bring out the deleterious influence Popper's ideas about refutation have had on them. Thirdly, this thesis aims to show how much of the plausibility of Popper's account of method relies on his rhetoric, in particular on various semantic fallacies. Popper writes in a simple, seemingly lucid way. But appearances can be deceptive, and many of his commentators have been lulled by the exercise of this skill. In the introduction to *The Philosophy of Popper*, for example, T.E. Burke writes that Popper "is lucid enough to have no need of a self appointed interpreter; his own words are by far the best source of information about the content of his thought."[5] But Popper needs interpretation quite as much as many a more obviously difficult writer.

Notes for the Introduction

1. Karl R. Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge*, 4th ed., rev. (London: Routledge and Kegan Paul, 1972), p. 51.

2. Bryan Magee, *Popper* (Glasgow: Fontana/Collins, 1975), p.9; Michael Mulkay and G. Nigel Gilbert, "Putting Philosophy to Work: Karl Popper's Influence on Scientific Practice", *Philosophy of the Social Sciences* 11(1981): 389-407.

3. Bill Hillier, John Musgrove, and Pat O'Sullivan, "Knowledge and Design", in *Environmental Design: Research and Practice 2. Proceedings of the EDRA 3/AR 8 Conference, University of California at Los Angeles, January 1972*, ed. William J. Mitchell (n.p., 1972), pp. 29-3-1 to 29-3-14.

4. Robert A. Fowles, "What Happened to Design Methods in Architectural Education? Part One - A Survey of the Literature", *Design Methods and Theories* 11 (January-March 1977): 17-31.

5. T.E. Burke, *The Philosophy of Popper* (Manchester: Manchester University Press, 1983), p. vii.

Chapter One

Popper on Knowledge and Refutation

1.1 Introduction

In this chapter, I analyse Popper's account of what refuting certain contingent propositions involves (1.2), and how this implausible account can seem plausible (1.3). David Stove has also discussed these matters in some detail and I criticise some important aspects of his analysis and conclusions (1.4). Next, I discuss two false doctrines about the refutation of major theories in science which Popper holds. I call these doctrines Strong Popperian Falsificationism (1.5) and Weak Popperian Falsificationism (1.6). The effect of accepting each of these doctrines, and the weak notion of refutation generally that Popper holds, is to make such refutations seem easier in principle to achieve than is the case. In turn, this makes the method of conjecture and refutation seem more attractive or realistic as an account of how science works than is the case. I conclude that Popper's method is the method of conjecture and conjecture (1.7).

1.2 Popper's Beliefs: Knowledge without Truth, Refutation without Falsity

What is it for a proposition to be refuted or falsified? In ordinary English and, in general, in philosophical discourse, 'refute' and 'falsify' both mean 'prove to be false', though 'falsify' does of course have other meanings.[1] A refutation or, in the relevant sense, a falsification is both a proof and an act: it is the act of providing a proof that some proposition is a falsehood. I propose, therefore, that some proposition, p, is refuted or falsified if, and only if, someone, S, shows or demonstrates that there is some other proposition, q, which S knows such that q entails $\sim p$. Since p is refuted only if p is false, 'knows' is used here, as it is in ordinary English and in philosophical discourse generally, such that S knows that q only if q (is true).[2] It is at this point that Popper parts company with ordinary English, and with most philosophers, so a good place to start in understanding his account of refutation in science is with his account of scientific knowledge, or at least with certain aspects thereof.

Popper has never accepted the common traditional account of knowledge as justified true belief, and at least since Edmund Gettier's short paper, "Is Justified True Belief Knowledge?",[3] there would be, in this respect, few philosophers with whom he is any longer out of step. Many now think that justified true belief is not sufficient, or that the notion of justification is fundamentally a mistake, and some quarrel with the notion of belief (though not for any Gettier-type reason).[4] But few would go as far as Popper has done. He replaces justification with corroboration and belief with acceptance, but the crucial shift in his thinking is the elimination of truth as a necessary condition for knowledge.[5] This shift is the product of a half-hearted skepticism on his part. He shares the skeptic's doubts about contingent knowledge, but is unable to make do without it if his portrayal of science is to be at all plausible. So he opts for an account of knowledge which stills those doubts, but which raises fresh doubts that what he gives an account of no longer resembles knowledge.

In general, he takes the view that truth is something to which, in effect, knowledge claims approximate, or which successive knowledge claims may approach. This idea or metaphor is the basis of the thesis of verisimilitude or truth-likeness.[6] The content of the claims he typically has in mind here are scientific theories. Our attention is drawn to the fact that the history of science is littered with false theories, many of which were once thought to be known to be true. A case in point is Newton's inverse square law of gravitation.[7] Important though this historical generality may be, no skeptic such as Popper is entitled to its use and in the present context it serves only to obscure the issue, namely, whether one has knowledge without truth.

A comparatively recent development in Popper's thought, and one to which he attaches some importance, is a distinction he has drawn between what he calls subjective or world two knowledge and objective or world three knowledge.[8] In general, 'subjective knowledge' and 'objective knowledge' mean, or rather would mean if 'knowledge' retained its ordinary meaning for Popper, what is otherwise meant by 'knowledge' and 'the content of knowledge' or 'what is known', respectively.[9] According to Popper, the former consists of states of mind or dispositions to act, the latter of "problems, theories, and arguments as such".[10] Renaming or alluding to the familiar distinction between the knowledge that q and the proposition or the fact that q in this fashion, however, is both otiose and misleading. In any case, there is no proposal to make truth a requirement for knowledge, so-called, in either form, so the distinction is of no consequence here.

Popper offers three arguments for why there can be no knowledge, traditionally so called. Each argument alleges, amongst other things, some fatal incapacity or limitation of experience as a source or ground for knowledge.

The argument to which he seems to attach the least weight turns on an alleged transcendence in description. He says:

> The statement, 'Here is a glass of water' cannot be verified by any observational experience. The reason is that the *universals* which appear in it cannot be correlated with any specific sense-experience. (An 'immediate experience' is *only once* 'immediately given'; it is unique.) By the word 'glass', for example, we denote physical bodies which exhibit a certain *law-like behaviour*, and the same holds for 'water'. Universals cannot be reduced to classes of experiences; they cannot be 'constituted'.[11]

Popper's second argument turns on the ineradicably logical component in justification. To justify is to provide reasons, and reasons involve logical relations. Such relations exist only between statements or propositions, however, so there are none between experience on the one hand and any statement of belief or acceptance on the other. There can be causal relations between experience and belief or acceptance, but whilst the former can "motivate" the latter it provides no more *justification* than does "thumping the table".[12]

Popper's third and most influential argument turns on the fallibility of experience or judgement. Since mistakes are always possible in observation or experiment, however skilful or meticulous we may be, we do not or cannot know whatever we may claim to have observed or demonstrated is the case.[13]

Popper settles for a weak form of conventionalism, and acknowledges that he does so, in his account of perceptual knowledge. He holds that any observation statement that is accepted - or 'known' - is one which merely "*plays the part* of 'a true statement of fact'". (Emphasis mine.)[14] It is worth noting here that in his account of science the logical form such statements may take is restricted to that of a so-called singular existential statement, for example, 'There is a brush-tail possum on that branch now.' These statements fill the role of basic statements in his account, and their acceptance (or rejection) as true (or false) is a matter for a form of collective decision making or agreement on the part of the members of the relevant scientific community.[15]

What are we to make of these arguments or of the conventionalist position they encourage Popper to adopt or retain?

His first argument above rests on a mistaken identification of universals, properly so called, with their instances. Suppose that a child is shown a glass and asked, "What is this?" If she replies, "A glass," then she is using a general term (not a universal) to correctly describe an *instance* of a universal. And there is, I take it, no objection to someone's being able to experience an instance of something. I agree that no universal can be "correlated" with any specific sense-experience. But this is because we do not experience universals; we experience their instances, which *are* so correlated.

It seems likely that this argument of Popper's developed as an analogy or by a confusion with the problem of induction,[16] that is, with the problem for the claim to know some universal generalisation, $(x)(Fx \supset Gx)$, that is posed by any *un*experienced instance of the universal F, namely, is it also a G? We do not know, and thus any such generalisation *transcends* experience in this way. Nonetheless, this is not a good analogy, for whatever may undermine my ability to know from experience that something is a glass, or that, say, it is chipped or spotted, it is not that there can be or is some *other* thing, of which I have no experience, that is also a glass, or that is also chipped or spotted. Moreover, in using words like 'glass' and 'water' correctly why am I committed to the objects so denoted behaving in some law-like manner? A basic statement like 'Here is a glass of water' does not *entail* any law-like generalisation, as Popper would otherwise be the first to point out. The child who describes a conjuring trick she has seen as one in which rabbits were pulled out of an empty hat may falsely believe that this was the case. But she can be right about the existence of the rabbits, and if she were wrong that the hat was at one stage empty (of rabbits) then there would have been no trick for her to witness.

The problems with Popper's second and third arguments are of a different kind. Here it is not the key premise that is mistaken, but the argument he develops from it.

If one is justified in accepting that q only if one is justified in accepting some other proposition, r, which entails q, then either an infinite regress of would-be justifying statements is generated, or one is forced to resort to a circular 'proof', or to 'justifying' q by means of some proposition which itself is alleged not to be in need of justification. Popper does or would recognize that all of these alternatives are unpalatable.[17] But that this is so is a reason for naturalising one's epistemology, for supposing that knowledge is something like reliably caused (true) belief, not for supposing that beliefs or acceptances about matters of fact can be rational without some basis or ground in experience. After all, one's desires or hallucinations can cause one to accept or agree that something is the case. Moreover, if one zoologist says, "That's a brush-tail possum over there," and another replies, "So it is," how does either of them come to know or rationally accept that the other agrees, even about the topic of conversation, except on the basis of his or her own experience?[18] Without this basis, agreement would be capricious or arbitrary.[19] In what sense would science even be empirical, asks A.J. Ayer, if experience were, epistemically speaking, on a par with blows on the table?[20] Popper now all but acknowledges the force of such objections to this second argument, for in replying to Ayer he agrees that "our experiences are *not only* motives . . . they may even be described as inconclusive *reasons*."[21] (Emphasis mine.)

The inconclusiveness of such reasons, Popper goes on immediately to say in this reply to Ayer, derives from our fallibility, which brings us to his third argument. Now one can be a fallibilist without being a skeptic, as Ian Hinckfuss, for example, has shown.[22] It is necessarily true that if one is mistaken that q then one does not know that q. But it does not follow from the *possibility* that one is mistaken that q that one *is* mistaken that q. So one can accept the fallibilist's premise that mistakes are always possible in observation or thought without being driven to the skeptical conclusion that one does not or cannot know anything. J.L. Evans has drawn a helpful analogy here between 'know' and other achievement verbs like 'win' and 'arrive'.[23] I can win a race, for example, though it was always possible for me to have lost it; I can arrive at my destination though I might have been waylaid.

Popper's conventionalist solution to reduce perceptual knowledge to mere acceptance or agreement is also unacceptable. As Ayer points out, a robust epistemic notion like 'learning from a mistake', a notion central to Popper's methodology, amounts on this view to nothing more than deciding not to abide by some previous decision.[24]

Whatever one may think of Popper's account of perceptual knowledge, or of his defence of it, however, the important consequence of the former for his account of refutation is that, as Max Deutscher has pointed out, even if some basic statement, q, is true and entails the negation of some universal generalisation, p, if we do not know or are unable to establish that q (is true) then we do not know or are unable to establish that p is false. If basic statements were unverifiable then universal generalisations would be unfalsifiable, and the contrast between the falsifiability and the unverifiability of universal generalisations, which the problem of induction otherwise provides, would collapse.[25] Not surprisingly, Popper resists such conclusions. He says:

> The contention that the falsification of a natural law is just as impossible as its verification . . . mixes two entirely different levels of analysis . . . . On the first level, there is a logical asymmetry: one singular statement - say about the perihelion of Mercury - can formally falsify Kepler's laws; but these cannot be formally verified by any number of singular statements. The attempt to minimize this asymmetry can only lead to confusion. On another level, we may hesitate to accept any statement, even the simplest observation statement.[26]

But it is Popper who is the source of this confusion, mixing the two "levels of analysis" he is supposed to be illustrating. His use of the word 'formally' blurs this distinction. Is "formally falsifies" a first level, logical notion; or a second level, epistemological one? Certainly, if he is to make the point about a *logical* asymmetry in respect of universal generalisations then "formally falsify" and "formally verified" above mean 'entail the negation of' and 'entailed', respectively. In which case, he fails to show how the falsification of any such generalisation, unlike its verification, is secure. To falsify is not merely to formally falsify, that is, to contradict; just as to verify is clearly not merely to entail. Falsification is an *epistemological* notion, as Popper himself elsewhere acknowledges when, for example, he claims (albeit falsely), "We say that a theory is falsified only if we have *accepted* basic statements which contradict it." (Emphasis mine.)[27] Deutscher suggests that Popper merely "appears to overlook" this point above, but if that were so it is doubtful he would have felt the need to invent the expressions 'formally falsify' and 'formally verify'.[28]

Similarly, it is not to the point to claim, as Popper sometimes does:

> While coherence, or consistency, is no criterion of truth, simply because even demonstrably consistent systems may be false in fact, incoherence or inconsistency do establish falsity; so if we are lucky, we may discover the falsity of some of our theories.[29]

Popper's premises here are all unobjectionable, but his conclusion does not follow from them. To point out that a system is (logically) false does not show that some part of it is (contingently) false. The conjunction of 'X is a non-white swan' and 'All swans are white' is false, and no inspection of the world is needed to determine that this is so. But if we would "discover the falsity" of the latter conjunct, our theory, we must first know or discover from experience the truth of the former, or of some other (contingent) proposition with which it is inconsistent, and so more than logic and good fortune are needed.

Popper has remained wholly impervious to criticism such as the above. For example, in the recently published *Postscript to The Logic of Scientific Discovery* (hereinafter, *Postscript*) he says, "If we accept as true the statement 'This swan here is black' [*q*] then we are bound, by logic, to admit that we have refuted the universal theory 'All swans are white' [*p*]."[30]

But we are not bound by logic, or by anything else, to make this admission. Whilever we accept q we are, or at least can be, bound by logic to *reject* p as false. But we are not obliged to admit that we have *refuted* p for on the above evidence we have not done so.

If we could falsify a proposition, r, merely by accepting one of its contraries then we could verify r merely by accepting any proposition which entailed r, and since every proposition entails itself we would have only to accept r to verify it. (Any verificationist who thought verification so easy would get short shrift from Popper, and properly so.) However, just as we are bound to reject any universal generalisation that is inconsistent with an accepted basic statement, so we are bound to *accept* any universal generalisation that is entailed by an accepted universal generalisation, given a commitment to consistency on our part in both cases.[31] Popper falsely denies that we are *ever* (conditionally) bound to accept a universal generalisation, because he has in mind that no accepted basic statement, or conjunction of accepted basic statements, should so bind us, which is correct. That we *can* be as conditionally bound to accept some universal generalisations as to reject some, however, provides a further objection to Popper's belief that he successfully defends the asymmetry between verification and falsification in respect of universal generalisations. This is because anyone who believes, as he does, that we can refute 'All swans are white' by accepting that something is a black swan should *also* believe that we can verify this same proposition by accepting that, say, all swans and geese are white, or merely by accepting that all swans are white. The fact that, other things being equal, there is a greater risk in accepting a universal generalisation than a basic statement is beside the point here, though it may obscure it.

Popper goes on in *Postscript* to claim that science would be unworkable if *some* basic statements were not accepted.[32] This is true, but it is not, as he also claims, an argument for the asymmetry not least because science would equally be unworkable, even on his own account, if no universal generalisation was ever accepted, for example, as an *explanans* or as what he calls a "falsifying hypothesis" (see p. 12 below).

In *Postscript*, Popper continues to skirt the point which Deutscher makes above that universal generalisations can be falsified only if basic statements can be known or verified. He says:

> Another objection often raised against asymmetry is this: no falsification can be absolutely certain, owing to the fact that we can never be quite certain that the basic statements which we accept are true. . . . As to the claim that this fact refutes the asymmetry between falsification and verification, the situation is really very simple. Take a basic statement or a finite set of basic statements. It remains forever an open question whether or not the

> statements are true: if we accept them as true we may have made a mistake. But *no matter whether they are true or whether they are false,* a universal law may not be derived from them. Even if we knew for sure that they were true, a universal law could still not be derived from [verified by?] them.
>
> However, if we assume that they are true, a universal law may be falsified by them.[33]

Popper concludes:

> Thus, the logical relation between basic statements and theories [universal generalisations], and the uncertainty of basic statements, enforce [reinforce?] rather than cancel each other: *both operate against verification*; *and neither operates unilaterally against falsification*.[34]

Since basic statements entail the falsity of some universal generalisations and never the truth of such propositions, Popper is right to claim that the logical relations operate against verification of universal generalisations. The entailment does not hold. If the basic statements are uncertain, there is indeed a dual reason to suspect the argument. But to the extent that the basic statements are uncertain, the argument to the falsity of any universal generalisation will be weak. So the uncertainty of basic statements *does* operate against falsification - not unilaterally, it is agreed, but against falsification for all that. If I apply for a job and do not get it there may be some consolation in learning that every other unsuccessful applicant missed out for two reasons whereas I missed out only for one. Nonetheless, I still did not get the job *for that one reason*. Popper comes close in his conclusion above to suggesting, if he does not actually do so, however, that uncertainty in the basic statements is not a problem for falsification. Clearly, the two factors concerned do not reinforce each other where falsification is concerned, and it is tautologous that if both operate against verification then neither operates only or "unilaterally" against falsification.

Popper's concepts of knowledge and refutation, shorn of certain complications and distinguished as knowledge$_{p}$ and refutation$_{p}$, respectively, may be defined in the following manner.

Knowledge$_{p}$: A scientific community, C, *knows$_{p}$* that q, where q is either a basic statement or a universal generalisation, if, and only if,

(i) C agrees to accept that q as true.

And in case q is a universal generalisation,[35]

(ii) q is well corroborated,

and

(iii) (i) because (ii).

By 'scientific knowledge', then, Popper means something like 'professional agreement' in the case of basic statements, and 'well corroborated professional agreement' in the case of universal generalisations.[36] The latter may *sound* more impressive, but corroboration ultimately depends upon agreements about basic statements. (Corroboration would amount to confirmation by tests, if Popper's skepticism did not lead him to reject confirmation.) In any event, the important point is that all three conditions for the knowledge_p that *q* can be satisfied even if *q* is false.

John Watkins has recently sought to defend this position by pointing out that 'knowledge' also means 'branch of learning', and that *The Oxford English Dictionary* does not specify that all such learning should be true. He provides the following example, "Medical knowledge in the eighteenth century was very defective and contained much that was downright false."[37] But even if Watkins's construal of 'learning' is correct, the fact that 'knowledge' has some *other* such meaning is beside the point. It is like pointing out to a farmer who remarks that the nearest bore is in the next paddock the fact that the nearest bore is in the next room watching television. In Watkins's example, if we replace 'medical knowledge' with the usual name for this branch of learning, namely, 'medicine', this point becomes clear. Watkins goes on immediately to address the question of whether or not the growth of knowledge is cumulative. Having made his objection, he thus gives up on 'knowledge' as meaning 'branch of learning' for it is of no use to him in *this* inquiry. If someone were to ask whether or not the growth of some branch of learning was cumulative, we should first want to know what sort of growth this person had in mind. Was he or she referring, for example, to the number or size of research institutes or schools, the number of scientists or other staff they employed, or all of these things? Clearly, this is *not* the sort of 'knowledge' Watkins had in when he framed his question. His concern lies with epistemology, not with sociology or history.

$\textit{Refutation}_p$ or $\textit{Falsification}_p$.: In principle, *p* is $\textit{refuted}_p$, where *p* is, for simplicity, a universal generalisation,[38] if, and only if,

(i) C knows_p that *q* , where, strictly speaking, *q* is a well corroborated "low level, empirical hypothesis" - a "falsifying hypothesis - and not merely the conjunction of "a few stray basic statements",[39]

and

(ii) *~p* is validly inferred from *q* , by some member(s) of *C*.

It may be that Popper would agree that a proof that *q* entails *~p* is also required. In any event, he is not consistent as to what is either necessary or sufficient for refutation$_p$, often implying less than even these conditions demand, as we shall see. Also, I have mentioned only universal generalisations above. However, Popper's basic statements - singular existential statements - are also falsifiable. But they are not, as he believes, falsifiable by other such statements, which his demarcation criterion requires.[40] For example, to falsify the singular existential statement,

(1) There is a pointer in motion at *k*,

where *k* is some space-time region, one needs to assert or imply that

(2) There is no pointer in motion at *k*.

Popper calls (2) "a singular non-existence statement".[41] He excludes such statements from science, even though they are falsifiable, because they are consistent with any universal generalisation. He holds the curious view that it is necessary for any observation-level proposition in science to be such that a universal generalisation can be falsified by means of it.[42] This is an arbitrary restriction on scientific propositions, and one that is his undoing, for one consequence is that no basic statement is inconsistent with any other, as David Stove has pointed out.[43] Popper tries to get around this problem, or would appear to be doing so, by claiming that the singular existential statement,

(3) There is a pointer at rest at *k*,

is logically equivalent to the conjunction of (2) and the singular existential statement,

(4) There is a pointer at *k*.[44]

But (3) entails the conjunction of (2) and (4) only if 'k' is so defined that *k* cannot contain any other pointer, in which case (3) is *not* a singular existential statement, that is, a statement which merely asserts the existence of something, for it is logically equivalent to,

(5) There is a pointer at rest (and no other pointer) at *k*.

For our purposes, however, the important point is that all of the above conditions for refutation$_p$ can be satisfied, even if *p* is true. In short, if we can do no more than to *corroborate and accept* the premises of any so-called falsifying argument or refutation then we can do no more than to *discorroborate and reject* the negation of its conclusion.[45] So by 'refute' or 'falsify' Popperians ought to mean, and sometimes do mean, 'discorroborate and reject'.

It would be quite a surprise if Popper ever said, "A theory can be both true and falsified." But were he to do so he would not, on falsification$_p$, be contradicting himself. In defence of falsification$_p$, Alan Musgrave is one of at most a few Popperians who notices or is prepared to acknowledge this meaning shift, and having done so accepts that he is obliged to defend it. He says:

> Arguments to the falsity of particular scientific hypotheses proceed from fallible premises. Now since an argument from fallible premises is not a conclusive proof a falsifying argument is not a conclusive disproof. In other words, 'falsify' cannot mean 'prove to be false'.
>
> Partisans of ordinary English usage will, of course, shudder at this. The best reply to them is that most interesting ideas violate ordinary English usage.[46]

Since meanings are conventions, 'falsify' *can* mean 'prove to be false'. It is irrelevant whether or not fallibilism is true. Curiously, what Musgrave thinks 'falsify' *does* mean he does not say. As with Watkins above he confuses what a word means with what, given that meaning, it can be used to help assert. His *problem*, however, is that if he were to hold that

(1) There are no contingent proofs, including disproofs,

and

(2) 'Falsify' means 'prove to be false',

he would be forced to conclude that

(3) There are no contingent falsifications.

So (2) is rejected not because those ideas of Popper's to which he is referring are interesting, but to avoid accepting (3). The selective use of 'conclusive' by Musgrave meanwhile suggests that a falsification$_p$ is at least *some* sort of disproof. Also, suppose that (2) were replaced by

(2)* 'Verify' means 'prove to be true',

and (3) by

(3)* There are no contingent verifications.

Would there be any qualms about accepting *this* argument? Of course not. This is one way in which a purely rhetorical advantage can be secured for Popper's account of falsification.

Finally, it should be noted that Popper often uses 'refute' and its cognates with various, often conveniently weaker or deviant meanings, and he is by no means alone in doing so.[47] Consider the following sentences, which occur cheek by jowl in Popper:

(1) We can then say that the theory rules out certain possible occurrences, and that it will be falsified if those possible occurrences do in fact occur,[48]

and

(2) We shall take it [the theory] as falsified only if we discover a *reproducible effect* which refutes the theory.[49]

In (1), 'falsified' means no more than 'false'. In (2), 'refutes' means no more than 'is inconsistent with'. In a similar vein, Popper talks of "refuting instances" when he means only 'counter-instances' or 'logically possible counter-instances'. An unknown counter-instance is not (yet) a refuting instance.

1.3 Popper's Rhetoric: Knowledge as Conjecture, Refutation as Discorroboration

How much less powerful would Popper's methodology have seemed, how much less interest or attention would it have attracted, if in its exposition 'knowledge' and its cognates had been replaced by 'well corroborated belief' and its cognates whenever the latter ought to have been used instead of the former? More significantly, suppose that 'refutation' and its cognates had likewise been replaced by, put simply, 'discorroboration' and its cognates whenever, once again, the latter ought to have been used instead of the former. While these meaning shifts remain obscure, however, the door is open to equivocation, and to the making of inflated or otherwise misleading or false claims. Thus Popper, and others, choose or fall in with a construal of 'knowledge' either as 'knowledge' or as 'well corroborated belief' or even 'conjecture', and with a construal of 'refutation' as 'disproof' or as 'discorroboration', to suit the circumstance. Consider the following range of examples.

Take, firstly, 'discovery'. Having said that he accepts the view that "we should call a state of affairs 'real' if, and only if, the statement describing it is true",[50] Popper remarks:

> Since I believe that science can make real discoveries I take my stand with Galileo against instrumentalism. *I admit that our discoveries are conjectural.* But this is even true of geographical explorations. Columbus's conjectures as to what he had discovered were in fact mistaken; and Peary could only conjecture - on the basis of theories - that he had reached the Pole. But these elements of conjecture do not make their discoveries less real, or less significant.[51] (Emphasis mine.)

In the passage above, for every sentence which expresses a proposition about discovery, *except* the one emphasised, 'the discovery that' or 'discovers that' entails 'it is true that'. Having thus framed his account with the concept of discovery, properly so called, Popper introduces his novel concept of discoveries as conjectures, suggesting as he does so (with the expression "I admit that") that the supposedly conjectural character of discoveries is really just a common or well known fact about them, something which he is prepared to acknowledge or concede.

Given that some conjectures are false, it is not surprising that, having conflated these two contrary notions of discovery, Popper should then fail to distinguish a false discovery claim from a discovery. If Columbus conjectured that he had discovered a western sea route to India, for example, but was "in fact mistaken", then he did not *discover* any such route. Perhaps Popper is misled here by a belief that some *other* discovery claims concerning this expedition were not false. The case of Peary, on the other hand, is at most a quibble about margins of error, and one which, as it happens, did not impress Peary.[52] Whatever force this case may seem to have derives largely from the failure to construe 'the Pole' as referring not to a certain *geometrical point* (as it often does) but rather to a certain *geographical region*, for it is the latter which an explorer would claim to have reached or discovered.

Turning to refutation, shortly following his remarks about meaning cited above (p. 14), Musgrave asks, "What is it to regard a theory as falsified? The answer is, I fear, very simple: one regards the theory as false."[53] This question of Musgrave's is so framed that his answer to it is not only *consistent* with the theory concerned being true, as Popper's epistemology requires, but also correct (or at least partly so) when 'falsified' is taken to mean, as it typically will be, 'proven to be false'. In this way, Popper's account can be made to seem plausible. But if Musgrave were to address some such question as, "According to Popper, what is it for a theory to have been falsified?" his answer, which should be no different, would reveal the comparative weakness of falsification$_p$. It would be clear that, as Musgrave sees it, a falsified theory is merely one that is *regarded* as false.

Popperians typically exploit or succumb to the notion of disproof, as they do with knowledge, because it promises a stronger and more plausible theory of method. So Popper says that "the method of trial and error is a *method of eliminating false theories*", omitting to point out that, on his account of trial and error, true hypotheses may also be eliminated, for true hypotheses can be discorroborated and rejected.[54] And Musgrave goes on to assert that by 1900 certain observations in astronomy had "*shown that something was wrong* with Newton's theory [of gravitation] - the theory had been falsified". (Emphasis mine.)[55] Clearly, a Popper-English companion dictionary is required, as the following passage from Popper further illustrates:

> If we test our conjecture, and succeed in falsifying it, we *see very clearly* that there was a reality - something with which it could clash.
>
> Our falsifications thus *indicate the points* where we have *touched* reality, as it were.[56] (Emphasis mine.)

Moreover, just prior to the above passage, Popper remarks that a false conjecture is one which "contradicts some real state of affairs",[57] contradicting his own view that logical relations obtain only between statements or propositions. The function of this deviant use of 'contradicts' is to draw attention away from that which such a false conjecture does contradict, namely, some true statement describing that state of affairs. And it is just such true statements, or rather our knowledge of them, which would "indicate the points where we have touched reality".

Equivocation enables people to adopt different stances on the same issue. Take, for example, the testing of theories. Ayer once asked, drawing attention to Popper's (unacknowledged) commitment to induction, "Why should a hypothesis which has failed the test be discarded unless this shows it to be unreliable; that is, except on the assumption that having failed once it is likely to fail again?"[58] Popper retorts, "Answer: because if it has failed the test, it is false," temporarily suspending his skepticism about empirical procedures and invoking the notion of disproof.[59]

On the other hand, when disproof is specifically attacked Popper retreats to a covert defence of discorroboration, or something even weaker still. Some philosophers, notably Pierre Duhem[60] and W.V. Quine,[61] have argued that individual theories do not fail any test since only (complex) theoretical systems can be tested, and so only such systems fail. Popper has replied chiefly by arguing at cross purposes with them, asserting what they do not or would not deny. What is "inexplicable" on their account, he suggests, is the fact that "we *are* reasonably successful . . . in attributing our refutations to definite portions of the theoretical maze".[62] But what is to count as a successful attribution? It may be nothing more, apparently, than "sheer guesswork" or an "inkling" as to "what has gone wrong with our system".[63] But Duhem and Quine were not denying that we can *guess* that some such portions are false or that such guesses may be consistent with whatever else we accept or confirm, so there is nothing for them to explain. They were pointing out that, amongst other things, a successful disproof of any portion of such a system is dependent upon knowing that the remainder is true, and *this* point is not in dispute.

Inconsistent skepticism, however, is a two edged sword. If presuppositions of knowledge or disproof can be used to provide a veneer of plausibility for Popper's account, they can equally be turned to the opposite effect at the hands of any similarly inconsistent but hostile skeptic. Thus Alan Chalmers, for example, concludes that no scientific theory can be falsified. His various arguments for this conclusion are self defeating, however, because they presuppose that there are cognitive achievements of just the above kinds, but these his conclusion denies.

Chalmers describes an episode in the history of astronomy in the following manner:

> Tycho Brahe claimed to have refuted the Copernician theory a few decades after the first publication of that theory. If the earth orbits the sun, Brahe argued, then the direction in which a fixed star is observed from earth should vary during the course of the year as the earth moves from one side of the sun to the other. But when Brahe tried to detect this predicted parallax with his instruments, which were the most accurate and sensitive ones in existence at the time, he failed. This led Brahe to conclude that the Copernican theory was false. With hindsight, it can be appreciated that it was not the Copernican theory that was responsible for the faulty prediction, but one of Brahe's auxiliary assumptions. Brahe's estimate of the distance of the fixed stars was many times too small. When his estimate is replaced by a more realistic one, the predicted parallax turns out to be too small to be detectable by Brahe's instruments.[64]

Chalmers claims that this account illustrates that no theory can be "conclusively falsified" - for the Duhem-Quine reason that "the possibility that some part of the complex test situation other than the theory under test is responsible for an erroneous prediction [such as Brahe's] cannot be ruled out."[65] But his account, if correct, does not illustrate that there are propositions of some privileged kind, the falsity of which, if they are false, cannot be discovered. Rather, it illustrates that we sometimes *do* discover which proposition in "the complex test situation" is false. Moreover, his account provides no reason for supposing that if this false proposition were a major theory, rather than some auxiliary assumption, we should be unable to discover its falsity.

A further reason for believing that theories cannot be falsified, Chalmers claims, is that "the observation statements that form the basis for the falsification may themselves *prove to be false* in the light of later developments".[66] (Emphasis mine.) This objection, once again, presupposes that there are falsifications of just the sort he objects to. He again turns to the Copernican theory in an attempt to illustrate his claim:

> Knowledge available at the time of Copernicus did not permit a legitimate criticism of the observation that the apparent sizes of Mars and Venus remain roughly constant, so that Copernicus's theory, taken literally, could be deemed falsified by that observation. One hundred years later, the falsification could be revoked because of new developments in optics.[67]

Since an observation statement is proved to be false only if there is some *other* observation statement that is known to be true, the latter observation statement is not one that will itself be falsified by any later development as it is true. So his objection collapses. The self-defeating nature of Chalmers's argument is partially obscured by his use of 'revoked', which is a proxy for 'falsified', and by his use of 'deemed', which performs a similar rhetorical function above to that of 'regard' in Musgrave's account (see p. 16 above).

'Falsifiability' is likewise not univocal for Popper. He says, for example, that "falsifiability is untouched by the problems that may affect empirical falsifications". It is a "purely *logical* criterion".[68] But this is to attribute another and weaker meaning to 'falsifiability' from its usual or conventional meaning of 'able to be falsified' or 'may be subject to falsification'. If, say, the fallibility of contingent knowledge were a problem for any alleged falsification of q, for example, then that would be a problem for the alleged falsifiability of q as well. If falsifiability were *purely* a logical matter, a falsifiability criterion would be merely an inconsistency criterion. And every proposition would satisfy *this* criterion since every proposition is inconsistent with some proposition. If q is falsifiable this is not merely because it is inconsistent with some proposition, but because at least one such proposition is or would be *knowable*. If there are "problems" with knowing any such proposition then these are problems for the falsifiability of q. So Popper does not stick to his novel meaning of 'falsifiability' - any more than he sticks to his novel meaning of 'falsification' - for he does not believe that the falsifiability criterion is merely an inconsistency criterion. If he did believe this then universal generalisations would be potential falsifiers no less than basic statements.

Consider, for example, Popper's treatment of existential generalisations, what he calls "strictly existential statements", for example, 'A fire-breathing animal exists'. Popper correctly asserts that no such proposition is falsifiable, for the reason that "we cannot search the whole world in order to establish that something does not exist, has never existed, and will never exist."[69] In other words, we cannot *verify* the antecedent of any counter-argument for an existential generalisation because that antecedent will contain a universal generalisation - in the above case, 'There is no fire-breathing animal'. But a universal generalisation can be well corroborated and accepted, so existential generalisations can certainly be *falsified*$_p$. I take it, for example, that zoologists generally believe, and with good reason, that there are no fire-breathing animals. Thus they can falsify$_p$ 'A fire-breathing animal exists', and would proceed to do so should they take seriously any medievalist going about the place asserting the existence of such creatures. Since Popper has made quite an issue of the unfalsifiability of existential generalisations, [70] this is an important case of his reliance on the notions he would reject, namely, falsifiability as disprovability and falsification as disproof.

1.4 Stove on Rhetoric, Refutation, and Popper

In Part One of his book *Popper and After: Four Modern Irrationalists*, David Stove reaches some general conclusions about Popper's accounts of knowledge and refutation, and about the place of rhetoric in its presentation, which are similar to my own.[71] Stove's analysis, however, is defective in at least two significant respects, the first of which is his analysis of language and rhetoric, the second is his analysis of refutation, and both have implications for his account of Popper.[72] I shall begin with the former.

Stove claims to have discovered two so-called literary devices in Popper's writings, as well as in those of Imre Lakatos, Thomas Kuhn, and Paul Feyerabend. The first of these devices Stove calls "neutralising a [cognitive] success word",[73] and the second "sabotaging a logical expression".[74] What Stove is attempting to explain, however, is the operation of a certain *rhetorical* or *sophistical device* in their *arguments*. The *means* for this device can be supplied by, but they are not identical with, such *literary devices* or *conceits* as he does indeed (sometimes) identify in their *sentences*. By failing to draw these distinctions, Stove's account leads to such absurd consequences as that many well formed sentences, such as 'Most logicians would suppose that *P* entails *Q*', are intrinsically misleading or deceptive.

Let us begin with the device of neutralisation. Consider the following two sentences:

(1) We know that the pointer reads zero,

and

(2) We 'know' that the pointer reads zero.

(2) is vague. It might mean:

(3) We believe or agree that the pointer reads zero,

or

(4) We think that we know that the pointer reads zero,

or

(5) So far as we can tell, the pointer reads zero.

And so on. The effect of enclosing the cognitive success word 'know' in quotation marks in (2) is to deplete or neutralise some or all of the cognitive success meaning of that word, and, in turn, of (2). But there is more, for (2) lends itself to equivocation between (1) and (3), (1) and (4), (1) and (5), and so on. Sentences of the form of (2) are thus attractive to those who are intent upon providing a plausible account of our knowledge of pointer readings but whose skepticism is such that they can consistently assert only such propositions as are expressed by sentences of the forms of (3), (4), (5), and the like. And as Stove drily observes, Lakatos "raises storms" of such quotation marks to this end, whilst Feyerabend just "keeps up a steady drizzle" of them.[75] Popper, on the other hand, rarely bothers with this device when he uses a word with a deviant meaning, as we have seen. (Stove calls Popper's practice of not using such marks, "bald neutralising".)[76]

One can make use of a sentence like (2), however, without committing any fallacy of equivocation. (Stove notices this at one point but overlooks its implications for his analysis.)[77] Consider the following two sentences, which are analogous to (2):

(6) The blacks were granted certain 'freedoms',

and

(7) Marxist writers 'explain' Darwin as though he were some simple, mechanical toy.

Clearly, the author of (6) can mean, and be understood to mean, that the so-called freedoms the blacks were granted were not genuine. The author of (7) is David Stove.[78] By 'explain' he most likely means 'explain away'. But even if there is some vagueness about (7), as there is about (2), there is no equivocation; it is clear that Stove rejects such explanations.

A device like neutralisation is a means of doing something *to* a sentence like (1), namely, of weakening its meaning. On the other hand, a rhetorical or sophistical device, of the kind for which such neutralisation may be useful, is a means of doing something *with* a sentence, like (2), namely, of persuading or tending to persuade others to believe or accept some proposition by irrational means, in this case, by equivocation. It is sophistry if the author *intends* to so persuade others.[79] The sophistical device to which Stove alludes is therefore that of *insinuation*. Sentences of the form of (2) allow the half-hearted skeptic to insinuate what he or she cannot consistently assert, namely, that there can be knowledge, or something resembling knowledge, of pointer readings.

Stove firmly believes that all four authors in his sights are sophists. Whether or not this is so, however, is not easy to decide from their philosophical writings alone. In any case, the question is chiefly one of biographical interest for we can assess the merits of their respective arguments, and determine the role that rhetoric plays in making them persuasive, without holding any opinion about whether the various fallacies committed by these authors are deliberate or not.

If we turn to Stove's account of his second device, sabotaging a logical expression, the consequences of conflating this distinction between a literary device and a rhetorical device - a conflation signalled by his choice of the pejorative term 'sabotage' - are apparent. Stove considers the following two *statements*:

(A) *P* entails *Q*,

and

(B) *P* entails *Q*, according to most logicians, ancient, medieval, and modern.[80]

He (correctly) classifies (A) as a logical statement because it implies something about a logical relation, in particular, that *P* entails *Q*. (B), however, is classified as "ghost-logical" by him for whilst it does not imply anything about any logical relation it "makes the strongest possible suggestion" of doing so, in particular, of also implying that *P* entails *Q*.[81] The "sabotage of a logical expression", Stove asserts, "is a literary device for appearing to make a logical statement, without actually doing so".[82]

What Stove has uncovered here, is *another* means of insinuation. In stating (B), someone can be insinuating that *P* entails *Q* , and it is thus reasonable to describe that person as having sabotaged a logical relation. The *literary* device upon which such sophistry depends, however, consists merely in attaching the weak cognitive attitude modifier 'according to most logicians, ancient, medieval, and modern'[83] to the *sentence*,

(8) *P* entails *Q*,

to form the sentence,

(9) *P* entails *Q*, according to most logicians, ancient, medieval, and modern.

Now no logical *word* or *expression* is "sabotaged" in (9), for 'entails' has the same meaning in both (8) and (9). (Likewise, 'falsified' retains its ordinary meaning when Musgrave or Chalmers employs the weak cognitive attitude modifier 'regard' or 'deemed' to a similar end, as we examined earlier - pp. 16 and 19, respectively). In addition, Stove talks of words having *implications* (in addition to meanings),[84] and accepting this falsehood would only encourage one to believe that a logical *word* is somehow sabotaged above.

The failure to distinguish the literary device of employing a weak cognitive attitude modifier from the sophistry of insinuation, for which this device can be useful, leads Stove to overlook two points. Firstly, such modifiers can equally be used with cognitive success words, as indeed logical words can equally be neutralised with quotation marks.[85]

Secondly, and more importantly, he overlooks that if in uttering (9) I am claiming that a certain belief remains common amongst logicians, I need *not* also be hinting or insinuating that this belief is true.

Stove next addresses a third statement:

(C) Some people think that P entails Q.[86]

(C) is like (B) - and unlike (A) - in not being a logical statement. Conversely, according to Stove, (C) is unlike (B) - and like (A) - in not being a ghost-logical statement. (C) is a "plain historical" statement, he says, one that "does not *pretend* to be anything else".[87] (Emphasis mine.) Plainly, however, (B) is an historical statement, no less than (C).[88] Stove presumably thinks otherwise, and believes that only (B) is ghost-logical, because someone would be much more likely, other things being equal, to draw the conclusion that P entails Q from (B) rather than from (C). This is because some suppressed premises are more plausible than others. It is more plausible to suppose that "most logicians" will be right about the logical relation between P and Q than will "some people". Nonetheless, a radical logician or a muddle-headed logic student would not be insinuating that P entails Q, for example, if that person, having stated (B), immediately added,

(D) But all these logicians are wrong; P does not entail Q.

Equally, someone stating (C) can add,

(E) But there is almost nothing that some people have not once thought was true.

Such a person can be thereby hinting or insinuating that P does *not* entail Q, which would also be (if it were stated) a logical statement.

The key point here is that such things as suggesting, hinting, pretending or insinuating are properties of *acts*, not of *statements*. This point is partially obscured by the fact that, in ordinary English, 'statement' means not only 'that which is stated' but also 'act of stating', and the same goes for 'suggestion', 'insinuation', and the like. Replace 'statement' with 'proposition' in Stove's analysis, however, and the effect of this influence is plain.

The second defective aspect of Stove's account that is of relevance here is his analysis of refutation. At first, he indicates that he would subscribe to a view of refutation that is at least very similar to my own. He says that to describe a Government Minister as having refuted some allegations that he had misled Parliament is to "ascribe to him a certain cognitive achievement: that of showing the allegations to be false".[89] From this point on, however, there are problems with his account, and it is as well to begin by looking more closely at 'refutation'.

Continuing with Stove's symbols to fit in with his text, recall that I have proposed that some proposition, *Q*, is refuted if, and only if, someone, *S*, shows that there is some other proposition, *P*, that *S* knows such that *P* entails *~Q*. 'Refute' and its cognates are thus complex epistemic words. They are, at least:

(i) *truth-value* words, since necessarily if *S* refutes *Q*, *Q* is *false*,

(ii) *logical* words, since necessarily if *S* refutes *Q*, *S* knows some *P* such that *P* *entails* *~Q*.

and

(iii) *cognitive success* words, since necessarily if *S* refutes *Q*, *S* *knows* that *Q* is false.

There are various other cognitive achievements involved in constructing a proof. In this case, for example, *S* needs to validly infer *~Q* from *P*. But more is needed, for *S* may have merely guessed correctly that this inference is valid. *S* would need to show that *~Q* can be arrived at by a series of recognizably valid inferences from *P*. Furthermore, in science or in other social practices, *S* would need to substantiate the knowledge claim that *P*, or at least to know that this can be done, in order to rule out cases where *S* has forgotten how he or she came by this knowledge. (There are also some institutional requirements on refutation in such practices, for example, a refutation would need to find expression in some institutionalised act, such as giving a seminar or publishing a paper.)

To return to Stove, he does not mention what I have called truth-value words, by which I mean words with such truth-value meanings as 'truth', 'fact', 'probability', and 'falsehood' possess. 'Refuted' is such a word, as I have suggested, because '*Q* is refuted' means, in part, '*Q* is false'. When Stove encounters 'truth'[90] and 'fact',[91] however, he misclassifies them as cognitive success words. But there can be truths or facts of which we are all ignorant. The success word in 'I saw the truth at last' is 'saw' not 'truth'. The proposition that *Q* is false does not imply anything about the cognitive attitude of anyone towards *Q*.

Secondly, Stove seems to believe that 'refute' and its cognates are sometimes not even logical words.[92] To this end, he contrasts the statement,

(F) *Q* is refuted,

with

(G) *Q* is refuted by *P*.

'Refuted', he says, is a logical word when it is used to state (G), but not when it is used to state (F). It would be wrong, however, to suppose that (F) does not imply that there is an entailment at hand between the negation of Q and *some* known proposition (such as P) merely because no reference is made to any such proposition in (F). Consider an analogous case: 'amused' does not change its meaning from

(10) I was amused,

to

(11) I was amused by the allegations against the Minister,

merely because *both* terms in the dyadic relation 'was amused by' are spelt out in (11) but not in (10). If I assert only that I was amused I nonetheless imply that *something* amused me.

Thirdly, having at first recognized that 'refute' is a cognitive success word, Stove switches, for no apparent reason, to classifying it and its cognates exclusively as failure words.[93] Now it is true that, for example, the Minister's refutation will expose some measure of cognitive failure on the part of anyone who happens to believe those allegations, since they are false. But the refutation itself is clearly not a cognitive failure, nor is it in any way dependant upon such failure for the Minister can refute the allegations even if no one believes them.

A further error in Stove's analysis is one we have already encountered in Popper's. Both philosophers write not only of *someone* refuting or falsifying a proposition, but of one *proposition* refuting or falsifying another.[94] There would be no harm in this practice if the latter expression were merely shorthand for the former and was understood as such by all concerned. Overlook the point, however, that propositions, unlike persons, do not have cognitive attitudes or draw inferences, which the latter expression makes much more likely, and we risk confining our attention to only those properties which propositions have, namely truth values and logical relations, in constructing our view of refutation. Thus it can appear to be sufficient that Q is refuted merely if for some proposition, P ,

P is true,

and

$P \dashrightarrow \sim Q$

Let us call this view of refutation, 'refutation$_S$' or 'falsification$_S$'. Thus Stove writes, "*P*'s falsifying Q cannot itself . . . cause abandonment of belief in Q".[95] But Q is falsified only if at least one person *has* abandoned the belief that Q. Refutation$_S$ is an otiose notion for, necessarily, if Q is false then Q is falsified$_S$ since it is a logical truth that for any false proposition, Q , there is some true proposition, $\sim Q$, which entails $\sim Q$. Moreover, since on any coherent view of falsification, Q is falsified only if Q is falsifiable it follows that a proposition cannot be both false and unfalsifiable$_S$. All existential generalisations, however, are unfalsifiable, in the normal sense of that word, yet some of them will be false. Presumably, for example, 'A fire breathing animal exists' is false.

With the above points in mind, consider the following argument which opens Stove's account of the sabotaging of logical expressions, in particular, of 'refutation'. He begins by pointing out that the logical relation of inconsistency is like a "frictionless pipe" along which knowledge can "travel without loss". So that if P is inconsistent with Q then someone who admits your claim to know that P cannot consistently deny your claim to know that Q is false".[96] (This is not right, for you may not know, but have merely guessed, that P is inconsistent with Q.) Stove continues:

> Suppose I first say that Q has been refuted, and then say that it is the truth of P which refutes Q, or is the refutation (or a refutation) of Q. In the first remark "refuted" is a failure-word, implying the falsity of Q; in the second, the cognate words are logical ones, since in virtue of them my remark implies that P is inconsistent with Q. But now suppose that in my first remark I was neutralising the failure-word, taking out its implication of falsity. Then, to be consistent, I clearly must do something, to the logical expression in my second remark, which is like neutralising a failure-word. Having admitted the truth of P, I will not be able to continue, as I began, avoiding the implication that Q is false, if I allow my logical statement to retain the implication that P is inconsistent with Q.[97]

He concludes that a writer "who often took the implication of falsity out of 'refuted' but never took the implication of *inconsistency* out of 'refutation', would be in a position hopelessly exposed to criticism. Our authors have not been so careless."[98]

This conclusion is false, as I have shown in 1.3 above. If I deny that

$$(S \text{ knows that } P) \dashrightarrow P$$

then I can deny that

$$(S \text{ refutes } Q) \dashrightarrow {\sim}Q$$

whilst continuing to assert that

$$P \dashrightarrow {\sim}Q$$

without contradicting myself. In general, this is Popper's approach, as we have seen.

Why then does Stove get this wrong? How can he later quote Popper's remark that "a few basic statements contradicting a theory will scarcely induce us to reject it as falsified" as an example of sabotaging the logical relation of inconsistency?[99] I suggest the following explanation. Stove begins above by setting out something approaching an adequate view of refutation, even though he misclassifies 'refute' as a failure word in his "first remark". His conclusion, however, is squarely based on refutation$_S$, a view which surfaces in his "second remark" and takes hold. Now since 'refutation', in the sense of 'refutation$_S$', is not even a cognitive attitude word, the saboteur's most promising line of attack is lost. A second line of attack would be on 'refutation' *qua* truth-value word. The saboteur could write, "It is the 'truth' of P which refutes Q." Stove elsewhere remarks, however, that 'truth' is such that it "does not lend itself to being neutralised", so presumably he does not consider this possibility here.[100] He is thus led to the false conclusion that the saboteur's only option is to attack 'refutation' *qua* logical word.

Stove is further encouraged to form this general conclusion because Popper's account of the refutation of a certain class of proposition, namely, unrestricted probability statements, does provide him with evidence for it. As Stove points out,[101] Popper's bind in respect of such statements is that they are not inconsistent with any basic statement, yet it would be implausible to exclude them from science, as his demarcation criterion would otherwise demand. So Popper claims that by adopting a methodological rule or convention - a privilege he resolutely withholds from the verificationist, who is in a similar bind here - unrestricted probability statements can be made falsifiable or regarded as falsified by the relevant basic statements. And this practice *is* one of suggesting or insinuating that a logical relation exists where none does.[102] Stove describes it as Popper's "most influential" act of sabotage, however, and this is not correct. (Even Stove does not believe the relevant passages in Popper have been widely read or discussed.)[103] Clearly, it is Popper's account of the refutation of universal generalisations which people typically have in mind when discussing his work, and he sabotages such refutations by removing the implication of cognitive success *not* the implication of inconsistency, as we have seen. Indeed, it is by *stressing* the latter implication that Popper has diverted much attention both from the unpalatable consequences of his skepticism about such cognitive success and from the sabotage it leads him to commit by equivocating between refutation as disproof and refutation as discorroboration.

1.5 Strong Popperian Falsificationism (SPF)

The easier refutation or falsification seems, or is made to seem, the stronger any supposedly falsificationist methodology will seem.

As we have seen in 1.2 above, one means by which Popper makes refutation seem easy is by relaxing the conditions he demands for knowledge. A second means of achieving this end, at least in respect of certain refutations, is to deny, gloss over, or otherwise ignore some of the premises required to construct such refutations, or to take it for granted that the required premises are known or at least are well corroborated. In this section, and the next, I discuss this aspect of his falsificationism.

The sort of refutation Popper deals with in this fashion is that which is central to his methodology, namely, the refutation of universal generalisations or scientific theories, especially important or fundamental scientific theories. And the set of premises he rejects or sweeps under the carpet is what I shall call the set of auxiliary propositions. This set is required, in the first instance, to deduce any existentially affirmative prediction from a universal generalisation and, in turn, to construct a deductively valid counter-argument for that generalisation from the negation of the prediction so deduced.

The logical form of such a counter-argument, a statement of which is not easily come by in Popper,[104] can be set out as follows:

T	:	universal generalisation, or theory;
A	:	set of auxiliary propositions, typically consisting of statements of initial conditions and boundary conditions, and perhaps other universal generalisations;
P	:	prediction;

and

O	:	observation statement, made to test or determine the truth value of P.

Now, if

(1) $(T \,\&\, A) \dashrightarrow P$

(2) O

and

(3) $O \dashrightarrow {\sim}P$

then

(4) ${\sim}P$ (2), (3); *Modus Ponens.*

and

(5) ${\sim}(T \,\&\, A)$ (1), (4); *Modus Tollens.*

So if

(6) A

then

(7) ${\sim}T$ (5); De Morgan's Law. (6); Disjunctive Syllogism

In short, the counter-argument for T is

$$(\sim P \;\&\; A) \dashrightarrow \sim T$$

or

$$(O \;\&\; A) \dashrightarrow \sim T$$

Popper espouses two false doctrines concerning the role of the auxiliary propositions, the acceptance of either of which makes the refutation of scientific theories look easy. The first of these doctrines I call Strong Popperian Falsificationism (SPF); the second, related doctrine I call Weak Popperian Falsificationism (WPF). WPF is discussed in 1.6 below.

SPF is the false doctrine that if P is refuted then T is refuted. It is yet another means by which Popper would sidestep the Duhem-Quine problem. SPF can be found not only in his early writings;[105] the following is a relatively recent and characteristic remark by him which illustrates his continuing commitment to SPF:

> Einstein was looking for crucial experiments whose agreement with his predictions would by no means establish his theory; while a disagreement, as he was the first to stress, would show his theory to be untenable.[106]

When circumstances dictate otherwise, however, it is equally characteristic of Popper to deny SPF. Thus, in a subsequent discussion of one of the very experiments he refers to above, he affirms and then denies SPF in saying:

> There were certain possible results [of Eddington's eclipse experiment] which, it was agreed in advance, would refute Einstein's theory - for example, a result indicating a zero deflection of the light rays. But there were, no doubt, also results which would have led first to a scrutiny of the experimental arrangement, and perhaps even to the overthrow of some of the more speculative auxiliary hypotheses.[107]

Popper's sporadic assertions of SPF often go unnoticed or unmentioned though some philosophers, notably J.O. Wisdom, make a point of denying that he displays any inconsistency of the kind in evidence above.[108] Alternatively, some, like Musgrave, do notice one or other instance of SPF in Popper but treat it as a trivial oversight or aberration on his part.[109] As one would expect, neither Wisdom nor Musgrave endorses SPF, since once the doctrine is exposed to scrutiny it is obvious that it is false. But SPF can be disentangled from the confused responses of some of Popper's critics. Kuhn, for example, claims that scientists do not "treat anomalies as counter-instances" of their theories, even though "in the vocabulary of philosophy of science that is what they are".[110] Now scientists would be correct in not treating an anomaly as a counter-instance. An anomaly is a discrepancy between P and O, and such a discrepancy can arise from an error in O, A, or in the inference from $T \& A$ to P, as it can from having found some counter-instance of T. Kuhn is simply wrong about the semantics of the philosophy of science, even if there remains a deal of loose, and consequently misleading, talk about counter-instances.[111]

Following Kuhn's lead, Lakatos extravagantly claims that all theories are beset with "undigested anomalies" at every stage of their scientific career, concluding that "all theories, in this sense, are born refuted and die refuted".[112] Since Lakatos employs the modifier 'in this sense', we should probably interpret his claim not as an expression of SPF, a doctrine he elsewhere rejects,[113] but as one in which 'refuted' is used in some deviant sense, probably best translated as 'at least weakly discorroborated'. But it is not hard to see how someone who engages in such equivocation is likely to give at least a passable impression of believing SPF. Thus, W.H. Newton-Smith, for example, clearly indicates in his critique of Popper in *The Rationality of Science* that he rejects SPF. He remarks, for example, that "the fact that we recognize the need to ignore anomalies where some extraneous factors have intervened makes theories particularly resistant to easy falsification".[114] Yet almost in the next breath, he bluntly announces, "All theories are born falsified." So much, one might think, for the difficulties of falsification. But Newton-Smith is merely engaged in equivocation, for his next sentence reads, "No theory has ever been totally free of anomalies."[115]

John Watkins, on the other hand, rejects the 'born refuted' thesis. Nonetheless, he too is similarly confused. He says:

> Taken at its face value, this thesis, if correct, would wreck our theory [of method] according to which it is more rational to adopt that theory, where there is one, that is better corroborated than its rivals, it being a necessary condition for its being better corroborated than them that it be unrefuted.[116]

Taken at face value, however, the 'born refuted' thesis is a fatuous claim. Clearly, all theories are not born *refuted* - some theories are true, and some false theories emerge before the conditions are ripe for their disproof. But even for those like Watkins for whom 'refuted' officially means 'well discorroborated', and does so above (if it does not mean 'disproved'), the corresponding thesis needs only to be understood to be dismissed. Why on earth should anyone believe that there is some well corroborated counter-argument which greets, or which can be immediately assembled for, *every* theory that is formulated? Watkins misses this point because he too equivocates. He acknowledges Lakatos's implicit definition of 'refuted' as 'weakly discorroborated' for immediately after the passage quoted above he notes that for Lakatos a theory is refuted merely when there is some "evidence conflicting with it".[117] But he does not contest this use of 'refuted', and slides from talk of conflicting evidence to that of "refuting evidence".[118] What Watkins *does* contest, and at some length, is the view that all or even most scientific theories are confronted with some counter-evidence at birth.[119] This may not be a worthless exercise, but it is not one that is relevant to the concern he voices above for his theory of method. For even if it were true that there has been no theory for which some counter-evidence does not date from the time that theory was first put forward, one theory can still be better supported by the total body of available evidence (including counter-evidence) at any time than any of its rivals.[120]

Since Watkins does seem to realise how easy it is for a theory to be refuted on Lakatos's view above, it is only the tug of the stronger meanings of 'refuted' which both prevents him from recognizing that the 'born refuted' thesis poses no threat (or at least no novel threat) to his account and that sends him off on an historical tangent to defend this account. If all theories were, *per impossible*, born refuted, or even if they were all born strongly discorroborated (and stayed that way), Watkins *would* have something to worry about, as would the rest of us.

Popper offers two defences of SPF. The first is an argument from crucial experiments, directed against Duhem; the second is an argument from what he calls crude tests, formulated in response to criticisms by Hilary Putnam and Imre Lakatos.

Taking Popper's first argument, it is difficult to escape the conclusion that he has not read or understood Duhem, or else that he felt it necessary to construct a straw man. Duhem's modest and plainly expressed response to the Duhem-Quine problem asserts no more than Popper himself is entitled to assert, though significantly less that Popper would like to. I concentrate on logical matters below, however, rather than these historical ones.[121]

In *Conjectures and Refutations* (hereinafter *Conjectures*), Popper asserts that Duhemian skepticism about the refutation of theories "overlooks the fact" that in a crucial experiment "we decide between two systems which differ *only* over the two theories which are at stake".[122] Whilst this is a fact about crucial experiments, it provides no cure for such skepticism. Let T_1 and T_2 be Popper's two theories; T_1 & A and T_2 & A Popper's two systems (though he thinks of A here as "background knowledge"); and let P_1 and P_2 be the predictions derived from each system, respectively. That is,

$$(1) \qquad (T_1 \ \& \ A) \dashrightarrow P_1$$

and

$$(2) \qquad (T_2 \ \& \ A) \dashrightarrow P_2$$

Now, even if

$$(3) \qquad \sim P_1$$

and

$$(4) \qquad P_2$$

it does not follow, as others have pointed out,[123] that

$$(5) \qquad \sim T_1$$

(5) is not a consequence of (1) and (3), which are the only relevant givens. Given (1), (2), (3), and (4), anyone who rejects T_1 and accepts T_2 may be rejecting what is true and accepting what is false.

Popper continues that Duhemian skepticism "further overlooks the fact that we do not assert the refutation of the theory as such [T_1], but of the theory *together* with that background knowledge [A]; parts of which, if other crucial experiments can be designed, may indeed one day be rejected as responsible for the failure".[124] Since the Duhem-Quine problem arises *only if* we do assert that T_1 & A is false, however, Duhem cannot have overlooked this point. Moreover, fancy *Popper* suggesting that we refrain from asserting that theories are refuted; and were we to do so, of course, there would be no disagreement with Duhem. Also, if Popper would back away from claiming that the false conjunct(s) of a refuted system can be prised from that system in *one* experiment, crucial or otherwise, he cannot consistently suppose that a *second*, or any subsequent, experiment will do the trick.[125]

I turn now to Popper's argument from so-called crude tests. Putnam points out that, for two hundred years or so, Newton's law of gravitation was unfalsifiable because at that stage no counter-argument could have been formulated in which the auxiliary propositions were known or knowable. He concludes that such theories are not, as Popper believes, "strongly falsifiable".[126] But Putnam is using 'falsifiable' here in a different sense from Popper. Popper's requirement for strongly falsifiable theories is a purely logical requirement (whatever one may think of this use of the word 'falsifiable'). Falsifiability is measured by the size of the class of *potential* or logically possible falsifiers, not the class of *discoverable* falsifiers. Since any statement of a law of nature or a true universal generalisation is unfalsifiable as a matter of fact, a requirement that a theory be falsifiable in Putnam's sense would indeed be undesirable.

Lakatos makes the stronger claim that a good scientific theory is, *in principle*, unfalsifiable.[127] Like Popper, Lakatos is a skeptic about contingent knowledge, and for much the same reasons.[128] And at least on this occasion, he takes his skepticism seriously. He considers the case of a planet which deviates from its predicted course, though his argument follows a familiar one. He contends that, even if we could know that this planet had so deviated, it is always possible that some undiscovered cause would explain this deviation. This is so even if the search for such a cause has been, for all practical purposes, exhaustive.[129] Lakatos points out that, at least for many disagreements between prediction and observation, that is, given

$$(T \& A) \dashrightarrow \sim O$$

we can cobble together some new set of auxiliary propositions, A', such that

$$(T \& A') \dashrightarrow O$$

If we are unable to corroborate A', we can try A'' such that, once again, agreement with observations is achieved:

$$(T \& A'') \dashrightarrow O$$

If we are unable to corroborate A'', then we can try A''' . . . and so on. Lakatos concludes with a flourish that "*exactly the most admired of scientific theories simply fail to forbid any observable state of affairs*".[130] But a *skeptical* premise will not support this conclusion. To profess ignorance about the world, as Lakatos does, gives one no reason to believe that it is one way rather than any other. In particular, if we *are* ignorant about the sum of the relevant forces on any planet, this *ignorance* is no reason to believe that the path that would be predicted for some planet, on a set of true auxiliary propositions, would not also be one from which the observed path would deviate. So where has Lakatos gone wrong, and what is he on about?

The premise that it is always possible that some unknown force sensibly perturbs a planet's motion is consistent with there being no such force. So the original set of auxiliary propositions, A, may be *true*. If A is true and the planet does deviate from its validly predicted path, then a state of affairs has been observed which T would "forbid", namely, that described by $O \& A$, whether or not Lakatos is right in denying that we can ever come to know that this is so.

Now the set of logically possible states of affairs that Lakatos has in mind does contain many members that are *not* forbidden or physically impossible on T, namely, those described by $O \& A'$, $O \& A''$, and so on. Thus Lakatos would be right in thinking that T does not forbid O; he is right in thinking that what I call SPF is false,[131] for another way of putting SPF is 'T forbids O'. In all probability, then, SPF is the real object of Lakatos's attack. But he overgeneralises and fails to see that T *does* forbid $O \& A$ (as well as $\sim O \& A'$, $\sim O \& A''$ and so on). What Lakatos ought to have said, which is true, is that exactly the most admired of scientific theories simply fail to forbid any observable state of affairs *such as Popper has in mind*, for example, 'The light ray deflected 15° in grazing the sun.' The key point here can be put more simply, however, for it does not depend upon distinguishing law-like from accidental conditionals: it is that T is consistent with O, but not with $O \& A$.[132]

Popper responds to both Putnam's and Lakatos's doubts about auxiliary propositions principally with SPF, for on SPF there are no auxiliary propositions that one is required to know. Thus he asserts that "*Newton's theory can be refuted without the use of initial conditions*";[133] and he offers the following cases as examples of "crude tests" from which, supposedly, the failure of that theory would be manifest:

> There are an infinity of possibilities, and the realization of any of them would simply refute Newton's theory. In fact, almost any statement about a physical body which we can make - say, about the cup of tea before me, that it begins to dance (and say, in addition, without spilling the tea) - would *contradict* Newtonian theory. This theory would equally be *contradicted* if the apples from one of my, or Newton's, apple trees were to rise up from the ground (without there being a whirlwind about), and begin to dance round [sic] the branches of the apple tree from which they had fallen, or if the moon were to go off at a tangent; and if all this were to happen, perhaps, without any other very obvious changes in our environment.[134] (Emphasis mine.)

This choice of bizarre, though logically possible, events is intended to highlight what Putnam seems to have overlooked and Lakatos falsely denies, namely, that there *are* infinitely many potential falsifiers of the theory. For all that, it is *obviously* not an immediate inference from, for example, 'My cup of tea began to dance' to the negation of either Newton's laws of motion or his law of gravitation.[135] No contradiction is involved in accepting this observation statement and in not rejecting anything of Newtonian mechanics, even should the behaviour of my cup of tea be non-Newtonian.

Popper once remarked that when he wrote *Logik der Forschung* he thought that "it was plain enough that from Newton's theory alone, without initial conditions, nothing of the nature of an observation statement can be deducible".[136] So it is not clear how he can swallow the contradiction that whilst T does not entail $\sim O$, O does (supposedly) entail $\sim T$, that whilst 'My cup of tea sat still' is not deducible from Newton's theory alone, 'My cup of tea began to dance' is inconsistent with that theory. It is clear, however, how Popper would persuade others to do so, for he prefaces his replies to both Putnam and Lakatos with the following remark:

> I feel obliged to say again, before I proceed to examine their arguments, that Newtonian theory is a falsifiable theory just as "All swans are white" is a falsifiable theory. That is, it

> is falsifiable in the simple logical sense of being logically incompatible with some basic statements. It has potential falsifiers.[137]

There is an important asymmetry, however, between the stock examples of a universal generalisation, such as 'All swans are white', and scientific theories like Newton's laws of motion or his law of gravitation. It is that none of the latter, unlike the former, is "logically incompatible" with any basic statement.

A basic statement is, for Popper, not only a singular existential statement; it is also an observation statement, and one of a rather elementary sort:

I have called simple descriptive statements, describing easily observable states of physical bodies, '*basic statements*', and I asserted that in cases in which tests are needed, it is these basic statements which we try to compare with the 'facts' and that we choose these statements, and these facts, because they are most easily comparable, and intersubjectively most easily testable.[138]

Now there are singular existential statements that are inconsistent with Newton's law of gravitation, for example:

> There is a pair of bodies at k gravitationally attracted to one another as the square root of the product of their masses and inversely as the cube of their separation.

This statement is a potential falsifier of the law but it is not an observation statement, much less a basic statement. Moreover, in order to know that the pair of bodies it refers to are so attracted, their movements over some period of time would need to be recorded, and for this a statement of initial conditions, amongst other things, is required.

On the other hand, stock universal generalisations, such as 'All swans are white', characteristically contain only observable predicates - what we may call 'basic predicates' - and the same goes, in general, for their potential falsifiers.[139] Furthermore, if T is a universal generalisation which contains only basic predicates (call it a 'basic universal generalisation'), it is easy to obscure the role of A in the counter-argument for T. This is because it is natural and convenient to express any potential falsifier of a basic universal generalisation in a sentence which gives no indication of the logical form of that potential falsifier *qua* antecedent in

$$(O \,\&\, A) \dashrightarrow {\sim}T$$

For example, suppose that someone sets out to test whether all dogs are carnivorous by training his puppy, Artichoke, to eat only cereals and vegetables. Thus we have,

T : All dogs are carnivorous,

A : Artichoke is a dog,

P : Artichoke is carnivorous,

and

O : Artichoke is vegetarian.

The potential falsifier, 'Artichoke is a vegetarian dog', can be expressed by the cumbersome sentence,

(1) Artichoke is vegetarian and Artichoke is a dog,

or, more naturally, of course, by the sentence,

(2) Artichoke is a vegetarian dog.

(2) conceals, however, what (1) does not, namely, the logical form of the antecedent in

(O & A) --> ~T

In his reply to Putnam, Popper offers a third argument for his belief that no statement of initial conditions is required to falsify any theory. This argument does not commit the fallacy of SPF, but it does depend for its plausibility on a further and misleading use of basic universal generalisations.

Putnam cites Newton's law of gravitation as one amongst many important theories from which no "basic sentence" or prediction can be derived without the assistance of some auxiliary proposition(s), and he offers two reasons for thinking this theory exemplifies his point. He notes, firstly, that there may be forces *other* than gravity at work on any body.[140] This is true, but the point is not generalisable. Newton's second law of motion, for example, concerns the *sum* of the forces on any body yet no prediction about, say, the position or rate of acceleration of any body follows immediately from this law, any more than it does from the law of gravitation. Secondly, Putnam points out that gravitational forces are not "directly measurable".[141] This misses the point, for even if these forces were directly measurable, and we had a value for some such force, nothing would follow about any of the observable properties of either of the bodies so acted upon. Moreover, this value would figure as an auxiliary proposition in the derivation of any prediction which employed it.

In any event, Popper ignores Putnam's reasons and generalises the claim they are intended to support, taking him to be asserting something about *all* theories or universal generalisations.[142] This move allows Popper to blur the focus of the dispute, as he fixes once again on basic universal generalisations. He does so because such propositions immediately entail existential denials which *can* be falsified by basic statements.[143] For example,

All dogs are carnivorous

entails

There is no non-carnivorous dog in my back yard now,

which can be falsified by

Artichoke is a vegetarian dog in my back yard now.

So Popper concludes that Putnam has simply overlooked that "a certain unusual kind of negative prediction can be derived from any theory", though he shies away from illustrating this claim with other than a basic universal generalisation.[144] Popper concedes that "in almost all cases" auxiliary propositions *are* employed in prediction making, which is to concede that in almost all cases his argument cuts no ice.[145] Moreover, this argument is in any case a desperate move for *Popper*, for in *The Logic of Scientific Discovery* (hereinafter *Logic*) he *excludes* from science any statement with the logical form of a "negative prediction" because no such statement is a potential falsifier of any universal generalisation, as we have seen (p. 13).[146] Furthermore, no existential denial which follows immediately from the law of gravitation, for example,

> There is no pair of bodies at k gravitationally attracted to one another as the square root of the product of their masses and inversely as the cube of their separation,

can be falsified by a basic statement. To falsify this so-called negative prediction one needs to assert its negation, which statement is not, as I pointed out above (p. 35), an observation statement, much less a basic statement.

It is not surprising that Popper should continually caution that his negative predictions are "unusual". Firstly, they are *arbitrary* consequences of the theories from which they are derived. For example, why k ? Why the *square root* of the product of the masses concerned? and so on. Secondly, the so-called negative predictions derivable from a theory such as the law of gravitation are as *abstract* as the theory itself. But whether predictions are made for evidence gathering or action guiding purposes, the practitioner aims to *descend* from such theoretical heights, drawing consequences from the theory that are testable by observation (however indirect the making of the relevant observations may be).

Finally, even if one could accept Popper's account of negative predictions, the making of any such prediction would still not provide the short cut to a refutation that he supposes. To falsify the basic universal generalisation, 'All dogs are carnivorous', we need to know above that Artichoke is a dog and that he is not carnivorous, whether our inquiry is conducted with a positive *or* a negative prediction. To falsify the law of gravitation above we need to know, amongst other things, the gravitational force between that pair of bodies at k , and this *presupposes* a propositional knowledge of various states of affairs (not to mention a knowledge of scientific techniques and equipment) that is no less extensive or sophisticated than would be required to falsify the law in the usual manner.

1.6 Weak Popperian Falsificationism (WPF)

WPF is the false doctrine that if *P* is refuted then *T* is *prima facie* refuted or apparently refuted or threatened with refutation. It is the doctrine that if *P* is refuted then *T* is refuted$_p$. When Lakatos and others say that all theories are born refuted they mean no more than that all theories are born refuted$_p$.

Popper typically retreats to WPF when confronted with a counter-example to SPF. In the literature, a classic case of this retreat occurs in his account of the oft discussed, anomalous perturbations in the motion of Uranus which troubled astronomers for much of the first half of the nineteenth century. This anomaly did not lead to the refutation of the law of gravitation for its cause was the unknown trans-Uranian planet, Neptune. So Popperians, and others, assert or imply that the anomaly of Uranus's motion was a *prima facie* or an apparent refutation of the law. (I discuss this case and its implications in detail in Chapters 3 and 4 below.)

Now, on *Popper's* account of refutation, there can be no such thing as a *prima facie* or an apparent refutation, as I shall explain in 2.1 below, but let us set this point aside for now and take WPF to be some such claim as that *P* is refuted only if there is a strong or at least moderately well corroborated counter-argument for *T*, or some *prima facie* good reason to disbelieve or reject *T*, or even perhaps that there is only more reason to disbelieve *T* than *A*. None of these claims, however, is true. If *P* is refuted then we know, or can readily come to know, only that just one of the following is true: *~T & A* , *T & ~A* , or *~T & ~A*. But we have no reason, *prima facie* or otherwise, to prefer one of these possibilities to any other. This is Duhem's lesson. *T* is thus no more threatened with refutation by the fact that *P* is false than is *A*.

Consider a simple analogy. Suppose that a teacher leaves a class of two students, Newton and Offsider, for a few seconds. She fails to notice upon her return that in the interim her chair has been rigged up for a practical joke. If we allow that she knows that no one other than either or both of these students had sufficient opportunity to play this joke then she knows that one or both of them is responsible for doing so. But it would be irrational for her *also* to conclude even that there was more reason to suspect Newton rather than Offsider (or the pair of them in cahoots). Clearly, to come to any such conclusion rationally she requires some evidence which distinguishes between these three possibilities, all equi-probable on the above evidence. She requires evidence that, for example, Newton has a talent or an inclination for playing such tricks whereas Offsider does not. Correspondingly, to justify a preference for the argument,

$$(\sim P \ \& \ A) \ \text{-->} \ \sim T$$

over

$$(\sim P \ \& \ T) \ \text{-->} \ \sim A$$

- the latter being rarely formulated or even alluded to in the literature - we need to know what is the prior support for each of T and A, or, in Popperian terms, what is the prior corroboration of each. If the prior support or corroboration of T is high compared with that of A, then the latter argument will be strong by comparison with the former, and *vice versa*.

Most, if not all, of the above points are rather obvious; so how has WPF come to seem plausible to Popper, and others? Well, anyone who finds SPF plausible will likely find WPF so, since SPF entails WPF. But the main reason for the plausibility of this doctrine lies, I suggest, in a certain aspect of Popper's idiosyncratic view of scientific practice.

Popper's general beliefs about what is interesting and fundamental in science were formed, so he tells us, shortly after the First World War, when he was just seventeen.[147] Characteristically, these beliefs have remained unchanged. At the time, relativistic mechanics was new and a serious rival to that long standing paragon of science, Newtonian mechanics. Most importantly, various crucial experiments had been proposed or conducted between them, notably Eddington's famous eclipse experiment of 1919. Popper was struck by the fact that this feature of physics, or the physical sciences generally, was in his experience nowhere in evidence when boasts about the scientific status of popular social theories, such as those of Marx or Freud, were made, as apparently they often were.[148] Popper has overgeneralised from this youthful encounter, however, in the beliefs he holds about the nature and occurrence of tests or testing in science. He does not bother to distinguish hypothetico-deductive reasoning generally from testing[149] and, more importantly, he tends to treat all tests as if they were epistemically and methodologically similar to the eclipse experiment, or to what he believes or assumes to be the case about this experiment. But there are two *dis*similarities of importance here. Before discussing them, however, some general remarks about tests are in order.

The primary scientific purpose of conducting a test is to gather evidence, favourable or otherwise, for some proposition, or conjunction of propositions, which I shall call the test candidate, X. In order to derive some test prediction, P, from X, let X be conjoined with some proposition(s), Y, such that

$$(X \& Y) \text{ --> } P$$

Intuitively, a test of X is stronger (that is, it provides more support or counter-support for X, as the case may be) if there is more reason to believe that, other things being equal,

(i) The determination of the truth value of P is correct,

or

(ii) Y is true.

Clearly, the test provides more reason to believe that X is false if there is more reason to believe that (i) and (ii) are true, that is, if there is more reason to believe that the antecedent, $\sim P \ \& \ Y$, of the counter-argument for X is true. Moreover, a test is strong only if there *is* good reason to believe that $\sim P \ \& \ Y$ is true.

Let us return to Popper. Firstly, he has what we may call a 'litmus test' in mind as a model of hypothetico-deductive reasoning in science. A litmus test is a *strong* test of X, one that has been *designed* as such. Thus Popper says, for example, that "We consciously test, as a rule, a certain *chosen* hypothesis, treating the rest of the theories involved as more or less *unproblematic* - as a kind of 'background knowledge'."[150] (Emphasis mine.) Now it is one thing to emphasise the value or importance of strong tests, as Popper often does, quite another to overlook that, unlike what at least he takes to be the case about the eclipse experiment,[151] some tests are weak or inconclusive. In discussing how we should respond to the refutation of a test prediction, however, Popper says, for example, that we need "special reasons" if we are not to accept that (what he thinks of as) the test candidate is false.[152] Any such case "must be exceptional", he tells us, otherwise "no test would count as a real test".[153] But the point is, rather, only that any such test would not be a strong or conclusive test.

Moreover, practitioners may not begin with a proposition they wish to test but rather with, for example, some puzzling fact they want explained. So they begin not with part of the antecedent, choosing instances of Y to maximise support (or counter-support) for X but rather with a consequent which is known. In the latter case, there is no corresponding freedom of choice in respect of either the antecedent or the consequent. They want that antecedent which explains the puzzling fact in question and no other.[154] So the antecedent is not designed to test any of its constituent parts and it may not do so, or at least it may do so only very weakly, even if it is that antecedent the practitioners are looking for.

Secondly, Popper's continual emphasis on theory testing leads him to assume that, as in the case of the eclipse experiment, T is always the test candidate. But some tests are tests of an initial condition or a boundary condition, that is, X is one of the propositions in A. (The common notational practice in philosophy of science of picking out in the set of premises from which P is derived that premise which is the most scientifically or historically important, and bundling the remainder of this set under the rubric 'auxiliary', can thus be misleading.) If an astronomer tests the claim of a colleague to have discovered an asteroid, or an engineer road tests a prototype of a new car, or a physicist attempts to explain why a projectile overshot its target, the test candidate is not Newton's laws of motion or his law of gravitation. This is not to say, however, as some philosophers have done,[155] that the laws are not tested in such cases - though such tests would be very weak for the laws are very well supported in such applications, especially by comparison with the relevant test candidates. There are reliable methods, for example, for calculating the position of an asteroid and for conducting a search for it. So a failure to find the alleged body would provide a poor reason for rejecting the law of gravitation, but a good reason for rejecting the discovery claim. This would especially be so if, say, the astronomer who made this claim was known to be unduly optimistic in such matters or to have been using inferior instruments. Similarly, in the other two cases above, the engineer's ideas about some of the prototype's performance characteristics may be highly speculative, as may the explanation the physicist formulates for the projectile's deviation. These practitioners do not need "special reasons" for not believing or supposing that, if their predictions are unfulfilled in such cases, they have *prima facie* falsified Newton's laws.

I conclude that there is a suppressed premise in WPF, one which makes its conclusion plausible to Popper and others, namely, that every test is a strong test in which T is the test candidate. With the addition of this false premise, Popper's argument becomes valid for, necessarily, if P is refuted in a strong test of T there is a strong counter-argument for T. Similarly, given the suppressed premise, 'This test is a conclusive test of T', if P is refuted in such a test so too is T - as SPF would have it. Whenever any proposition in A is not well supported, however, as the various cases above illustrate, there is not even a *prima facie* or an apparent refutation of T.

1.7 Conjecture and Conjecture

Are SPF and WPF Popper's only responses to the Duhem-Quine problem, apart from obfuscation? No, he is inconsistent. In *Logic*, for example, he says that "it cannot be asserted of any one statement of the system [*T & A*] that it is, or is not, specifically upset by the falsification" of *P*. This is because, via *modus tollens*, "we falsify *the whole system* (the theory plus the initial conditions)."[156] Where, then, is the method of conjecture and *refutation*, the method of conjecturing and refuting *theories*, instances of *T* ? In "Replies to My Critics" (hereinafter "Replies"), Popper can only add:

> Attributing the blame for a falsification to a certain subsystem [of *T & A*] is a typical hypothesis, a conjecture like any other, though perhaps hardly more than a vague suspicion if no definite alternative suggestion is being made. And the same applies the other way round: the decision that a certain subsystem is not to be blamed for the falsification is likewise a typical conjecture. The attribution or nonattribution of responsibility for failure is conjectural, like everything in science; and what matters is the proposal of a new alternative and competing conjectural system that is able to pass the falsifying test.[157]

On the above account, there are no refutations of theories. Popper's method is the method of conjecture and conjecture. *All* refutations would be conjectures if Popper were right in thinking that all knowledge is conjectural, a conclusion that more than one of his commentators has reached.[158] If the antecedent of every counter- argument that *p* is but a conjecture, so is any alleged refutation of *p*. Furthermore, on the above account, Popper's advice to practitioners faced with predictive failure is, "Try replacing either *T* or *A* or both." This is better advice than that which either SPF or WPF can provide, but that does not prevent it from being trivial.

Notes For Chapter 1

1. *The Shorter Oxford English Dictionary*, 3d ed., rev. 1973, s.v. 'falsify' and 'refute'; A.R. Lacey, *A Dictionary of Philosophy* (London: Routledge and Kegan Paul, 1976) s.v. 'refute'; Jennifer Speake, ed., *A Dictionary of Philosophy*, 2d rev. ed. (London: Macmillan Press, 1983), s.v. 'refute'; John A. Passmore, "Popper's Account of Scientific Method", *Philosophy* 35 (1960): 327. Not all philosophers outside the Popperian tradition, however, stick to ordinary English here. For example, Peter D. Klein, in *Certainty: A Refutation of Skepticism* (Brighton: Harvester press, 1981), p. 212, takes 'refute' to mean 'show to be implausible', and this will not do for what is implausible may not be false, just as what is plausible may not be true.

2. See, for example, *The Shorter Oxford*, s.v. 'knowledge'; Lacey, *Dictionary*, s.v. 'epistemology'; Speake, *Dictionary*, s.v. 'epistemology'; Anthony Quinton, s.v. "Knowledge and Belief", in *The Encyclopedia of Philosophy*, reprint ed., 1972.

3. Edmund L. Gettier, in "Is Justified True Belief Knowledge?" *Analysis* 23 (June 1963): 121-23, drew attention to the point that an entailment of a justified but false belief can be a justified true belief. The latter would not be knowledge, however, if it were inferred from a false belief such as the former.

4. See, for example, Michael D. Roth and Leon Galis, eds., *Knowing: Essays in the Analysis of Knowledge* (New York: Random House, 1970).

5. David M. Armstrong, in *Belief, Truth and Knowledge* (Cambridge: Cambridge University Press, 1973), p. 137, remarks, "I do not think there has ever been any serious doubt that it is a necessary condition for knowledge that what is known is true." Nonetheless, this is what Popper, and others, doubt (see also n. 37, p. 45 below).

6. See, for example, Karl R. Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge*, 4th ed., rev. (London: Routledge and Kegan Paul, 1972), p. 226; and his *Objective Knowledge: An Evolutionary Approach* (Oxford: Clarendon Press, 1972), secs. 7-11.

7. See, for example, Karl R. Popper, *Unended Quest: An Intellectual Autobiography*, rev. ed. (London: Fontana, 1976), sec. 8.

8. Popper, *Objective Knowledge*, pp. 108-109.

9. 'Objective knowledge' has other meanings for Popper; for example, it also means 'shared knowledge' and 'branch of learning' - see *Objective Knowledge*, p. 110.

10. Ibid., pp. 108-109.

11. Karl R. Popper, *The Logic of Scientific Discovery* (London: Hutchinson and Co., 1972), p. 95; see also John W.N. Watkins, *Science and Skepticism* (Princeton: Princeton University Press, 1984), pp. 82-84.

12. Popper, *Logic*, p. 105.

13. See, for example, Karl R. Popper, *The Open Society and its Enemies*, 5th ed., rev., 2 vols. (London: Routledge and Kegan Paul, 1966), 2: 374-75; Popper, *Conjectures*, pp. 25, 229 and 238; and his *Objective Knowledge*, pp. 41 and 64.

14. Popper was originally a strong conventionalist; he believed that an account of science could be given without any appeal to truth values. This attitude is preserved in *Logic*, sec. 84.

15. Ibid., secs. 28-30.

16. Ibid., p. 425.

17. Ibid., secs. 25 and 29.

18. This point is well made by Max Deutscher in "Popper's Problem of an Empirical Basis", *Australasian Journal of Philosophy* 46 (December 1968): 283.

19. As A.J. Ayer points out in "Truth, Verification and Verisimilitude" in *The Philosophy of Karl Popper*, ed. Paul A. Schilpp, The Library of Living Philosophers, vol. 14 (La Salle, Ill.: Open Court Publishing Co., 1974), bk. 2: 687. In "Replies to My Critics", in *The Philosophy of Karl Popper*, ed. Paul A. Schilpp, The Library of Living Philosophers, vol. 14 (La Salle Ill.: Open Court Publishing Co., 1974), bk. 2: 1110, Popper accuses Ayer of completely missing his point, and of concentrating on his use of the words 'convention' and 'decision' in the mistaken belief that "every decision or convention must be arbitrary". But if Ayer believed that all decisions were arbitrary he would surely have criticised Popper merely for *incorporating* decision making in his account, which he does not do. Neither 'convention' nor any synonym for 'convention' appears in Ayer's paper.

20. Ayer, "Truth", bk. 2: 687.

21. Popper, "Replies", bk. 2: 1114.

22. Ian C. Hinckfuss, "A Note on Knowledge and Mistake", *Mind* (October 1971): 614-15; see also Richard Feldman, "Fallibilism, and Knowing That One Knows", *Philosophical Review* 90 (April 1981): 266. John Kekes, for example, mistakenly identifies fallibilism with skepticism in "Fallibilism and Rationality", *American Philosophical Quarterly* 9 (October 1972): 301-9.

23. J.L. Evans, *Knowledge and Infallibility* (New York: St Martin's Press, 1978), p. 40.

24. Ayer, "Truth", bk. 2: 687.

25. Deutscher, "Popper's Problem", p. 280.

26. Popper, *Conjectures*, p. 41 (n. 8).

27. Popper, *Logic*, p. 86.

28. Deutscher, "Popper's Problem", p. 280 (n. 2).

29. Popper, *Conjectures*, p. 226 (see also p. 28 for a similar argument).

30. Karl R. Popper, *Postscript to The Logic of Scientific Discovery*, ed. W.W. Bartley III, 3 vols. (Totowa, N.J.: Rowman and Littlefield, 1983), vol. 1, *Realism and the Aim of Science*, p. 186.

31. Talk of our being logically bound or committed to reject p once we accept q can be misleading. We are not *logically* bound to reject (or accept) any *contingent* proposition. What we are, or can be, logically bound to do is not to contradict ourselves, for example, not to accept $p \& q$.

32. Popper, *Postscript*, p. 186.

33. Ibid., p. 185.

34. Ibid., p.186.

35. If doubt should arise concerning some basic statement that has been accepted corroboration is required - see Popper, *Logic*, p. 104. It is not clear whether or not an individual scientist can know something in advance of his or her colleagues' agreement, but this problem can be set aside here.

36. Deutscher, "Popper's Problem", p. 284.

37. John W.N.Watkins, "On Stove's Book, by a Fifth Irrationalist", *Australasian Journal of Philosophy* 63 (September 1985): 261. This argument can also be found in Watkins, *Science*, p. 11. Tom W. Settle, in "Deutscher's Problem is not Popper's Problem", *Australasian Journal of Philosophy* 47 (August 1969): 217, denies that knowledge entails truth. Alan E. Musgrave, in "Logical versus Historical Theories of Confirmation", *British Journal for the Philosophy of Science* 25 (1974): 5 (n. 3), says that "it almost goes without saying that our 'background knowledge' may in fact be false."

38. Popper, *Logic*, p. 86.

39. See, for example, Popper, *Logic*, p. 76.

40. Popper, *Logic*, p. 84.

41. Ibid., p. 102.

42. Ibid., pp. 100 - 101.

43. David C. Stove, "Popper on Scientific Statements", *Philosophy* 53 (January 1978): 85.

44. Popper, *Logic*, p. 102.

45. Harold I. Brown, in *Perception, Theory and Commitment: The New Philosophy of Science* (Chicago: University of Chicago Press, 1977), p. 75, makes this point, but without the term 'discorroboration', or some equivalent, the ambiguities found in Popper remain. Watkins sometimes uses 'discorroboration' perhaps as a synonym for 'falsification' - see, for example, his "The Popperian Approach to Scientific Knowledge" in *Progress and Rationality in Science,* eds. Gerard Radnitzky and Gunnar Andersson. Boston Studies in the Philosophy of Science, vol. 58 (Dordrecht: D. Reidel Publishing Co., 1978), p. 36.

46. Alan E. Musgrave, "Falsification and its Critics", in *Logic, Methodology and Philosophy of Science IV: Proceedings of the Fourth International Congress for Logic, Methodology and Philosophy of Science, Bucharest, 1971*, eds. Patrick Suppes, Leon Henkin, Athanase Joga, and Gr. C. Moisil (Amsterdam: North-Holland Publishing Co., 1973), pp. 394-95.

47. For example, Imre Lakatos, in "Falsification and the Methodology of Scientific Research Programmes", in *Criticism and The Growth of Knowledge*, eds. Imre Lakatos and Alan E. Musgrave (Cambridge: Cambridge University Press, 1970), p. 116, implies that 'This theory is falsified' means something like 'This theory is sufficiently inferior to some rival theory to warrant its rejection in favour of that rival'. Later (pp. 116-17), Lakatos uses 'refutation' when he means 'anomaly' in describing the positive heuristic of a scientific research programme as being capable of predicting and digesting refutations.

48. Popper, *Logic*, p. 88. See also, for example, Gerard Radnitzky, "Popperian Philosophy as an Antidote Against Relativism", in *Essays in Memory of Imre Lakatos*, eds., Robert S. Cohen, Paul K. Feyerabend, and Max W. Wartofsky, Boston Studies in The Philosophy of Science, vol. 39 (Dordrecht: D. Reidel Publishing Co., 1976), p. 535.

49. Popper, *Logic*, p. 86.

50. Popper, *Conjectures*, p. 116.

51. Ibid., p. 117.

52. Robert E. Peary, *The North Pole* (London: Hodder and Stoughten, 1910), p. 260 (n. 1).

53. Musgrave, "Falsification", p. 403.

54. Popper, *Conjectures*, p. 56. For other cases in which Popper's appeal to refutation as disproof is clear, see his *Logic*, pp. 275 and 279; *Conjectures*, pp. 37, 51, 55, 230, 231, 235, and 240; *Objective Knowledge*, pp. 13, 14, 69 and 353; and *Unended Quest*, p. 38.

55. Musgrave, "Falsification", p. 402.

56. Popper, *Conjectures*, p. 116.

57. Ibid.

58. A.J. Ayer, *The Problem of Knowledge* (London: Macmillan & Co., 1958), p. 79.

59. Popper, "Replies", bk. 2: 1110. T.E. Burke fails to grasp this point in *Popper*, cf. pp. 59 and 64.

60. Pierre Duhem, *The Aim and Structure of Physical Theory*, trans. P.P. Wiener (Princeton: Princeton University Press, 1954), pp. 186-90.

61. W.V. Quine, *From a Logical Point of View; Nine Logico-Philosophical Essays* (Cambridge: Harvard University Press, 1953), p. 41.

62. Popper, *Conjectures*, p. 243.

63. Ibid., p. 239.

64. Alan F. Chalmers, *What is this Thing called Science? An Assessment of the Nature and Status of Science and its Methods*, 2d ed., (St. Lucia, Qld.: University of Queensland Press, 1982), p. 65.

65. Ibid., p. 64.

66. Ibid., p. 63.

67. Ibid.

68. Popper, *Postscript*, p. 189.

69. Popper, *Logic*, p. 70.

70. See, for example, Popper, *Conjectures*, pp. 195-96, 248-50.

71. David C. Stove, *Popper and After: Four Modern Irrationalists* (Oxford: Pergamon Press, 1982).

72. Reviews (of Part One) of Stove, *Popper and After*, have been mixed. However, Andrew Lugg, in *Philosophy of Science* 50 (June 1983): 350-52; John F. Fox, in *Australasian Journal of Philosophy* 62(March 1984): 99-101; and J.M.B. Moss, in *British Journal for the Philosophy of Science* 35 (September 1984): 307-10, do not point out the deficiencies in Stove's analysis discussed below. Stove has extended his critique of Popper's rhetoric, in "How Popper's Philosophy Began", *Philosophy* 57 (July 1982): 381-87, to include an alleged equivocation about 'irrefutable'. But James M. Brown, in "Popper had a Brand New Bag", *Philosophy* 59 (October 1984): 512-15, shows that this allegation is baseless.

73. Stove, *Popper and After*, chap. 1.

74. Ibid., chap. 2.

75. Ibid., p. 10.

76. Ibid., p. 11.

77. Ibid., p. 7.

78. Ibid., p. 46.

79. John L. Mackie, s.v. "Fallacies", in *The Encyclopedia of Philosophy*, reprint ed., 1972.

80. Stove, *Popper and After*, p. 23.

81. Ibid.

82. Ibid., p. 27.

83. Stove's term is "epistemic embedding" - see *Popper and After*, p. 26.

84. Ibid., pp. 22 - 23.

85. When a logical word is neutralised as in, for example, " *P* 'entails' *Q* ", Stove misclassifies this as sabotage - see *Popper and After*, p. 26. But all manner of words can be neutralised, not just the cognitive success words Stove considers; for example, "The 'door' is open", "He 'danced' for us", "The table is 'flat'", and so on.

86. Stove, *Popper and After*, p. 26.

87. Ibid.

88. Stove recognizes this at one point in *Popper and After*, p. 23.

89. Ibid., p. 7.

90. Ibid., p. 17. There are also truth-functional words, of course, such as 'and', 'not', 'or', and the like.

91. Ibid., p. 10.

92. Ibid., p. 22.

93. Ibid., pp. 16 and 22.

94. Ibid., pp. 22 and 41. For this mistake in Popper see, for example, p. 29 above; see also pp. 14-15 and 34 above, for example, for the related mistake of supposing that events or states of affairs can refute propositions.

95. Stove, *Popper and After*, p. 41.

96. Ibid., p. 21.

97. Ibid., p. 22.

98. Ibid., p. 23.

99. Ibid., p. 33.

100. Ibid., p. 17.

101. Ibid., pp. 29 and 30.

102. See Popper, *Logic*, pp. 191 (n.*1), 192 (n.*1), and 204.

103. Stove, *Popper and After*, p. 29.

104. See Popper, *Logic*, p. 102; but where else?

105. In addition to the instances in Popper's writings cited in 1.5 above see, for example, *Logic*, p. 33; *Conjectures*, pp. 36, 38 (n. 3), and 244; *Objective Knowledge*, p. 38 (n. 5); "Replies", pp. 979-80, 986, and 1187 (n. 78).

106. Popper, *Unended Quest*, p. 38.

107. Popper, "Replies", bk.2: 1035.

108. J.O. Wisdom, "The Nature of Normal Science", in *The Philosophy of Popper*, ed. Paul A. Schilpp, The Library of Living Philosophers, vol. 14 (La Salle, Ill.: Open Court Publishing Co., 1974), bk. 2: 826.

109. Alan E. Musgrave, "Evidential Support, Falsification, Heuristics, and Anarchism", in *Progress and Rationality in Science,* eds. Gerard Radnitzky and Gunnar Andersson, Boston Studies in the Philosophy of Science, vol. 58 (Dordrecht: D. Reidel Publishing Co., 1978), p. 198 (n. 18).

110. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2d ed., enlarged. International Encyclopedia of Unified Science: Foundations of the Unity of Science, vol. 2, no.2 (Chicago: University of Chicago Press, 1970), p.77.

111. See, for example, Lakatos, "Falsification", p. 133; and Brown, *Perception*, p. 97.

112. Imre Lakatos, "Introduction: Science and Pseudoscience" in *Philosophical Papers*, eds. John Worrall and Gregory Currie, 2 vols. (Cambridge: Cambridge University Press, 1978), vol. 1, *The Methodology of Scientific Research Programmes*, p. 5.

113. Lakatos, "Falsification", pp. 116-17.

114. W.H. Newton-Smith, *The Rationality of Science* (Boston: Routledge and Kegan Paul, 1981), p. 71; he concludes that we can now "see how difficult it is going to be to falsify a theory that has something going for it" (p. 72).

115. Ibid., p. 72.

116. Watkins, *Science*, p. 330.

117. Ibid.

118. Ibid.

119. Ibid., pp. 330-34.

120. Since Watkins conflates counter-evidence with refuting evidence above, it is not clear that he believes that in deciding the level of support for an unrefuted$_p$ theory counter-evidence may need to be taken into account. Following Popper he does distinguish weak from strong corroboration, which implies that there can be both weak and strong *dis*corroborations. In *Science*, p. 299, Watkins seems to employ the term 'discorroboration' in place of 'refutation' when he has something like this distinction at the back of his mind. Also, John Worrall, in "The Ways in which the Methodology of Scientific Research Programmes Improves on Popper's Methodology", in *Progress and Rationality in Science*, eds. Gerard Radnitzky and Gunnar Andersson, Boston Studies in the Philosophy of Science, vol. 58 (Dordrecht: D. Reidel Publishing Co., 1978), p. 52, uses "degree of corroboration *minus* 1" as a synonym for 'refutation'; Watkins in *Science* does not object that there are no degrees of discorroboration.

121. In *Logic*, p. 78 (n. 1), Popper claims that Duhem, in *Aim*, p. 188, "denies . . . the possibility of crucial experiments because he thinks of them as verifications, while I assert the possibility of crucial *falsifying* experiments". This is false. On p. 188 of *Aim*, Duhem claims only that if the effect predicted by one of the two theories in some crucial experiment *is* observed, and so that of the other is not, the latter theory will be "condemned" whilst the former will be held to be "indubitable". He then entertains the possibility Popper claims he does not think of, for he makes the point that even if we could "condemn once and for all" the latter theory the former would still not be "a demonstrated truth" because the two theories are contraries, not contradictories (p. 189). As Duhem observes (p. 190), "light may be a swarm of projectiles, or it may be a vibratory motion . . . ; is it forbidden to be anything else at all? In *Conjectures*, p.

112 (n. 26), Popper corrects this mistake, claiming instead that "Duhem, in his famous criticism of crucial experiments . . . succeeds in showing that crucial experiments can never *establish* a theory. He fails to show that they cannot *refute* it." However, Duhem is not required to state the immediate consequences of every claim he makes, for the section immediately preceeding that to which Popper refers above contains Duhem's classic statement of *the Duhem/Quine problem.* It is entitled "An Experiment in Physics Can Never Condemn an Isolated Hypothesis but Only a Whole Theoretical Group" (pp. 183-88). If Duhem denies that a theory can be falsified in any experiment, he denies that it can be falsified in any crucial experiment.

In *Conjectures*, p. 112, Popper suggests that, against his own view, "one might be tempted to object (following Duhem) that in every test it is not only the theory under investigation which is involved but . . . more or less the whole of our knowledge". But if anyone were to believe that in testing Ohm's law we are also testing our beliefs about, say, the Spanish Civil War, that person would not be following Duhem in this regard. How much clearer does Duhem need to be than when he describes, in *Aim*, p. 185, the propositions in question as those "*used* to predict the phenomenon [concerned] and to establish whether it would be produced"? (Emphasis mine.) Or when, in the case of Foucault's test of the emission theory of light (p.185), he describes them as "the theory from which we *deduce* the relation between the index of refraction and the velocity of light in various media"? (Emphasis mine.)

What was *Duhem's* advice concerning the Duhem/Quine problem? In *Aim*, p. 216, he says,"No absolute principle directs this inquiry, which different physicists may conduct in very different ways without having the right to accuse one another of illogicality." On the choice between rival theories, Duhem says, in *Aim*, p. 218, that "the day comes when good sense comes out so clearly in favour of one of the two sides that the other side gives up the struggle even though pure logic would not forbid its continuation." Is this advice so very different from that which Popper can offer?

122. Popper, *Conjectures*, p. 112.

123. Adolf Grünbaum, "Is Falsifiability the Touchstone of Scientific Rationality? Karl Popper versus Inductivism", in *Essays in Memory of Imre Lakatos*, eds. Robert S. Cohen, Paul K. Feyerabend, and Max W. Wartosfsky, Boston Studies in the Philosophy of Science, vol. 39 (Dordrecht: D. Reidel Publishing Co., 1976), pp. 247-49. Grünbaum believes (p. 248), however, that A need only be "presumed to be true" to "permit the valid deduction of the falsity" of T_1 , but it would be valid only to presume the falsity of T_1 on this premise. See also, Musgrave,"Evidential Support", p. 198 (n. 25).

124. Popper, *Conjectures*, p. 112.

125. See Popper, *Logic*, p. 76 (n. 2) for a similar expression of this belief.

126. Hilary Putnam, "The 'Corroboration' of Theories" in *The Philosophy of Popper*, ed. Paul A. Schilpp, The Library of Living Philosophers, vol. 14 (La Salle, Ill.: Open Court Publishing Co., 1974), bk. 1: 226-28.

127. Imre Lakatos, "Falsification", pp. 98-103.

128. Ibid., pp. 98-100.

129. Ibid., pp. 100-101.

130. Ibid., p. 100.

131. SPF is one of a cluster of doctrines Lakatos calls dogmatic falsificationism. Lakatos counts anyone a dogmatist, however, who believes there is contingent knowledge - see his "Falsification", pp. 95-97.

132. William Berkson, in "Lakatos One and Lakatos Two: An Appreciation", in *Essays in Memory of Imre Lakatos*, eds. Robert S. Cohen, Paul K. Feyerabend, and Max W. Wartofsky, Boston Studies in the Philosophy of Science, vol. 39 (Dordrecht: D. Reidel Publishing Co., 1976), makes this point but comments, "I still don't understand how Lakatos could make this mistake" (p. 52). I try to explain why above.

133. Popper, "Replies", bk. 2: 998. He makes a similar claim in a reply to Grover Maxwell - see "Replies", bk. 2: 1037. The latter claim is immediately preceded (p. 1037) by the following curious statement:

> [Maxwell says] that, in my view, a theory is refuted by deducing from it an "(observation) statement inconsistent with [a] . . . potential falsifier . . . [which] turns out to be true". This is not my view. According to me the "falsifier" is the *conjunction* of Maxwell's "(observation) statement" and his "potential falsifier"; in other words, the "falsifier" on its own contradicts the theory.

But the conjunction of propositions to which Popper refers above is logically false. Also, the motley statement attributed to Maxwell is nowhere to be found in his "Corroboration without Demarcation" in *The Philosophy of Karl Popper*, ed. Paul A. Schilpp, The Library of Living Philosophers, vol. 14 (La Salle, Ill.: Open Court Publishing Co., 1974), bk. 1: 292-321.

134. Popper, "Replies", bk. 2: 1005.

135. Stove, *Popper and After*, pp. 92-93. See also Putnam, "Corroboration", bk.1: 225.

136. Popper, *Logic*, p. 101 (n.*1).

137. Popper, "Replies", bk. 2: 987.

138. Popper, *Conjectures*, p. 267; see also his *Logic*, p. 103.

139. Some stock universal generalisations do not have potential falsifiers which contain only observable predicates though they themselves do, for example, 'All men are mortal'.

140. Putnam, "Corroboration", p. 225.

141. Ibid.

142. Popper, "Replies", bk.2: 994 and 997. John L. Mackie, in "Failures in Criticism: Popper and his Commentators", *British Journal for the Philosophy of Science* 29 (1978): 363, remarks that "Putnam's article . . . is a disaster: his main 'criticism' of Popper fails to take account of Popper's actual argument, as Popper has no difficulty in showing in his reply". But this is not correct, for what Popper takes to be Putnam's main criticism is that SPF is false - see "Replies", bk. 2: 994 and 997. And it is *Popper* who overlooks his own argument on this point, as we have seen above (p. 35). He continues to overlook his own arguments (on other matters) elsewhere in his reply to Putnam too, for example, he identifies a "falsifying hypothesis" that would refute P with a rival theory to T - see "Replies", bk. 2: 995.

143. Ibid., bk. 2: 997-98.

144. Ibid., bk. 2: 997.

145. Ibid.

146. Popper, *Logic*, pp. 101-2.

147. Popper, *Unended Quest*, sec. 8; and *Conjectures*, pp. 33-38.

148. Popper, *Conjectures*, p. 35.

149. It can be important, however, merely to determine what some consequences of our current system of beliefs are so as to know what we are also committed to believe, or to determine what we should believe are the likely consequences of some event whose occurrence we would not welcome. On the latter point, consider the recent research on the probable climatic and social effects of a large scale nuclear war. The principal scientific purpose of such research is merely to predict what, for all we know and assume, such effects would be. No researcher, one hopes, would ever be moved to try to test such predictions.

150. Popper, *Postscript*, p. 188.

151. In *Unended Quest*, p. 37, Popper states: "In May, 1919, Einstein's eclipse predictions were successfully tested by two British expeditions. With these tests a new theory of gravitation and a new cosmology suddenly appeared . . . as a real improvement on Newton - a better approximation to the truth."

152. Popper, "Replies", bk. 2: 1035.

153. Ibid.

154. For discussion, see p. 81 and chap. 3 below.

155. Thomas S. Kuhn," Logic of Discovery or Psychology of Research?", in *Criticism and the Growth of Knowledge*, eds. Imre Lakatos and Alan E. Musgrave (Cambridge: Cambridge University Press), pp. 5-7; Putnam, "Corroboration", p. 234.

156. Popper, *Logic*, p. 76.

157. Popper, "Replies", bk. 2: 982.

158. See, for example, Max Deutscher, "What *is* Popper's Problem of an Empirical Basis?", *Australasian Journal of Philosophy* 47 (December 1969): 354; and Burke, *Popper*, p. 128.

Chapter Two

Popper on 'Avoidance' of Refutation

2.1 Introduction

Popper is inclined to describe any case of predictive failure which does not lead to the refutation of the theory from which that prediction was derived as one in which a refutation has been avoided or evaded, or in which the theory concerned has been rescued from or immunized against a refutation. To reinforce his portrayal of the practice of refuting or of attempting to refute major theories as the rational core of science, Popper depicts the so-called practice of avoiding such refutations as the exception to the rule, and much of what he calls avoidance he dismisses as unscientific or pseudo-scientific.[1] It should already be clear from my remarks about tests in 1.6 that I do not agree with this judgement of Popper's.

In this chapter I shall criticise Popper's notion of avoidance of refutation. I conclude that his writings on this notion are incoherent. Popper, and others, are driven to believe that refutations can be avoided or theories protected from refutations because they conflate sentences with propositions, or propositions with beliefs (and often both). Moreover, I shall argue that relativism is a consequence of Popper's notion of refutation, and his desire not to be saddled with this consequence also pushes him to claim that refutations can be avoided. This desire is further evidence that Popper would like to endorse the view that refutations *are* disproofs (2.2). *Ad hoc* hypotheses, according to Popper and several others, are the most important of the means of avoiding a refutation. I shall argue, however, that many hypotheses that are thought to comply with Popper's definition of an *ad hoc* hypothesis do not do so. It appears otherwise to those who think so largely because of Popper's use of rhetoric. Moreover, the desire of such people to make Popper's view plausible leads them to describe as *ad hoc* some hypotheses that for *other* reasons are unacceptable. I conclude that to know that a hypothesis is *ad hoc* in Popper's sense does not illuminate scientific practice; worse still, because his account of such hypotheses is incoherent one can be misled into thinking that many a hypothesis is *ad hoc*, or otherwise vaguely undesirable, when this is not so (2.3). Lastly, Popper has attempted to explicate *ad hoc*ness in terms of certain undesirable, or allegedly undesirable, properties of hypotheses or the explanations they would provide. The first such property is circularity, which is undesirable; the second such property is reduction in empirical content, which is not undesirable. In the former case, I argue that non-circularity is clearly preferable to non-*ad hoc*ness as a criterion for a satisfactory explanation or *explanans*, as the case may be (2.4). In the latter case, I argue that Popper and others are barking up the wrong tree (2.5).

2.2 The Logic and Rhetoric of Avoidance

Can a theory *avoid* a refutation as, say, someone might avoid a missile which is headed in that person's direction? Someone, S, avoids a missile, M, only if S might have been hit by M and S is not hit by M. But it is necessarily false either that a true theory might have been refuted or that a false theory is not refuted by any refutation of it that is produced. It follows that *no theory, true or false, can avoid any refutation*. If there can be no refutation of any true theory there can be no refutation that any true theory avoids or is rescued from. On the other hand, if a false theory is not refuted by some counter-argument for it then that argument is not, whatever else it may be, a refutation of that theory. Unlike missiles, which sometimes do miss their targets, those things which are proofs do not, else they would not be proofs.

It might be said, however, that in being true a theory is spared or avoids the possibility of refutation altogether. But this is a different point from that which Popper would make, which is that particular refutations can be avoided. Alternatively, someone might remark of a false theory that it had avoided a refutation on some occasion when the point is that but for some untoward event, such as a malfunction in a necessary piece of the experimental apparatus, a refutation of that theory would have been produced on that occasion. This point may be of some historical interest, but it has no epistemic significance. We know that the refutation *would* have been so produced only if we know that the theory in question has since been refuted by means of such an experiment. Similarly, if some apparent counter-example should turn out not to be genuine no *refutation* is thereby avoided, though the appearance of one has been dispelled.

Whether or not a theory can avoid a $refutation_p$, however, is another question, and one which, amongst other things, I shall address below.

There are two logically distinct procedures by which, on Popper's account, a refutation (or refutation$_p$) can be avoided. I shall call these procedures 'rejecting the antecedent' and 'rejecting the consequent', and begin by discussing the latter for it is the less important of the two.

My criticism of rejecting the consequent stands or falls whichever concept of refutation - discorroboration or disproof - is employed, for this criticism is a logical one and there is no logical difference between these two concepts. So I shall, for simplicity, not distinguish refutation from refutation$_p$ in discussing this procedure. Also, for simplicity, let *R* stand for the antecedent in

(O & A) --> *~T*

Thus,

R --> *~T*

Now to avoid the refutation of T via R we can, according to Popper, formulate a new theory, T' , such that

R -/-> *~T'*

and, for the sake of plausibility, typically,

T --> *T'*

We then accept *T'*.

But if *T* *is* refuted by *R* how has *T* 's refutation been avoided by this procedure? The fact that *R* does not entail *~T'* does not alter the fact that *R* *does* entail *~T*. Moreover, if we know or continue to accept *R* we ought to reject *T*. It may be that by rejecting *T* for *T'* *we* avoid believing something which is contrary to fact, but that is a separate matter. And in any case what would be rationally undesirable about having one fewer counter-example to our beliefs? Popper's objection here is partly that we should not retreat to believing or accepting *T'* , for *T'* has, or is thought to have, less empirical content than *T*. But this too is a separate matter, and one that I shall deal with in 2.5 below.

Consider this case of rejecting the consequent which Popper offers:

> The case of Marxism is interesting. As I pointed out in my *Open Society*, one may regard Marx's theory as refuted by events that occurred during the Russian Revolution. . . . The reinterpretation of Marx's theory of revolution to evade this falsification immunized it against further attacks, transforming it into the vulgar-Marxist (or socioanalytic) theory which tells us [only] that the "economic motive" and the class struggle pervade social life.[2]

By allowing himself some flexibility here as to the referent of 'Marx's theory of revolution', Popper fosters the impression that since so-called vulgar-Marxism, *T'* , is not refuted by our knowledge of certain events during the Russian Revolution, R, then Marx's original theory of revolution, T, is itself somehow no longer refuted. The referential equivocation on which this impression depends occurs in the last sentence above. Popper's claim concerning immunity from further attacks which he expresses there would be true only if the first instance of 'it' in that sentence referred to T', but the sentence as a whole makes sense only if 'it' in both instances refers to T.

The effect of this equivocation is enhanced by the metaphor of immunization - a metaphor, incidentally, that is redolent of induction. If I immunize my cat against feline enteritis, for example, I make this animal safe from a disease to which it is otherwise vulnerable. This process of immunization, however, is one of bringing about certain changes in the properties of one and the same cat; it is not one of finding some cat which is not so vulnerable and substituting it for one which is. Like the avoidance and rescue metaphors, the immunization metaphor makes the false suggestion that T and T' are identical, and this encourages us to believe that T is not, after all, refuted by R since T' is not. To further encourage this belief, Popper disingenuously avoids stating or implying above that Marx's theory *was* refuted by the events of the Russian Revolution. This is in striking contrast to what he actually believes, for in a footnote to the very passage of *The Open Society and its Enemies* to which he refers above he describes these events as a "striking refutation" of that theory.[3] But not only does *Popper* believe that this theory was so refuted, his vulgar-Marxists must do so too else why would they set about reinterpreting Marx? The avoidance Popper has in mind here is rather that by reinterpreting Marx vulgar-Marxists avoid admitting that Marx was wrong, but this is only a matter for the historians. Clearly, even if Marx is reinterpreted such that certain events are not a counter-example to his *reinterpreted* position, this is irrelevant to whether or not those events are a counter-example to his *original* position, or to what Popper takes to be that position.

The belief that refutation can be avoided in this manner is not, however, an idiosyncratic one of Popper's. It is common in the literature to be told that a theory can avoid or evade a refutation through being added to, or watered down, or otherwise modified.[4] Larry Laudan, for example, states:

> Whenever a theory encounters a refuting instance, it is possible to modify the interpretative rules associated with the theory so as to disarm the "refuting" data. If, for instance, we have a theory, T, that "all planets move in ellipses" and then discover a satellite of the sun, S, which moves in a circle, we can always modify the interpretative rules governing the term "planet" so as to exclude S, thus preserving our theory intact and eliminating any appearance of refutation.[5]

But even if S were a counter example to T - it is not since a circle is a special case of an ellipse - changing Laudan's interpretive rules would not "disarm the 'refuting' data". If 'planet' now meant, say, 'satellite of a sun with a mass greater than the mass of S', it is no longer T that we assert when we write or utter with assertive intent the sentence, 'All planets move in ellipses,' for we are now asserting not that all satellites of the sun move in ellipses but that all satellites of the Sun heavier than S do so, which is a new proposition, T'. What is preserved intact by Laudan's move? It is not T for a *proposition* is not, to use Laudan's expression, "cognitively threatened" by refuting data, though the corresponding *belief* may be, as it is above. However, nor do we preserve the *belief* that T ; for of course with the discovery of S we no longer believe that T (unless we allow ourselves to equivocate, as Laudan does, between T and T' , in which case our beliefs in this regard will be confused). What *is* preserved by Laudan's move is the belief, or at least the commitment to believe, that one of T 's entailments, namely that which we now call T' , is true.

Popper and Lauden conflate sentences with propositions above, so they do not notice that different propositions can be expressed by the same sentence. Thus, for example, Popper says:

> A biologist offers the conjecture that all swans are white. When black swans are discovered in Australia, he says that it is not refuted. He insists that these black swans are a new kind of bird since it is *part of the defining property* of a swan that it is white. In other words, he can escape the refutation, though I think that he is likely to learn more if he admits that he was wrong.[6]

If 'swan' means, in part, 'white' for this biologist then, in uttering the sentence 'All swans are white', he is asserting an analyticity, which fact clearly needs to be pointed out to him. No refutation of the contingent proposition that all swans are white is avoided by assigning a novel meaning to the word 'swan' such that in uttering the sentence, 'All swans are white,' a logical truth is expressed. The fact that the same *sentence* can be used to express a truth as it can a falsehood is what leads Popper and this biologist to believe that a refutation has been avoided above.

One final point here: if we were to 'add' some hypothesis, H , to T in the hope of thereby avoiding a refutation of T , as we are often advised to do, we would be even more disappointed, for if T is refuted it is a simple matter to refute the conjunction of T and H since

$$\sim T \; \text{-->} \; \sim(T \;\&\; H)$$

Turning now to rejecting the antecedent, given again that

$$R \dashrightarrow {\sim}T$$

we can formulate some proposition, or conjunction of propositions, R', such that

$$R' \text{ -/-> } {\sim}T$$

and

$$R' \text{ --> } {\sim}R$$

If we accept R' we should reject R. If we do so, then we no longer have the reason for rejecting T which accepting R originally supplied. So Popper believes that T's refutation has thereby been avoided.

Reviving the distinction between the two concepts of refutation we are concerned with, it is clear that the *refutation* of T cannot be avoided by this procedure. If we *know* that R, as we would need to if we are to refute T, we can immediately refute R' since

$$R \text{ --> } {\sim}R'$$

We are thus being asked above to reject what we know in favour of something that we can recognise is false.

But what if T is merely refuted$_p$? Consider the following case which Popper discusses:

> There are what one might call "unsophisticated" theories like "All swans are white" or the geocentric "All stars other than planets move in circles". Kepler's laws may be included (though they are in many senses highly sophisticated). These theories are falsifiable, though falsifications can, of course, be evaded: immunization is *always* possible . But the evasion would usually be dishonest: it would consist, say, in denying that a black swan was a swan, or that it was black; or that a non-Keplerian planet was a planet.[7]

But what is the cognitive significance of such denials? Consider a case in which I know or believe that I am harbouring a fugitive. If I were to deny that this is so, say, to my neighbours or to whoever is in pursuit of this person, this denial would make no difference to my cognitive attitude to the proposition that I am harbouring that fugitive, for I would still know or believe that I was doing so. Indeed, it is in part *because* I know or believe this fact that I would deny it.

Notice that Popper's intuition would prompt him to *agree* with my analysis of this case, for he admits that the denials he contemplates would "usually be dishonest". This admission shows that he too does not believe that such denials make any difference to the cognitive attitudes to what is denied by those who utter them for, necessarily, someone *dishonestly* denies that p only if that person knows or believes that p. Also, notice that in criticising someone who denied that "a black swan was a swan", Popper is not implying that this person's mistake is that he or she has asserted a logical falsehood. Rather, Popper means that this person is denying that something which *everyone else concerned knows or can recognize is a black swan* is even a swan. Popper is correct in thinking that any observer who would utter such denials is, if not incompetent or distracted, then dishonest. But this only shows that he cannot swallow his conventionalist account of 'seeing that'. As Ayer would point out, Popper cannot distinguish a case of dishonesty from one in which the practitioner freely decides not to accept the decision of his or her colleagues that the wet and feathery thing in front of them is a swan.[8]

On Popper's conventionalist account of basic knowledge, then, if R' is a basic statement, as it is in this case, we have merely to agree that R' (and to infer that $\sim R$ from R') in order to refute$_p$ R, and thus to *un*refute$_p$ T. So the question arises:

Refutation$_p$: where is *thy* sting?

Introducing corroboration does not help, for even if R is well corroborated and accepted by a scientific community, C, at some time, t_0, R' can still be well corroborated and accepted by C at some later time, t_1. Corroboration is only a measure of performance in tests, and R may perform well in tests to t_0 but poorly to t_1.. As Popper says:

> We can never simply say of a statement that it is as such, or in itself, 'corroborated' (in the way in which we may say that it is 'true'). We can only say that it is *corroborated with respect to some system of basic statements* - a system accepted up to a particular point in time. 'The corroboration which a theory has received up to yesterday' is *logically not identical* with 'the corroboration which a theory has received up to today'. Thus we must attach a subscript, as it were, to every appraisal of corroboration - a subscript characterising the system of basic statements to which the corroboration relates (for example, by the date of its acceptance).
>
> Corroboration is therefore not a 'truth value'; that is, it cannot be placed on a par with the concepts 'true' and 'false' (which are free from temporal subscripts); for to one and the same statement there may be any number of different corroboration values.9 (Latter emphasis mine.)

Thus T can be unrefuted$_p$ at t_1 because C *then* knows$_p$ that $\sim R$. It is also possible, of course, that at some time after t_1, R may again be well corroborated, and so reaccepted by C. If so, then the hapless T would, once again, be refuted$_p$ by R. But it is not true that T *avoided* being refuted$_p$, either at t_0 or at t_1. If any theory is *ever* refuted$_p$, T is refuted$_p$ at t_0. How T fares in any test after t_0 makes no difference to how T fared, or to how it was agreed that T fared, in any test before t_0. R may be poorly corroborated by the research done *after* t_0 but this does not alter the fact that R was well corroborated by the research done *before* t_0. C may reject R at t_1, but C did accept R at t_0.

To return to Popper's simple case of rejecting the antecedent, the avoidance which disturbs him there is merely that the *person* who utters the dishonest denial thereby avoids admitting, if not revealing, something that he or she knows or believes to be the case. This is an instance of the same kind of avoidance that vulgar-Marxists allegedly indulge. Moreover, what is possible to immunize or protect is not a proposition but a *belief*, though Popper's case does not illustrate even how this can be achieved. But suppose that I habitually refuse or find some excuse not to examine any counter-argument that is produced for one of my pet theories such that the strength of my belief that this theory is true is preserved intact by this avoidance behaviour. I have thereby successfully immunized or protected this belief from such arguments by my dogmatism. I have done nothing, however, to immunize or protect *what* I believe from refutation, even in Popper's diminished sense. Someone with a more open mind than I possess, who took the trouble to examine one or other of these counter-arguments, may discover that one or other of them is indeed sufficient to refute my pet theory, and proceed to do so. Whether or not I am moved by any such disproof is beside the point.

The premise that such protective behaviour or dogmatism is always possible is a variation on the theme that mistakes are always possible, which I examined earlier (p. 8). This premise likewise fails to support the skeptical conclusion drawn from it, namely, that no belief - or theory, if the two are conflated - can be refuted, though Quine, for example, seems to believe otherwise.[10] Popper naturally resists such skepticism when it is directed against *refutation*, even though it is an argument of the same kind which takes him from fallibilism to skepticism about knowledge. Quine says that "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system".[11] But it does not follow that because we *can* thus behave dogmatically that we cannot or do not behave otherwise, so this premise of Quine's does not support his skepticism about refutation.

Quine, however, goes further. From the premise:

> Any statement can be held true come what may,

he concludes:

> It becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements, which hold come what may.[12]

But even if synthetic statements, or what we like to call synthetic statements, were things of the sort that one could protect from experience, it does not follow that they would be no different from statements which had no need for such protection in the first place. In any case, there is a slide in Quine's argument from, 'These statements are claimed or believed true' to 'These statements are true' which his argument largely depends upon for its plausibility, and which is obscured by the presence of a meaning shift from 'held' to 'hold'. 'Held' means 'claimed' or perhaps 'believed', whereas 'hold' means 'obtain' or 'are true'. Moreover, if 'held' does mean 'believed' above, and this is at least its typical or central meaning, then there are many statements that most people could *not* hold true come what does. For example, if I mistake a shadow for my cat then, necessarily, once I look again and see that it is a shadow or I remember that my cat is dead, I do not continue to believe that the shadow I thought was my cat is my cat.

Whereas Quine would collapse the analytic/synthetic distinction, Harold Brown, for example, thinks that we need to distinguish a *third* category of proposition to accommodate at least some of the synthetic propositions which, as Quine would put it, have been held true come what may.[13] Following Kuhn, the propositions Brown has in mind are what he calls 'paradigmatic propositions',[14] such as Newton's laws of motion or his law of gravitation. Is Brown's new category of proposition a discovery, or a superfluity? He points out that Newton's laws are not analytic because counter-instances to them are logically possible. "On the other hand", says Brown, "such propositions are not ordinary empirical propositions, exactly because they are protected from straightforward empirical refutation".[15] Whilst he holds back here from claiming that a new category of proposition is needed, he soon takes the plunge, declaring that paradigmatic propositions "constitute an epistemically distinct class in that they do not fit the traditional division of all propositions into a priori [sic] and empirical".[16]

As with Quine, however, it is merely Brown's conflation of the belief that p with p that has led him to the conclusion that a new class of *proposition* is called for. Is there some problem, then, with the retention of so-called paradigmatic beliefs in the face of counter-evidence or counter-arguments which needs to be addressed? Let me just say this for now. If I know that I know that p there is no dogmatism, no protection racket, involved in my holding p true *come what may*. Even if I know only that my belief that p is well supported there need be no dogmatism in my holding p true come much of what may. Since propositions are generally counted as paradigmatic only when they are well supported, there need be nothing unusual, much less irrational, therefore, in the longevity of such beliefs. Popper's epistemology, however, makes the refutation of major theories in science seem so easy that the longevity of the corresponding beliefs requires explanation. Has Brown been misled, therefore, into thinking that such theories (since he conflates them with beliefs) *must* somehow be protected if the stings from the swarm of would-be refutations Popper has led him to expect prove to be anything but fatal? We shall see, in the following chapters.

With the removal of truth from the conditions he thinks necessary for knowledge, Popper becomes a relativist about knowledge, and hence about refutation. Someone is a relativist about knowledge, an epistemic relativist, who believes or implies that a proposition that is known by one person can be inconsistent with a proposition that is known by someone else, or by the same person. Epistemic relativists do not usually believe that this is true of *every* two inconsistent propositions which figure in our beliefs, or which could do so. Rather, they are motivated to count as knowledge those strong or important beliefs that are held by the members of a society or social group, or by numerous individuals at one time, and a *consequence* of this is that some of the beliefs they call knowledge will be mutually inconsistent. (As things go, of course, many such beliefs will be.)

A typical belief of an epistemic relativist is, for example, that whilst white nutritionists know that bad diets cause much sickness in Aboriginal communities, the Aborigines themselves know that such sickness is caused by bad spirts.[17] Popper would reject this sort of cultural relativism outright, and in a way that would make him appear not to be a relativist for he would side squarely with the nutritionists. (He would also probably count only the nutritionists' beliefs here as 'appropriately formed' in his terms, for only they are supposedly the product of a critical attitude.)[18] Moreover, Popper is not, as he often emphasises, a relativist about truth values, for he does not believe that the truth value of a proposition is relative to, say, cognitive attitudes or times, as indeed he makes clear above (p. 59).[19] For all that, he *is* a relativist about knowledge because, on his account, C can know that $\sim R$ at t_1 even though C knew that R at t_0. For Popper, T can be *un*refuted at t_1 even though T was refuted at t_0. Thus, scientific knowledge is (non-trivially) relative to times. Popper makes much of the fact that successive theories in the evolution of a scientific discipline - for example, mechanical theories in physics - are mutually inconsistent, and he does not balk at describing such theories as scientific knowledge.[20]

Since T can be refuted$_p$ at t_0 and unrefuted$_p$ at t_1, it is only Popper's desire for the plausibility of a non-relativist position that prevents him from recognizing, much less pointing out, that on his unappealing account a refutation is, metaphorically speaking, something which can be defeated, quashed, or overturned, or from which a theory can be revived or resurrected. The metaphors of avoidance and immunization, however, offer the compensation of suggesting that the theory concerned was *not* in any sense refuted, which talk of defeated refutations or resurrected theories would not do. And since there is no requirement to *un*do what has not been done, Popper can thus give the impression that he is in agreement with the non-relativist on the point, which is a crucial one, that we cannot or do not *un*refute any theory.

In addition to his use of the faulty metaphors of avoidance and immunization which obscure his relativism, Popper exploits, if sometimes unthinkingly, such resources of language as weak cognitive attitude modifiers, modal auxiliaries, and tenses to the same end. Consider again the sentence:

> (1) As I pointed out in my *Open Society*, one may regard Marx's theory as refuted by events that occurred during the Russian Revolution,[21]

and this sentence which occurs close by (1):

> (2) The observed motion of Uranus might have been regarded as a falsification of Newton's theory.[22]

Firstly, the use of the weak cognitive attitude modifier 'regard as' ensures that such propositions as are expressed by sentences like (1) and (2) do *not* imply that the theories concerned are or were in any sense refuted. So not only is Popper's task of explaining how a refutation was avoided made easier if he refrains from implying that there was one to avoid, but, once again, he will appear to be opposed to relativism if he does not concede that on his account those concerned were in some sense engaged in unrefuting a theory. This is obviously so in the case of Marx's theory above since he has elsewhere described the events of the Russian Revolution as a "striking refutation" of that theory.[23] It is less obvious in the case of Newton's theory above because Popper does not describe as a refutation any argument that he *would* otherwise do so but for the fact that it has since been defeated. (By 'defeated' I mean that the argument is, if not demonstrably unsound, at least rejected on reasonable grounds.) Now the fact that an argument has been defeated *is* a good reason for not describing that argument as a refutation, but it is not one that is available to Popper. Even if we overlook his commitment to SPF, which does waver, he nowhere implies that the counter-argument for Newton's theory from the deviant motion of Uranus was not well corroborated.[24] The important point is that Popper only counts as refutations those arguments which, in his estimation at least, are *undefeated* refutations$_p$, and the job of the weak cognitive attitude modifier in this context is to give the appearance of relieving him of the commitment to counting *defeated* refutations$_p$ amongst their number and so disguise his relativism.

Secondly, the use of a modal auxiliary such as 'may' in (1) and 'might' in (2) ensures that such propositions as are expressed by sentences like (1) and (2) do *not* imply that the theories concerned are or ought to be in any sense even *regarded* as refuted. The rhetorical purpose of removing this implication is to make it easier still for the reader to come to regard the theories concerned as in some sense unrefuted.

But more is needed for suppose that, in place of (1), Popper had written:

> (1') As I pointed out in my *Open Society*, one might have regarded Marx's theory as refuted by events that occurred during the Russian Revolution,

or, in place of (2):

> (2') The observed motion of Uranus may be regarded as a falsification of Newton's theory.

The reader would be more likely in each case to twig that there was something odd about Popper's view of refutation - in short, that he was a relativist. (1') would be likely to jog the reader to ask: "But should Marx's theory not *still* be regarded as so refuted?" Once that happens, how is Popper to convince the reader to regard the theory as in any sense *un*refuted by the actions of those vulgar-Marxists? (2') invites one of two disagreeable responses, depending on how 'may' is construed. One such response would be, "I agree that one *may* [can] regard Newton's theory so but as this would be a *mistake* - the theory was not refuted - why are we not being advised against doing so?" The other such response (which 'might' or 'can' would not evoke) would be, "One *may not* [is not entitled to] regard the theory as refuted, since it was not refuted." But however 'may' is construed, once the reader *grasps* that there is no refutation of Newton's theory in this case, the claim about immunization loses its foothold. So Popper switches to the past tense in (2) and the mistake of regarding this theory as refuted does not then appear to be one that he is committed to making or is urging upon us, for that we do not *now* regard a theory as refuted by some argument is consistent with our having done so.

If 'may' in (1) and 'might' in (2) were each replaced with, say, 'should', as Popper ought to do to remain consistent with his epistemology, it would be difficult to convince the reader in either case that the theory concerned had avoided a refutation.

Popper is a relativist who doesn't want to be a relativist, as his use of language above reveals. It is one thing, however, to obscure one's relativism in this manner, it is another to claim a distinction in doing so which no skeptic such as himself is entitled to. I have in mind the distinction between a refutation and an apparent refutation. The notion of an apparent refutation is one that Popper, and others, invoke when it is a defeated refutation$_p$ they have in mind but want to avoid the appearance of relativism. So they describe the counter-argument for Newton's theory cited above as an apparent refutation or a *prima facie* refutation of that theory.[25] When they do so, however, they are invoking the notion of refutation as disproof.

Let us consider the following simple case. Suppose that a group of professional ornithologists agree that there is a tawny frogmouth at a certain spot in a nearby tree, p , but later discover, to their chagrin, that what they had thought was a frogmouth is in fact an extraordinarily shaped branch. In relation to this alleged sighting, the group reports:

> "We were wrong, though it did appear as though we were right."

This report presupposes a distinction between 'knowing that' and 'apparently knowing that'. These ornithologists are distinguishing, or claiming to distinguish, between knowing that $\sim p$ and having apparently known that p. They are conceding that they did not *know* that p , though they would be correct in believing that they did know_p that p. A Popperian member of this group, however, with a taste for consistency, could only report:

> "It appears that we were wrong, though it did appear as though we were right."

To defend the distinction his colleagues have drawn this ornithologist would have to relinquish Popper's belief that knowledge is conjectural or provisional. Popperians believe, in other words, that we have at best only apparent knowledge, and so the above distinction lies beyond *their* grasp (whether or not they are right in thinking it lies beyond everybody else's too). When Popperians talk about knowledge they do not mean that their well corroborated agreements only *appear* to be so, they mean that such agreements appear to be true.

Thus Popper cannot distinguish a refutation from an apparent refutation, a point which Musgrave inadvertantly concedes when, with just such cases as the case of the mistaken ornithologists in mind, he asks:

> Can fallible falsifications, falsifications which are not disproofs, play the significant role in science which falsificationists attribute to them? *Or to put it another way:* what should scientists do when confronted with a clash between a theory and some *apparently* respectable counterevidence?[26] (Emphasis mine.)

Musgrave believes that the questions he asks above reveal "what we might call the *problem of falsification*."[27] What these questions reveal, however, is the problem of *falsification*$_p$. Musgrave goes on to say, for example, that he thinks that "it is a perfectly rational policy, when faced with a clash between a scientific hypothesis and some *apparently respectable* evidence, to try to explain away the evidence."[28] (Emphasis mine.) In other words, it is perfectly rational on Popper's account of refutation to try to defeat a refutation, to try to unrefute a refuted proposition. What price a work entitled, *Conjectures and Apparent Refutations*?

2.3 The Logic and Rhetoric of *Ad Hoc* Hypotheses

An *ad hoc* hypothesis, according to Popper, is one of the rationally undesirable or unscientific means of avoiding a refutation, and it is the means he has most often attacked. The problem of *ad hoc* hypotheses or *ad hoc*ness is, or has been, a relatively popular topic in the philosophy of science generally. I shall argue, however, that in so far as Popper has had anything to do with this popularity it is undeserved. His ideas concerning *ad hoc* hypotheses are incoherent.

What is an *ad hoc* hypothesis? Recall that in rejecting the antecedent,

$R' \dashrightarrow \sim R$

where R is shorthand for $(O \& A)$. Now R' can take various forms, for example,

O'

where O' is a new test observation statement (obtained by repeating the test observation, or the entire test). Alternatively, R' may consist of

A'

where A' is a revised or wholly new set of auxiliary propositions; or, of course,

$O' \& A'$

Alternatively, R' may comprise some proposition which contradicts one of R's presuppositions, for example, the presupposition that the test was conducted by a sober or clear sighted practitioner. Any instance of R', however, must contain at least one proposition which is inconsistent with at least one proposition in R, or that is a presupposition of R. Now in Popper's scheme, as he describes it in "Replies", an instance of such a proposition in R' is one of two logically different kinds, namely, an auxiliary hypothesis and an *ad hoc* hypothesis.[29] Popper cites the familiar trans-Uranian planet hypothesis, introduced to remove the anomaly in Uranus's motion, as an instance of the former kind, and says, "I call a conjecture '*ad hoc*' if it is introduced (like this one) to explain a particular difficulty, but if (in contrast to this one) *it cannot be tested independently*."[30] Clearly, the hypothesis of an unknown planet is not *ad hoc* in this sense for it has potential falsifiers or testable consequences independently of any disturbances that such a planet would cause in the motion of a neighbouring planet. (The proposition in R this hypothesis contradicts, of which more later, is that Uranus's sensible perturbations are caused solely by the planets then known.)

In rejecting the consequent, *T'* would be *ad hoc* in the above sense if

$$(T' \,\&\, A) \; \text{-->} \; O$$

and *T'* was not independently testable of *O*. Note that in this case, as opposed to those we examined in 2.2 above,

$$T \; \text{-->} \; {\sim}T'$$

where *T* , the reader will recall, is the original theory for which *O* is anomalous.

Popper's attitude to both *ad hoc* and auxiliary hypotheses is negative because both would remove the empirical challenges which confront major theories. But since, *ex hypothesi*, *ad hoc* hypotheses alone fail to leave themselves open to challenges of just the same kind they alone are proscribed. This move is, as Musgrave points out, a strengthening of the demarcation criterion, for Popper is asking in such cases not merely for testability, but for independent testability.[31] Musgrave also claims that "generations of scientists have been suspicious of *ad hoc* explanations" and that "Popper gives their suspicion a very strong rationale".[32] We shall see.

The problem of *ad hoc* hypotheses is, in the first instance (and not just with Popper), a problem of *language*. The literal meaning of '*ad hoc*' in English is 'for this or the particular purpose'.[33] The expression is ordinarily used to describe something that is designed or adopted only so as to satisfy some narrow or limited requirement, or set of requirements. I shall call this ordinary English use of '*ad hoc*', '*ad* hoc_{oe}'. An *ad* hoc_{oe} measure is not a revolutionary initiative but a conservative response; it is not a major innovation but a minor addition or modification to some established thing. Such a measure is typically justified on the ground of expediency rather than by any appeal to basic principles or general considerations (though one may have a principle that certain cases should be dealt with in an *ad hoc* fashion). The distinction between a measure that is *ad* hoc_{oe} and one that is not, however, is clearly very much like the bald/hairy distinction. That is to say, the boundary is not well-defined.

'*Ad hoc*' is often used pejoratively in ordinary discourse, in which case it also means, 'arbitrary', 'fabricated', 'makeshift', 'stop-gap', or the like, and anyone resorting to such measures would be thought of as muddling on, tinkering with something that should be overhauled or scrapped, or engaged in what my old physics teacher used to call ' jiggery-pokery'.

Reviewing the use of '*ad hoc*' by scientists, Gerald Holton concludes that a hypothesis will be described as *ad hoc* by them, irrespective of any of its "logical properties", if it is specifically created to account for some "bothersome result or feature" of a theory.[34] Used pejoratively, and he maintains that it need not be, '*ad hoc*' also means 'artificial', 'cooked up', 'contrived', 'implausible', 'unreasonable' 'unnecessary', 'ugly', and so on.[35] On Holton's account, then, the use of this term in scientific discourse shows no discernible or interesting difference from ordinary English.

Popper's use of '*ad hoc*' in '*ad hoc* hypothesis', which I shall call *ad* hoc_p, clearly *retains* its ordinary English meaning, and since he disapproves of *ad* hoc_p hypotheses we can reasonably anticipate some of the pejorative shades of meaning that are often part of the ordinary use of that expression. For example, Popper says that "it is well known that *ad hoc* hypotheses are disliked by scientists: they are, at best, stop-gaps, not real aims".[36] The difference between '*ad* hoc_p' and '*ad* hoc_{oe}' (apart from the latter having referents other than propositions) is that '*ad* hoc_p' *also* means 'not independently testable'. On the face of it, then, one is entitled to be cautious, if not skeptical, of Musgrave's (unsubstantiated) claim that Popper has explicated something which has worried "generations of scientists". Holton, for example, does not believe that scientists are "likely to be much helped" by Popper's criterion.[37] In any event, the best that can be said for Popper's novel use of an ordinary English expression is that it is unfortunate, for two reasons. Firstly, since a hypothesis can be *ad* hoc_{oe} *without* being *ad* hoc_p, as is *any* auxiliary hypothesis on his definition of 'auxiliary hypothesis' above, this novel use of '*ad hoc*' simply invites equivocation if, as is the case, Popper draws no distinction between its two senses. Secondly, the meaning of '*ad* hoc_p' is a mixture of the psychological and the logical which, in theory at least, Popper usually and correctly insists upon separating. To describe a hypothesis as *ad hoc*, in Popper's sense, is to describe both the *purpose* of that hypothesis (to remove some anomaly), and to pick out one of its logical properties (non-independent testability).

It is possible of course to use '*ad hoc*' to mean '*ad* hoc_p' without bearing false witness against any hypothesis, committing any fallacy, or even failing to make one's meaning plain. (If this were not so, for example, I could not succeed in this criticism of Popper.) But it is not very likely that someone would avoid these pitfalls if that person continued to use '*ad hoc*' in the ordinary sense in contexts where the two are easily confused but without distinguishing between them. Those who champion Popper's analysis of *ad hoc* hypotheses often fare badly in this regard, as we shall see, and we can understand why it is likely they would do so by inspecting the following sample of Popper's writings:

> (1) It is always possible to find some way of evading falsification, for example, by introducing *ad hoc* an auxiliary hypothesis or by changing *ad hoc* a definition.[38]

(2) I also realized that we must not exclude all immunizations, not even all which introduced *ad hoc* auxiliary hypotheses. For example the observed motion of Uranus might have been regarded as a falsification of Newton's theory. Instead the auxiliary hypothesis of an outer planet was introduced *ad hoc*, thus immunizing the theory.[39]

(3) Suppose we were to produce an unbroken sequence of explanatory theories each of which would explain all the explicanda in its field, including the experiments which refuted its predecessors; each would also be independently testable by predicted new effects; yet each would be at once refuted when these predictions were put to the test. . . .

I assert that, in this case, we should feel that we were producing a sequence of theories which, in spite of their increasing degree of testability, were *ad hoc*, and that we were not getting any nearer to the truth. And indeed, this feeling may well be justified: this whole sequence of theories might easily be *ad hoc*. For if it is admitted that a theory may be <u>*ad hoc*</u> [emphasis mine] if it is not independently testable by experiments of a new kind but merely explains all the explicanda, including the experiments which refuted its predecessors, then it is clear that the mere fact that the theory is also independently testable cannot as such ensure that it is not *ad hoc*. This becomes clear if we consider that it is always possible, by a trivial stratagem, to make an *ad hoc* theory independently testable, *if we do not also require that it should pass the independent tests in question*: we merely have to connect it (conjunctively) in some way or other with any testable but not yet tested fantastic *ad hoc* prediction which may occur to us (or to some science fiction writer).

[This requirement] . . . is needed in order to eliminate trivial and other *ad hoc* theories.[40]

Both instances of '*ad hoc*' in (1) mean '*ad hoc*$_{oe}$' not '*ad hoc*$_{p}$', as anyone alive to this distinction or who pays attention to the syntax of (1) will recognise. The first instance of '*ad hoc*' in (2), however, is ambiguous, until one reads the remainder of the quotation when it becomes clear that, once again, both instances mean '*ad hoc*$_{oe}$'. But Popper equivocates in (3). In the third sentence of the second paragraph in (3), the first instance of '*ad hoc*' (emphasised) is '*ad hoc*$_{p}$' whilst the second is '*ad hoc*$_{oe}$' - as are all the remaining instances in (3). Popper backs away from acknowledging this meaning shift with the weak circumlocution "for if it is admitted that a theory may be '*ad hoc*'", when he means or would otherwise say, simply, "a theory is '*ad hoc*'". But his *point* is that some hypotheses that are not *ad hoc*$_p$ will yet strike us as *ad hoc*, in some vague ordinary pejorative sense of that word. If '*ad hoc*$_p$ hypothesis''is supposed to explicate '*ad hoc* hypothesis', as that term is understood in philosophical or scientific discourse, however, then Popper fails to notice or acknowledge that he is in the business of putting forward counter-examples to his own explication. The first case in (3) is that of a series of theories in which each theory satisfies the narrow or limited requirement of entailing (not explaining) the relevant known facts, but the wider requirement of explaining all the relevant facts is one that it fails to satisfy by failing at its first attempt in a novel application. A succession of such theories, each one doing little if anything it was not designed to do, *would* lead to the criticism that such theories or theorising was contrived, arbitrary, artificial, or the like. In the second case in (3), Popper recognizes that the particular requirement that a hypothesis, H, be independently testable can always be met by cooking up (as with grue-like predicates) some artificial hypothesis, $H \;\&\; p$, where p is any contingent proposition we please not entailed by H. Again, any such arbitrary hypothesis would very likely be described as *ad hoc*; but it is worth noting that it would *still* be so described even if it were to pass the independent test in question, that is, if p were found to be true.

In the light of the above inspection of Popper's use of '*ad hoc*', consider the following accounts of his position - one by Gerard Radnitzky, the other by Alan Musgrave, both of whom believe there is great merit in it.

In a long defence of Popper's methodology, Radnitzky claims that "the prohibition of immunization methods plays an important role in research" and that, on a wider stage, it is "indispensable in combating the pollution of the intellectual environment".[41] For all that, Radnitzky is stymied by Popper's semantics. He says that "introducing an additional hypothesis *ad hoc* is illegitimate" if, amongst other things, "this is done to preserve the theory from falsification".[42] Yet almost in the next breath he acknowledges that "the as yet unsolved difficulty consists in defining '*ad hoc*' objectively - to speak of the intention of the researcher [as Radnitzky has just done] would be to lapse back into psychologism".[43] Just so. He also says, falling under the spell Popper weaves in such passages as (1) and (2) above:

> A falsifiable theory can always be rescued from a falsification by adding *ad hoc* hypotheses. From this it follows that a general method, a policy, is scientific if and only if auxiliary hypotheses are not introduced *ad hoc* or, if such an introduction is expressly

> declared to be a temporary, purely heuristic measure, then the method is scientific if and only if these hypotheses are retained only if they lose their *ad hoc* character.
>
> . . . .
>
> Everything hinges upon whether or not the potential explanation can be processed into an authentic explanation, i.e., *whether or not the new component originally introduced* ad hoc *into the explanans REMAINS* ad hoc.[44]

When Radnitzky talks of a hypothesis introduced *ad hoc* which remains *ad hoc*, he means a hypothesis introduced for a particular purpose which is used only for that purpose. He considers the case in which, given

$(T \,\&\, A) \text{ --> } {\sim}O$

A' is formulated such that

$(T \,\&\, A') \text{ --> } O$

T & A' , says Radnitzky, only potentially explains *O* ; it is *ad hoc* and will remain so "until *A'* has been corroborated, until there is independent evidence for *A'* ".[45] An $ad\ hoc_p$ hypothesis, however, *cannot* acquire any independent evidence. Radnitzky has simply failed to distinguish Popper's *ad hoc* hypotheses from Popper's auxiliary hypotheses because he is misled by the fact that *both* are $ad\ hoc_{oe}$.. It is also not true, as Radnitzky believes, that *everything* hinges upon obtaining independent evidence for *A'*. The sighting of Neptune, for example, provided independent evidence for the trans-Uranian planet hypothesis and helped demonstrate that Uranus's perturbations were not due entirely to already known causes, but Uranus could have had *worse* residual perturbations once the effects of Neptune were included in the calculations.[46]

Let us turn now to Musgrave's account, he says:

> Popper has noticed that *some* explanations of apparent refutations are far too easy. A Newtonian could easily render his theory completely immune from criticism with the following explanation: "Any experimenter who claims to have refuted Newton is a liar who has fabricated his results out of envy of Newton's great achievement." It is also too easy to say: "Any experimenter who claims to have refuted Newton has simply betrayed his scientific incompetence." The trouble with these explanations is that the only evidence produced in support of the idea that the Newtonian critic is a liar, or is incompetent, is that he has criticized Newton. In other words, such explanations are not independently testable - they are *ad hoc*. To exclude them, Popper . . . introduces the rule that if we try to explain away a refutation as erroneous, we must do so in an independently testable fashion.[47]

It should be equally obvious, however, that the would-be explanations favoured by Musgrave's dogmatic or reactionary Newtonian are *also* independently testable. If they were not there would be no prospect of identifying any such unreliable practitioner. My dishonest claim to have calibrated some instrument, for example, or my poor attempt at doing so, may explain both why a subsequent reading made with it disagrees with a predicted value and why, say, the next person to use that instrument, much to her annoyance, is forced to recalibrate it. The latter fact would provide independent evidence of my unreliability in this regard. Since Musgrave later asserts that Newton's theory *was* refuted he must suppose that at least one critic was both honest and competent, and that the claims of this reactionary Newtonian are therefore false.[48] Nor can he believe, of course, that Newton's *theory* was rendered "completely immune from criticism" by such claims, though the corresponding belief of his reactionary physicist is another matter. Musgrave thinks that it is "perhaps worth noting" that sometimes experimenters *are* at fault, but that "the rule against *ad hoc*ness only means that when the idiosyncracies . . . of an experimenter *is* invoked [sic], other evidence must be produced apart from the fact that he has reached unwelcome results".[49] But a non-ad hocness rule implies only that such evidence should be producible, not that it should be produced (though that too would be desirable). Musgrave conflates, as Radnitzky probably does, the mere absence of independent evidence with the logical impossibility of obtaining any. Finally, it is ironic that Popper should be credited with having noticed that some "explanations of apparent refutations are far too easy". He is the one who is responsible for the false impression that all refutations are easy, and hence that such explanations - which are refutations of R - are easy. It is *Popper* who gets Musgrave into this bind, not the one who points the way out of it.

Popper rarely supplies, much less discusses, what he considers are instances of *ad hoc*$_p$ hypotheses. He once claimed that the Lorentz-Fitzgerald contraction hypothesis was *ad hoc*$_p$, but later accepted Adolf Grünbaum's criticism that it was not,[50] (though it has been widely regarded by scientists and philosophers as in some sense *ad hoc*.)[51] Popper then suggested, however, that perhaps this hypothesis illustrates "degrees of *ad hoc*ness".[52] But this is a false comparative; there can be degrees of independence, but no degrees of non-independence. Nothing can be more, or less, not independently F-able of anything else. Either a theory is not independently testable, or it is. I shall later discuss some of Popper's other suggestions for *ad hoc*$_p$ hypotheses, but consider first the following cases which Chalmers and Musgrave propose and discuss. All are *ad hoc*$_{oe}$; none is *ad hoc*$_p$.

Chalmers suggests that the hypothesis of the negative weight of phlogiston may be *ad hoc*.[53] Since phlogiston was believed to be released from any substance undergoing combustion or calcination, weight increase of the residue is anomalous for this hypothesis and the notion of the negative weight of phlogiston was introduced to explain such results.[54] But even if phlogiston chemists had been able to give a plausible explanation of how something can have negative weight, this hypothesis faced straightforward counter-examples. For example, some substances, notably wood, *decrease* in weight upon combustion.

Moreover, it is a fundamental mistake in the approach Chalmers takes to the problem of evidential support, that he assumes that p is evidence for q only if p is a logical consequence of q. For if, conversely, q is a logical consequence of p and one has good evidence (or counter-evidence) that p then one has good evidence (or counter-evidence) that q. Thus, once Antoine Lavoisier had provided good evidence that phlogiston did not exist there was good evidence that phlogiston did not have negative weight.[55]

Chalmers later strengthens the non-ad hocness criterion, as Popper sometimes does, though he does not notice that he does so. He remarks that the negative weight hypothesis would be *ad hoc* if "it led to no new tests".[56] By "new tests" Chalmers presumably means at least tests of a different sort from weighing substances before and after combustion. But it is not necessary to have a test of a different sort for any hypothesis if one of the usual sort will do, as it will here. In the case we considered earlier, Chalmers would presumably *not* think that we should entertain the suggestion that my calibration of the instrument in question was unreliable only if someone can come up with a different test for this suggestion from whatever is the routine method for checking the calibration of that instrument.

A second case proposed by Chalmers is the following:

> Having carefully observed the moon through his newly invented telescope, Galileo was able to report that the moon was not a smooth sphere but that its surface abounded in mountains and craters. His Aristotelian adversary had to admit that things did appear that way when he repeated the observations for himself. But the observations threatened a notion fundamental for many Aristotelians, namely, that all celestial bodies are perfect spheres. Galileo's rival defended his theory in the face of the apparent falsification in a way that was blatantly *ad hoc*. He suggested that there was an invisible substance on the moon, filling the craters and covering the mountains in such a way that the moon's shape was perfectly spherical. When Galileo inquired how the presence of the invisible substance might be detected, the reply was that there was no way in which it could be detected. There is no doubt, then, that the modified theory led to no new testable consequences and would be quite unacceptable to a falsificationist. An exasperated Galileo was able to show up the inadequacy of his rival's position in a characteristically witty way. He announced that he was prepared to admit that the invisible undetectable substance existed on the moon, but insisted that it was not distributed in the way suggested by his rival but in fact

> was piled up on top of the mountains so that they were many times higher than they appeared through the telescope. Galileo was able to outmanouvre his rival in the fruitless game of the invention of *ad hoc* devices for the protection of theories.[57]

Since Chalmers claims above only that the Aristotelian's hypothesis has "no new testable consequences" he believes or at least allows that it does have *some* testable consequence. But what can this consequence be, for he also thinks that the stuff Galileo's critic imagines as existing is undetectable? The failure of the Aristotelian's hypothesis, as Chalmers describes it, is not that, as it were, it grips the world at one (convenient) point and no other; its failure is that it does not grip at all. Bearing in mind that this hypothesis supplies no information about this would-be lunar stuff except that it is undetectable, what possible state of affairs can one describe that is not consistent with this suggestion? None.

Chalmers's account of this lunar hypothesis, however, is historically inaccurate. Galileo's Aristotelian critic was Lodovico della Colombe and he claimed that the moon was encased in a *smooth transparent crystal*, a claim which does make for an independently testable, albeit fanciful, hypothesis.[58] One could go mountain climbing on Galileo's moon but not on Colombe's. Interestingly, Galileo too overstated the case against Colombe's hypothesis, with a logical criticism similar to that of a modern Popperian. He remarked that this hypothesis "proposed nothing more than simple non-contradiction", which is false.[59] Colombe's hypothesis is no less contingent than, say, the canard that the moon is made of green cheese. Both of these hypotheses were refuted with the first lunar landing, if not before.[60]

What cognitive attitude, then, should a practitioner adopt towards a hypothesis such as Colombe's? As Galileo's acerbic reply indicates, the distribution of crystal that Colombe postulated was improbable in the extreme. It would be less so if some weight were given to Aristotle's belief that the moon was perfectly spherical, but this belief was itself supported only by (naked eye) observations of the sharpness of the moon's crescents, and in any case Colombe's hypothesis *presupposes* the efficacy of Galileo's telescopic observations of the lunar mountains and craters. Moreover, there was no *evidence* in the first place of the existence of this crystal. So there is no reason to *believe* Colombe's hypothesis. It is but one of an infinite number of such merely possible truths, and so poses no *special* problem. Is one to believe all such propositions? Clearly it would not be rational to do so, not least because many such possibilities are mutually inconsistent, or physically impossible.

The two cases Chalmers offers above are both instances of rejecting the antecedent. In the former case, the belief in the existence of phlogiston is preserved at the expense of one of the presuppositions of the counter-argument for it, namely, that weight is 'positive'. In the latter case, the belief that the moon is perfectly spherical is preserved by rejecting the presupposition that Galileo's observations provide sufficient information to conclude that the lunar surface is irregular. Let us turn now to a case of rejecting the consequent.

Late in the nineteenth century, Asaph Hall made a minute adjustment to the inverse square relation between force and distance in Newton's law of gravitation. He did so in order to accommodate the recalcitrant advance of Mercury's perihelion and without generating any anomaly where the original law did not do so, and he succeeded on both counts.[61] Musgrave describes Hall's modified 'law' - the Newton-Hall theory, as I shall call it - as "artificial and *ad hoc*" on one occasion,[62] and as "dangerously *ad hoc*" on another.[63] In so doing, however, Musgrave smuggles a new referent for 'not independently testable' into his analysis, for in this case 'not independently testable' does not refer to the relation between an *explanans* and its *explanandum* but to the relation between two *explananses*, namely, T' and T, which have a common *explanandum*, O. Clearly, any such modified 'law' is independently testable of the case of Mercury's advancing perihelion.

Was the Newton-Hall theory *ad hoc*, then, on Musgrave's novel definition of '*ad hoc*', which I shall distinguish as '*ad hoc*$_m$'? Whilst there was no crucial test of the two theories which could have been conducted at the time, so far as I am aware, that does not make the theory *ad hoc*$_m$ for clearly it has (numerous) *potential* falsifiers which Newton's law does not, and *vice versa*. It is a matter of how the world happens to be whether or not any test is a *practical* possibility. Some current research in physics, for example, is attempting to detect similarly minute discrepancies in the inverse square law.[64] In any event, the Newton-Hall theory so closely shadows the original that it will fail in any application where the latter is relatively wide of the mark as in, for example, the bending of light in strong gravitational fields. So the period of grace for Hall's modification was short for the first attempt to detect this phenomenon was made by Eddington in 1919, and with some success.[65]

Musgrave is fickle in his opposition to Hall's modification to Newton's law. Elsewhere, when his purpose is to criticise Lakatos for believing that scientists treated the law of gravitation as dogma, he says that "the list of those who contemplated such modifications adds up to quite a distinguished galaxy", though curiously Hall does not figure amongst them.[66] And he asks, "What if their modifications to the law of gravity had been successful?"[67] Quite so; Hall's modification *was* (temporarily) successful, at least in the way Musgrave's question presupposes. And is this "distinguished galaxy" amongst Musgrave's "generations of scientists" suspicious of such modifications?

Again, whilst the Newton-Hall theory can now be rejected, what cognitive attitude should one have adopted to it at the time? Musgrave quotes Simon Newcomb as arguing in 1910 that the only plausible explanation available for the anomaly in Mercury's orbit was some modification to the law of gravitation, and that he favoured Hall's as the simplest. Musgrave does not criticise Newcomb for holding this view. But one need not in any case *believe* that the Newton-Hall theory is true. One can truly and rationally believe that it is *instrumentally* superior to its rivals, and use it accordingly, without also believing that there is a gravitational force between any two masses corresponding to the value given by this theory. Moreover, there is a conceptual problem with the latter belief. There were, for example, accepted law-like generalisations of the same inverse square type for the luminance at a distance from a point source of light, similarly for the quantity of radiant heat from a point source or for the strength of an electrical or magnetic field. And if one conceives of gravity as something emanating from masses one would expect the gravitational force to accord with a law of this type, for the surface areas of uni-centred spheres increase with the square of their radii. "The inverse square law of force", wrote Laplace, "is that of all the emanations which come from a centre like that of light".[68]

Like Musgrave, I take Popper's independent testability criterion to be a refinement of his original testability or falsifiability criterion, and therefore to be a purely logical criterion. A theory is testable or falsifiable in this sense if and only if it has at least one potential falsifier, that is, at least one contrary which describes a logically possible state of affairs. This is clearly a special or technical sense of 'testable' - let us call it the *logical* sense of the term - for one could construe 'testable', as presumably many scientists do (or would do), in various stronger and partly pragmatic senses, of which there are basically two. A theory might reasonably be defined as testable only if it is *physically* possible to observe the state of affairs, or at least some trace thereof, described by a potential falsifier; or, more strongly still, as testable only if it is *practically* possible to observe such a state of affairs, or some trace thereof. Let us call these latter senses of 'testable' its physical and practical senses, respectively. Thus, when a hypothesis is described as '*ad hoc*' the author may have in mind not that an independent test is logically impossible but merely that one is physically impossible, or that one is practically impossible.

Now many contingent claims are not physically testable, for example, the claim that some event occurred when no trace of that event remains. But it would be a mistake to erect a demarcation criterion on this basis, for it would be consistent with such a criterion that we may have to entertain 'unscientific' theories in order to try to gather evidence to see whether or not those theories *are* 'unscientific'. That is, scientists may have to entertain unscientific theories, which is queer. (There is no problem of *belief* or *acceptance* here for we are certainly not required to believe or accept any such theory when there is no evidence for it.) A practical testability criterion would be worse still. As well as inheriting the above mistake it would contribute one of its own, for if there *were* some such trace but we had no way of detecting it (and perhaps little prospect of doing so) we would be dissuaded from the attempt by this criterion. But what manner of *Popperian* is it who would circumscribe the adventure of science in this fashion?

The basis of both Chalmers's dismissal of Colombe's hypothesis and Musgrave's of the Newton-Hall theory as *ad hoc*$_p$, however, is a demand that the independent consequences of these hypotheses should be *practically* testable. I conjecture that these philosophers are attracted to this weak construal of 'untestable' in a bid to find *some* hypothesis to illustrate the alleged importance of Popper's rule against *ad hoc*ness. Popper too has favoured this construal, as it happens, on the very occasion in "Replies" where he distinguishes an *ad hoc* from an auxiliary hypothesis. The act of doing so makes him dimly aware of this popular piece of semantic elasticity in his terms for he suggests, to begin with, that the distinction between such hypotheses is "a little vague".[69] He later redefines '*ad hoc* hypotheses' as "*at the time* untestable auxiliary hypotheses".[70] (Emphasis mine.) In between, he provides the following example of a hypothesis that is *ad hoc* in this novel sense:

> [Wolfgang] Pauli introduced the hypothesis of the neutrino quite consciously as an *ad hoc* hypothesis [to explain the energy loss, assuming the conservation laws are true, in beta-decay]. He had originally no hope that one day independent evidence would be found; at the time this seemed practically impossible. So we have an example here of an *ad hoc* hypothesis which, with the growth of knowledge, did shed its *ad hoc* character. And we have a warning here not to pronounce too severe an edict against *ad hoc* hypotheses: they may become testable after all, as may also happen to a metaphysical hypothesis. But in general, our criterion of testability warns us against *ad hoc* hypotheses; and Pauli was at first far from happy about the neutrino which would in all likelihood have been abandoned in the end, had not new methods provided independent tests for its existence.[71]

Popper's criterion of (logical) testability warns us against no such thing, however, for a hypothesis can be testable in the logical sense without being testable in the practical sense, and he is well aware of this distinction. In *Conjectures*, for example, he says:

> "Einstein's theory of gravitation clearly satisfied the criterion of falsifiability. Even if our measuring instruments at the time did not allow us to pronounce on the results of the tests with complete assurance, there was clearly a possibility of refuting the theory."[72]

Pauli's conjecture, which led to an important discovery, highlights the weakness in the demand for practical testability that Chalmers and Musgrave make, and which is obscured by their choice of hypotheses that now seem wildly implausible or are known to be false.

It may seem odd that Popper should champion conjectures as part of the life-blood of science yet remain unenthusiastic about conjectures as to what novel things (a trans-Uranian planet) or novel sorts of things (the neutrino) the universe contains. It may seem equally odd that he should insist that it is of no consequence how or why a conjecture is generated yet continually badger us about dealing with, or at least attempting to deal with, anomalies in, as Musgrave puts it, this "perfectly reasonable" fashion. The truth is that in spite of his rhetoric Popper is simply *biased* against some conjectures, and some refutations, in science. Whilst he eulogizes the practice of conjecturing and refuting major theories, he grudgingly tolerates *the same practice* in relation to auxiliary propositions. He also misrepresents the latter practice so that we seem to be faced with a choice between two radically different kinds of practice, in essence, between accepting a refutation and avoiding one. Since there is no *a priori* reason, however, to suppose that predictive failure will lead to the refutation and replacement of instances of T rather than A, the contrast he draws between the desirability or the probability of success in attempting the former compared with the latter collapses. I shall examine this matter in more detail in the next chapter, but for now consider the way in which Popper's selective use of the notion of immunization and the word '*ad hoc*' help to paint this contrast.

Take, once again, his account of the introduction of the hypothesis of a planet exterior to Uranus:

> We must not exclude all immunisations, not even all which introduced *ad hoc* auxiliary hypotheses. For example the observed motion of Uranus might have been regarded as a falsification of Newton's theory. Instead the auxiliary hypothesis of an outer planet was introduced *ad hoc*, thus immunising the theory.[73]

But the argument can just as easily go the other way. Suppose that this Uranian anomaly had caused some scientist to invent a new mechanical theory to replace Newton's. One could then say, with equal justice:

> We must not exclude all immunisations, not even all which have introduced *ad hoc* theories. For example, the observed motion of Uranus might have been regarded as a falsification of the set of auxiliary propositions. Instead a new mechanical theory was introduced *ad hoc*, thus immunising this set.

Notice too that by the expression 'not even all' in his first sentence above, Popper implies that *most* hypotheses which are introduced for some particular explanatory purpose are destined to be excluded from science. For this implication to seem at all plausible one needs to equivocate about '*ad hoc*', slipping from '*ad* hoc_{oe}' to '*ad* hoc_{p}', as one is encouraged to do, not least by the placement of '*ad hoc*' in that first sentence. (As we have seen, it cannot be determined from this sentence alone whether '*ad hoc*' leans on 'introduced' or on 'hypotheses'.)

But Popper goes further. Having claimed that the trans-Uranian planet hypothesis immunized Newton's theory, he immediately adds, "This turned out to be fortunate; for the auxiliary hypothesis was a testable one, even if difficult to test, and it stood up to tests successfully." Again, it is only by equivocating between '*ad* hoc_{oe}' and '*ad* hoc_{p}' that anyone would believe it is "fortunate" when auxiliary hypotheses turn out to be testable. But to then try to prop up this belief with the suggestion that the trans-Uranian planet hypothesis was "difficult to test" is sheer humbug. Leaving aside the fact that this suggestion concerns a practical matter and not a logical one, it took the astronomers who tested this hypothesis and discovered Neptune only a few hours of routine work on each of two successive evenings to do so, such was the accuracy of the data they were supplied with, and the quality of their telescope.[74]

Charles Sanders Peirce called the "form of inference" by which hypotheses such as those of the neutrino or of a novel planet come to be formulated or entertained, 'abduction' or 'retroduction'. Peirce set out abduction (though using different symbols) as follows:

> The surprising fact, F, is observed;
>
> But if H were true, F would be a matter of course.
>
> Hence, there is reason to suspect that H is true.[75]

The form of *this* inference, however, is that of a deductively valid argument with the suppressed premise that any observed consequence of H that is surprising, such as F, is a reason to suspect that H is true. Peirce *contrasted* abduction or retroduction with deduction, however, so he failed to grasp this point, as have several of his commentators.[76] Pierce also contrasted abduction with induction though when he did so he meant by 'abduction' (in the above symbols) 'act of inferring H from F', and others have followed or tolerated this usage of logical terms as well.[77] But the inference from F to H is an *inductive* one.

Nonetheless, Peirce, and later N.R. Hanson in particular[78], did appreciate that as a form or pattern of argument so-called abduction, as set out by Peirce above, is both important and common in scientific inquiry, as it is in everyday life. The fact that H is *ad hoc* in the ordinary sense, or that there is no independent evidence for H, should not deter us, as

Peirce and Hanson recognized, from entertaining H (though of course other considerations may do so). We cannot always or even often be so fortunate in our inquiries as to have independent evidence for our conjectures already to hand.

Unlike Peirce and Hanson, however, Popper and many of his students do not accept the suppressed premise in Peirce's abductive schema, and hence do not accept Peirce's conclusion (at least in theory). They do not believe that F is evidence for, or corroborates, H, for the simple reason that H was devised to explain F. As Gerard Radnitzky and Gunnar Andersson put it: "a fact cannot be used twice" - once to construct a theory and then to support it.[79] Popper came to this radical view of positive evidence, so he tells us, because he was dismayed at the many people he encountered in his youth who would interpret seemingly any relevant case of human behaviour to fit snugly with their entrenched social or psychological beliefs, counting every such case as yet more evidence for those beliefs.[80] Popper concluded, or rather decreed, that an *explanandum* is evidence for an *explanans* only if it was not or could not have been known to the person who formulated that *explanans* at the time he or she did so. In effect, positive evidence or corroboration can be had only from consciously designed tests, for only then is the *explanandum* not known in advance. In fact Popper went further, claiming that a test prediction should also be *unlikely* in the light of the existing background knowledge, and thus should provide a crucial test between the novel theory and the relevant portion of that background knowledge.[81]

The agile dogmatists from Popper's youth demonstrated, amongst other things, the considerable freedom of choice of possible truths with which to flesh out a pet theory into an *explanans* that fits the facts, whatever the facts may be. This is the point, indeed the only point, of Lakatos's deviant planet story, and Popper is rightly skeptical that our ability to formulate such *explananses* is anything like as impressive an achievement as philosophers like Lakatos and Quine seem to think it is. What is intellectually shoddy about the practice Popper describes, however, is that alternative explanations are not considered by those concerned, and that such people look no further for evidence to support their acceptance of the *explanans* in question than the particular *explanandum* they want explained. But neither of these deficiencies need drive us to a radical theory of positive evidence like Popper's or, for that matter, to Lakatosian skepticism or Quinean dogmatism.

It is surely contrary to almost everyone's intuitions, and Popperians are importantly no exception here, to believe that F is evidence for H only if H was not formulated to explain F. This is pure psychologism. Suppose there are two facts, F_1 and F_2, of which only F_1 is known at t_0. H is formulated to explain F_1, and is tested by predicting that F_2

obtains. But what if, of the two facts in question, only F_2 had been known at t_0 ? H might then have been formulated to explain F_2 and tested by predicting that F_1 obtained. If F_2 is a reason for believing that H in the former case, why not in the latter? Conversely, if F_1 is a reason for believing that H in the latter case, why not in the former?

Why do we typically entertain or test such hypotheses if not because we think we *already* have some evidence or reason to believe that they are true (or to accept them as true, if Popper prefers)? How is rational action possible on Popper's theory of evidence? It is one thing to recognize that, for example, Leverrier did not have *sufficient* evidence for the existence of the trans-Uranian planet he postulated because it would remove Uranus's existing residuals, quite another to pretend that he had *no* evidence at all for this postulate prior to having it tested at the telescope. In unguarded moments, Popperians do not persist with this counter-intuitive requirement for evidence. For example, recall Musgrave's remark (p. 71 above) that the "only evidence" produced for the hypothesis that those who claim to have refuted Newton are liars or cheats is that they have criticized Newton. But it is the fact that such people criticised Newton that *led* to the formulation of this hypothesis. Moreover, Popperians often talk of the need for *independent* evidence in such cases, having forgotten that on their account there is no *other* kind of evidence that one can have. And Musgrave says:

> Scientists themselves regard genuine confirmations of, say, Relativity Theory as very difficult to come by indeed - which suggests that by genuine confirming evidence for a new theory they mean evidence which is not also explained by existing theories.[82]

If scientists do think that this is so then they are badly mistaken, but I think Musgrave has misread the situation. In general, a theory would not be *counted* as a rival or replacement candidate for another if it did not make at least a reasonable fist of duplicating the success of the theory it would replace. If Relativity Theory did not yield Newtonian Mechanics as an approximation, for example, would it have been in the race to replace the latter? There is no point in looking for *new* confirmations for such a theory unless one is assured that it is confirmed by the evidence which confirms the theory it would replace. The situation Musgrave has in mind is one in which the scientists concerned are looking only for evidence that will *discriminate* between the two (or more) theories in question, so we need to be careful how we interpret their remarks about the difficulties of obtaining evidence for one of them. Someone may say, "Evidence which confirms Relativity Theory is hard to come by" but mean, and be understood to mean *in this situation*, "Evidence which confirms Relativity Theory *alone* is hard to come by."

If some scientists do believe, as Musgrave suggests, that Relativity Theory is confirmed or supported *only* by such phenomena as the bending of light in strong gravitational fields or the redshift of spectral lines, whereas Newtonian Mechanics captures the support of observations of falling apples, the movements of the planets, the tides, and so on, then these scientists believe that the two theories are *incommensurable* in respect of the positive support each can command. This is scarcely a palatable consequence, not least for Popperians.

Popper's concept of an *ad hoc* hypothesis is the same as, or very similar to, that of several other philosophers of science. For example, Carl Hempel in his *Philosophy of Natural Science* gives an account of *ad hoc* hypotheses and their alleged dangers that is along the same lines as Popper's.[83] Grover Maxwell reports that a hypothesis is "often said to be *ad hoc* with respect to a certain fact, if and only if it explains the fact in question and no others".[84] David Miller in *Dictionary of History of Science* defines '*ad hoc* hypotheses' as those which are "designed for a specific purpose, that accomplish nothing else".[85] W.V. Quine and J.S. Ullian claim that "the vice of an *ad hoc* hypothesis" is that (in the worst case) it "covers only the observations it was invented to account for, so that it is totally useless in prediction".[86]

Technical definitions of '*ad hoc* hypothesis', such as those provided or implied above, have in common not merely that they retain the ordinary pejorative meaning of '*ad hoc*' but also that the technical component in each of them is itself shaped by this ordinary meaning. If their authors grasped this point they would be unlikely to hold many of the views they do about so-called *ad hoc* hypotheses. Take Maxwell's suggestion that many people believe there are hypotheses which explain only that which they were intended to explain. Miller does not preclude the possibility that he believes there are such hypotheses, and nor perhaps do Quine and Ullian. Anyone who holds this belief, however, has a very queer view indeed of the relations between outcomes and intentions.

To see why it appears otherwise to such people, however, and how they arrive at the notion of such a hypothesis, consider the following three propositions:

(1) The hypothesis, H, is designed or intended only to explain the alleged fact, or set of alleged facts, F,

(2) H is not intended to explain anything other than F,

and

(3) H is not able to explain anything other than F.

If (3) is conflated with (2) then (3) will seem entirely plausible, since (2) follows immediately from (1) and (1) is obviously true for many instances of H. There are many instances of *ad hoc*$_{oe}$ hypotheses.

But (3) is false. If H explains F, then H will explain many *other* facts besides F. It is simply irrelevant that whoever formulated H had no *intention* that H should do so. If I conjecture that a friend's car has broken down because she is late for an appointment, for example, I may have in mind only to explain why she is late. But whatever my purposes or intentions, if this conjecture is true and does explain why she is late it will explain other facts besides such as, for example, how she came to know a mechanic in a part of town she rarely visits or why she had fresh grease stains on her sleeve when she finally did arrive for our appointment.

The concept of an *ad hoc* hypothesis which Maxwell describes above is stronger than Popper's, but this does not affect the points I have just made. To form his concept of an *ad hoc* hypothesis, Popper slides not from (2) to (3) but from,

> (2) 'H is not intended to have any testable consequence other than F (and its testable consequences),

which likewise follows immediately from (1) above, to

> (3) 'H does not have any testable consequence other than F (and its testable consequences).

Necessarily, H is not independently testable of F just when (3)' is true.

The reason Popperians continually make false assessments of the extent of the testable consequences of the hypotheses Popper has led them to disapprove of as *ad hoc* is that they equivocate between (2)' and (3)'. This equivocation also explains why they mistakenly believe that *ad hoc*$_p$ hypotheses are numerous or troublesome. But given that H entails F, (3)' is true only if H is at least *logically equivalent* to F, which shows how meagre in fact is the class of genuinely *ad hoc*$_p$ hypotheses. *A hypothesis is ad hoc*$_p$ *only if it is logically equivalent to its explanandum.* The mere fact that Popper and others continue to employ the term 'hypothesis' in this context, however, is a further indication that they have a larger or different class of proposition in mind, for hypotheses are generally supposed to *transcend* observation statements or their *explanandums* .

Maxwell concludes that we are justified in dismissing a would-be explanatory hypothesis as *ad hoc* only when it is logically equivalent to its *explanandum.* This is a sensible conclusion, if one believes there is some value in distinguishing propositions as *ad hoc*.[87]

It suggests, however, that Maxwell's concept of an *ad hoc* hypothesis is not that which he says is common but in fact the same as Popper's. In any event, having drawn this conclusion, Maxwell is thus "extremely wary" when the charge '*ad hoc*' is levelled at any hypothesis, and justifiably so. He cites the neutrino hypothesis as a paradigm of a hypothesis that is "intuitively labelled" as *ad hoc* but which eventually "opened up an extensive and exciting new area of inquiry, neutrino astronomy".[88] In reply, Popper says that he does not "greatly disagree" with Maxwell on these matter.[89] If so, then why the fuss about *ad hoc* hypotheses?

Those who slide from the psychological claim in (2) or (2)' to the logical claim in (3) or (3)' above do so only because they disapprove of H, or rather they disapprove of the motive, or the alleged motive, of the author of H, and this slide appears to deliver a reasonable objection to H itself. Such people thus end up believing in hypotheses with bizarre explanatory powers because they find the alleged motive of the authors of those hypotheses distasteful. The motive they find distasteful is of course that someone should want to save one of his or her cherished beliefs from a counter-argument when *they* happen to think that this belief ought to be relinquished. No doubt many people are prone to believing or mouthing bad arguments, if not to irrationality, when one of their cherished beliefs is threatened, but the remedy for bad arguments is not more of the same. It is both unavoidable and desirable that people should often *want* to undermine counter-arguments to their own beliefs, for many beliefs are well supported or known to be true. And to suppose that if people have this desire it must vitiate or taint their attempts at undermining such arguments would be to commit the *ad hominem* fallacy. Thus Larry Laudan is certainly correct in saying:

> What seems to lie behind many discussions of *ad hocness* is a conviction - often present but rarely defended - that there is something suspicious about any change in a theory which is motivated by the desire to remove an anomaly. The presumption is that if we know what the anomaly is, it is little more than child's play to produce some face-saving change in the theory which turns the anomaly into a positive instance. I doubt that where "real" science is concerned, this task is such an easy one.[90]

Hempel concludes his analysis by asserting that "there is, in fact, no precise criterion for *ad hoc* hypotheses", a conclusion which merely reflects the fact that if people identify with a threatened belief they will be much less likely than they otherwise would to count any saving hypothesis as *ad hoc*, whatever its logical properties.[91] Likewise, Popper argues that the distinction between an *ad* hoc_p hypothesis and an auxiliary hypothesis is "a little vague" when he comes to consider, as we have seen, the successful neutrino hypothesis.[92] Hempel goes on to say, however, that "some guidance" can at least be had in this matter. The *first* question we should ask, he suggests, is whether or not the hypothesis concerned has been proposed "just for the sake of saving some current conception against adverse evidence".[93] It is *this* question that we need to remove from the agenda if hypotheses are to be assessed objectively.

2.4 *Ad hoc*ness and Circularity

I turn now to Popper's attempts to explicate *ad hoc*ness in terms of certain undesirable, or allegedly undesirable, properties of propositions or the explanations they would provide.

Popper *has* attempted to distinguish a hypothesis that is *ad* hoc_p from one that is logically equivalent to its *explanandum*, though he does not mention this fact in his reply to Maxwell cited above. In "The Aim of Science", Popper says that he intends there, amongst other things, to "elucidate his use of the expression 'independent', with its opposites, *ad hoc*, and (in extreme cases) 'circular'".[94] In this section, I shall examine his attempt at doing so. Popper states:

> Let *a* be an *explicandum*, known to be true. Since *a* trivially follows from *a* itself, we could always offer *a* as an explanation of itself. But this would be highly unsatisfactory, even though we should know in this case that the *explicans* is true, and that the *explicandum* follows from it. *Thus we must exclude explanations of this kind because of their circularity.*
>
> Yet the kind of circularity I have here in mind is a matter of degree. Consider the following dialogue: 'Why is the sea so rough today?' - 'Because Neptune is very angry' - 'By what evidence can you support your statement that Neptune is very angry?' - 'Oh, don't you *see* how *very* rough the sea is? And is it not always rough when Neptune is angry?' This explanation is found unsatisfactory because (just as in the case of the fully circular explanation) the only evidence for the *explicans* is the *explicandum* itself. The feeling that this kind of almost circular or *ad hoc* explanation is highly unsatisfactory, and the corresponding requirement that explanations of this kind should be avoided are, I believe, among the main motive forces of the development of science: . . .
>
> In order that the *explicans* should not be *ad hoc*, it must be rich in content: it must have a variety of testable consequences, and among them, especially, testable consequences which

> are different from the *explicandum*. It is these different testable consequences which I have in mind when I speak of *independent* tests, or of *independent* evidence.[95]

Before considering the novel aspects of this argument, notice that Popper's alleged example of a hypothesis or *explanans* (his *explicans*) that is *ad* hoc_p fails in the same way that several of those we examined above do. The rough sea, r , is the only evidence that is *considered* for the hypothesis that r is caused by Neptune's anger, N , but it does not follow that N is *ad* hoc_p. Moreover, N is *not ad* hoc_p for it rules out any state of affairs in which the sea is only partly rough, and hence partly smooth, since Neptune cannot be both angry and placid at one and the same time. It would not be difficult, of course, to formulate some modified hypothesis to accommodate such a state of affairs; for example, one that allows that Neptune can be angry with one fishing village but not another would do the job. But what would then have to be shown is that any such modified hypothesis was itself neither open to a similar objection nor untestable. Alternatively, one can appeal to simplicity and take the razor to this supernatural baggage.

But to our present concern: why does Popper believe that the explanation of r which N would provide is "almost circular"? If I suggest that a scatter of broken china about the kitchen is explained by Narelle's recent fit of anger, no circularity, or near circularity, is involved. In Popper's dialogue above, N can be replaced with *any explanans*, including the correct one, and he would still be obliged to conclude that the explanation it would provide of r was "almost circular". So it is the *structure* of this dialogue that has led Popper astray and that we need to examine more closely.

If Popper's dialogue appears to display a circularity, or near circularity, of some kind, and it does so not just to Popper, this is I suggest because it appears to resemble a circular proof, or rather a circular proof claim. Newton-Smith, for example, describes the argument in this dialogue as "a justification which runs in a circle", and this is false.[96]

A circular proof claim, in its simplest form, is an argument in which one proposition, p , is offered in support of a second proposition, q , when q has already been offered in support of p , or clearly depends upon that support. But no such circularity occurs above. The question, "Why is the sea so rough today?" is not a request for evidence but for an explanation, and it is treated as such by both participants in the dialogue. The *only* occasion on which evidence is solicited or offered above is in response to the question, "By what evidence can you support your statement that Neptune is very angry?" In short, r is offered as evidence of N , but N is not offered as *evidence* of r.[97] We can assume that both participants can see for themselves that the sea is rough.

Moreover, the latter question is both otiose and misleading. It comes at a point in the dialogue when both participants have *already* committed themselves to one answer to it, namely, r (even though a Popperian would be otherwise unlikely, as we have seen, to accept r as evidence of N). So this question needs to be reframed, if as a request for evidence for N then as a request for evidence *other than* r . In which case, r would be an obviously unacceptable answer, and would not give rise to any appearance of circularity in the dialogue. Even so, it should have been obvious to the explainer in Popper's dialogue that his or her interlocuter believed, and rightly so, at least that r was not sufficient evidence for *N*. The point is that if the explainer is aware of this belief it is unhelpful to respond to a request for evidence for N with, as happens above, an assertion which obviously entails r.

Now whilst a proof claim is circular just when what needs supporting is called upon, at some stage in the argument, to do the supporting, a would-be explanation is circular just when what needs explaining is called upon to do the explaining. (We call such arguments 'circular' because, as it were, we set off seeking support for, or an explanation of, some proposition, p , only to end up where we started, offering p as the support, or as the basis of the explanation, we were seeking.) Popper's case of

$$a \dashrightarrow a$$

is a circular 'explanation' in its simplest logical form. This argument is sound and known to be so but, clearly, though we may know that something is the case and argue validly that if it is the case then it is the case, this does not *explain* how or why it is the case. Knowing that an *explanans* is true and that it entails the *explanandum* (Popper's *explicandum*) is thus not sufficient for explanation.

A circular 'explanation' is defective because if what needs explaining could provide that explanation there would not be this need. If such would-be explanations were acceptable the job of looking for explanations, which we have all experienced in every day life if not in science, would *disappear*. We would have only to know that something was the case in order to explain it. In short, what we find puzzling or do not understand will not *itself* relieve our puzzlement or enlighten us, otherwise we would not be puzzled or ignorant in the first place. Moreover, when a causal explanation is called for, as it is in Popper's case above of the rough sea, a would-be explanation that is circular has the counter-intuitive consequence that events or states of affairs can be self-caused.

Popper, however, fails to grasp why a circular 'explanation' is defective. He seems to believe it is because the evidence for the *explanans* is inadequate, and this is a mistake. Consider again

$$a \dashrightarrow a$$

Clearly, it would be rational to doubt the *explanans* in this case only if one already doubted the *explanandum*. Yet it is part of the pragmatics of explanation that we usually only seek an explanation of something, *p* , if we already know or at least have good reason to believe that *p*. So there would not usually be a problem of *evidence* for the *explanans* in any would-be explanation that is circular. Moreover, it so happens that in *a* --> *a* we know that the *explanandum* is true, so there is obviously no *need* for any additional or independent evidence for the *explanans* in this case (even if *per impossible* there were independent evidence for such an *explanans*). In this regard, however, all that Popper says of *a* --> *a* is that "the only evidence for the *explicans* is the *explicandum* itself", and even this is not correct. Rather, the only evidence for the *explanans* is (or can be) *the evidence for the explanandum*. It is presumably Popper's belief that the only evidence for Neptune's anger is the rough sea that is the origin of this error.

Is there nothing, however, to choose between Popper's requirement that an *explanans* should be independently testable of its *explanandum* and the traditional requirement that a would-be explanation should not be circular? There are at least three reasons for preferring the latter requirement.

Firstly, non-circularity is a more *comprehensive* criterion than non-*ad hoc* ness$_p$, and not only because it applies to other kinds of arguments such as proofs and definitions. The hypothesis, 'The sea is rough and was liked by Homer', for example, provides a circular 'explanation' of why the sea is not smooth. But it is independently testable of this *explanandum* for all that. 'The sea was not liked by Homer', for example, is a potential falsifier of the *explanans* but not the *explanandum* in this case.[98]

Secondly, the independently testable *explanans*, 'The sea is rough and was liked by Homer', provides no better an explanation of why the sea is not smooth than does an *explanans* that is *not* independently testable, for example, 'The sea is rough.' The point of testing any proposition is to gather evidence (or counter-evidence) for that proposition to help determine one's cognitive attitude to it. But there need not be, and usually there is not, a problem of belief or acceptance with the *explanans* in a circular explanation, as we have seen. So a testability criterion misses the point as to why such an *explanans* is defective.

Thirdly, if the alleged explanation some hypothesis provides is circular, then it is the alleged *explanation* that is unacceptable. The hypothesis which provides that explanation may be otherwise perfectly acceptable. So Popper's criterion is misleading whereas the criterion of non-circularity is not, for only *arguments* can be circular. Since *every* proposition, and hence every piece of scientific knowledge, can provide a circular 'explanation' of *some explanandum* (itself, for example), it is important to draw this distinction.

Consider the following case. Immediately following the definition of '*ad hoc* hypotheses' he supplies above, Miller suggests that the "supposedly explanatory" hypothesis,

It is the dormitive virtue of opium that induces sleep,

is *ad hoc*.[99] Miller thus conflates an *explanans* with an explanation, almost certainly because he has been misled by Popper. The "supposedly explanatory" hypothesis or *explanans* in this case is,

Opium has a dormitive virtue.

The *explanandum* is,

Opium induces sleep.

We should reject the would-be *explanation* that it is the dormitive virtue of opium that induces sleep because it is circular, though Miller does not point this out. The proposition which constitutes the *explanans,* however, should not be rejected (as false), since it is known to be true. We may want to express this proposition somewhat differently these days as, for example,

Opium has the property of being sleep inducing,

or simply,

Opium is a sedative.

But truths about the dormitive virtue or sedative property of such substances can of course provide, or assist in providing, any number of sound explanations, for example, of why people fall asleep, why they miss their buses, get the sack, keep such substances where children cannot easily get at them, and so on. Such propositions are thus not *ad hoc* in Miller's sense.

In addition, however, to rejecting Popper's requirement that a hypothesis should not be *ad hoc*$_p$, in favour of a requirement that no would-be explanation should be circular, it is important to notice that *ad hoc*$_p$ hypotheses have no plausible role in the practice of rejecting the antecedent, though this is where Popper believes such hypotheses do their main damage. (This is a further reason, if one were needed, for believing that it is not, in general, the class of hypotheses that are logically equivalent to their respective *explanandums* that Popper has in mind when he talks about *ad hoc* hypotheses.) It is certainly true that an *ad* hoc_p hypothesis and its *explanandum* can be made to fit the logical form of

R' --> $\sim R$

for example,

a --> $\sim(\sim a)$

Thus, one person may remark, 'It is because the sea is rough that it is not smooth,' in the vain hope of explaining to another why the sea is not smooth. But this remark is plausible only if *both* participants in this dialogue or inquiry accept that the sea is not smooth. In rejecting the antecedent, however, whilst *one* participant does believe that the sea is not smooth, the other holds a contrary view.

2.5 *Ad hoc*ness and Empirical Content

Finally, let us turn to the relations, or the alleged relations, between *ad hoc*ness_p and empirical content. In particular, we need to examine Popper's contention that the practice he calls avoiding a refutation is rationally acceptable only when R' is more falsifiable or bolder or has more empirical content than R, or T' is more falsifiable or bolder or has more empirical content than T, as the case may be. Replacement candidates which do not satisfy this requirement are said or hinted to be *ad hoc*;[100] Chalmers, for example, even *defines* an *ad hoc* theory in this way.[101]

But to begin with, what is empirical content? If the class of potential falsifiers of some contingent proposition, p, includes, but is not exhausted by, the class of potential falsifiers of another contingent proposition, q, that is, if the former class includes the latter as a proper sub-class, then p is more falsifiable than q. There is more chance, other things being equal, of falsifying p than q. The degree of falsifiability of any such proposition, Popper claims, is a measure of how much, as it were, that proposition says about the world, which is intuitively its empirical content. Thus on his account p has more empirical content than q, and this accords with our intuitions, at least in respect of p and q.[102]

Popper is, of course, primarily concerned to compare the falsifiability or empirical content of universal generalisations, and on the above method it follows that, for example,

All penguins have light coloured fronts, T_1,

is less falsifiable and has less empirical content than,

All penguins have light coloured fronts and dark coloured backs, T_2.

Similarly, an observation statement such as, *X* is a penguin with a dark coloured front, R_1,

is less falsifiable than,

X is a penguin with both a dark coloured front and back, R2.

But there are some well known problems with comparing classes of potential falsifiers to measure either falsifiability or empirical content. For example, since existential generalisations have no potential falsifiers they can have no empirical content on this method. Thus, for example,

There are penguins,

apparently says nothing about the world. Moreover, it would therefore say no less about the world than,

There are penguins, icebergs, and snow storms.

Also, a theory such as,

All penguins have dark coloured fronts, T_3,

has potential falsifiers which neither T_1 nor T_2 has, for example,

X is a penguin with a light coloured front, R_3,

and *vice versa*, for example, R_1 is a potential falsifier of both T_1 and T_2 but not T_3. And the same goes for falsifiability comparisons of observation statements; for example, R_3 has potential falsifiers which neither R_1 nor R_2 has, and *vice versa*.

Popper claims that such comparisons can be made using what may be called an erotetic method, namely, that if the class of questions which can be answered with either 'yes' or 'no' by appealing to *p* includes as a proper sub-class the class of questions which can be answered with either 'yes' or 'no' by appealing to *q* , then *p* is more falsifiable or has more empirical content than *q*. This method overcomes the problem that whilst, for example, R_3 is intuitively *less* falsifiable than R_2 it still has potential falsifiers that R_2 lacks, for example, R_1. But an appropriate answer to the question,

Is *X* a penguin with a dark coloured front?

is forthcoming not only from R_3 - 'no' - but also from R_2 - 'yes'; and R_2 answers questions that R_3 cannot, for example,

Is *X* a penguin with a dark coloured back?

An objection raised or implied by David Miller to this method,[103] however, is that there are some questions, such as

Does *X* have a light coloured front or a light coloured head?

which can be appropriately answered by appealing to R_3 - 'yes' - but *not* to R_2. R_2 denies that *X* has a light coloured front so whatever answer it supplies to the second part of this question is the answer it supplies to the question as a whole. But R_2 implies nothing about the colour of X 's head and so cannot supply either 'yes' or 'no' to the second part of this question. Also, questions such as

Are there penguins with dark coloured fronts?

or

Do all penguins have light coloured fronts?

would have to be ruled out, even though most people would rightly regard such questions as empirical, otherwise Popper would have to concede that existential generalisations do have empirical content.

There are penguins with dark coloured fronts,

for example, supplies the answers 'yes' and 'no', respectively, to these questions.

The general problem of measuring empirical content is not, however, one that I propose to dwell upon. There is, to say the least, no general agreement that the problem has been solved, but, even if it had been, the question would remain: should we go along with Popper in his demand that R' and T' have more empirical content than R and T, respectively. It is this question I am concerned with here, and for the points I wish to make we can get by with clear cut cases of content comparison, or at least with cases that it would be difficult to interpret as unfavourable to the points I wish to make.

What, then, is the relation, at least in a clear cut case, between *ad hoc*ness$_p$ and empirical content? If a hypothesis or theory *is ad hoc$_p$*, and thus logically equivalent to its *explanandum*, it will have the *same* empirical content as that *explanandum*. Thus, if one demands that a theory not be *ad hoc$_p$*, one should demand that it have *more* empirical content than its *explanandum*. But how is this relevant to the demand that a replacement theory should have more empirical content than the theory it would replace? The short answer is that it is not. If one equivocates about the referent for 'not independently testable' in '*ad hoc$_p$*', however, one may easily come to believe otherwise. T' may thus be held to be *ad hoc$_p$* if it is not independently testable of T, or, as in the case of Musgrave's account of the Newton-Hall theory, if it is falsely believed to be not independently testable of T.[104] Thus Popper would bolster his demand for greater empirical content in replacement theories by claiming or suggesting that it is a way of avoiding *ad hoc*ness$_p$. The equivocation on which this suggestion rests is discernible, for example, in the following passage:

> At any time *t*, the theoretician will be especially interested in finding the best testable of the competing theories in order to submit it to new tests. I have shown that this will at the same time be the one with the greatest information content [or empirical content] and the greatest explanatory power. It will be the theory most worthy of being submitted to new tests, in brief '*the best*'' of the theories competing at time *t*. . . . In what has just been said

> about '*the best*'' *theory* it is assumed that a good theory is not *ad hoc*. The idea of *ad hocness* and its opposite, which may perhaps be termed 'boldness', are very important. *Ad hoc* explanations are explanations which are not independently testable; independently, that is, of the effect to be explained.[105]

Clearly T' can be less testable or bold than T without being logically equivalent to the latter's *explanandums*.

Since the requirement that an explanation not be circular is obviously not a reason for requiring that a novel theory (or observation statement) should have more empirical content than that which it replaces, why should we go along with this requirement of Popper's?

Consider, firstly, the practice of rejecting the antecedent, beginning with this simple case. Suppose we reject

X is a penguin with a dark coloured front, R_1,

for

X is a penguin with a light coloured front, R_3.

Intuitively, R_3 does not have more empirical content than R_1, but an equivalent amount. Yet clearly there need be nothing unscientific or irrational about replacing R_1 with R_3. Perhaps we simply made a mistake in accepting R_1 in the first place. (We probably did; so far as we know, there *are* no penguins with dark coloured fronts.)

Musgrave has noticed this point too, and promptly makes an *ad hoc* adjustment to Popper's requirement. In considering the case of Uranus's deviant orbit, he says:

> The revised Newtonian system of Leverrier and Adams, which led to the discovery of Neptune, was not *more* falsifiable than its predecessor (which implies that no new planet would be found at the calculated position). The difference between the two systems lay in their degrees or [of] corroboration, not their degrees of testability. And we certainly do not want to exclude the revised system as being *ad hoc,* so we must adopt the weaker definition of *ad hoc*ness [that the revised system should not be less testable than its predecessor].[106]

The analysis of this case is not quite correct, however, for the original system implies only that no new planet *which sensibly perturbs Uranus* would be found at the calculated position. The calculation of a planetary orbit takes no account of any body which does not, or would not, have any sensible effect on the planet concerned. (The moon, for example, has no such effect on Uranus and so was not included in the calculations of Leverrier or Adams, but it does not follow that either astronomer was thereby denying the existence of the moon.) For all that, Musgrave would be correct if he were to think that, The sum of the planets sensibly perturbing Uranus is n , A_1,

has an equivalent empirical content to

The sum of the planets sensibly perturbing Uranus is $n + 1$, A_2.

A_2 does not assert more about the causes of Uranus's perturbations than A_1 does, even though it does assert that there are more such causes.

What Musgrave does not mention is that Popper provides an account - one that is both sketchy and misleading - of why he believes Leverrier's (or Adams's) system *does* have more empirical content than its predecessor. Popper says:

> A prima facie falsification *may* be evaded, . . . as in the Uranus/Neptune sort of case, by the introduction of testable auxiliary hypotheses, so that the empirical content of the system - consisting of the original theory plus the auxiliary hypothesis - is greater than that of the original system. We may regard this as an increase of informative content - as a case of *growth* in our knowledge.[107]

This argument involves, as Musgrave would concede, a slide from the idea of increased empirical content to the idea of increased empirical knowledge. It takes advantage of the fact that in the case mentioned our empirical knowledge did increase to convince us that so too did the empirical content of the relevant system of beliefs or acceptances. Positioned between Popper's claims about empirical content and empirical knowledge is the ideal banana peel - a claim about increased *informative* content. 'Informative content' is a Popper-synonym for 'empirical content' *and* it is misleadingly suggestive of truth. Indeed, Popper all but identifies informative content with knowledge in the passage quoted above. If this seems an uncharitable reading of his argument, why are we not told what he takes "the original system" to consist of? We can infer that it includes "the original theory" but if there is no more to it than that why distinguish system from theory? Does *Popper* believe there is more? Since he does not describe the new system as a *modification* of the old, nor mention that one auxiliary proposition, A_1 , had to make way for another, A_2 , are we to believe that we have only to conjoin A_2 to the original system to form the new system of Leverrier's? At first glance, the empirical content of such a system may *seem* greater than that of the original. But all that would be achieved by this move would be to produce a system which, because it still contained A_1 , had *no* empirical content at all for A_2 is inconsistent with A_1 , and a contradiction does not imply anything about the world.

Yet even if one recognizes that A_2 *replaced* A_1 , one can still overlook the logical consequences that the original system has that the new system does not, and so mistakenly conclude that the latter has more empirical content than the former. John Worrall, for example, does so, and implies that such an increase in empirical content is a *sine qua non* of the scientific approach to anomalies, and an object lesson to Marxists. He says:

> The 1846 Newtonian does not content himself . . . with the claim that the irregularities in Uranus's orbit do not refute Newtonian theory, but rather some (unspecified) auxiliary or observational assumption. Nor even with simply specifying the faulty auxiliary assumption and replacing it with a new assumption. Instead he replaces the faulty assumption with a new assumption of a special kind - one which makes the new total theory capable of receiving genuine support from more facts than the previous total theory. Here, of course, one *extra* empirical prediction concerned the existence of a hitherto unknown planet.
>
> This prediction was subsequently confirmed. Thus the Newtonian's shift was from one set of assumptions to another set which received support from more facts; whereas the Marxist's shift was to a set of assumptions which was incapable of receiving support from more facts than its predecessor. (Emphasis mine.)[108]

Like Popper, Worrall conflates '(logically) capable of receiving more support' with 'receiving more support' above. This is, presumably, at least in part why he and Popper overlook some of the logical consequences of the original system, though this oversight may also be due to the fact that A_1 was a tacit assumption whereas A_2 was a novel and public hypothesis, one that was spectacularly confirmed. The view of science Worrall and Popper think that such cases support has the absurd consequence that when an anomaly is due to some error in the calculations (as it partly *was* in this case) then merely to correct this error is not sufficient for doing *science*.

If rejecting the antecedent can be rational when the empirical content of R' is equivalent to that of R , is it not possible for it to be rational if R' has even less content than R ? Is Musgrave's *ad hoc* move to his "weaker definition of *ad hoc*ness" reasonable? Consider the following case. Suppose we are using remote cameras to study a penguin rookery and we accept:

X is a penguin with both a dark coloured front and back, R_2

which would refute,

All penguins have light coloured fronts and dark coloured backs, T_2.

We then discover that the film which enabled us to make R_2 was defective. A search of all our reliable photographs yields only

X is a penguin with a light coloured front, R_3.

Perhaps we even return to the rookery, when we are eventually able to do so, but cannot find X. What would be irrational in rejecting R_2 for R_3 (especially as, for all we know, T_2 is true, and it is therefore antecedently probable that R_2 is false)?

Let us turn now to the practice of rejecting the consequent, beginning with some cases where the empirical content of T' is equivalent to that of T.

The Newton-Hall theory has an empirical content that is intuitively *equivalent* to that of the law of gravitation. Both universal generalisations purport to describe the gravitational attraction between any two masses. One does not need to believe that the Newton-Hall theory would have been a suitable replacement for Newton's law, however, in order to accept the general point that it can be rational to change one's mind about the relation between certain variables, even though the empirical content of one's changed belief in this regard is no greater, or smaller, than that of one's previous belief.

We can even discover that some variable we previously overlooked, or had no knowledge of, is relevant, without T' then having or requiring *more* empirical content than T. For example, compare:

> All Red Winged Parrots have black backs, T_4,

with

> All male Red Winged Parrots have black backs and all female Red Winged Parrots have green backs, T_5,

and

> All mature male Red Winged Parrots have black backs and all immature or female Red Winged Parrots have green backs, T_6.

The world would be a simpler place if, other things being equal, T_4 were true rather than T_5 or T_6, though for all we know T_6 *is* true. T_4 is intuitively *simpler* than T_5 and, in turn, T_6, but we should not confuse simplicity with empirical content. T_4 intuitively says as much about the world as do either T_5 or T_6. It is true that, say, T_6 implies that back colour in Red Winged Parrots is a function of both sex and age but it does not follow that it has more empirical content than, say, T_4, since T_4 *denies* that back colour in this species is a function of either of these two variables.

To return to the Newton-Hall theory, Musgrave is in a further bind in respect of this theory for he describes it as "artificial and *ad hoc*" in the very paper where he proposes his "weaker definition of *ad hoc*ness", on which it is not thus *ad hoc*.[109] Musgrave gets into this bind purely because he equivocates between a technical sense of '*ad hoc*' and the ordinary pejorative sense of that word. But it is indicative of how deeply embedded this equivocation is in his thought that he can fail to notice both that the theory does not have less empirical content than Newton's law and that it is independently testable of this law.

What, then, if T' has *less* empirical content than T? Chalmers considers the old adage, 'Bread nourishes', T_7, and says:

> This low-level theory, if spelt out in more detail, amounts to the claim that if wheat is grown in the normal way, converted into bread in the normal way and eaten by humans in a normal way, then those humans will be nourished. This apparently innocuous theory ran into trouble in a French village on an occasion when wheat was grown in a normal way, converted into bread in a normal way and yet most people who ate the bread became seriously ill and many died. The theory, "(All) bread nourishes" was falsified. The theory can be modified to avoid this falsification by adjusting it to read, "(All) bread, with the exception of that particular batch of bread produced in the French village in question, nourishes." This is an *ad hoc* modification. The modified theory cannot be tested in any way that was not also a test of the original theory. The consuming of any bread by any human constitutes a test of the original theory, whereas tests of the modified theory are restricted to the consuming of bread other than that batch of bread that led to such disastrous results in France. The modified hypothesis is less falsifiable than the original version. The falsificationist rejects such rearguard actions.[110]

Chalmers is right that his "modified theory", T_8, is less falsifiable than T_7, but is that a fatal or even a significant objection to it? Is that what is wrong with T_8?

A practitioner would accept T_8 only because he or she *already* accepted,

> The particular batch of bread produced in the French village in question did not nourish, R_7.

Now the conjunction of T_8 and R_7 is not less falsifiable than T_7. Moreover, it is intuitively closer to the truth or empirically more adequate than T_7 for in only one case does T_8 & R_7 entail something different about the nutritional value of a batch of bread than does T_7, and in that case the former entailment is true whilst the latter is false. Chalmers is concerned that if T_7 is replaced by a *theory* that is less falsifiable than itself, "scientific progress" will be thwarted.[111] But T_7 would be replaced by T_8 & R_7 (or rather the belief that T_7 would be replaced by the belief that T_8 & R_7) so how would progress be thwarted? The former belief is *not* less falsifiable than the latter, and it is empirically more adequate to boot.

On Laudan's analysis of this sort of case, cited earlier (p. 56), he would presumably *approve* of a theory like T_8 provided that the scientists who employed it were also prepared to risk some semantic confusion, not to mention people's lives, by *saying*, 'All bread nourishes,' whenever they *meant* what 'All bread, with the exception of that particular batch of bread produced in the French village in question, nourishes' means in ordinary English.

The modest aim of T_8 is to preserve the considerable predictive success of T_7 whilst avoiding its one dreadful failure, and in this aim T_8 is successful. But in the light of the events in that French village there is obviously something fishy about any theory, whatever its retrodictive adequacy, whose *predictive* consequences are *indistinguishable* from 'All bread nourishes'.

To grasp what is wrong with T_8, however, we need to distinguish accidental from law-like generalisations. Causal or physical laws, or law-like generalisations, are universal generalisations, but they are not *merely* universal generalisations. Chalmers's original theory, T_7, once "spelt out" along the lines he indicates, *does* purport to describe a causal regularity, namely, that if bread is baked and consumed in the traditional manner this is causally sufficient to nourish the human beings concerned. What the French village incident teaches us is that this is false. Anyone who asserts T_8, however, even though it might be true, has not learnt this lesson. The fact that the batch of bread concerned was baked on some day in a French village is accidental. It is no more causally relevant than, say, the fact that the bread was baked 5000 kilometres from the nearest mono-lingual Chinese market gardener whose name rhymes with the name of that French village. *That* is not why this bread did not nourish. An accidental generalisation such as T_8 implies that if bread were baked and consumed just as it was in that French village - including the presence of the disease, ergot, in the grain, which was the cause of the suffering and death of those villagers - it would yet nourish those who consume it. This implication is both counter-intuitive and, should one believe it, potentially lethal.

An instructive example of how *reduction* in empirical content from T to T', and in turn to T'', T''', and so on, is a feature of some important cases of scientific progress, however, is that of Hooke's (so-called) law of elastic behaviour.

As a result of his cogitations on the springiness of certain bodies and, in particular, his experiments in loading and unloading helical wire springs and long straight wires, Robert Hooke formulated his sweeping generalisation, '*ut tensio sic vis*' or 'As the extension, so the force', which he published in 1679.[112] This generalisation is still known, simply, as 'Hooke's law'.

Now even if we assume that Hooke had in mind only elastic materials loaded within their breaking stresses, the behaviour of such materials is not always in accordance with Hooke's law (or Hookean) in some rather obvious ways.[113] Elastic materials can have a simple set of forces applied to them such that they behave as a plastic material would in that they do not recover their shape when those forces are removed.[114] Think, for example, of how a teaspoon can be bent, or indeed how one of Hooke's springs would have been made. Moreover, complex structures like bridges or trees do not behave in a Hookean manner, though Hooke apparently believed otherwise.[115] And even in the case of a simple structure like a spring which, in general, does, Hooke was relying on an unstated analogy between a spring and a wire - for in the case of a spring it is the *structure* which is stretched whereas in the case of a wire it is the material itself.

But even if Hooke's law is modified to take account of such objections, there are important limits to the behaviour of elastic materials loaded in just the way Hooke envisaged. I mention two: elastic limit and fatigue limit.

Early in the nineteenth century it was discovered that various elastic materials have a limiting stress beyond which those materials behave plastically, and so Hooke's linear relation between force and extension no longer holds.[116] This limiting stress of an elastic material is called its elastic limit. Secondly, at about the same time, according to J.E. Gordon,

> It began to be noticed that the moving parts of machinery would sometimes break at loads and stresses which would have been perfectly safe in a stationary component [and *were* safe earlier in the life of those same parts]. This was especially dangerous in railway trains whose axles would sometimes break off suddenly and for no apparent reason after they had been in service for a time. The effect soon became known as 'fatigue'.[117]

Presumably, the Ancients had been acquainted with the effects of fatigue in soft metals, but as a topic of scientific interest the study of fatigue dates from only the middle of last century. What we now know is that the breaking stress of an elastic material *reduces* with the number of stress reversals the material undergoes, that is, with the number of times the material is stressed and unstressed. The curve of the breaking stress of an elastic material against the number of stress reversals that material has undergone describes its fatigue limit. In some materials, such as steel, this curve flattens out after a time but in others, such as aluminium, it does not - as the Comet aircraft disasters of the early 1950s tragically demonstrated.[118]

In the case of Hooke's law, then, scientific progress has come about, *contra* Popper, by continual *reductions*, over a long period, in the empirical content of the scientific community's belief in the Hookean behaviour of materials. Similarly, in the case of the poisoned bread, whereas it was once believed that bread had only to be baked in the traditional manner for it to nourish, we now think that a means of preventing grain from becoming ergot infected, or similarly diseased, is also needed. So the class of potential falsifiers for what we now believe are causally sufficient conditions for nourishing bread is *smaller* than the class of potential falsifiers for the corresponding belief of those French villagers. Their belief can be falsified whether a means of preventing ergot infection is part of the process by which a batch of bread is made or not; our belief can be falsified only if such a means *is* a part of that process.

Such cases are not isolated and they are a consequence of our behaving, in part, just as Popper would have us do - formulating bold conjectures, well beyond the available evidence, as in Hooke's case. To suppose that it is a backward step to assert T' rather than T, because in so doing we are asserting less about the world, is to devalue truth and to overlook that our knowledge has advanced on other fronts. We know a good deal more about the *non*-Hookean behaviour of materials than Hooke ever did, or about the *dangers* of traditional bread making practices that those French villagers were unfortunately blind to.

None of this, however, is to deny that if, say, two fields of inquiry, such as celestial mechanics and terrestrial mechanics, are unified, then some unified theory will not have more empirical content than any theory in either of the original fields. Even so, it does not follow, assuming the sums can be done, that the bodies of accepted theory in those fields have together less empirical content than the new body of unified theory.

In her review of the Popperian approach to theory appraisal, Noretta Koertge asks the pertinent question:

> What's so wonderful about theories with high content? Why, other things such as truth status being equal, should scientists give a higher mark to stronger theories?

Koertge immediately replies, giving Popper's primary reason for valuing such theories:

> One answer is trivial - such theories, if true, are more informative.[119]

But this answer cannot be *trivial*, that is, trivially correct, because Koertge has reneged on her *ceteris paribus* commitment. She fails to keep the very thing equal, namely, truth status, she chose to illustrate what does need to be kept equal. The advantage that Koertge, and Popper, would secure from not doing so is that if T' is true and has more empirical content than T then, other things being equal, T' *is* a better theory than T. This is so either because T is false, or in case T is true because in asserting T' we are asserting more about the world *that is true* than if we were to assert T. Popper glosses over this flaw when, for example, he falsely implies above (p. 93) that greater empirical content implies greater explanatory power or that the best testable theory is the best theory. Truth or more truth, not more empirical content, is what makes T' better than T here. The fact that T' has more *empirical* content than T is not even *part* of the reason why T' is better than T. p need not be a reason, much less a good reason, for something just because q is a good reason for that thing and p is a logical consequence of q. For example, if I return some empty soft drink bottles to a shop for the deposit on them I no longer own those bottles. But whilst it may be a good thing from the point of view of my household finances or storage space, or my desire to preserve the ecology of some distant sand island, that I return those bottles, it does not follow that it is a good thing, either from my point of view or from anybody else's, that I no longer own those bottles.

Koertge remarks elsewhere, in an analysis of how to deal with the Duhem-Quine problem, that "by always replacing bits of a system with new parts which are at least as testable as the old we insure that we don't lose ground in our search for comprehensive explanatory theories".[120] The point is, however, that if we need to replace such bits then the ground cannot already have been *won*. Moreover, Koertge does not point out that the more empirical content we demand from a replacement candidate the more chance there is that, other things being equal, it will be *false*. Popper does do so, however, and tries to make a virtue out of this necessity, claiming there is thus more chance of our making a discovery - the discovery that the replacement candidate is false.[121] This is true; but it is of little, if any, comfort, for our aim is not to find theories which do *not* explain what we want explained. Moreover, the fact that the replacement candidate with the most empirical content will have the most testable consequences need not dispose us, even if other things are equal, to select it for testing, notwithstanding Popper's extravagant claims to the contrary. Suppose that T' has more empirical content than T''. If T' entails T'' (and is not logically equivalent to it) there are two kinds of tests we can perform on these two theories: those which test both T' and T'', and those which test T' alone. Why should we always prefer to conduct the latter tests? Furthermore, other things may not be equal. Both T' and T'' may be inconsistent with some other theory we hold, or many people may hold T'' whilst few even entertain T', or the latter tests may be less convenient to conduct than the former.

The basic fact that scientific knowledge has been continually growing for a long time lends Popper's view a superficial attractiveness or plausibility for it follows from this fact that the empirical content of what is known to science has been continually growing for a long time. But this is consistent, as I have attempted to show, with some, indeed many, replacement theories or observation statements having less or no more empirical content than their predecessors.

Notes for Chapter Two

1. Popper, *Logic,* p. 82; Popper, *Conjectures*, pp. 36-37; Popper, *Unended Quest* , pp. 42-43.

2. Popper, *Unended Quest* , p. 43.

3. Popper, *Open Society*, 2: 326 (n. 13).

4. See, for example, Popper, *Logic*, pp. 82-83; Chalmers, *What is Science?*, p. 51; and Jarret Leplin, "The Concept of an *Ad Hoc* Hypothesis", *Studies in the History and Philosophy of Science* 5 (1975): 317.

5. Larry Laudan, *Progress and Its Problems: Towards a Theory of Scientific Growth* (Berkeley: University of California Press, 1977), pp.118-19.

6. Popper, "Replies", bk. 2: 982.

7. Popper, *Unended Quest*, p. 43.

8. See Ayer's objection to Popper's account of 'learning from one's mistakes', p. 8 above.

9. Popper, *Logic*, p. 275.

10. Quine, *Point of View*, p. 43.

11. Ibid.

12. Ibid.

13. Brown, *Perception*, pp. 101-6.

14. Ibid., p. 105.

15. Ibid.

16. Ibid.

17. The desire to avoid ethno-centrism probably lies behind many a relativist's move to count as knowledge the important beliefs of other cultures, irrespective of the truth values of those beliefs, because of the fact that knowledge is superior to mere belief. 'Knowledge' is an achievement word, 'belief' is not.

18. See Popper, *Objective Knowledge*, pp. 247-48, and 261.

19. In addition, see Popper, *Conjectures*, pp. 223-28; and *Unended Quest*, sec. 32.

20. See, for example, the papers "Three Views Concerning Human Knowledge" and "Truth, Rationality, and the Growth of Scientific Knowledge" in Popper, *Conjectures*, pp. 97-119 and 215-50, respectively.

21. Popper, *Unended Quest*, p. 43.

22. Ibid., p.42.

23. Popper, *Open Society*, 2: 326 (n. 13).

24. Whether or not this was such an argument, however, I shall consider in Chapter 3.

25. See 3.1 below (also 3.2-3.5, and 4.1-4.3).

26. Musgrave, "Falsification", p. 395.

27. Ibid.

28. Ibid., p. 397.

29. Popper, "Replies", bk. 2: 986.

30. Ibid.

31. Musgrave, "Theories of Confirmation", p. 6.

32. Ibid.

33. *The Shorter Oxford*, s.v. '*ad hoc*'.

34. Gerald Holton, "Einstein, Michelson, and the Crucial Experiment", *Isis* 60 (Summer 1969): 178.

35. Ibid. See also Putnam, "Corroboration", p. 236.

36. Popper, *Conjectures*, p. 287.

37. Holton, "Einstein", p. 182.

38. Popper, *Logic*, p. 42.

39. Popper, *Unended Quest*, p. 42.

40. Popper, *Conjectures*, p. 244.

41. Gerard Radnitzky, "Progress and Rationality in Research", in *On Scientific Discovery: The Erice Lectures 1977*, eds. Mirko D. Grmek, Robert S. Cohen, and Guido Cimino, Boston Studies in the Philosophy of Science, vol. 34. (Dordrecht: D. Reidel Publishing Co., 1981), p. 72.

42. Ibid., p. 71.

43. Ibid.

44. Ibid., pp. 71 and 93.

45. Ibid., p. 92.

46. Ibid., p. 93.

47. Musgrave, "Falsification", p. 398.

48. Ibid., p. 402.

49. Ibid., p. 398.

50. Popper, *Logic*, p.83.

51. See, for example, Holton, "Einstein", sec. viii; and Leplin, "*Ad Hoc* Hypothesis", pp. 309-16.

52. Popper, *Logic*, p.83 (n.*1).

53. Chalmers, *What is Science?*, pp. 52-53.

54. Stephen F. Mason, *Main Currents in Scientific Thought: A History of the Sciences* (London: Routledge and Kegan Paul, 1956), p. 242.

55. Ibid., chap. 26.

56. Chalmers, *What is Science?*, p. 53.

57. Ibid., p. 52.

58. Galilei Galileo, *Le Opere di Galileo Galilei*, (Edizione Nazionale), vol. 11, pp. 141-43; Stillman Drake, Introduction to *Discoveries and Opinions of Galileo*, trans. Stillman Drake (New York: Anchor Books, 1957), p. 73.

59. Galileo, *Opere*, 11: 142; Stillman Drake, *Galileo at Work: His Scientific Biography* (Chicago: University of Chicago Press, 1978), p. 169.

60. Ludovico Geymont, in *Galileo Galilei: A Biography and Inquiry into his Philosophy of Science*, trans. Stillman Drake (New York: McGraw Hill, 1965), p. 54, falsely remarks, "It follows that since it [Colombe's crystal infill] cannot be seen, nothing can be proved against its existence".

61. Simon Newcomb, "Discordances in the Secular Variations of the Lunar Planets", in *A Source Book in Astronomy*, eds. Harlow Shapely and Helen E. Howarth, Source Books in the History of Science (New York: McGraw Hill Book Co., 1929), pp. 336-38. For a further discussion, see Simon Newcomb, "The Abnormal Behaviour of the Perihelion of Mercury", in *A Source Book in Astronomy*, eds. Harlow Shapely and Helen E. Howarth, Source Books in the History of Science (New York: McGraw Hill Book Co., 1929), pp. 338-44.

62. Musgrave, "Evidential Support", p. 195.

63. Alan E. Musgrave, "Method or Madness? Can the Methodology of Scientific Research Programmes be Rescued from Epistemological Anarchism?", in *Essays in Memory of Imre Lakatos*, eds. Roberts S. Cohen, Paul K. Feyerabend, and Max W. Wartofsky, Boston Studies in the Philosophy of Science, vol. 39 (Dordrecht: D. Reidel Publishing Co., 1976), p. 462.

64. For example, the work of Prof. Frank Stacey at the University of Queensland.

65. H. von Kluber, "The Determination of Einstein's Light Deflection in the Gravitational Field of the Sun", in *Vistas in Astronomy 3*, ed. Arthur Beer (London: Pergamon Press, 1960), pp. 47-77.

66. Musgrave, "Falsification", p. 400.

67. Musgrave, "Method", p. 460.

68. Quoted in Mason, *Main Currents*, p. 236.

69. Popper, "Replies", bk. 2: 986.

70. Ibid.

71. Ibid.

72. Popper, *Conjectures*, p. 37.

73. Popper, *Unended Quest*, p. 42.

74. Morton Grosser, *The Discovery of Neptune* (Cambridge: Harvard University Press, 1962), pp. 116-19.

75. Charles Sanders Peirce, *The Philosophy of Peirce: Selected Writings*, ed. J. Buchler (London: Kegan Paul, Trench, Trubner & Co., 1940), p. 151.

76. Ibid., pp. 153-54. See for example, K.T. Fann, *Peirce's Theory of Abduction* (The Hague: Martinus Nijhoff, 1970), pp. 8-9, 43-44; Martin Curd, "The Logic of Discovery: An Analysis of Three Approaches", in *Scientific Discovery, Logic, and Rationality*, ed. Thomas Nickles, Boston Studies in the Philosophy of Science, vol. 56 (Dordrecht: D. Reidel Publishing Co., 1980), pp. 212-14; and Gary Gutting, "The Logic of Invention", in *Scientific Discovery, Logic, and Rationality,* ed. Thomas Nickles, Boston Studies in the Philosophy of Science, vol. 56 (Dordrecht: D. Reidel Publishing Co., 1980), p. 225-26. Arthur W. Burks does not mention the above setting out of abduction in "Peirce's Theory of Abduction", *Philosophy of Science* 13 (October 1946): 301-6.

77. Peirce, *Philosophy*, p. 151. See also Fann, *Abduction*, pp.53-58; Curd, "Logic of Discovery", pp. 213-14; Gutting, "Logic of Invention", p. 226.

78. N.R. Hanson, "Retroductive Inference", in *Philosophy of Science: The Delaware Seminar*, ed. Bernard Baumrin (New York: Interscience Publishers, 1963), pp. 21-37; and N.R. Hanson,"Leverrier: The Zenith and Nadir of Newtonian Mechanics", *Isis* 53 (September 1962): 364-65.

79. Gerard Radnitzky and Gunnar Andersson, "Objective Criteria of Scientific Progress? Inductivism, Falsificationism, and Relativism", in *Progress and Rationality in Science*, eds. Gerard Radnitzky and Gunnar Andersson, Boston Studies in the Philosophy of Science, vol. 58 (Dordrecht: D. Reidel Publishing Co., 1978), p. 10.

80. Popper, *Conjectures*, pp. 33-38.

81. Ibid., p. 36.

82. Musgrave, "Theories of Confirmation", p. 17. Musgrave shifts from this position in "Evidential Support", p. 186, where he remarks, "Relativity Theory has lots of evidential support, [but] the facts which lead us to prefer it to Newton's Theory are very few indeed."

83. Carl G. Hempel, *Philosophy of Natural Science*, Foundations of Philosophy Series, (Englewood Cliffs, N.J.: Prentice-Hall, 1966), pp. 28-30.

84. Maxwell, "Corroboration", bk. 1: 298.

85. David W. Miller, s.v. '*ad hoc* hypotheses', in *Dictionary of History of Science*, eds. W.F. Bynum and E.J. Brown (Princeton: Princeton University Press, 1981).

86. W.V. Quine, and J.S. Ullian, "Hypothesis", in *Introductory Readings in the Philosophy of Science*, eds. E.D. Klemke, Robert Hollinger, and A. David Kline (New York: Prometheus Books, 1980), p. 204.

87. Maxwell, "Corroboration", bk. 1: 298.

88. Ibid., bk.1: 299.

89. Popper, "Replies", bk. 2: 1039. In *Conjectures*, p. 288 (n. 84), Popper says:

> The rule for the exclusion of *ad hoc* hypotheses may take the following form: the hypothesis *must not repeat* (except in a completely generalized form) the evidence or any conjunctive component of it. That is to say x = 'This swan is white', is not acceptable as a hypothesis to explain the evidence y = 'This swan is white'.

90. Laudan, *Progress*, p. 116.

91. Hempel, *Philosophy*, p. 30.

92. Popper, "Replies", bk. 2: 986.

93. Hempel, *Philosophy*, p. 30.

94. Popper, *Objective Knowledge*, p. 192.

95. Ibid., pp. 192-93. Cf. note 89 above.

96. Newton-Smith, *Rationality*, p. 73. I discuss Newton-Smith's argument in detail in 4.4 below.

97. Confusing justification with explanation is easy to do. David Armstrong does so, for example, in attempting to make this very point in *What is a Law of Nature?* (Cambridge: Cambridge University Press, 1983), p. 40. He says:

> That all the observed *Fs* and *Gs* [Q] may well constitute a good reason for thinking that all *Fs* are *Gs* [P]. But a good reason for P is not necessarily an explanation of P. The presence of smoke is a good reason for think that fire is present. But it is not an explanation of the presence of fire.

But no one would suppose that Q explains P, least of all Armstrong who is in the process of showing that P does not (even) explain Q. (What he wants to assert here, presumably, is that Q can be a good reason for P even though P is not an explanation of Q.)

98. Thus Maxwell in "Corroboration", bk. 1: 298, is wrong in saying that "to be actually *ad hoc* is to be circular - nothing more, nothing less" (given that he thinks that an *ad hoc explanans* is one that is logically equivalent to its *explanandum*). Also, there are no circular hypotheses.

99. Miller, "*Ad hoc* hypotheses".

100. Popper, *Logic*, pp. 82-83, and *Conjectures*, p. 287.

101. Chalmers, *What is Science?*, p. 51.

102. Popper seems to believe that the falsifiability criterion distinguishes empirical or contingent propositions which are scientific from those which are not or which are analyticities or contradictions. If so, he faces the following problem. Let r be any basic statement. I assume that Popper believes in classical logic at least as far as believing that any proposition entails a tautology. Now since,

$$r \dashrightarrow \sim(s \,\&\, \sim s)$$

where s is any proposition we please, then any contradiction has more potential falsifiers than any universal generalisation and hence, on Popper's account, more empirical content than any universal generalisation. This is an important objection to Popper's account, though not one that is relevant to my present concerns, and I have excluded contradictions from falsifiability comparisons above.

103. David W. Miller, "The Accuracy of Predictions", *Synthese* 30 (1975): 159.

104. Musgrave, "Method", p. 462.

105. Popper, *Objective Knowledge*, pp. 15-16.

106. Musgrave, "Evidential Support", p. 197 (n. 24).

107. Popper, *Unended Quest*, pp. 43-44.

108. Worrall, "Research Programmes", p. 56.

109. Musgrave, "Evidential Support", cf. pp. 195 and 197 (n. 24).

110. Chalmers, *What is Science?*, pp. 51-52.

111. Ibid., p. 51.

112. J.E. Gordon, *Structures, or Why Things Don't Fall Down* (Harmondsworth, Middx: Penguin Books, 1978), pp. 36-41; and F.F. Centore, *Robert Hooke's Contribution to Mechanics: A Study in SeventeenthCentury Natural Philosophy* (The Hague: Martinus Nijhoff, 1970), pp. 87-91.

113. This is probably assuming too much - see Gordon, *Structures*, p. 40; and Centore, *Hooke's Contribution*, p. 91.

114. E. Williams, "Hooke's Law and the Concept of the Elastic Limit", *Annals of Science* 12 (March 1956): 76.

115. Gordon, *Structures*, p. 41.

116. Williams, "Hooke's Law", pp. 79-82.

117. Gordon, *Structures*, pp. 333-34.

118. Ibid., pp. 334-37.

119. Noretta Koertge, "The Problem of Appraising Scientific Theories", in *Current Research in Philosophy of Science: Proceedings of the P.S.A. Critical Research Problems Conference (1977)*, eds. Peter D. Asquith and Henry E. Kyberg Jr. (East Lansings, Mich.: Philosophy of Science Association, 1979), p. 233.

120. Noretta Koertge, "Towards a New Theory of Scientific Inquiry", in *Progress and Rationality in Science*, eds., Gerard Radnitzky and Gunnar Andersson, Boston Studies in the Philosophy of Science, vol. 58 (Dordrecht: D. Reidel Publishing Co., 1978), p. 259.

121. Popper, *Conjectures*, pp. 230-31.

Chapter Three

The Problem of Uranus's Orbit: A Case Study of the So-Called Practice of Avoidance of Refutation

3.1 Introduction

The discovery of Neptune in September 1846 from a prediction based on the belief that there were residual or unexplained perturbations in the orbit of Uranus is a case that is often raised in the discussion of Popper's methodology. At the very least, it would seem to be an unfavourable one for him. The manifest failure of successive attempts to predict the motion of Uranus did *not* lead to the refutation of the law of gravitation, yet significant scientific progress obviously did result. How is Popper to account for this?

I shall begin by examining how Popper avoids this issue (3.2). He retreats from SPF to WPF in this as in other such cases, claiming here that the counter-argument from Uranus's residuals (that is, the discrepancies between the planet's predicted and observed positions) constituted a "*prima facie* falsification" of the law of gravitation, or that the motion of Uranus was "*prima facie* non-Newtonian".[1] Many have unthinkingly accepted this pseudo-scientific claim of Popper's.[2] This claim is pseudo-scientific because it is not based on any consideration of the support, or lack of it, for some of the premises in the above counter-argument. I shall therefore discuss the history of the problem of Uranus's orbit and the probable cause of its residuals (3.3). I try to show that if one *does* consider the support for at least one of the said premises (3.4), it is such that one should conclude that Newton's law was not *prima facie* or apparently refuted and, in general, this has been the view of astronomers (3.5). I conclude with some general remarks about how anomalies are rationally dealt with in science, as in everyday life (3.6).

3.2 Popper's Rhetoric

Popper says that he has discussed this case "many times" in his lectures.[3] It is mentioned or briefly discussed on only a few occasions in his published work, however, with the following passage from "Replies" being his most detailed account:

> There is one important method of avoiding or evading refutations: it is the method of auxiliary hypotheses or *ad hoc* hypotheses.
>
> If any of our conjectures goes wrong - if, for example, the planet Uranus does not move exactly as Newton's theory demands - *then we have to change the theory*. But there are in the main two kinds of changes; *conservative and revolutionary*. And among the more conservative changes there are again two: *ad hoc hypotheses* and *auxiliary hypotheses*.
>
> In the case of the disturbances in the motion of Uranus the adopted hypothesis was partly revolutionary: what was conjectured was the existence of a new planet, something which did not affect Newton's laws of motion, but which did affect the much older "system of the world". The new conjecture was auxiliary rather than *ad hoc*: for although there was only this one *ad hoc* reason for introducing it, it was *independently testable*: the position of the new planet (Neptune) was calculated, the planet was discovered optically, and it was found that it fully explained the anomalies of Uranus. Thus the auxiliary hypothesis stayed within the Newtonian theoretical framework, and the threatened refutation was transformed into a resounding success.[4]

Popper also remarks that "because a number of . . . critics appear not to have understood" his position, he has tried "very carefully" above to restate it.[5] One can only agree that he does just that. Popper would have us believe that the refutation of major theories is easy, and Strong Popperian Falsificationism (SPF), if it were true, would make that so. Since this case is an obvious counter-example to SPF, however, Popper has to settle for the next best thing - Weak Popperian Falsificationism (WPF). But he does not do so easily. In the first sentence of the second paragraph above, he *reaffirms* SPF even though he is about to attempt to explain how astronomers were *not* forced to change Newton's theory, but rather an assumption about a set of forces - on a body unknown to Newton. Shortly after the passage cited above, Popper even spells out Newton's theory as "his laws of motion plus his law of gravitation" - propositions which all concerned know were not changed.

But one fallacy can obscure another and Popper equivocates in the second paragraph above between 'theory' and 'theoretical framework' or 'theoretical system', that is, between T and $(T \& A)$. His suggestion that changes can be either conservative or revolutionary is the means by which this equivocation is concealed. He begins by alluding to changes to *theories* in these terms and ends by describing changes to *auxiliary propositions*, via *ad hoc*$_p$ or auxiliary hypotheses, as "among the more conservative". The distinction between 'conservative' and 'revolutionary' is left vague and this helps to obscure the equivocation by enabling him to describe the auxiliary hypothesis in question as "partly revolutionary", which in *another* sense perhaps it was.

On WPF, the counter-argument for Newton's theory from Uranus's residuals would be a *prima facie* falsification, as Popper himself describes it elsewhere. Popper's talk above of this counter-argument as a "threatened refutation" amounts to the same thing, for a weak counter-argument does not pose any threat of refutation. Whenever

$$R \dashrightarrow {\sim}T$$

is a *prima facie* refutation or a strong counter-argument, however, then the grounds for believing that R is true must be strong, and any case in which R did turn out to be false would be untypical. Thus WPF goes a long way towards explaining why Popper is untroubled by this case, or ones like it. For we do *not* typically avoid accepting the conclusions of strong arguments, setting out instead to overturn them. It would clearly be irrational to do so. If we did succeed in overturning

$$R \dashrightarrow {\sim}T$$

with

$$R' \dashrightarrow {\sim}R$$

are we then to turn around and try to overturn *this* would-be refutation with

$$R'' \dashrightarrow {\sim}R'$$

and so on? Furthermore, if one insists upon associating a hypothesis such as the trans-Uranian planet hypothesis with *ad* hoc_p hypotheses, this association only reinforces the impression that inventing such a hypothesis is a barely rational activity, untypical of *science*. It does not matter that Popper agrees that in *this* case the hypothesis was a "resounding success", for we can always be lucky.

In addition to the doctrine of WPF , there are two rhetorical devices Popper commonly employs in such cases, and both are in evidence above. Both focus attention on the possible counter-argument for T from $\sim(T \ \& \ A)$ so that the possible counter-argument for A is thereby ignored.

The first device is Popper's habitual use of expressions such as 'avoiding or evading refutations' when he *means* 'avoiding or evading refutations of T'. The effect of this ellipsis or meaning shift is to obscure the fact that in the cases he describes something *is* refuted, namely, A. Popper is simply not interested in counter-arguments or refutations of the sort,

$$R' \dashrightarrow {\sim}R$$

but only in those of the sort,

$$R \dashrightarrow {\sim}T$$

The discovery of Neptune, however, *refuted* the proposition that the residuals of Uranus were caused only by previously known bodies, a point which is often not understood.[6]

Popper's second device, which reinforces the first, is his habit of employing euphemisms for 'refute' and its cognates whenever *A* is refuted. With some notable exceptions, Popper has no qualms about *saying* of a refuted prediction, theoretical system, or theory, that it has been refuted, because his falsificationism depends for its plausibility upon his doing so. Equally, however, it depends upon his denying or glossing over the refutation of auxiliary propositions. Were he to *say* that *A* was refuted this would only jog the reader to ask, 'Why, then, was *T* threatened with refutation?' So Popper shifts or resorts to euphemisms such as 'affected', 'held responsible', 'blamed', 'called into question', and the like, whenever an auxiliary proposition is refuted.[7] He further distances himself from admitting that this was so above by describing a *consequence* of *A* as 'affected', rather than *A* itself. What concerned astronomers in this case, however, was that they were wrong about the number of planets sensibly perturbing Uranus, not that they were wrong about a consequence of this proposition, namely, the number of planets in the solar system.[8] Popper's suggestion that the hypothesis of a planet exterior to Uranus merely "stayed within the Newtonian theoretical framework" falls short of implying that this hypothesis *replaced* an existing belief or assumption in that framework, an implication which, if it were made obvious, would only draw attention to the fact that this belief was replaced because it was *refuted*.

3.3 The Problem of Uranus's Orbit and its Probable Cause

I have claimed that Popper, and others, believe that the counter-argument for the law of gravitation from the residuals in the orbit of Uranus constituted a *prima-facie* refutation of this law only because they presuppose WPF. But perhaps this argument, unbeknownst to them, *was* a *prima facie* refutation. Was there even at least more reason to suppose that the law of gravitation was false rather than some auxiliary proposition? Musgrave claims that, at the time, it *had* "seemed a defeat" for the law.[9] Is that true? Did it seem that way to either Leverrier or Adams? Were these two astronomers trying to rescue one of their fundamental beliefs, to show that an apparent defeat of the law of gravitation was nothing *more* than that? I shall try to provide, in this section and the next two, reasons for believing that these claims are false, that the answer to these questions - epistemic or historical - is 'no'. I begin with an examination of the problem of Uranus's orbit.

On March 13, 1781, William Herschel observed what he recorded in his journal of observations as "a curious either nebulous star [planetary nebula] or perhaps a comet".[10] It was in fact the seventh planet, Uranus.

Various attempts over the next twenty years or so to construct satisfactory tables for the planet met with some success, but it was transient.[11] Alexis Bouvard took up this problem in 1820 and published his tables in the following year.[12] By then, at least seventeen pre-discovery or so-called ancient observations of the planet had been recovered in searches of astronomers' records.[13] With the post-discovery or so-called modern observations, Bouvard's data covered one and a half revolutions of the planet. He could find no orbit that was remotely satisfactory, however, and he took the simple expedient of rejecting all the ancient observations (though, as was later shown, they were not inaccurate).[14] Even so, Bouvard's tables could accommodate the modern observations only "moderately well", the largest discrepancy being nine seconds of arc.[15] Moreover, it was implausible to suppose that each of the four astronomers responsible for the ancient observations should have been prone to quite uncharacteristic errors whenever they happened to observe Uranus.[16] In any event, before the end of the decade, observations of the planet showed significant disparities with Bouvard's tables.[17] In 1832, George Airy, later Astronomer Royal, reported that Uranus was half a minute behind its tabular position.[18] The planet continued to fall steadily further behind and by 1844 the discrepancy was two minutes.[19] The standard of accuracy for planetary theory at the time approached the limits of observational error, which itself was of the order of just a few seconds of arc.[20] So clearly there was something wrong somewhere.

Urbain J.J. Leverrier took up this pressing problem in the summer of 1845. He had by then "earned a reputation as a gifted analyst", one well suited to such a task;[21] and by all accounts his analysis of this problem was meticulous.[22] Before the end of that year, Leverrier published his first paper on Uranus in which he overhauled Bouvard's tables and succeeded in reducing the above discrepancy considerably.[23] Even so, the revised tables were still in error by "more than forty seconds of arc" for the opposition of 1845. Moreover, since these tables took no account of the ancient observations, Leverrier was convinced that, amongst other things, there were residual perturbations in the orbit of Uranus, since the law of gravitation was manifestly well supported.[24]

John Couch Adams had formed the same conviction much earlier and arrived at his first solution to the problem of Uranus's orbit, based on the hypothesis of an exterior planet, in 1843, soon after he graduated from Cambridge at the age of twenty-three.[25] Adams later acquired the reputation of a consummate theoretical astronomer.[26] Before the discovery of Neptune, however, or rather before the controversy this discovery precipitated, he was relatively unknown, though evidently extraordinarily talented.[27] It was a missed opportunity that his calculations did not lead to the discovery of Neptune before Leverrier's first paper on Uranus was published.[28]

In June 1846, Leverrier published his second paper on the problem in which he concluded that, amongst other things, for the entire range of observations, ancient and modern, once the perturbations due to Jupiter and Saturn had been removed - the other known planets have no sensible effect on Uranus - there was no ellipse that would satisfy these observations, even on the most favourable distribution of errors in them.[29] As Leverrier put it:

> I have demonstrated . . . a formal incompatibility between the observations of Uranus and the hypothesis that this planet is subject only to the action of the sun and of other [known] planets acting in accordance with the principle of universal gravitation.[30]

Thus, we can agree with Popper that at least with the publication of Leverrier's second paper an instance of $(T \,\&\, A)$ has been refuted. But Leverrier was not then *obliged* to argue:

$$(\sim(T \,\&\, A) \,\&\, A) \dashrightarrow \sim T$$

If there was no good reason to believe that A is true then, notwithstanding Leverrier's demonstration, there was no good reason to believe, as Putnam in general recognizes, that T is false.[31] And it so happens that in this instance A was *not* well supported or corroborated, as there was very little support for the proposition that Uranus's sensible perturbations were caused *solely* by known bodies. (If one does not know or assume that the set of forces on a planet is *complete* one cannot predict its position.) It had been convenient in the first instance to *assume* that this set of forces was complete. Moreover, there was no good reason at the time to believe or assume otherwise. But that is all. Why, then, had it been merely a convenient assumption rather than a reasonable belief that the known sensible influences on the motion of Uranus were the only such influences?

Let us concede that astronomers did have good reason to believe that there were no such *non*-gravitational influences on Uranus. Some had suggested, for example, that the residuals of Uranus would be explained if the planet had collided with a suitable comet around the time of its discovery.[32] Such a collision would have dislodged the planet from its original orbit and so two ellipses would be needed to account for its motion. But Leverrier had finally put paid to this suggestion by showing that no ellipse would satisfy even the modern observations. Others had revived Descartes' speculation about a cosmic fluid, claiming that such a fluid would offer some measure of resistance to the planet's motion.[33] But there were no discernible effects of such a fluid, as W.M. Smart notes, under "much more favourable circumstances" elsewhere in the solar system.[34]

There were also good theoretical or empirical reasons for disbelieving in the existence of many such possible *gravitational* influences. For example, a planet of an appropriate mass in the region of Saturn would also produce significant perturbations in Saturn's orbit which had not been detected. Moreover, it was very probable that any such planet would already have been noticed. A planet in the region of Uranus itself, or a satellite of it, would not produce perturbations such as those detected, and a satellite would need to be so massive it could scarcely have escaped detection since 1781. Finally, a planet far beyond Uranus would likewise need to be so massive it would produce sensible perturbations once again in the orbit of Saturn, for its distance from Saturn would be comparable with its distance from Uranus.[35]

There was *no* theoretical objection to a planet, or planets, located at some intermediate distance beyond Uranus, however, and the proposition that no such planet existed was then untested. No search for a trans-Uranian planet was planned or begun until at least 1845, and only then by those entertaining the possibility that Uranus had residual perturbations caused by this unknown planet.[36] The important remaining question, then, is what was the probability that a suitable trans-Uranian planet - that is, one capable of producing the Uranian residuals Leverrier had precisely described - would already have been discovered by chance?

3.4 The Low Probability of a Chance Discovery of a Trans-Uranian Planet Prior to 1846

Whilst it is impossible to be definitive about this probability, for our purposes we do not need to be. I shall argue only that the probability of such a discovery was then, if not remote, at least sufficiently low that there was no good reason to *believe* that Jupiter and Saturn were the only sensible forces on Uranus, apart from the Sun. Some people outside the scientific community, however, were obviously bemused by the fact that Neptune had not been discovered much earlier, as Figure 1 illustrates.

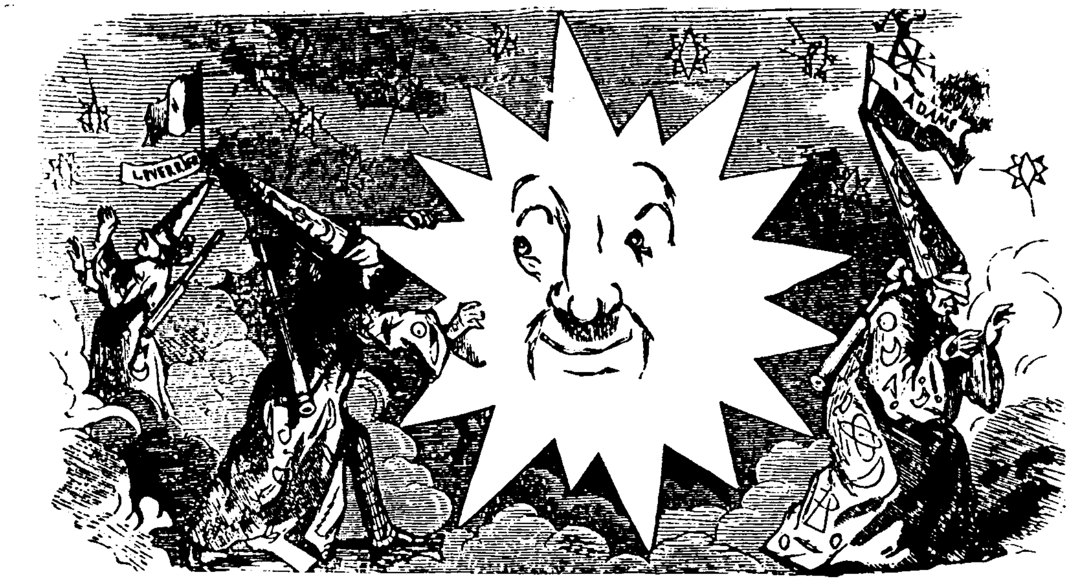


Figure 1. The Discovery of Neptune.
Cartoon by Cham (*Le Charivari*, January 1, 1847). Reprinted from Camille Flammarion, *The Flammarion Book of Astronomy*, trans. Annabel and Bernard Pagel (London: George Allen and Unwin, 1964), p. 321.

I can find no astronomer or historian of science, including those who did not at the time support the hypothesis of a trans-Uranian planet, however, who has expressed any doubt or reservation that such a planet should hitherto have escaped detection. This stands in marked contrast, for example, to the hypothesis of a large Uranian satellite.

Before setting out to explain why this probability was so low, however, it is worth noting that the question we need to address here is not how probable it was that *Neptune* would already have been discovered by chance but how probable it was that, as I have put it above, a suitable trans-Uranian planet would. There is a *family* of possible planets, or combinations of possible planets, any member of which could have caused the known residuals in the orbit of Uranus. Some members of this family would have been easier to discover than Neptune while others would have been so dark, dense, and distant as to have been practically impossible to detect. As it happens Neptune provides, if anything, a conservative example of the difficulties confronting such a discovery for it is not, as a member of this family, an inconspicuous planet. Neptune's albedo and density compare favourably with those of Uranus and the other Jovian planets,[37] and its mean distance from the Sun of about 30 Astronomical Units (AU) is appreciably less than that of any of the planets postulated by Leverrier or Adams. Their mean distances range from more than 33 AU to almost 39 AU.[38] For our purpose, then, it is safe to employ Neptune as an example of a suitable trans-Uranian planet.

A good way of coming to understand the probability of such a discovery is to consider the circumstances surrounding the discovery of Uranus. Uranus is a considerably less prominent or eye-catching planet than its interior neighbours, as Table 1 illustrates.

Table 1

The Relative Prominence of the Superior Planets to Uranus

	Mean Opposition Magnitude[a] (Uranus = 1)	Angular Diameter at Mean Distance[b] (Uranus = 1)	Mean Daily Apparent Motion (1970)[c] (Uranus = 1)
Mars	1100	2	20
Jupiter	1500	10	4
Saturn	90 – 230	5	2
Uranus	1	1	1

Sources: Arthur P. Norton and J. Gall Inglis, *A Star Atlas and Reference Handbook for Students and Amateurs*, 15th ed., ed. R.M.G. Inglis (Edinburgh: Gall and Inglis, 1966), pp. 17[a] and 32[b]. Ruth J. Northcott, ed., *Observer's Handbook 1970*, The Royal Astronomical Society of Canada (Toronto: University of Toronto Press, 1970), pp. 28-30.[c]

Note: The apparent motion of a planet varies from one year to the next, and more so for a planet close to the Earth like Mars, but this variation is of no consequence here.

But Uranus is by no means an inconspicuous object. It is sufficiently bright as to be just visible to the naked eye in good conditions; yet it was not discovered for more than a century and a half after telescopes were in common use by astronomers. The planet had been observed and recorded on a number of occasions before its discovery, as we have seen, but each time it was thought to have been a fixed star. A.F.O'D. Alexander lists twenty-two such observations, by four good observers, and there may have even been more.[39] Given the beliefs, aims, and instruments of such astronomers, or most of Herschel's contemporaries, however, it is not surprising that Uranus was not discovered earlier.

In general, astronomers had believed or assumed that the solar system contained only those planets known to the Ancients. For two months or so after its discovery, Uranus "received the attention of all the leading observers of Europe" but was widely accepted as a comet, even though that was not the most probable interpretation of its appearance.[40] Herschel's belief that Uranus was a comet was in no small way the product of wishful thinking on his part, and he took longer than most to come around to the view that it was after all a planet.[41] In general, astronomers were more interested at the time in the foreground of the solar system, at least as it was then conceived, than in the background of the (so-called) fixed stars from which Uranus would have to be prised.[42] The further a planet is from an Earth bound observer the more it looks and behaves like a fixed star, and Uranus is, relatively speaking, a long way away. Such interest as there was in this background lay principally in determining the positions of a limited number of stars to serve as a reference frame for planetary theory and observation, and for the practical purpose of navigation.[43] The instruments and practices which developed to satisfy this aim were not conducive to the making of chance discoveries of planets, especially distant planets.

A planet can be distinguished from a star either by its appearance or by its apparent motion relative to some neighbouring star, or to the celestial sphere. (The celestial sphere is an imaginary sphere of infinite radius, centred on the Earth, onto which the stars and the apparent paths of the Sun and the other planets are projected.) Planets characteristically have well defined discs and a steady light, although so too do some planetary nebulae and both Uranus and Neptune can be confused with such stars.[44] (J.L.E. Dreyer remarks that had Herschel previously seen one of these stars he might well have missed discovering Uranus.)[45] Furthermore, stars can readily give rise in the telescope to disc-like images (called spurious discs) and at the time of discovery of either Uranus or Neptune only a good telescope, employing a much higher power of magnification than was used for positional work, could have distinguished the two images in the case of either planet. The instruments of those astronomers who observed Uranus prior to its discovery were of poor optical quality, with objectives too small to magnify and resolve the planet's disc.[46]

When Herschel discovered Uranus he was probably better placed than anyone else at the time to have done so. He was a musician, doing astronomy in his spare time, and did not have the narrow interests of his professional counterparts. As J.A. Bennett points out:

> Herschel approached the heavens in quite a different way. His concern for sidereal astronomy as such led him to make telescopes suited to observing faint and distant objects, and his goals were light grasp and quality of definition. . . . Only with a fine telescope was Uranus noticeably nonstellar, and so likely to be sought out again for further investigation.[47]

Whilst the optical quality of telescopes in general use improved considerably by the time Neptune was discovered, it requires a better telescope again to detect its disc for Neptune is about six times duller than Uranus and a third its optical size.[48] Many instruments in use in the first half of the nineteenth century could not have done so.[49]

To illustrate his claim that scientific revolutions involve "transformations of vision", however, Kuhn remarks of the discovery of Uranus:

> A celestial body that had been observed off and on for almost a century was seen differently after 1781 because, like an anomalous playing card [a black four of hearts, for example], it could no longer be fitted to the perceptual categories (star or comet) provided by the paradigm that had previously prevailed.[50]

But the fact is that to these earlier astronomers Uranus would have been visually indistinguishable from a star. Kuhn describes these astronomers as "eminent observers", and Pierre Lemonnier as "one of the best observers in this group". He recounts Lemonnier's missed opportunity to discover Uranus saying that he had "actually seen the star on four successive nights . . . without noting the motion that could have suggested another identification".[51] But if Lemonnier had discovered Uranus it would not have been by noticing a difference in the configuration of certain stars in his telescope from one day to another, but by noticing a difference in the configuration of certain numbers in his journal of observations for those days.

Turning to the second means by which a planet could have been discovered - its apparent motion - positional astronomers were unlikely to notice any such motion. The instruments that were the stock in trade of positional astronomy, such as the quadrants or transit instruments of the seventeenth and eighteenth centuries and the meridian circle of the nineteenth, are held fixed at some point on the meridian and the star field drifts across the field of view. Lower powers of magnification are better suited to this work for they widen the field of view, slowing down the movement of stars across it. The right ascension of a star, the celestial equivalent of longitude, is then measured by the time of transit of the star and its declination, the celestial equivalent of latitude, from a graduated arc or circle. Under such conditions, no astronomer could detect the apparent motion of any body (nor, of course, the disc of a distant planet). On the evening in 1756 when Tobias Mayer measured the position of Uranus, for example, he did the same for a hundred or so stars, in less than three and a half hours.[52]

This is not to say that such a discovery was impossible for these astronomers. Mayer returned more than once to the area where he had observed Uranus and remeasured "all stars of comparable magnitude in the same vicinity".[53] So if he ever noticed that the body we call Uranus was missing he probably thought his only observation of the planet had been a mistake, or perhaps that he had observed a variable star. But even in the unlikely event that he had thought otherwise, the search area for any such body would have soon become impossibly large. Lemonnier observed Uranus, as Kuhn notes, four nights in succession in 1769, but with no reason to examine his records it was only when Bouvard was searching for ancient sightings of the planet, in 1820 well after Lemonnier's death, that these observations came to light.[54] One astronomer, however, did make a discovery of the sort that Lemonnier at least had missed. In 1801, Giovanni Piazzi discovered the first of the minor planets or asteroids, Ceres.[55] Piazzi apparently took three evenings to convince himself of its motion.[56] If that is so then had Ceres been Neptune it is doubtful that he would have formed this conviction for the mean apparent motion of Neptune is less than one quarter of the apparent motion of Ceres when it was noticed by Piazzi.[57]

Finally, in the years to the discovery of Neptune there are no reports of astronomers puzzling over any potential ancient observations of some new planet.

Anyone entertaining the trans-Uranian planet hypothesis was therefore entitled to conclude that there was no better than a slim chance that such a planet would already have been discovered by any positional astronomer. This conclusion is supported not only by the above considerations of the practice of astronomy and the 'ancient history' of Uranus, and by the fact that there would presumably have been some, perhaps many, ancient observations of one or other of the several minor planets discovered early in the nineteenth century. But it was also likely that there would have been significantly fewer ancient observations of any such trans-Uranian planet, from which it might have been discovered, than had been the case with Uranus - even allowing for the fact that by the time Neptune was discovered a further sixty-five years had elapsed for such observations to be made. This is principally because there were bound to be very many more stars down to the magnitude of a planet such as Neptune compared with Uranus, as Table 2 below indicates.

Table 2

Number of Stars Brighter Than Visual Magnitudes 5.0 to 10.0 in the Sky

Visual Magnitude	Star Numbers
5.0	1, 620
6.0	4, 850
7.0	14, 300
8.0	41, 000
9.0	117, 000
10.0	324, 000

Source: Arthur S. Eddington and Harold S. Jones, s.v. "Star", in *Encyclopedia Britannica*, 1951 ed.
Note: Uranus is a 6th magnitude body (mag. 5.5-6.5); Neptune is an 8th magnitude body (mag. 7.5-8.5).

Apart from Galileo, whose notebooks reveal that he observed Neptune over a period of a month when it was near Jupiter, and noted that this 'star' appeared to move,[58] only the

French astronomer, Joseph Lalande, is known to have accidentally sighted the planet before it was discovered. And he did so at a time when astronomers had reason to be more open minded about the outer limit of the solar system. On May 8 and 10, 1795, Lalande observed Neptune whilst recording faint stars for a catalogue.[59] He was using a mural quadrant which would have given no hint of Neptune's nonstellar character.[60] But the apparent motion of the planet was then relatively fast as it was only a fortnight or so from opposition.[61] Moreover, its motion is (very nearly) along the ecliptic, as one would expect of a planet. Lalande discarded his first observation, however, which was rare, and entered the second in his catalogue as 'doubtful'.[62] This indicates that he believed he *had* observed the same body on those two evenings but (falsely) concluded that the difference in the positions he measured was due to some error on his part.

A good idea of the difficulties attendant upon a chance discovery of Neptune can be had from the circumstances of its actual discovery, by Johann Galle and Heinrich D'Arrest. Galle located a likely candidate for Leverrier's planet - it was a suitably faint body, not marked on the star chart they were using, and within a degree of the position predicted. They were able to observe this body for at least a few hours with a telescope that was "the pride of the Berlin Observatory", but the disc was "difficult to resolve" and they remained unconvinced of the body's motion.[63] The conviction that they had found the eighth planet had to wait until the following evening when the weather improved and they could observe its disc. By then, too, the planet's apparent motion, which was a little less than its mean, was easily discernible.[64] Neptune was an easy prize for astronomers looking for such a planet, and in the right place, otherwise it was like any one of tens of thousands of equally faint and unremarkable stars in the sky.

The discovery of a new planet in the solar system might not have been as unconnected with the aims of some astronomers, however, as it would have been for the positional astronomers principally considered thus far. Apart from Herschel who might have discovered Neptune as he did Uranus, though it was appreciably less likely to have attracted even his attention, some astronomers had been actively looking for new members of the solar system. There were comet hunters, for example, like Charles Messier. But the comets then discovered were large showy objects; and comets are best looked for with lower powers of magnification.[65] Moreover, the slow apparent motion of a distant planet would easily have escaped their notice. Messier, nicknamed 'the ferret of comets', was astonished to learn of Herschel's discovery of the slow-moving Uranus, for he naturally assumed that such an undistinguished object would have been discovered by its apparent motion.[66]

In 1800, however, six German astronomers met in Lilienthal and formed a society - the 'Lilienthal Detectives' - the founding aim of which was to organise and conduct a systematic search for a new planet in the same band of the celestial sphere where Neptune is located.[67] They hoped to find a planet between Mars and Jupiter, one corresponding to the only gap in what has become known as Bode's law.[68] This so-called law is an arbitrary formula for the mean distance of a planet from the Sun, first put forward (though not by Bode) in the early 1770s - see Table 3.

Table 3
Bode's law (So-Called) Of Planetary Distances

	n	Mean Distance from Sun in AU (Earth = 1AU) a	Mean Distance from Sun by Bode's Law: ($a = 0.4 + 0.3[2^n]$)
Mercury	$-\infty$	0.39	0.4
Venus	0	0.72	0.7
Earth	1	1.00	1.0
Mars	2	1.52	1.6
????	3		2.8
Jupiter	4	5.20	5.2
Saturn	5	9.54	10.0
Uranus	6	19.18	19.6

Source: Ake Wallenquist, *The Penguin Dictionary of Astronomy*, trans. Sune Engelbrektson, (Harmondsworth, Middx.: Penguin Books, 1966) s.v. 'Bode's law'.

Clearly, a planet so close to the Earth as one corresponding to $n = 3$ would need to be relatively small and dull, otherwise how would it hitherto have escaped discovery? Since Kepler, however, German astronomers had been given to looking for some such mathematical relation, and the discovery of Uranus strikingly confirmed the above formula, or so many believed.[69]

The Lilienthal Detectives hatched an ambitious plan to search for this would-be planet, one which called for twenty-four astronomers to cooperate in searching a different portion of the zodiac each. But their plan was never put into practice as Ceres was discovered before they might have done so.[70] The detectives were convinced Ceres was the missing planet in Bode's series when its mean distance was calculated at 2.767 AU,[71] very close to the value given by Bode's law (as Table 3 above indicates). They were forced to think again, however, when one of the detectives, Heinrich Olbers, discovered a second minor planet, Pallas, in 1802.[72] The mean distance of Pallas is almost identical to that of Ceres.

Moreover, it was soon clear that these two bodies were tiny, probably of the order of only a few hundred kilometres each in diameter.[73]

Nonetheless, what had been intended as a search for one major planet then became a search for more minor planets, with Olbers predicting that such bodies would be numerous.[74] Another of the detectives, Karl Harding, picked up the third minor planet, Juno, in 1804, and Olbers found the fourth, Vesta, in 1807.[75] Within a couple of years, however, there was a "distinct lessening of interest" in the search.[76] By 1815, the society had disbanded and after another year or two the only remaining searcher, Olbers, had given up, with no further discovery having been made.[77] A German postmaster, Karl Hencke, revived the search in 1830, and others may have done so too around this time. Hencke made his first discovery, the fifth minor planet, Astrea, in 1845, shortly after Leverrier's first paper on Uranus was published, and he found the sixth, Hebe, less than two years later.[78]

How probable, then, was it that these early asteroid hunters would have discovered a trans-Uranian planet such as Neptune? How adequate were their searches as a *de facto* search for such a planet?

Had Neptune been observed under favourable conditions by, say, Olbers or Hencke, he would have had a reasonable chance of picking it up. These two probably missed at least a few ninth magnitude asteroids in their search areas and several a magnitude duller, and perhaps one or two a magnitude brighter.[79] Also, they were looking for, and found, bodies whose apparent motions were very much faster than that of any distant planet. When Pallas and Astrea were discovered, for example, they were moving several times faster than Ceres had been when it was discovered by Piazzi.[80] On the other hand, a distant planet would spend very much longer in any search area because of its slow progress around the celestial sphere. Also, most of the first five minor planets were either as dull as, or duller than, Neptune when they were discovered, and all are optically very much smaller.[81] Still, there is or was no good reason to believe that a suitable trans-Uranian planet would have been observed, much less discovered, by them.

Before the use of photography in the early 1890s, the only reliable way to conduct a systematic search for a faint object over a wide area was with a good star chart. If a chart is accurate it is relatively easy to isolate, within the limiting magnitude of the chart, any body in the corresponding area of the heavens that is not marked on the chart, and *vice versa*. Barring such things as variable stars, any find will thus be either a body which has moved into this area or a trace of one that has left, respectively, since the chart was constructed.

There were no charts in existence in the early part of the nineteenth century, however, that would do this job for objects as faint as Neptune, as Figure 2 illustrates, nor even adequate catalogues of stars from which to construct a set of such charts.

Figure 2. Portion of a Typical Early Nineteenth Century Star Chart.
From Bode's *Uranography* 1801. Reprinted from Flammarion, *Flammarion Astronomy*, p. 401. Cf. number of stars mapped above with the number in the portion of the star chart from the 1840s shown in Fig. 3, p. 126 below, which covers an area of the sky less than a quarter of the size of that covered by Bode's chart above.

In 1824, the German astronomer Friedrich Bessel wrote to the Berlin Academy pointing out these impediments to further discoveries of planets, major or minor, and work began on a series of charts the following year under the auspices of the academy.[82] Such work was then slow and laborious,[83] and the first of these charts did not appear until 1830, presumably encouraging Hencke to begin his search. The last was published only in 1859.[84] The academy charts (*Akademische Sternkarten*) aimed to map all stars down to the ninth magnitude and some, perhaps many, fainter ones in an area extending fifteen degrees either side of the celestial equator.[85] There are in the order of 150,000 such stars in this band of the heavens.[86] Figure 3 shows the portion of the academy chart with which Neptune was found.

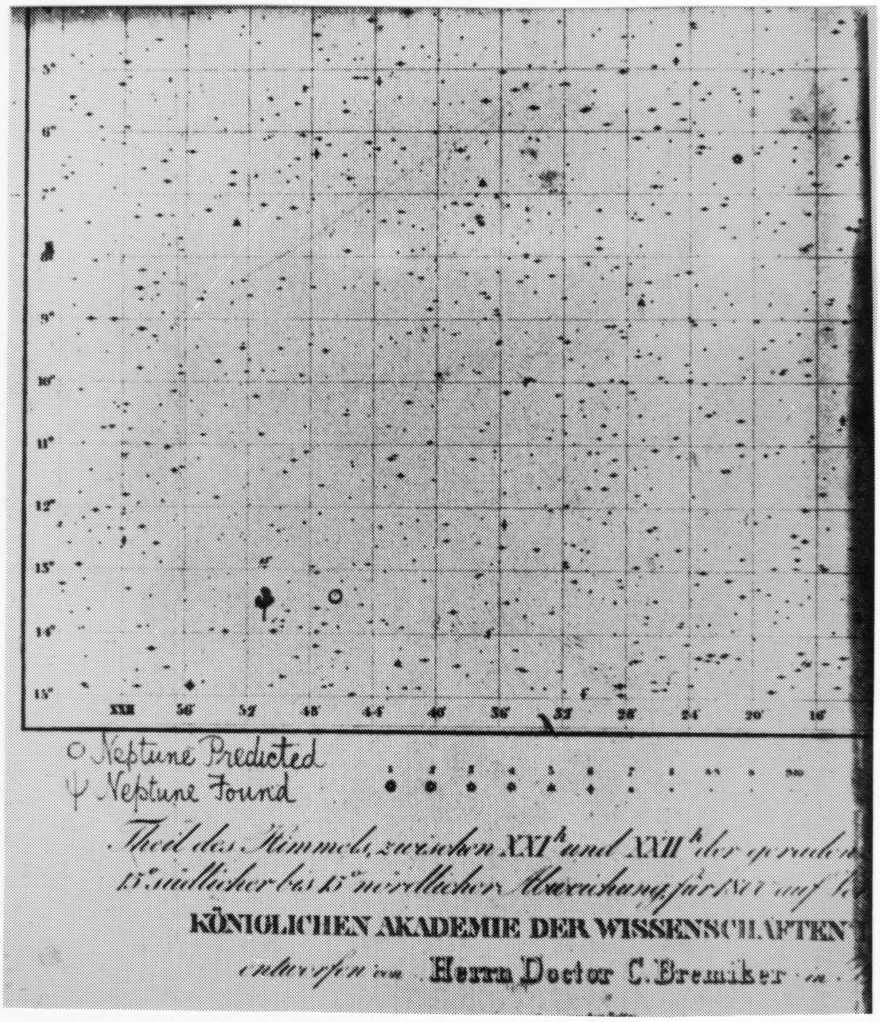


Figure 3. Portion of the Berlin Academy Star Chart (for the 21st Hour of Right Ascension) used to find Neptune.

Reproduced from F.A. Bellamy, "Johann Gottfried Galle", *Knowledge* 34 (September 1910): 374. Morton Grosser, in *The Discovery of Neptune* (Cambridge: Harvard University Press, 1962), p. 117, says that Galle was reluctant to use a star chart for the search as he had "recently used Harding's chart of the same area and knew how inadequate it was". Harding's atlas contains "above 40,000 stars down to mag. 9" for the northern hemisphere and clearly to at least 15° south, so it cannot be anywhere near complete - see George Chambers, "A Handbook of Descriptive Astronomy", 3d ed. (Oxford: Clarendon Press, 1877), p.863.

Now it was probable that the orbit of any unknown planet would be inclined at no more than a few degrees to the ecliptic, and would very probably be contained within the zodiac. (The ecliptic is the apparent path of the sun around the celestial sphere, and the zodiac extends eight or nine degrees either side of this path.) The orbits of all the planets then known lay within the zodiac, and those of the known Jovians lay within three degrees of the ecliptic.[87] Moreover, the residuals of Uranus were very largely confined to the plane of its orbit, which is almost coplanar with the ecliptic, so if an unknown planet were the cause of those residuals the plane of its orbit would probably lie close to the ecliptic as well.[88] Neptune's orbit is inclined at less than two degrees to the ecliptic.[89]

The ecliptic is itself inclined at about twenty three and a half degrees to the celestial equator, however, corresponding to the tilt of the earth's axis, so a complete set of the Berlin Academy charts cover *less than half* the ecliptic, and obviously the same proportion of the zodiac, as Figure 4 indicates.

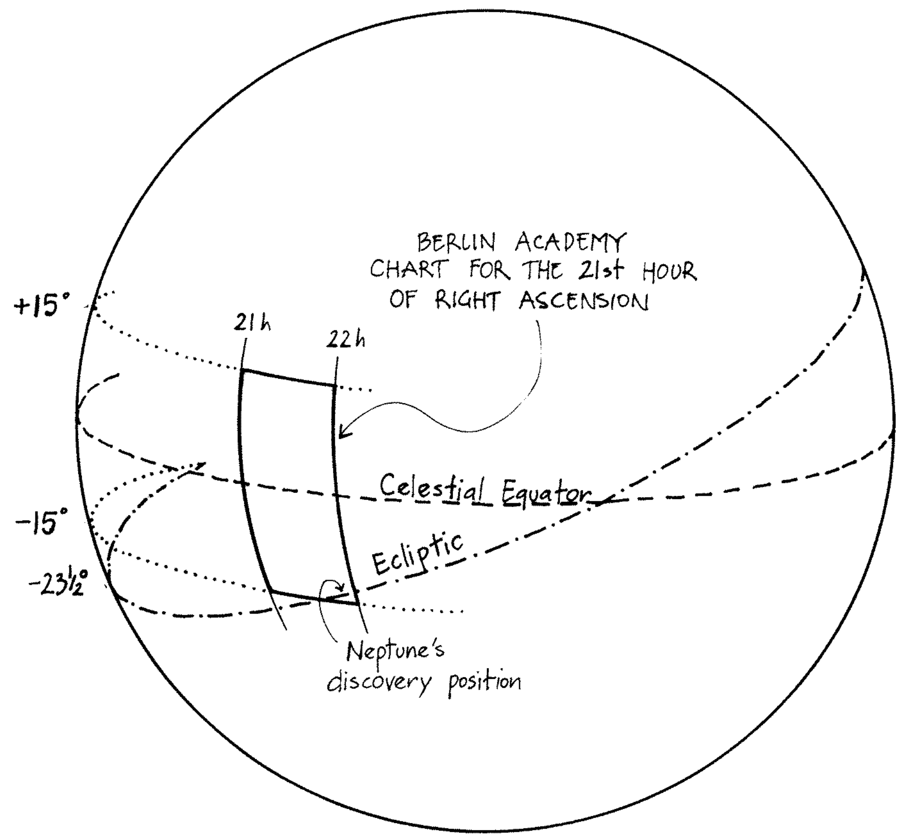


Figure 4. The Coverage of the Ecliptic by the Berlin Academy Star Charts (*Akademische Sternkartern*).

This partial coverage of the ecliptic would not matter for the chance of finding a planet with a very short sidereal period as any such planet, if it were moving along the ecliptic outside the area covered by these charts, would soon enter that area. But since a distant planet has a very long sidereal period, as we know from Kepler's third law, it would be confined to a small portion of the celestial sphere for the duration of even a protracted search, such as those conducted by Olbers or Hencke. Neptune's sidereal period is almost 165 years;[90] and during a search that lasted, say, fifteen years, the planet would trudge back and forth along less than one ninth of the ecliptic (see Figure 5).

Figure 5. Neptune's Slow Angular Progress.
Based on star chart in Roy L. Bishop, ed., *Observer's Handbook 1985*, The Royal Astronomical Society of Canada (Toronto: Toronto University Press, 1985), p. 107. Stars are shown to the 9th magnitude.

This slow angular progress of Neptune is faster, however, than that which any of the planets postulated by Leverrier or Adams would make, for their periods would range from roughly 190 to 240 years.[91] Moreover, as I have pointed out, the academy charts were incomplete at the time of Neptune's discovery. (As it happens, the chart which Neptune was found with *had* recently been completed, though it had not yet been distributed to any other observatory.)[92]

The lack of good charts for faint objects would have obliged the early asteroid hunters to confine their searches to relatively small portions of the celestial sphere.[93] Moreover, these searches were not coordinated. An asteroid hunter would be likely to favour a search area with which he was very familiar, or for which there happened to be a good chart. Conversely, he would be likely to avoid any area which presented special difficulties, such as the portion of Sagittarius shown in Figure 5 above, which lies in the centre of the Milky Way and is not well placed in the sky for observers in northern Europe.

Furthermore, since the orbits of Ceres and Pallas, and later those of Juno and Vesta, were found to be sharply inclined to the ecliptic (see Table 4), the prospect of finding an asteroid away from the ecliptic was much brighter than would be the case for a planet, even though the ecliptic remains the most likely place to find either.[94]

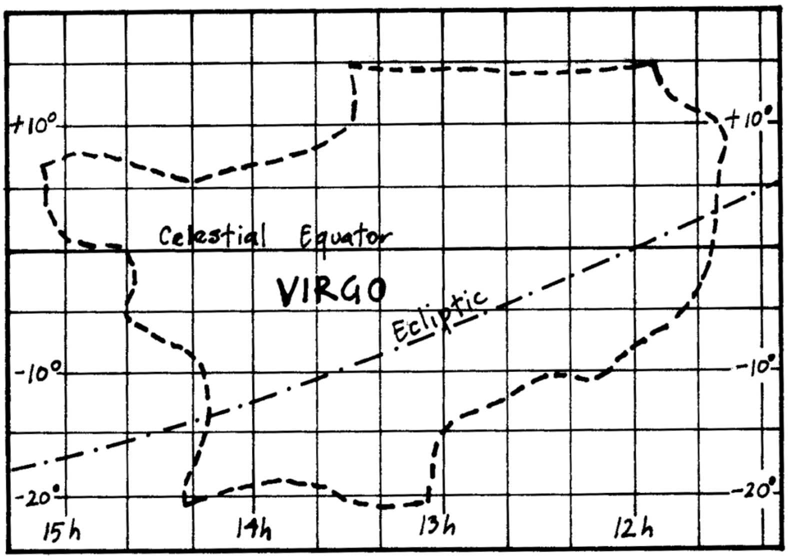
Table 4

Inclinations of the Orbits of the First Four Minor Planets to the Ecliptic

Ceres	$10^{o}.6$
Pallas	$34^{o}.7$
Juno	$13^{o}.0$
Vesta	$7^{o}.1$

Source: Chambers, *Handbook*, p. 900.

Most of the discoveries of these early asteroid hunters were made away from the ecliptic. In the case of Olbers, for example, we know that his main or only search areas were just two patches of the celestial sphere, each to one side of the ecliptic. When the mean solar distance of Pallas was found to be almost identical to that of Ceres, Olbers hit upon a would-be saving hypothesis for Bode's law: he argued that these "cosmical potsherds" were but two remnants of the planet the detectives had originally hoped to find. Olbers concluded that all other such remnants would pass through the two areas in space where the orbits of Ceres and Pallas almost intersected, and he nominated two search areas - directly opposite one another on the celestial sphere - which he described as the north-western portion of Virgo and the western end of Cetus (see Figure 6 below).[95]



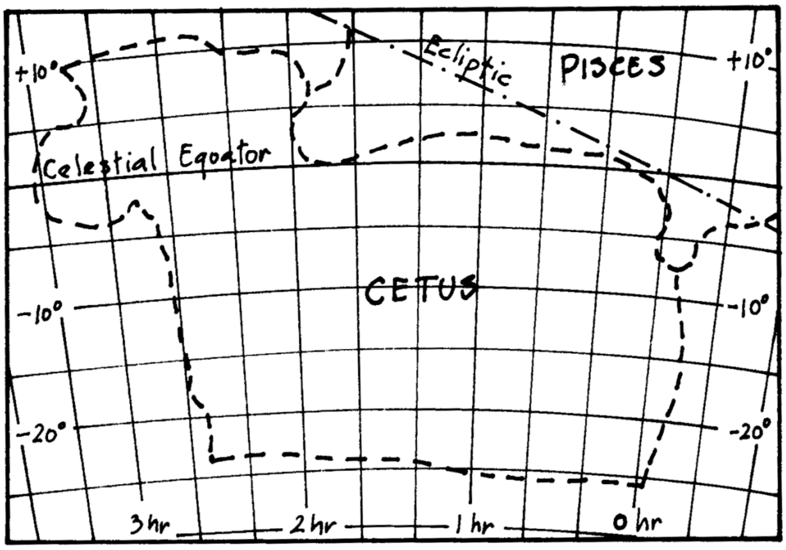


Figure 6. Olbers's Search Areas.

Based on Arthur Cottam's star charts (1891) in T.E.R. Phillips and W.H. Steavenson, *Hutchinson's Splendour of the Heavens: A Popular Authoritative Astronomy* (London: Hutchinson and Co., 1923), pp. 935 (Cetus) and 939 (Virgo). North-western Virgo is the upper right hand portion of the constellation. The western end of Cetus is the right hand end of the constellation, and since Olbers's search areas were opposite one another on the celestial sphere he was presumably not searching the bottom portion of this area. (The boundaries of the constellations were not fixed until the twentieth century, so a description such as 'north-western Virgo' is even somewhat vaguer than it may at first seem).

When Ceres is visible in or near either of these search areas it is beyond the zodiac, as an inspection of its orbital data reveals and as Figure 7 below illustrates for the search area in

Virgo.[96] We can therefore safely conclude that at least the centres of Olbers's areas were well removed from the ecliptic.

Figure 7. Ceres in Northern Virgo in 1977.
Based on the star chart in John R. Percy, ed., *Observers Handbook 1977*, The Royal Astronomical Society of Canada (Toronto: Toronto University Press, 1977), p. 76.

Olbers found both Pallas and Vesta in this search area. Pallas was close to the path of Ceres, and therefore well removed from the ecliptic too.[97] Vesta was no closer than roughly the edge of the zodiac, as an inspection of its orbital data also reveals.[98]

Harding probably found Juno near the ecliptic for he came upon it in Pisces, near the search area Olbers had designated in Cetus (see Fig. 6 above).[99] Harding was engaged at the time, however, in constructing a series of charts to aid the general search which would show "all the small stars near the paths of Ceres and Pallas", an exercise that would have taken him well away from the ecliptic much of the time (see Table 4, p. 129 above).[100]

The discovery of Astrea by Hencke was made or shortly afterwards verified with the academy chart for the fourth hour of right ascension, and Hencke found Hebe using the academy chart for the seventeenth hour.[101] As Figure 8 below illustrates, however, the ecliptic is well removed from both of these charts.

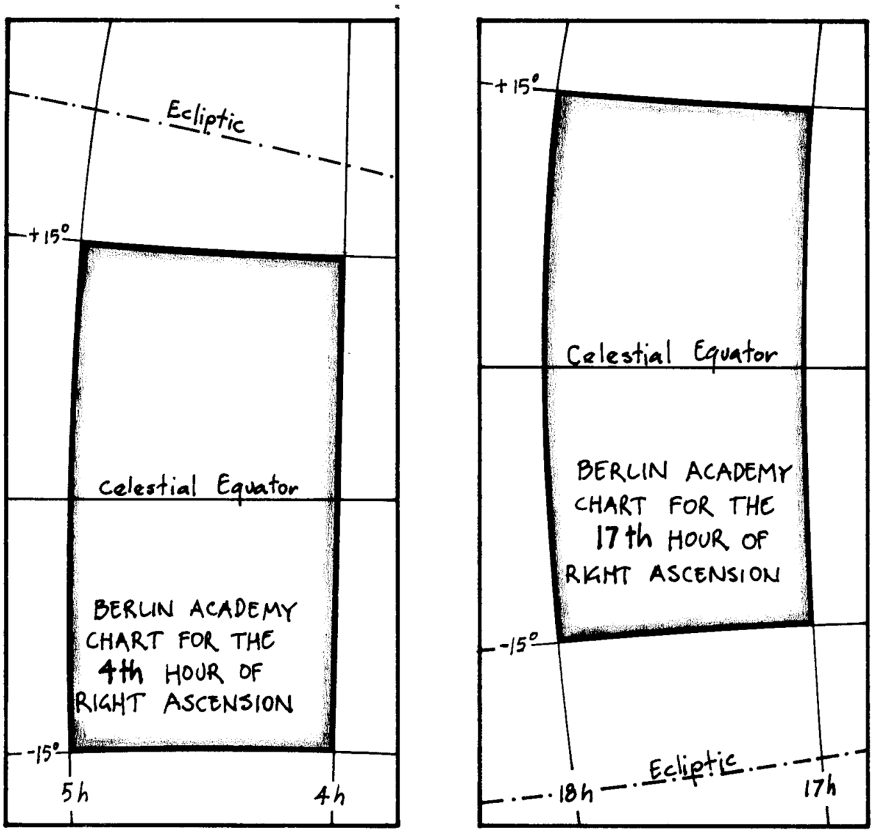


Figure 8. The Ecliptic in Relation to the Berlin Academy Charts for the 4th and 17th Hours of Right Ascension.
The 7th and 8th asteroids, Iris and Flora, were discovered in the same year as Hebe, by John Hind. He was using the academy charts for the 19th and 5th hours of right ascension, respectively. The ecliptic is as distant from the former chart as it is from the chart for the 4th hour, and as distant from the latter as it is from the chart for the 17th hour. (Robert Grant, *History of Physical Astronomy* (n.p., 1852); reprint ed., The Sources of Science, no. 38 (New York: Johnson Reprint Corporation, 1966), p. 243.

To sum up, there is no good reason to believe that the efforts of these early asteroid hunters collectively amounted to a systematic or reliable search for a distant planet.

Once again, Kuhn believes that the discovery of these minor planets was probably due ("though the evidence is equivocal") to the "shift of vision", the change in "scientific perception", that had originally "enabled astronomers to see Uranus, *the planet*". (Emphasis mine.)[102] But no Kuhnian shift of vision was involved in their discovery. Unlike a black four of hearts which one can mistake for a (conventional) four of hearts before noticing the difference between the two, asteroids were visually indistinguishable from stars for those who discovered them. What such astronomers *saw* was a different pattern of stars, or star-like bodies, at different times, and they *inferred* that this difference was due to the apparent motion of one (or more) of these bodies.

Kuhn concedes that an asteroid, unlike a planet, is not visually distinguishable from a star but he claims that, "nevertheless, astronomers *prepared* to find additional planets were able, *with standard instruments*, to identify twenty [thirteen] of them in the first fifty years of the nineteenth century".[103] (Emphasis mine.) In the context of a discussion about "changes in scientific perception", this claim is highly misleading. It suggests that a significant, and perhaps the major or only, impediment to their discovery had been merely that earlier astronomers were not primed to notice them. What was principally required, however, was a *practice* that was designed for, or that was at least conducive to, finding such objects, and which I have described above. As Herschel himself remarked in 1802:

> From the appearance of Ceres and Pallas it is evident that the discovery of asteroids requires a particular method of examining the heavens which hitherto astronomers have not been in the habit of using. I have already made five reviews of the heavens without detecting any of these concealed objects. Had they been less resembling the small stars of the heavens I might have discovered them.[104]

Anyone impressed by Kuhn's interpretation of these events, however, would be inclined to think it very much more probable that a planet such as Neptune would have been discovered earlier.

I conclude that when Adams and Leverrier took up the problem of Uranus's orbit in the 1840s astronomical knowledge of the contents of the trans-Uranian region was so slight that the working assumption that the known planets of Jupiter and Saturn were the only sensible perturbing forces on Uranus was poorly supported.

3.5 The Strength of the Counter-Arguments from Uranus's Residuals and the Opinions of Astronomers

If the above conclusion is correct then there was no *prima facie* refutation of, or strong counter-argument for, the law of gravitation based on the proof of Uranus's residuals. On the other hand, the prior corroboration or support for the law of gravitation was good - a point on which both Popper and his critics can (and do) agree - so the stronger counter-argument from $\sim(T \,\&\, A)$ was

$$(\sim(T \,\&\, A) \;\&\, T) \dashrightarrow \sim A$$

What is the *point* of corroborating theories if we then shy away from using them in the premises of such arguments?[105] It is a testimony to the strength of Popper's falsificationism that this point is so successfully obscured.

Since the presupposition of WPF cultivates indifference to the question of evidence for the auxiliary propositions, support for the assumption that Jupiter and Saturn alone sensibly perturb Uranus could have been even weaker still, and there is no reason to believe that the relevant opinions of Popper, or those who follow him, would change. There are many possibilities, any one of which, had it been the case, would have significantly *reduced* the already small probability that a suitable exterior planet would have been discovered by chance. For example, if there were two or three times as many stars to the eighth or ninth magnitude sprinkled about the ecliptic such a planet would be correspondingly less likely to have been previously observed. Or if Uranus were less massive and farther out, and therefore less conspicuous, the planet perturbing it would probably have been likewise. Further, if Ceres had been a known major planet whose mean distance was, say, 4 AU, or if no asteroid were ever brighter than, say, the tenth magnitude, it is unlikely that any asteroid search would have got underway during this period or the Berlin Academy charts been produced. And so on.

By the same token, Popper *might* have been lucky in believing the law of gravitation was *prima facie* refuted prior to the discovery of Neptune for history might have been such as to favour his belief. For example, it so happens that Uranus and Neptune were in heliocentric conjunction at about the time Bouvard prepared his tables for Uranus (see Figure 9).

Figure 9. The Relative Positions of Uranus from 1780 to 1840.
Based on Fig. 475 in Flammarion, *Flammarion Astronomy*, p. 323.

So Bouvard's modern data was from a period when Uranus was being strongly accelerated by Neptune, whilst his immediate predictions were for a period when it was being strongly decelerated by the unknown planet. W.M. Smart points out that had Uranus been half a revolution behind its actual position at the time of its discovery - and, it should be added, some of the ancient observations of this planet not been made or recovered - the problem with its orbit would have been delayed until much later in the nineteenth century. This is because the two planets would have been widely separated in their orbits until the 1870s.[106] Had that been the case, the counter-argument from Uranus's residuals for the law would by then have been much stronger, perhaps amounting to a *prima facie* refutation by, say, the turn of the century. Amongst other reasons, this is because the search for minor planets in the second half of the nineteenth century would still have been conducted much as it was - by numerous observers, equipped with increasingly accurate and comprehensive charts.[107] Not a year went by after the discovery of Hebe in 1847 in which another minor planet was not added to the list. By 1875, the total was more than 150; by 1890, it was around 300.[108] The minor planets then being discovered were typically several magnitudes fainter than Neptune, and there was a sharp increase in the rate of discovery of these bodies in the 1890s with the use of photography.[109]

Let us now return to the historical questions I raised earlier. Firstly, what did the astronomical community believe was the explanation for Uranus's residuals? It is clear that no proposed explanation was ever *generally* preferred to that of the hypothesis of an exterior planet. In introducing his tables for Uranus, Bouvard had raised the general possibility of some "extraneous influence" on the planet when he could not accommodate the ancient observations.[110] Bessel suggested an exterior planet as this influence in private correspondence early in the 1820s,[111] and by the late 1830s this hypothesis was publicly generally preferred.[112] Few astronomers ever advanced the claim that it was more probable that the law of gravitation was false.[113]

Adams and Leverrier would seem to have been the only astronomers to get further than holding some such general opinion about how this problem would be solved to formulate a testable solution candidate for it, and their position was clear. In a letter to Airy shortly after Neptune was discovered, Adams wrote, "I entertained from the first the strongest conviction that the observed anomalies were due to the action of an exterior planet; no other hypothesis appeared to me to possess the slightest claims to attention".[114] And there is no reason to believe that Adams was being wise after the event in making this remark. Elsewhere, he said, "the law of gravitation was too firmly established" to be doubted "till every other hypothesis had failed".[115] Leverrier was of a like mind.[116] The historian Morton Grosser, however, in his otherwise characteristically astute account of this episode, remarks that "there was plenty of precedent for such a doubt". As he puts it, "since the beginning of the eighteenth century, alteration of the Newtonian law had provided a dependable escape hatch for theoreticians trapped by their own hypotheses".[117] But it is doubtful Grosser means what he says here: there was plenty of precedent *of* such doubts, but as Adams himself emphasised this *would-be* escape hatch had proved entirely *un*dependable.[118] "There can be no doubt of their clarity of mind and scientific correctness," R.A. Lyttleton remarks in his detailed analysis of this episode, "in perceiving that the hypothesis of another planet was the one to be examined to the utmost limits before considering a change in the law of gravitation, or such like."[119]

There is, in short, no evidence that it has ever seemed to the astronomical community in general, much less that it seemed to Adams and Leverrier in particular, that the law of gravitation was refuted. Musgrave implies, however, that these two young theorists were trying to explain away an apparent refutation of the law. I have claimed that there was no such argument; but even if there had been, neither of these astronomers can have been *trying* to explain away something they did not believe existed. Just as they were not, *contra* Popper, *avoiding* accepting the conclusion that the law was false if they thought the case against it was a weak one. I do not have to *avoid* accepting the proposition that, say, the moon is made of green cheese if there is nothing that would rationally induce me to do so in the first place.

3.6 Some General Remarks on the Rationality of Scientific Practice where Anomalies are Concerned

Some further methodological remarks are also in order here for to understand scientific practice, or how it can be rational, it is not sufficient merely that we know which *beliefs* are formed by scientists in such circumstances, nor even which of these beliefs are *warranted* or *rational*.

Whether or not a practitioner has evidence against either conjunct of *(T & A)* aside, he or she can have *other* reasons for *supposing*, that is, for entertaining the possibility, that one of these conjuncts is false. Some such reasons can *also* be (broadly) described as epistemic, even though they are non-evidential. And there are typically good *pragmatic* reasons for making some such suppositions as well, since choosing a course of action is at stake here. These two distinctions - between a reason for believing and a reason for (merely) supposing that a proposition is true, or false, and the additional epistemic and pragmatic reasons that one can have for suppositions - are easily and often overlooked in this debate. This is especially so when the terms 'accept', 'reject', and their respective cognates are used, as they often are of course by both Popperians and their critics. 'Accept', for example, can mean 'suppose', but it can also mean or act as a surrogate for 'believe'.

Consider a practical case to illustrate the above distinctions, and some of their implications. It can be rational to begin by checking the wiring in a piece of faulty electrical equipment even though I may have *more* reason to believe that the fault lies elsewhere in that equipment. There is, we may suppose, a reliable check for wiring faults in such equipment (epistemic reason), and this check is one that can be carried out in a matter of seconds (pragmatic reason). But I would check the wiring only because I entertained the possibility, however slight, that it was faulty. If I were certain that it was not I would not do so, or at least if I did I would need some other reason for doing so in order to be acting rationally. For example, I may want to allay the suspicions of a friend who is helping me with the repairs and is unacquainted with this piece of equipment.

Thus, if a practitioner supposes that A is false in $(T \& A)$, and formulates a would-be replacement, A', for A, it does not follow either that this person does or ought to believe that there is more reason to believe that A is false rather than T. Nor does it follow that such a person is acting less than rationally in formulating A' even if he or she knew that there was more reason to believe that T was false.

The prospect of both a strong and a convenient independent test of a trans-Uranian planet hypothesis were two good additional non-evidential reasons - the first epistemic, the second pragmatic - for the course adopted by Leverrier and Adams. The independent test I have in mind is of course that of telescopic observation. A planet which would cause such residuals in a planet of Uranus's mass would itself require a large mass, and so resemble its interior neighbours, the known Jovian planets, in this regard. Since their density is low it was therefore probable that, like them, the planet would have a large volume. Moreover, the albedos of these Jovians are relatively high.[120] Thus, this planet would probably not be difficult to find, if one had a tolerably accurate prediction of its position. Both Leverrier and Adams concluded that the planet would be recognizable by its disc, and Adams at least believed that it would be no fainter than a star of the ninth magnitude.[121] Thus, a strong and convenient test for such a planet was very probable.

Those who doubted the law of gravitation, on the other hand, were themselves sufficiently pragmatic not to propose a revolution in celestial mechanics. Hoping that Uranus would turn out to be a special case, Airy, for example, plumped for a variation in the gravitational force, from that given by Newton's law, at the large distances which separated Uranus from the Sun.[122] However, if an unknown planet *were* the cause of Uranus's residuals the state of planetary theory was then such that no modified law would have any purchase once that planet was found, as proved to be the case. On the other hand, even if some modified law had seemingly removed this anomaly, and to my knowledge no one has shown how this could be done, that would not rule out the possibility that an unknown planet *was* the cause of Uranus's residuals.

To rule out this possibility and strengthen the counter-argument for Newton's law the proponents of any new or modified law would have had to show that there was no such planet. And to do *that*, of course, they too would have had to entertain the possibility that there *was* such a planet and set out to determine whether or not it existed, albeit in the expectation that it did not. Thus, if practitioners *had* attempted to make use of Uranus's residuals to refute the law of gravitation they would have been best advised, given their aim, to adopt in part the same general course of action as that of Adams and Leverrier. Furthermore, unless those who believed or suggested that the law of gravitation was false could discover *some* such body as another trans-Saturnian planet, there was *no* independent test for their option. Notice also that Popper would be obliged to describe such practitioners as avoiding a refutation (of Newton's law), though that is just what they would have been trying to achieve. Popper has often talked of the desirability of practitioners setting out to refute their theories; but anyone with this aim in mind here would have regarded the discovery of Neptune as merely a consolation prize, a sentiment for which I can find no evidence.

Once any such anomaly has been successfully removed, practitioners have both an epistemic reason and a pragmatic reason for tackling a second anomaly of the same kind in the same way. The epistemic reason arises from the fact that the second anomaly is of the same kind as the first, which of course is an inductive reason; the pragmatic reason arises from the fact that a method for tackling this second anomaly is already at hand. This is the ideal case, of course, for the second anomaly may be only analogous to the first, or the method used to remove that first anomaly may have been cumbersome or its success fortuitous. Nonetheless, this way of tackling anomalies is common in science, as it is in human affairs generally. Kuhn and Lakatos in particular have drawn our attention to it with their notions of the exemplar in the practice of normal science and the positive heuristic in a scientific research programme, respectively.[123]

The success of Adams and Leverrier induced some later astronomers to engage in wishful thinking about small residuals in the orbits of Uranus or Neptune, in the hope of predicting the position of a trans-Neptunian planet, so such patterns are not always rational.[124] But suppose that the relevant properties of the planets exterior to, say, Jupiter were such that Saturn had been accidentally discovered as Uranus was. And suppose that Uranus, Neptune, and Pluto were then each discovered, in no less rational a manner than was done in the case of Neptune, in one continuous research programme of solving such perturbation problems.[125] By the time Pluto's turn had come in this programme, any talk of the law of gravitation being apparently refuted by some unexplained residuals in Neptune's or Uranus's orbit would be *obviously* empty. Astronomers would have had good reason to believe that the solar system contained yet another distant planet, and good reason to proceed along the same lines as they had previously and successfully done.

Notes for Chapter Three

1. Popper, *Unended Quest*, p. 43; and "Replies", bk. 2: 1006.

2. See, for example, Musgrave, "Falsification", p. 397, and "Method", p. 459, who describes this anomaly as an "apparent refutation"; Radnitzky, *Progress*, p. 93 ("alleged falsification"); O'Hear, *Popper*, p.100 ("apparent counter-example"); Brown, *Perception*, p.147 ("apparent counter-instance") For Lakatos it would count, of course, as a "'falsification'" (see "Falsification", p.137). Chalmers, in *What is Science?*, p. 53-54, describes the trans-Uranian planet hypothesis as a "move to save Newton's theory from falsification".

3. Popper, "Replies", bk. 2: 1187 (n. 77).

4. Ibid., p. 986.

5. Ibid., p. 987.

6. J.N. Hattiangadi, in "The Role of Auxiliary Hypotheses", *Ratio* 16 (1974): 117, recognizes that auxiliary propositions are often the ones refuted. Hanson in "Retroductive Inference" fails to grasp this point - see, for example, pp. 24, 29-30, and 34. He supposes that a proposition such as the trans-Uranian planet hypothesis was merely conjoined with the existing set of "initial conditions", namely, A. As I have pointed out above (pp. 94-95), however, this would be to entertain a contradiction. Putnam in "Corroboration", bk. 1: 228, points out that auxiliary propositions can be "highly risky suppositions", so that "we cannot regard a false prediction as definitively falsifying a theory". Like Hanson, however, Putnam fails to see that an auxiliary proposition was refuted here. He believes that the novel auxiliary proposition,

> There is one and only one planet in the solar system in addition to the planets mentioned in S_1,

where S_1 is what I call A, was conjoined with S_1 (Putnam, "Corroboration", bk. 1: 232). And he says (p. 232) that S_1 contains the proposition,

> The solar system consists of at least, but not necessarily of only, the bodies . . . Mercury, Venus, Earth, Mars, Saturn, Jupiter, and Uranus.

Apart from the extraneous information this proposition contains (see p. 94 above), it does not allow one to predict Uranus's position as it does not imply that the set of forces on that planet is complete (see p. 115 above). Putnam also falsely believes that from the conjunction of the law of gravitation, which he calls $U.G.$, S_1, and his novel auxiliary proposition above, which he calls S_2, one can *deduce* that "the unknown planet must have a certain orbit", S_3 (p. 232). It is unclear how Putnam could ever imagine that this is the case - that it is not logically possible for a planet to have some other orbit than it does. Uranus's residuals had only one cause, but there were many possible causes; there were many possible trans-Uranian planets which could have been caused those residuals (see p. 117-18 above). Furthermore, it is unnecessary and false to deny, as S_2 does, the existence of a planet such as Pluto, in order to solve the problem of Uranus's residuals. Putnam then says that from the conjunction of $U.G.$, S_1, S_2, and S_3, one can deduce the orbit of Uranus (p. 233). In effect, then, Putnam thinks that the following two propositions are true:

(1) $(T \& A) \dashrightarrow A'$

and

(2) $(T \& A \& A') \dashrightarrow O$

If (1) and (2) *were* true, however, then

(3) $(T \& A) \dashrightarrow O$

would also be true. In other words, there would have been no problem with Uranus's orbit in the first place.

7. See, for example, Popper, *Logic*, p. 76; *Conjectures*, p. 244; and "Replies", bk. 2: 1035.

8. To make matters worse above, Popper asserts both that Newton's laws of motion were *not* affected, when no one would have supposed otherwise, and that the "much older 'system of the world'" *was* affected, when it was the discovery of Uranus (not Neptune) that refuted what I take it he is referring to here, namely, the long standing belief that the solar system contained only those planets known to the Ancients.

9. Musgrave, "Falsification", p. 397.

10. Quoted in Grosser, *Neptune*, p. 19.

11. Grosser, *Neptune*, pp. 24-27, and 41; Dieter B. Herrmann, *The History of Astronomy from Herschel to Hertzsprung* (Cambridge: Cambridge University Press, 1984), p. 36.

12. Grosser, *Neptune*, pp. 40-41; E.G. Forbes, "The Pre-Discovery Observations of Uranus", in *Uranus and the Outer Planets* , ed. Garry Hunt, Proceedings of the IAV/RAS Colloquium, no. 60 (Cambridge: Cambridge University Press, 1982) p. 72.

13. Grosser, *Neptune*, p. 41. Others suggest 19 ancient observations: Robert Grant, *History of Physical Astronomy* (n.p., 1852), reprint ed., The Sources of Science, no. 38 (New York: Johnson Reprint Corporation, 1966), p. 164; W.M. Smart, "John Couch Adams and the Discovery of Neptune", *Occasional Notes of the Royal Astronomical Society* 2 (August 1947): 4-5; A.F.O'D. Alexander, in *The Planet Uranus: A History of Observation, Theory, and Discovery* (London: Faber and Faber, 1965), p. 102.

14. Grosser, *Neptune*, p. 42. The mean of the errors in the ancient observations is now known to be only a little over 6" - see Alexander, *Uranus*, p. 275.

15. Smart, *Occasional Notes*, p. 5. Grosser is less charitable to Bouvard in *Neptune*, pp. 43-44; Grant in *History*, p. 178, is characteristically more so.

16. On the accuracy of the ancient observers, see Grosser, *Neptune*, pp. 42-44; also V.V. Podobed, *Fundamental Astronomy: Determination of Stellar Coordinates*, ed. A.N. Vyssotsky (Chicago: University of Chicago Press, 1965), p. 10. See also Alexander, *Uranus*, pp. 102-3, and 274-75.

17. Smart, *Occasional Notes*, p. 5; Grosser, *Neptune*, pp. 45-46.

18. Grosser, *Neptune*, p. 46.

19. Ibid., p. 100. See also Alexander, *Uranus*, p. 94; and George Chambers, *A Handbook of Descriptive Astronomy*, 3d ed. (Oxford: Clarendon Press, 1877) p. 168.

20. On the accuracy of planetary theory, see Grosser, *Neptune*, p. 45; Smart, *Occasional Notes,* p. 22; Alexander, *Uranus*, pp. 208-9. On the accuracy of the modern observations, see Alexander, *Uranus*, p. 102.

21. Grosser, *Neptune*, ch. 3, in particular, p. 69. See also Smart, *Occasional Notes*, p. 3; Grant, *History*, p. 175; Hanson, "Leverrier", p. 359; A. Pannekoek, "The Discovery of Neptune",

Centauras 3 (1953): 127; R.A. Lyttleton, *Mysteries of the Solar System* (Oxford: Clarendon Press, 1968), p. 216.

22. Grosser, *Neptune*, pp. 96-99; Grant, *History*, pp. 175-77; Smart, *Occasional Notes*, pp. 25-26.

23. Grosser, *Neptune*, p. 97; Smart, *Occasional Notes*, p. 25.

24. Grosser, *Neptune*, p. 97.

25. Ibid., pp. 75-80; Grant, *History*, pp. 168-70.

26. Smart, *Occasional Notes*, pp. 51-55; Pannekoek, "Neptune", pp. 132-33; Lyttleton, *Mysteries*, p. 216; Gilbert E. Satterthwaite, *Encyclopedia of Astronomy* (London: Hamlyn, 1970), s.v. "Adams".

27. Grosser, *Neptune*, chap. 4; Smart, *Occasional Notes*, pp. 12-18.

28. W.M. Smart, *Celestial Mechanics* (London: Longmans, Green & Co. Ltd., 1953), p. 248; Smart, *Occasional Notes*, p. 28; Grosser, *Neptune*, p. 87.

29. Grant, *History*, pp. 176-77.

30. Quoted in Hanson, "Leverrier", p. 361.

31. Putnam, "Corroboration", bk. 1: 226-28.

32. Grosser, *Neptune*, p. 47.

33. Ibid., pp. 46-47.

34. Smart, *Occasional Notes*, p. 7. See also Grant, *History*, p. 177. Putnam suggests in "Corroboration", p. 227, that astronomers might have turned to this suggestion or else contemplated "significant nongravitational forces" if the hypothesis of an exterior planet had not been successful, which indicates that his knowledge of this episode is slight.

35. Grant, *History*, pp. 177-78. See also Grosser, *Neptune*, p. 47; and Smart, *Occasional Notes*, p. 7.

36. Several proposed searches after October 1845 were thwarted, delayed, or proved to be unsuccessful - see Grosser, *Neptune*, pp. 92, 107-110, 120-23, and 139; Chambers, *Handbook*, p. 169; O.M. Mitchell, *The Planetary and Stellar Worlds: An Exposition of the Discoveries and Theories of Modern Astronomy* (London: T. Nelson & Sons, 1859), pp. 183-84.

37. Ake Wallenquist, *The Penguin Dictionary of Astronomy*, ed. and trans. Sune Engelbrektson (Harmondsworth, Middx.: Penguin Books, 1966), s.v. 'albedo' and 'density', supplies the following data:

	Albedo	Mean density (water = 1)
Jupiter	0.41	1.33
Saturn	0.42	0.68
Uranus	0.45	1.60
Neptune	0.54	2.25

38. Smart, *Occasional Notes*, pp. 21-23, 26, 29, and 49.

39. Alexander, *Uranus*, p. 90; Forbes in "Pre-Discovery Observations", p. 75, concurs. See also Herrmann, *History*, p. 36, who attributes at least 17 ancient observations to the English astronomer, James Bradley. This would bring the total to 36, or thereabouts.

40. Chambers, *Handbook*, p. 157; Grosser, *Neptune*, pp. 19-22.

41. R.H. Austin, "Uranus Observed", *British Journal for the History of Science* 3 (June 1967): 275-84.

42. Mason, *Main Currents*, p. 238; J.A. Bennett, "The Discovery of Uranus", *Sky and Telescope* 61 (March 1981): 191.

43. Bennett, "Uranus", p. 191.

44. Wallenquist, *Dictionary*, s.v. 'planetary nebula'.

45. J.L.E. Dreyer, Biographical Introduction to *The Scientific Papers of Sir William Herschel*, by William Herschel, 2 vols., ed. J.L.E. Dreyer (London: The Royal Society and The Royal Astronomical Society, 1912), 1: xxix.

46. A. Pannekoek, *A History of Astronomy* (London: George Allen and Unwin, 1961), p. 294. Bradley's objective, for example, was 1".6. Roy L. Bishop, ed., *Observer's Handbook for 1988*, The Royal Canadian Astronomical Society (Toronto: University of Toronto Press, 1987) gives the following sizes for (modern) telescopes to observe their discs: Uranus 3" (p. 128); Neptune, 4" (p. 129). T.W. Webb, in *Celestial Objects for Common Telescopes* (London: Longmans, Greens and Co., 1873), p. 168, says that in his 5".5 Frauenhoffer achromatic the disc of Neptune is only ever "dull and ill defined", and that his 3.7" telescope is "not sufficient to define" Uranus's disc "perfectly".

47. Bennett, "Uranus", p. 191; see also Grosser, *Neptune*, pp. 17-18; and Grant, *History*, pp. 533-34.

48. Arthur P. Norton and J. Gall Inglis, in *A Star Atlas and Reference Handbook for Students and Amateurs*, 15th ed., ed. R.M.G. Inglis (Edinburgh: Gall and Inglis, 1966), supply the following data for mean opposition visual magnitude (p. 17) and angular diameter at mean distance (p. 32):

	Mean opposition at visual magnitude	Angular diameter mean distance
Uranus	5.74	3".8
Neptune	7.65	2".1

49. Grant, *History*, p. 191.

50. Kuhn, *Scientific Revolutions*, pp. 115-16.

51. Ibid., p. 115.

52. Forbes, "Pre-Discovery Observations", p. 69.

53. Grosser, *Neptune*, p. 24.

54. Ibid., pp. 40-41.

55. Ibid., p. 30.

56. Ibid. Eric G. Forbes, in "The Correspondence between Carl Friedrich Gauss and the Rev. Nevil Maskelyne (1802-5)" *Annals of Science* 27 (September 1971): 236, reports Piazzi as remarking, "If I had not been in the habit of observing the stars four, five, six times and even more, I should certainly not have discovered my present one [Ceres]."

57. The mean daily apparent motion of Neptune is approx. 1'20" - see Fig. 5, p. 128 above. The daily apparent motion of Ceres when it was discovered by Piazzi was 5'19" (from data in Grant, *History*, p. 238). Grosser, *Neptune*, p. 30, gives Ceres's motion in right ascension at this time as 4^{m}; this should read 4'.

58. Charles T. Kowal and Stillman Drake, "Galileo's Observation of Neptune", *Nature* 287 (September 25, 1980): 311-13.

59. Grosser, *Neptune*, p. 139; Smart, *Occasional Notes*, p. 49.

60. Camille Flammarion, *The Flammarion Book of Astronomy*, trans. Annabel and Bernard Pagel (London: George Allen and Unwin, 1964), p. 324.

61. Neptune was at opposition on April 28, 1960 (1960.321), and was therefore at the same point in its orbit, one revolution earlier, at 1795.529 (T = 60, 190 days). The Earth was then 0.211 years ahead of Neptune, so approx. 0.211 year earlier opposition occurred. 1795.529 - 0.211 = 1795.318, or approx. April 27, 1795.

62. Smart, *Occasional Notes*, p. 49.

63. Grosser, *Neptune*, pp. 117-18., See also J.S. Encke, "Galle's Discovery of Neptune", in *A Source Book in Astronomy*, eds. Harlow Shapely and Helen E. Howarth, Source Books in the History of the Sciences (New York: McGraw Hill Book Co., 1929), p. 253.

64. Grosser, *Neptune*, p. 118; Encke, "Galle's Discovery", pp. 253-54.

65. Norton and Inglis, *Star Atlas*, p. 42.

66. Alexander, *Uranus*, pp. 29-30.

67. Grosser, *Neptune*, p. 30. 'Lilienthal Detectives' is Grosser's description. See also Grant, *History*, pp. 237-41, for an account of their efforts.

68. Grosser, *Neptune*, pp. 27-30.

69. Ibid.

70. Ibid., pp. 30-31.

71. Ibid., p. 34.

72. Ibid., pp. 34-35.

73. Ibid. Herschel estimated the diameter of Ceres, for example, as 161.6 miles (Grosser, *Neptune*, p. 34). Wallenquist, *Dictionary*, s.v. 'minor planets', gives its diameter as 427 miles, still tiny in astronomical terms. Mercury, the smallest major planet in the solar system, has an equatorial diameter of 3,000 miles (Wallenquist, *Dictionary*, s.v. 'Mercury').

74. Grosser, *Neptune*, p. 36.

75. Ibid., pp. 36-37.

76. Ibid., p. 39.

77. Patrick Moore, *Guide to the Planets* (London: Eyre and Spottiswood, 1955), p. 117.

78. Grant, *History*, p. 242.

79. A reasonable idea of the effectiveness of at least Olbers's search in this respect can be had from the following consideration. Of the asteroids discovered after Vesta, those listed below are the brightest asteroids which were in a good position for viewing, in or near one or other of Olbers's search areas, at some time during 1981-88 (approx. half the length of his search):

Minor Planet	1981	1982	1983	1984	1985	1986	1987	1988
Hebe		x				x		
Flora				x		x		
Metis		x		x				
Parthenope				x				
Irene								x
Melpomene								x
Lutetia	x							
Laetitia							x	
Dapne					x			
Nysa	x							
Nausika					x			
Prokne						x		
Herculina							x	

In general, these asteroids were 9th or 10th magnitude bodies at the time. See "Ephemerides for the Brightest Asteroids" in John R. Percy, ed.,Observer's Handbook 1981, The Royal Astronomical Society of Canada (Toronto: University of Toronto Press, 1981); Roy L. Bishop, ed., Observers Handbook 1982, The Royal Astronomical Society of Canada (Toronto: University of Toronto Press, 1982), and for the years 1983-88 under the same editor . Of the asteroids discovered after Vesta, Astrea is not especially conspicuous. For the years given above, Hebe, Iris, Flora, Metis, Hygiea, Eunomia, Melponene, and Amphritite, for example, are all appreciably more conspicuous than Astrea

80. The following data is calculated from or supplied by Grant, *History*:

Apparent motion at discovery
(shortly after, in the case of Astrea)

Ceres	5' 19"	(p. 238)
Pallas	22' 22"	(p. 239)
Astrea	14' 21"	(p. 242)

See also Forbes, "Correspondence", p. 235, who quotes Maskelyne as saying, "If astronomers would observe on two successive nights, they would run a chance of discovering new planets. Or if they observed stars twice in the same night, with an interval of one, two, or three hours, with a good equatorial instrument, they would find them out by their motion in the interval." Clearly, this would not be an adequate strategy in the case of Neptune (see notes 63 and 64, p. 144 above).

81. Neptune is a relatively bright 8th magnitude body. Ceres was an 8th magnitude body on discovery (Grosser, *Neptune*, p. 30). It is the largest of the minor planets; but Herschel was so "disconcerted" by its optical smallness when he tried to measure its size that he was moved to check his instruments (Grosser, *Neptune*, p. 34). Juno is described by Grosser, in *Neptune*, p. 36, as having been "faint" when Harding found it. It is typically duller than Ceres - see Bishop, *Observer's Handbook*, for the years, say, 1982-88. Astrea was a 9th magnitude body upon discovery (Grant, *History*, p. 242).

82. Herrmann, *History*, p. 30; Grant, *History*, p. 241.

83. Pannekoek, *History*, p. 467; G. van Biesbroeck "Star Catalogues and Charts", in *Stars and Stellar Systems*, 9 vols., ed. Gerald P. Kuiper, vol. 3, *Basic Astronomical Data*, ed. Kaj A. Strand (Chicago: University of Chicago Press, 1978), p. 479.

84. Chambers, *Handbook*, p. 863.

85. Ibid. Grant, *History*, p. 242. Pannekoek in *History*, p. 353, claims that the charts "cover the zodiacal zone"; van Biesbroeck in "Star Catalogues", p. 479 claims that they "cover the ecliptic" to a limiting visual magnitude of 11. Both claims are false - see Figure 3, p. 126 above.

86. The first two sections of Argelander's *Bonner Durchmusterung* contain 216,059 stars to magnitude 9.5 (9th magnitude), from -2^{o} to $+41^{o}$ declination. See Chambers, *Handbook* , pp. 860-861.

87. Norton and Inglis, *Star Atlas*, p. 32.

88. Hanson, "Leverrier", p. 362. Both Adams and Leverrier assumed that the planet perturbing Uranus was coplanar with the ecliptic (Smart, *Occasional Notes*, p. 20); see also Urbain J.J. Leverrier, "Prediction of the Position of Neptune" in *A Source Book in Astronomy*, eds. Harlow Shapely and Helen E. Howarth, Source Books in the History of the Sciences (New York: McGraw Hill Book Co., 1929), pp. 249-50.

89. Norton and Inglis, *Star Atlas*, p. 32.

90. Ibid.

91. This is because their mean distances were appreciably greater than the mean distance of Neptune (see p. 118 above), and the square of a planet's period is proportional to the cube of its mean distance (Kepler's third law).

92. Grosser, *Neptune*, p. 117.

93. This does not matter nearly so much if one is looking, as they were, for any one of perhaps many such bodies.

94. This is because the path of any body in orbit around the sun intersects the ecliptic; moreover, most such orbital planes are relatively flat.

95. Grosser, *Neptune*, p. 36; Gunter D. Roth, *The System of Minor Planets*, trans. Alex Hein (Princeton: D. Van Nostrand Company Inc., 1962), pp. 28-29.

96. The ascending node of Ceres is approx. 80°. Assuming a circular orbit (eccentricity = 0.07), Ceres reaches its highest point above the plane of the ecliptic 90° further on, in Leo, just by Olbers's search area in north-western Virgo. The mean distance of Ceres is approx. 2.77 AU and the inclination of its orbit to the ecliptic is approx. $10^{\circ}.6$. So when it is in opposition at this point in Leo it is 1.77 AU from the Earth and thus approx. $16^{\circ}.5$ above the ecliptic. So when it is visible nearby in Virgo (at opposition,or either side of opposition), Ceres is well removed from the ecliptic, beyond the zodiac. See Chambers, *Handbook*, pp. 900-901, for orbital data.

97. Grant, *History*, p. 239.

98. The ascending node of Vesta is approx. 103°. Assuming a circular orbit (eccentricity = 0.09), Vesta reaches its highest point above the ecliptic in north-western Virgo. The mean distance of Vesta is approx. 2.36 AU and the inclination of its orbit is approx. $7^{\circ}.1$. So when it is in opposition in this part of Virgo it is 1.36 AU from the Earth and thus approx. 12° above the ecliptic. Vesta was in retrograde motion when it was discovered (Grant, *History*, p. 241), so it was roughly near opposition at the time. Thus Olbers was not searching closer to the ecliptic at the time than, say, the edge of the zodiac. See Chambers, *Handbook*, pp. 900-901, for orbital data.

99. Grant, *History*, p. 240.

100. Grosser, *Neptune*, p. 36.

101. Grant, *History*, p. 242.

102. Kuhn, *Scientific Revolutions*, p. 116.

103. Ibid.

104. William Herschel, *The Scientific Papers of Sir William Herschel*, 2 vols., ed. and with a Biographical Introduction by J.L.E. Dreyer (London: The Royal Society and The Royal Astronomical Society, 1912), 2: 197.

105. Putnam, "Corroboration", bk. 1: 226.

106. Smart, *Occasional Notes*, p. 51.

107. Basil J.W. Brown, *Astronomical Atlases, Maps and Charts: An Historical and General Guide* (London : Search Publishing Company, 1932), chap. iv; Pannekoek, *History*, p. 354. See also Chambers, *Handbook*, p. 864.

108. Chambers, *Handbook*, pp. 900 - 909; Andrew C. Cromelin and Fred L. Whipple, s.v. "Minor Planet", in *Encyclopedia Britannica*, 1951 ed.

109. Pannekoek, *History* , p. 354.

110. Grosser, *Neptune*, p. 42.

111. Herrmann in *History*, p. 36, says Bessel suggested this hypothesis in a letter to Olbers in 1823; Forbes in "Pre-Discovery Observations", p. 72, says he suggested it in a letter to Gauss in 1824. See also Grosser, *Neptune*, pp. 46-52.

112. Grosser, *Neptune*, pp. 52-56.

113. Alexander, *Uranus*, p. 97; Grosser, *Neptune*, p. 48; Smart, *Occasional Notes*, p. 8.

114. Quoted in Smart, *Occasional Notes*, p. 25.

115. John Couch Adams, *The Scientific Papers of John Couch Adams*, 2 vols., ed. William G. Adams (Cambridge: Cambridge University Press, 1896), 1: 7.

116. Grosser, *Neptune*, p. 100; Hanson, "Leverrier", p. 376.

117. Grosser, *Neptune*, p. 47.

118. Adams, *Scientific Papers*, 1: 7.

119. Lyttleton, *Mysteries*, p. 228. Following the centenary of the discovery of Neptune, there has been something of a revival of interest amongst mathematical astronomers in the problem Leverrier and Adams tackled, and the methods each adopted. There is some disagreement amongst these astronomers about the amount of luck, if any, that either or both of them enjoyed, but no disagreement that luck need not have been a major or decisive factor. Cf. Lyttleton *Mysteries*, p. 233, and Smart, *Occasional Notes*, p. 50, and *Celestial Mechanics*, chap. 16.

     Lyttleton, in "The Rediscovery of Neptune", in *Vistas in Astronomy 3*, ed. Arthur Beer (London: Pergamon Press, 1960), pp. 25-46, offers an accurate and simpler method by which the planet could have been found, based on a determination of heliocentric conjunction of

Uranus and Neptune (see also Fig. 9, p. 134 above). C.J. Brookes, in "On the Prediction of Neptune" *Celestial Mechanics* 3 (1970): 79-80, concludes that "the approximate position of the planet could have been predicted with sufficient accuracy to lead to its discovery, for any value of the mean distance from 30 AU to 40 AU . . . any time after 1830 when sufficient observational material became available . . . [though] there is nevertheless an element of chance present".

Bode's law led both Adams and Leverrier to assume too large a value for the mean distance of the unknown planet; and hence a correspondingly larger value for the mass of this planet was required to produce the same forces on Uranus as Neptune does. This is only to say, however, that they had described another member of the family of possible planets causing Uranus's residual perturbations; but not one whose *longitude* differed significantly from that of Uranus. See Smart, *Occasional Notes*, pp. 49-51.

120. Wallenquist, *Dictionary*, s.v. 'albedo'.

121. Grosser, *Neptune*, pp. 108, 112, and 120.

122. Ibid, p. 48.

123. Kuhn, *Scientific Revolutions*, pp. 186-91; Lakatos, "Falsification", pp. 134-38.

124. Morton Grosser, "The Search for a Planet Beyond Neptune", *Isis* 55 (August 1964): 163-83. See also Owen Gingerich, "The Solar System Beyond Neptune", in *Frontiers in Astronomy: Readings from Scientific American* (San Francisco: W.H. Freeman and Co., 1970), pp.78-84.

125. Following the discovery of Neptune, Leverrier remarked that "this success allows us to hope that after thirty or forty years of observing the new planet, we will be able to use it in turn for the discovery of the one that follows it in order from the Sun". Quoted in Grosser, "Search", p. 164.

Chapter Four

Uranus's Orbit and Other Anomalies: The Popperian Legacy

4.1 Introduction

Popper's commentators offer a variety of accounts of the general practice, so-called, of avoiding a refutation, and of the problem of Uranus's orbit in particular. In this chapter I shall discuss a sample of these accounts. My principal aims are, firstly, to show how common is the acceptance of Popper's mistakes, even amongst some philosophers who claim to reject his falsificationism. Secondly, I shall analyse the consequences for their accounts of this unthinking acceptance of elements of his falsificationism. In the process, I shall have occasion to further criticise Popper, as well as some aspects of the positions taken up by these other philosophers, for the light it sheds on rationality in scientific practice where a problem like that of Uranus's orbit is concerned.

As I pointed out in the previous chapter, the law of gravitation would have been *prima facie* refuted by the argument from Uranus's residuals only if there had been good reason to believe as false what is true, namely, that some unknown body was responsible for those residuals. Had there been this reason then the course adopted by Leverrier and Adams would have been unlikely to succeed, and this case would be, as Popper believes it is, untypical of science. Many of Popper's commentators correctly reject this belief of his. It is *not* untypical, they point out, for anomalies to be resolved by such changes in the auxiliary propositions or in the presuppositions upon which the inquiry is based. Where many of these commentators still go wrong, however, is in *retaining* the presupposition of WPF which encouraged Popper to hold this belief about anomalies in the first place. So although these commentators have a better grasp of history (at least in this respect) than Popper does, he has a better grasp of his epistemology than they do.

The methodology of scientific research programmes which Lakatos devised is a pertinent case in point, and I begin by critically examining the relevant aspects of this methodology.

A belief that major theories have or need to be protected from the supposedly continual threat of refutation is one of the irrational conclusions that Lakatos and others are driven to by this problem of how to reconcile Popper with the history of science. Kuhn arrives at effectively the same conclusion about the need for such dogmatism, even if by a rather different route, and I examine his account too (4.2). On the other hand, some philosophers do not see that Popper has this problem in the first place, or they find some creative alternative to the Lakatosian protection racket that would allegedly solve it. I analyse two such accounts and pick up the points I made in 3.6 on how practitioners rationally deal with anomalies, to further show how these points are obscured or not understood in this debate (4.3). Finally, I return to that other strand in Popper's thought on 'avoidance' of refutation, namely, the alleged dangers of *ad hoc* hypotheses. I examine the accounts of three philosophers of science who have been misled by one or other of these strands in his thought and imply or assert that there *is* something fishy about the hypothesis of a trans-Uranian planet devised by Leverrier and Adams. I provide a further defence of the rationality of entertaining this hypothesis at the time, and detail how the historical interpretations such philosophers are driven to prefer are badly mistaken (4.4).

4.2 The Negative Heuristic, Paradigm Commitment, and the Belief in Dogmatism

As we saw in 1.5, Lakatos rejects SPF - it is part of what he calls "dogmatic falsificationism".[1] But he retains WPF, and in no small way Lakatos's project should be understood as a doomed attempt to rationalise the history of science for the irrationality this false doctrine injects. He says:

> All scientific research programmes may be characterized by their '*hard core*'. The negative heuristic of the programme forbids us to direct the *modus tollens* at this 'hard core'. Instead, we must use our ingenuity to articulate or even invent 'auxiliary hypotheses', which form a *protective belt* around this core, and we must redirect the *modus tollens* to *these*. It is this protective belt of auxiliary hypotheses which has to bear the brunt of tests and get adjusted and re-adjusted, or even completely replaced, to defend the thus-hardened core.[2]

And of Newtonian mechanics in particular, which Lakatos regards as the paradigm of a scientific research programme, he adds:

> In Newton's programme the negative heuristic bids us to divert the *modus tollens* from Newton's three laws of dynamics and his law of gravitation. This 'core' is 'irrefutable' by

> the methodological decision of its protagonists: anomalies must lead to changes only in the 'protective' belt of auxiliary, 'observational' hypotheses and initial conditions.[3]

So for Lakatos every anomaly would 'falsify' the core. Yet core beliefs remain intact. Thus he came to believe that only obedience to some *rule or convention of method* explains why these (supposedly) beleaguered cores are not in some sense refuted, and why scientists retain them. But Adams and Leverrier had no need of Lakatos's rule. Nor is there any reason, as we have seen, to believe that they were following it, even if in querying and then replacing an auxiliary proposition they were acting as they would have done had they been following this rule. Moreover, take into account the good non-evidential reasons that practitioners can have for first exhausting such options, especially when the core in question is something like Newtonian mechanics, and there is even less reason to believe in the existence of 'minders' for core beliefs.[4]

This is not to say, of course, that there were no dogmatic Newtonians; simply that Lakatos is mistaken in believing or implying that Newtonians needed to be part of some protection racket to practice their craft. And whether or not *every* anomaly prior to that of Uranus's residuals also failed to provide a *prima facie* refutation of the core is a question we can leave to the historians, except to say that it would be surprising if *any* such anomaly did so, for in the area in which it was then applied the law is a good approximation to the truth. Consider one further example, for future reference. Early in the eighteenth century some French physicists had grave doubts about the law of gravitation, citing the increase in gravitational acceleration that had been detected away from the equator.[5] These doubts likewise rested on some hitherto convenient *assumptions*, in particular, that the Earth was a sphere. Newton pointed out, however, that the increase in gravity would be accounted for if the Earth were an oblate spheroid (a sphere with an equatorial bulge), which it is.[6]

To return to Lakatos, the firmness of his commitment to WPF is revealed in his elementary logical blunder above that *modus tollens* is redirected or diverted from the core, a mistake that has been repeated by more than one of *his* commentators.[7] Leaving aside the fact that the logical relations in *modus tollens* are not subject to the will of any scientist, to be directed or redirected as he or she sees fit, *modus tollens* is not directed at the core. It is directed, to continue the metaphor, at the *conjunction* of the core and Lakatos's would-be protective belt, like so:

$(T \ \& \ A) \dashrightarrow P$

$\sim P$

therefore $\sim(T \ \& \ A)$

If scientists do not continually mount arguments of the form:

$(T \ \& \ A) \dashrightarrow P$

$\sim P$

therefore $\sim T$

it is not because they are practitioners of that special activity we call science, in possession of some fraternal heuristic principle. It is just that, like many non-scientists, scientists can generally be relied upon when they see an argument of the latter sort to recognize that it is *invalid*.

Some of those who would defend the methodology of scientific research programmes, like John Worrall, have abandoned the negative heuristic altogether, even though it is one of the most distinctive features of that methodology. But not only does Worrall fail to acknowledge this departure, he even says in response to a critic of the methodology, who defines this rule as Lakatos does above, that one would be "mad" to advise anyone to follow it (which is true).[8] According to Worrall, the methodology places "no restrictions" on any scientist who attempts to modify or replace the core, though he does comment that the business of replacing the core is such an "enormous undertaking" it is "no wonder . . . that some theories (those which form integral parts of powerful research programmes) have had such long lives, surviving many clashes with experiment".[9] Since a research programme would not *be* powerful, however, if it did not survive there is no cause for such wonderment. And Worrall's comment is a good example of the mistake of conflating the lack of sufficient evidential reason for believing or accepting that a proposition is false with having a good pragmatic reason for (provisionally) not entertaining the possibility that it is so. If the reason for the longevity of core beliefs like Newtonian mechanics were that it was too much trouble to replace them, science would not be a rational enterprise. It was certainly easier, for example, to come up with the suggestion that the Earth was an oblate spheroid than to look for a replacement for the law of gravitation. But it was evidence obtained from expeditions to Lapland and Peru, which measured the length of a degree of latitude in each location, that convinced scientists to accept Newton's suggestion and removed the associated doubts about the law.[10]

In contrast to Worrall, Anthony O'Hear articulates a firm Lakatosian line in *Karl Popper*. He says that Popper's "way of looking at saving hypotheses obscures the fact that their role is *precisely* to deflect criticism from the original 'core' theory, so as to treat it as virtually unfalsifiable".[11] (Emphasis mine.) Furthermore, he says, "The demarcation criterion overlooks the importance of the metaphysical or unfalsifiable status of research programmes to the growth of science, for growth would actually be hindered were theories not allowed to stand despite 'falsifications'."[12]

O'Hear also describes the strategy of employing saving hypotheses as one of "directing attention away from the theory under test onto criticism of the assumption that other things are not equal in the test situation".[13] Now apart from the fact that the assumption O'Hear is referring to here is the assumption that other things *are* equal, this description makes Lakatos's position out to be innocuous, for there is obviously nothing to object to in the suggestion that we can or do switch our attention from one counter-argument to another. But this is *not* Lakatos's position. If the redirection or deflection metaphor is taken seriously, Lakatos and O'Hear are urging us to treat a counter-argument for the core as if it were a counter-argument for the "protective belt". But if we were to treat counter-arguments for the core in this fashion we would sink into self-contradiction. For example, if the criticism of the law of gravitation from the evidence of Uranus's residuals had been deflected by Newtonians onto the assumption that other things were equal in the test situation, they would have contradicted themselves for this criticism *presupposes* that these other things were equal. In particular, it presupposes that there were no sensible influences on Uranus other than those that had already been taken into account.

The case of Uranus's residuals also does not provide O'Hear with any reason to doubt the demarcation criterion. This case is simply one in which it so happened that an auxiliary proposition was refuted rather than a core proposition. O'Hear comes to believe otherwise because he embraces the deflection metaphor. He thinks that the core of a research programme is the target for a swarm of anomaly based counter-arguments, all of which are in some incoherent sense refutations of the core but which are "deflected" from it. This target is thus virtually unhittable by such arguments, hence O'Hear's skepticism that we can distinguish hittable targets from unhittable ones, at least where core propositions are concerned.

Popper does not employ the deflection metaphor but his evasion and immunisation metaphors amount to the same thing, and a shadow of a doubt about the demarcation criterion even falls across *his* thinking. This case, he says, shows that "falsifiability or testability cannot be regarded as a very sharp criterion".[14] For Popper to make such a concession, much less where none is required, emphasises the strength of his commitment to SPF *cum* WPF.

If one *were* to try to engender doubts about the demarcation criterion, however, by doubting the refutability of core propositions, one plausible line of attack would be to choose a case in which it was generally believed that a core proposition *was* refuted and then attempt to undermine that belief. But why pick a case in which all concerned, O'Hear included, believe that no core proposition was refuted? The fact that a counter-argument for p does not refute p because it contains a premise that turns out to be false is not a reason to believe that p should have been treated as "virtually unfalsifiable" or that the demarcation criterion is defective, even if the counter-argument in question did once *seem* to be a refutation of p.

I can only think that the reason O'Hear does not see this point is his flirtation with skepticism, a flirtation which does much to make the deflection metaphor seem plausible to him. If skepticism about contingent knowledge were true then no contingent proposition could be refuted and the demarcation criterion would collapse. It would not matter that we could still distinguish those contingent propositions whose contraries included descriptions of logically possible states of affairs (Popper's potential falsifiers) from those contingent propositions whose contraries did not, if we could never know that any such description was true. A genuine skeptic would simply make these points and be done with Popper's criterion.

But O'Hear takes a more circuitous route for he is, like Popper, an inconsistent skeptic. Whilst holding that no refutation is "conclusive", O'Hear has no compunction in describing those contingent propositions as refutable, or as refuted, that many or all non-skeptics characteristically do, or would do. For example, he says that the Adams-Leverrier hypothesis could have been refuted,[15] and that the law of gravitation was eventually refuted.[16] But if any alleged refutation is inconclusive it does not establish that the proposition allegedly refuted is false, and so it is not a refutation. Hence the inconsistency. And someone who believes that refutations *are* inconclusive, as O'Hear does, would be inclined to doubt the value of a logical distinction between that which can be refuted and that which cannot when whatever is refuted today, so that person is obliged to believe, may be unrefuted tomorrow. O'Hear says:

> The demarcation criterion loses any practical value it might have in enabling us to know when we are being truly scientific in rejecting a theory that could be saved from falsification, as opposed to being unscientific in attempting to save it from falsification. The point is that if falsification can never be conclusive, it is hard to see how apparent falsification can be absolute grounds for rejection.[17]

But the point is that if falsification can never be conclusive, *falsification* cannot be "absolute grounds for rejection". It is because O'Hear cannot surrender the belief that there *are* falsifications, properly so called, that he resorts to 'apparent falsification' in trying to make his point above. He does not notice that he thereby shows himself to be an inconsistent skeptic, part of the reason being that it is *true* that apparent falsifications do not provide absolute grounds for rejection. That is a point on which skeptics and non-skeptics alike can agree.

O'Hear insists that to protect core propositions from counter-arguments "is not, of course, to deny that counter-evidence is and should be taken seriously".[18] But what else can it be? For all that, O'Hear *does* recognize, as Lakatos did, that he is advocating that "dogmatism has an essential role to play in science".[19] Dogmatism, O'Hear emphasises, is one of the "essential elements of the scientific attitude".[20] This view is not uncommon. It is implicit, for example, in *The Structure of Scientific Revolutions*, whilst in a relatively neglected paper, "The Function of Dogma in Scientific Research", published soon after, Kuhn spells it out.[21] The case of Uranus's residuals, he says on that occasion, is one of those anomalies which "calls established techniques and beliefs in doubt", and shows the need for such techniques and beliefs to be dogmas if they are to survive.[22] Newton-Smith is often sharply critical of Lakatos and Kuhn; nonetheless, he believes that Lakatos discerned one of the "important facets of scientific procedure" in recognizing that "scientists properly have a sufficient degree of *faith* in their basic theoretical postulates . . . that anomalies are explained away".[23] (Emphasis mine.) Feyerabend thinks that "scientists must develop methods which permit them to retain their theories in the face of plain and unambiguously refuting facts, even if testable explanations for the clash are not immediately forthcoming". He calls this alleged requirement "the principle of tenacity".[24] Popper agrees with these commentators on the need for such an attitude or such a principle. The case of Uranus's residuals, he says, shows that "*some* dogmatism is fruitful in science". (Emphasis mine.)[25] Thus, the only significant difference between Popper's position and that of his commentators here is that he thinks there is less need for dogmatism because he thinks the practice, so-called, of avoiding a refutation is less common in science.

If these philosophers, Feyerabend aside, did not falsely believe that dogmatism was necessary they would presumably reject this cognitive attitude as inimical to any rational activity, which they all suppose science at least ought to be. 'Dogmatism' is so unambiguously a derogatory term that for such people to *advocate* dogmatism is a sure sign of the grip on their thinking of *some* false belief or presupposition, and which I claim is typically SPF or WPF. Holding either of these presuppositions forces one to mistake weak arguments for strong or compelling ones. Once it then becomes apparent that it is as well scientists have *not* been moved to accept the conclusions of those arguments, one option is to conclude, as all of the above philosophers do, that it is also as well not to be moved by strong or compelling arguments, at least in such cases. This irrational attitude they correctly describe as dogmatism or faith. The failure to distinguish supposition from belief or acceptance makes it harder to resist the allure of dogmatism here. This is because the possibility of rationally *supposing* that p is true even when there is a strong counter-argument for p is not a possibility that is countenanced.

But there is more, for the attempt to *justify* such (all?) dogmatism is self-defeating. To show that practitioners ought sometimes or often hold core propositions dogmatically, Popper and others are obliged to proffer some cognitive benefit that stands to be gained from adopting this attitude. In the case of the law of gravitation, for example, that benefit would consist of such things as the discovery that the Earth is an oblate spheroid or that the solar system does not end at Uranus, together with the removal of the associated anomalies. But if there is the prospect of such a benefit practitioners have only to be made aware that this is so in order to remove the alleged need for them to hold core beliefs dogmatically, as they would then have a good reason for not doing so, namely, the prospect of that cognitive benefit. This prospect is good, of course, only when the counter-argument for those core beliefs is weak, which is the real reason practitioners do not need to relinquish those beliefs.

In short, to get the argument for dogmatism off the ground here an epistemic reason for not relinquishing core beliefs is required. But if such a reason exists there is no need for dogmatism. Watkins, for example, echoes Popper when he says, "It is desirable that a theory should be defended with a certain dogmatism, so that it is not knocked out too quickly before its resources have been explored".[26] But if untapped resources were a reason for continuing to accept (or perhaps entertain) a theory where would the dogmatism be in continuing to accept (or entertain) that theory for that reason? It is *Popper*, of course, who would have us knock theories off too quickly, and if Watkins were to give up the belief that refutation is easy he would presumably recognise that dogmatism is unnecessary.

Let us turn now to Kuhn. Dogma is more basic or pervasive in science for Kuhn than Popper or even Lakatos and O'Hear allow. Scientists, Kuhn tells us, are *converted* to the paradigms that form the basis of their practice of normal science,[27] and so paradigms are dogmas or "quasi-dogmatic commitments" *before* (most) anomalies arise.[28]

It is sometimes a moot point in Kuhn's later writings, however, whether or not a community (or sub-community) of scientists is so converted or committed. For example, to underscore this supposedly unswerving commitment on their part, Kuhn says, "In the arts, in particular, the work of men who do not succeed in innovation is described as 'derivative', a term of derogation significantly absent from scientific discourse which does, on the other hand, repeatedly refer to 'fads'."[29] There would be no fads, however, if those responsible for them had in fact had an unswerving commitment to their paradigms. Nonetheless, Kuhn continues to believe that dogmatic commitment is necessary not just to tough out and *resolve* anomalies, as Popper and Lakatos believe, but to *produce* anomalies in the first place. In his "Reflections on My Critics", from which the above remark is drawn, Kuhn goes on to say that "because they can ordinarily take current theory for granted, exploiting rather than criticising it, the practitioners of mature sciences are freed to explore nature to an esoteric depth and detail otherwise unimaginable."[30] Anomalies are a *by-product* of this exploration, and most are resolved by further paradigm-based research. Kuhn adds that "because that exploration will ultimately isolate severe trouble spots", practitioners "can be confident that the pursuit of normal science will inform them when and where they can most usefully become Popperian critics", which is "the strategy appropriate to those occasions when something goes wrong with normal science."[31]

Kuhn's distinction between exploiting a theory and criticising a theory, however, cannot be sustained. The critic, or potential critic, of a theory is obliged to entertain the possibility that that theory is true if he or she is to derive any consequence from it for the purpose of testing or criticism. Moreover, the critic is not *obstructed* in this exercise because he or she does not also identify with, or have some quasi-dogmatic commitment to, that theory. The critic can do the same research, perform the same experiment, as the convert does. When the French Academy of Sciences dispatched expeditions to Lapland and Peru, for example, to measure a degree of latitude in each of those distant locations, their manifest desire to "explore nature to an esoteric depth" was fuelled, in part, by *opposition* to the Newtonian theory of universal gravitation.

Thus Kuhn overlooks the possibility, as Popper does, that in formulating a hypothesis such as the trans-Uranian planet hypothesis a practitioner *could* have been trying to refute, or at least to provide good reason to doubt, the law of gravitation. The critic of the law who formulated this hypothesis would have been trying to show, as I have pointed out, that no planet capable of removing Uranus's residuals existed. And what is Kuhn to make of any crucial, or potentially crucial, experiment? Dogma aside, no one can be rationally committed to two *rival* theories - such theories are inconsistent. Moreover, any Kuhnian who was committed to either of two such theories would, in performing any such experiment, still collect evidence (or counter-evidence) for the other.

On Kuhn's account, Leverrier and Adams were trying to solve a *puzzle*, namely, 'What is the cause of Uranus's residual perturbations?' This is certainly closer to the truth than Popper's view, but it is still false. As a heuristic device, it may be useful to treat such research *as if* it were a puzzle-solving exercise, but that is all. Neither Leverrier nor Adams thought that the problem of Uranus's motion was a mere puzzle for both recognized that it might necessitate at least a change in the law of gravitation. Uranus might have had no residual perturbations. ('What is the cause of Uranus's *residuals*?' does not describe a Kuhnian puzzle, since the answer to *this* question might be, 'Newtonian mechanics is false.') A Kuhnian puzzle is, I take it, a problem for which there is a 'correct' paradigm based solution, as a crossword puzzle is a problem for which there is a correct clues-and-diagram based solution. If, say, '4 across' is a seven letter word meaning 'deduct', then 'magical' (wrong meaning) or 'take-away' (too long) will not do; what fits the framework of the crossword puzzle is (say) 'subtract'.

Kuhn's attempted distinction between exploiting and criticising a theory follows from his distinction between puzzle-solving and testing, practices which he thinks are confined to "normal science" and "extraordinary science", respectively.[32] Leaving aside what purpose testing the allegedly incommensurable theories of extraordinary science would serve, testing takes place quite as much in the practice *he* calls normal science as in that which *he* calls extraordinary science.

Kuhn would count Bouvard's preparation of tables for Uranus, for example, as one of the tasks of normal science, a task he would classify as the "matching of facts with theory" or perhaps "the determination of significant fact".[33] But this exercise was a test of Newton's theory, whatever Bouvard's attitude to that theory may have been. Indeed, the exercise of accounting for the motion of Uranus was a significant test of the theory, if then a relatively weak one, for Uranus is a good deal farther from the Sun than the farthest of the other planets then known (see Table 3, p. 123 above).

Nevil Maskelyne's attempt in 1774 to estimate the mean density of the Earth using the law of gravitation is a classic case of what is, supposedly, puzzle-solving normal science. But it too was a significant test of the law. Maskelyne measured the faint gravitational pull on a plumb line exerted by a suitable Scottish mountain, Schehallien.[34] By comparing the pull of this mountain (5".8) with that of the Earth, and estimating the mass of Schehallien, Maskelyne obtained a value for the mass of the Earth. This was a novel application of the law for the mass of a mountain is small compared with that of a planet or a satellite; so too was the distance between the centres of mass of Maskelyne's plumb line and Schehallien compared with, say, the radius of the Earth or the distance of the Moon from the Earth. It is clear from Maskelyne's conclusions that *he* knew this experiment was a test of the law. He said:

1. It appears from this experiment, that the mountain Schehallien exerts a sensible attraction; therefore, from the rules of philosophizing, we are to conclude, that every mountain, and indeed every particle of the earth, is endued with the same property, in proportion to its quantity of matter.

2. The law of the variation of this force, in the inverse ratio of the squares of the distances, as laid down by Sir Isaac Newton, is also confirmed by this experiment. For, if the force of attraction of the hill has been only to that of the earth, as the matter in the hill to that of the earth, and had not been greatly increased by the near approach to its centre, the attraction must have been wholly insensible. But now, by only supposing the mean density of the earth to be double that of the hill, which seems very probable from other considerations, the attraction of the hill will be reconciled to the general law of the variation of attraction in the inverse duplicate ratio of the distances, as deduced by Sir Isaac Newton from the comparison of the motion of the heavenly bodies with the force of gravity at the surface of the earth; and the analogy of nature will be preserved.

3. We may now, therefore, be allowed to admit this law; and to acknowledge, that the mean density of the earth is at least double of that at the surface, and consequently, that the density of the internal parts of the earth is much greater than near the surface. Hence also, the whole quantity of matter in the earth will be at least as great again as if it had been all composed of matter of the same density with that at the surface; or will be about 4 or 5 times as great as if it were all composed of water.[35]

In 1798, Henry Cavendish measured the very much fainter gravitational pull of a large lead sphere on a small lead ball, in the comfort of his laboratory. By comparing the pull of the sphere with that of the Earth, Cavendish estimated the mean density of the Earth at 5.488 times that of the density of water, which is close to modern estimates.[36] Kuhn cites this experiment as a classic of normal science.[37] Once again, however, it is a significant test of the law of gravitation. Apart from the greater accuracy of Cavendish's measurements, the mass of his lead sphere was obviously small compared with the mass of Schehallien; so too was the distance between the centres of mass of the sphere and the lead ball compared with the distance between the centre of Schehallien's mass and Maskelyne's plumb-line. Cavendish's experiment was thus a novel application of the law.

Now if the gravitational force between such masses as Cavendish employed were, say, ten times greater than it is - a phenomenon that would still have gone hitherto undetected -

Cavendish's experiment would have produced strong counter-evidence for the law of gravitation. This is because his estimate for the mean density of the Earth would then have been about *half* that of water. A Newtonian would have been quite correct, of course, to scrutinise Cavendish's equipment or his experimental method, or to scour his laboratory for hidden forces, had this result been obtained. But the important point here is that if one were a non-believer or a critic of the notion of universal gravitation, or of the inverse square law in particular, then the experiments of Maskelyne or Cavendish were a good means of trying to obtain evidence to support such a position.

Thus, so-called normal scientific practice is or can be *independent* of the mental states Kuhn and others who follow him, like Putnam, think are necessary or desirable for its practitioners to have. Kuhn and Putnam are wrong: Maskelyne or Cavendish *could* have been trying to falsify or to find counter-evidence for 'the Newtonian paradigm' in performing the above experiments.[38] On the other hand, Popper is also wrong in thinking it necessary or desirable for them to have been trying or intending to do so, for they were not doing so, yet science did not suffer.

Let us return to Popper and conclude with some remarks on his use of 'dogmatism'. As with his use of the term '*ad hoc*', Popper deploys 'dogmatism' for the rhetorical advantage it would secure for the practice of conjecture and refutation over the practice, so called, of avoiding a refutation. He says, for example:

> Clearly, one can say that if you avoid falsification *at any price*, you give up empirical science in my sense. But I found that, in addition, supersensitivity with respect to refuting criticism was just as dangerous: there is a legitimate place for dogmatism, though a very limited place. He who gives up his theory too easily in the face of apparent refutations will never discover the possibilities inherent in his theory. *There is room in science for debate*: for attack and therefore also for defence.

Popper continues:

> I did not propose the simple rule: "Look out for refutations, and never dogmatically defend your theory." Still, it was much better advice than dogmatic defence at any price. The truth is that we must be constantly critical; self-critical with respect to our own theories, and self-critical with respect to our own criticism.[39]

Are we seriously expected to believe that if we are self-critical of our criticism of some theory then we are dogmatically defending that theory? There is no substance to this use of 'dogmatism'; it is purely rhetorical. Influenced by Popper and Lakatos here, however, Chris Mortensen and Tim Burgess, for example, believe that a "healthily dogmatic defence of a thesis is a cautiously sceptical attitude to its criticism".[40] No one is a dogmatist merely for defending a theory from counter-argument, an act that can equally be described as an attack (on that counter-argument) *and which may itself be a refutation*. Whether or not one is a dogmatist does not depend on *what* one chooses to attack - 'X is a black swan' rather than 'All swans are white' - but on *how* one goes about the task. Thus, any alleged refutation of a theory may be the work of a dogmatic opponent of that theory, someone who merely siezes upon an anomaly with no thought for the strength or otherwise of the counter-argument it provides. Such a person can also be reasonably described as an uncritical thinker.

In *Unended Quest*, Popper takes his rhetorical use of 'dogmatism' and its cognates one step further, contrasting what he calls "dogmatic thinking" with what he calls "critical thinking". He says:

> Most (or perhaps all) learning processes consist in theory formation; that is, in the formation of expectations. The formation of a theory or conjecture has always a "dogmatic", and often a "critical", phase . . . . The critical phase consists in giving up the dogmatic theory under the pressure of disappointed expectations or refutations, and in trying out other dogmas . . . .
>
> I looked on this method of theory formation as a method of learning by trial and error.[41]

This passage raises more questions than it would settle. For example, how can someone like Popper, who continually urges us to treat all theories as tentative or provisional, describe all theories as dogmas? If someone's dogma-fuelled expectations are disappointed would that person have other *dogmas* to try out, as Popper claims? Someone can be a dogmatic Newtonian, for example, or a dogmatic Einsteinian, but is anyone *both*? Is anyone a dogmatic atheist *and* a dogmatic Christian? Would a belief even be a dogma if one were moved by a disappointed expectation to give it up? And in what conceivable sense is a dogma something one puts on trial?

Moreover, *Popper* cannot sustain the distinction he refers to above between dogmatic thinking and critical thinking. Since theories or hypotheses or conjectures figure in the antecedents of all the arguments he calls refutations, so-called *critical* thinking is merely arraying one dogma against another if all theories or conjectures are dogmas. If Popper can succeed in having us identify dogmatic thinking with forming or holding or defending theories, however, whilst critical thinking is identified with attacking or surrendering them, then the so-called practice of avoiding a refutation will appear less than rational by comparison with refutation.

4.3 An Excess of Apparent Refutations, the Alleged Need for Wise Men, and More on the Rejection of a Trivially True Account of Rationality in Science

Of Popper's commentators who presuppose WPF, some reject or at least do not always turn to dogmatism or faith in an effort to explain or justify the retention of core beliefs in the face of anomalies. Two such commentators - one sympathetic, the other not - are Alan Musgrave and Harold Brown, respectively. I shall begin with Musgrave.

Musgrave is in two minds on this problem. Sometimes he simply wears the consequences of WPF. Thus, in concluding an analysis of Lakatos's account and its relation to the history of Newtonian mechanics, he says, "We surely do find this: some hypotheses have persisted over long periods despite many apparent refutations of them," a conclusion which others have been driven to as well.[42] Almost in the next breath, however, forced to explain "the 'continuity' of Newton's theory", Musgrave says, "It survived simply because it was a very good theory, it was not made unfalsifiable by fiat, but was difficult to falsify in fact."[43] This is a good point, but it is striking that Musgrave does not notice the problem of how it can supposedly have been *easy* to produce *prima facie* or apparent refutations of a theory that *was* difficult to refute? It is breathtaking that so many people should ever feel the need to *explain* the survival of a "very good theory" in what is supposed to be a paradigm discipline. As to Musgrave's first claim above, we surely do *not* find that hypotheses are apparently refuted as often as he would have us believe is the case. The world would be a queer place if we did, as the following analogy illustrates.

In the genre of the detective story, one of the stock devices writers employ is to construct the narrative in such a way that it first appears as though the crime was committed by some character other than the criminal. But what if life were to imitate art in this respect as often as Musgrave believes that theories which appear to be responsible for false predictions turn out to be innocent of them? In this bizarre world detectives would quickly develop a healthy skepticism concerning any fact they would otherwise have counted as good evidence of a criminal act except that it happened to come to light early in the investigation. Thus, experienced detectives would be disinclined to take someone into custody, for example, whom they surprised at the scene of the crime shortly after it was committed, in possession of the necessary implements and with a *prima facie* good reason for committing it. Some other suspect, however, who had, say, a reasonable alibi and little apparent motive for committing the crime would not receive such favourable treatment.

Turning to Brown, in *Perception, Theory and Commitment*, he claims that an outstanding feature of what he calls the "new philosophy of science" is the "rejection of formal logic as the primary tool for the analysis of science" in favour of the "detailed study of the history of science".[44] According to Brown, rationality in science is no longer characterised by algorithmic decision making, which in principle machines can do, but by decision making of the sort for which there is no algorithm or rule but where decisions require the informed and deliberative judgement of the skilled practitioners themselves. "I offer the making of these decisions", he says, "as a model of rational thought", and those who make such decisions are "men of practical wisdom" in Aristotle's sense.[45] For Brown, Leverrier and Adams are paradigm cases of such decision makers, two of his favourites. Brown's view of these two scientists stands in marked contrast to that of Popper and others we have examined for whom they were Newtonian dogmatists (though none of these philosophers is ever moved to *describe* them as such).

To develop his model of rational thought in science, Brown appeals to cases where informed and deliberative judgements in the application of social rules are called for. He says:

> Aristotle gives the following description of equity: "A correction of the law where it is defective owing to its universality." The problem Aristotle is concerned with is that we sometimes encounter a situation that falls under existing laws, so that justice requires that we act in accordance with the law, but in which it seems unfair to apply the letter of the law. This may occur because in the formulation of universal laws it is impossible to foresee and make provision for every circumstance. The man of practical wisdom must be able to recognize this and correct the universal law in accordance with the demands of a particular situation.
>
> An analogous point holds in the scientific case. Suppose, for example, we adopt a methodology which requires us to reject any theory inconsistent with a well confirmed falsifying hypothesis, and regard the postulation of Uranus [a trans-Uranian planet] . . . in this light. We can now view Leverrier and Adams . . . as scientists who applied the general rule to the particular case and judged that although the rule applied, the case in question required special consideration and thus the rule was suspended.
>
> It is the ability to decide how an exceptional case should be handled that is characteristic of rationality.[46]

The first thing to be said about Brown's argument is: where is the concern for history that is supposed to characterise the new philosophy of science? Brown merely assumes that the counter-argument from Uranus's residuals was a well confirmed falsifying hypothesis. And he makes no attempt to support his notion that Leverrier and Adams were mavericks who refused to apply a certain rule because they saw the case in question as an exception to that rule. Did they even know of this rule, much less refrain from applying it? What was the "special consideration" they supposedly realised this case called for? Brown does not say.

There is no evidence that either scientist regarded this case as exceptional in the way Brown suggests, and each of them recognized that his approach was in line with how all previous such anomalies had been successfully removed. Brown comes to believe this case is exceptional because Popper induces him to believe that the argument from Uranus's residuals was "well confirmed", and therefore unlikely to fail.

Brown also thinks, however, that the so-called new philosophy of science starts (roughly) with Kuhn. So it is not surprising that he elsewhere implies that this case was *not* exceptional, by selecting it to illustrate Kuhn's normal science - a notion he essentially approves. When Brown is following Kuhn he thinks of the increase in gravitational acceleration away from the equator or of the irregularities in the orbit of Uranus as "observational discoveries which, logically speaking, could have been taken as counter-instances [but which] became, instead, research problems to be solved by the proper application or further development of the theory".[47] That is to say, they are cases to be handled *in the normal way*, not in any exceptional way.

Furthermore, a major influence on Brown's thinking that logic should take second place to history in the analysis of science is clearly faulty logic, something else he shares with Kuhn. An observation statement, O , is (or perhaps describes) a counter-instance of a theory, T , if, and only if, O is true and O and T are contraries. If 'Artichoke is a vegetarian dog' is true then it is a counter-instance of 'All dogs are carnivorous'. But the observation that freely falling bodies accelerate at different rates across the surface of the Earth, for example, is *not* a counter-instance of the law of gravitation. These two propositions are not contraries for *both* can be true, a point that is emphasised by the fact that Newton showed that an *explanans* which retained the law but which included a different assumption about the shape of the Earth could explain the observed variation in gravitational acceleration. And no satisfactory *explanans* can be the contrary of its *explanandum*. The fallacy that if a true observation statement is inconsistent with a prediction then it is a counter-instance of the theory from which that prediction was derived is, as we have seen, the fallacy which underpins SPF and WPF.

But what of Brown's analysis of the "practical wisdom" which, let us allow, Leverrier and Adams did display? Does his analogy with inequities in the application of social rules cut any ice? Consider the rule that a competitor in a breast stroke race finishes such a race just when that competitor, having swum the required distance, touches the end of the pool with both hands simultaneously. Let us call this rule the finishing rule. It is an unintended consequence of the finishing rule that no one handed swimmer can finish such a race. In one Australian competition recently this fact was brought to the attention of race officials by the coach of a beaten two handed swimmer. The rule was invoked, and a one handed swimmer was stripped of the third place he had just been awarded. Taking this to be the sort of case Brown has in mind, what should be done about it or about cases like it? If we merely suspend the finishing rule, as Brown believes Leverrier did in allegedly analogous circumstances, how is any swimmer, one handed or two handed, to finish such a race? Since it would not be fair to let *anyone* begin a race which cannot officially be completed, Brown's suggestion is not one that would lead to an equitable result. We should *ammend* the finishing rule, or invent a new one, not merely suspend it.

On the other hand, I do not deny that an informed and deliberative judgement may need to be made by race officials in this case to remove the inequity. But the informed and deliberative judgement Brown has in mind is one that would prevent inequities *of this sort* from occurring, for he is concerned with how laws or rules should be, as he puts it, corrected. We would still have to decide whether or not it was equitable to apply any amended rule to the case which led us to make that amendment. And it would be a mistake to think that in amending one rule we may not be following another. In amending the two hands rule, for example, we may well be following, albeit amongst doing other things, the general rule that equality of opportunity should not be denied people with disabilities.

So even if Brown's analogy with inequitable social rules were to hold good it would show little more than that some old philosophers of science needed to change the rules they thought scientists ought to follow, not that they needed to change their belief in the importance of rule following to rationality in science.

But there is, in any case, an important *dis*analogy between a case such as the two hands rule and the Popperian rule, 'Reject any theory for which there is a well confirmed falsifying hypothesis'. In the former case, we need to encounter or imagine a situation such as that of the one handed swimmer to see what is wrong with the rule in question. But you don't need to know an aphelion from a perihelion to advise any astronomer not to follow the latter rule. There is no consideration that is special to any case such as that of Uranus's residuals which supplies the reason for rejecting this rule, a fact which goes a long way to explaining Brown's silence on just what *was* the special consideration he believes Leverrier and Adams exercised. This Popperian rule is open to the logical objection that it is not rational to reject a $\textit{falsified}_p$ theory as a truth candidate, for such a theory may be true.

In a review of Brown's analysis, Harvey Siegel remarks that it is "fine as far as it goes". He says that "Brown is on firm ground in holding that it was rational for scientists like Leverrier . . . to ignore methodological rules in certain instances". Where Brown falls short, Siegel thinks, is in failing to see that "we need to know when it is *justifiable* to hold on to hypotheses in the face of contradictory evidence, and when not".[48] If by 'hold on to' is meant 'believe' or 'accept', then the short answer to Siegel is that it is *never* justifiable to hold on to such hypotheses, if one accepts that evidence. However, Siegel does then say that "Brown's account offers no criteria by which we can assess the justifiability of [such] decisions", which suggests that he does recognize that Brown has failed to explain how Leverrier's decision was rational.[49] Brown does not see this failure for when Siegel points out that "informed human judgement may well be a necessary condition of rationality, [but] it is surely not a sufficient condition",[50] Brown replies, "What more does Siegel want?"[51]

Since Leverrier's judgement was correct - Uranus *was* sensibly perturbed by an exterior planet - Brown is not prodded to consider whether or not it was also rational. However, suppose that Leverrier had shown that *all* Uranus's residuals were merely errors in Bouvard's tables, yet still insisted on rejecting the assumption that only Jupiter and Saturn sensibly perturb the planet. What would Brown say in this case? He could say, "Well, that's an informed judgement too, so that's rational", from which it follows that anything goes. Or he could say, "Leverrier was misinformed, so his judgement was not rational". But what would Leverrier have been misinformed about? Not about the relevant astronomical facts. And if Brown were to suggest that Leverrier did not understand that a successful prediction is not a reason for *rejecting* any of the propositions on which that prediction is based, he would be implying that there is a condition or criterion of rationality against which Leverrier had been measured, and (correctly) found wanting. On the other hand, if an informed judgement that p is for Brown only one in which the person making that judgement has good reason to believe or entertain p , then his proposal is empty. It is merely a stipulation that 'informed' means, in part, 'rational'.

I have suggested that scientific practice is rational only if the approaches its practitioners adopt to the removal of anomalies are guided by the epistemic and pragmatic reasons available for such courses of action. Unless one doubts that there are reasons for courses of action this is an innocuous suggestion, and one that would be almost trivial were it not for the fact that many people have come to believe otherwise. Many fail to see this point, even if they reject the negative heuristic or do not follow Popper in holding every course of action other than attempting to replace the major theory concerned, assuming there is one, to be a form of avoidance behaviour. I consider a sample of such cases below, to add to that of Brown and others above, beginning again with Musgrave.

At first, Musgrave considers the approach I have suggested. But he passes over it in favour of a suggestion of the later Kuhn that a "diversity of response" is desirable.[52] Musgrave favours diversity so that "no promising line of research gets neglected", but he also agrees with Kuhn that it is "the community's way of distributing risk".[53] Now I do not deny that one can often find such diversity or that it is often rationally desirable, just as one can often find that practitioners replace propositions in the so-called protective belt and that this is often rationally desirable. But Musgrave misses the point here much as Lakatos did.

I have several points about Musgrave's blanket recommendation of diversity. Firstly, if there is no diversity of promising lines of research in a particular case why recommend a diversity of response in that case? The anomaly of Uranus's orbit, which Musgrave often discusses, might have alerted him to this point. Of the possible lines of inquiry that were suggested in this case only the hypothesis of a trans-Uranian planet was ever promising. (It might be thought that the possibility that the anomaly was an artifact was also promising, since Uranus's residuals were in part errors in Bouvard's analysis, as Leverrier demonstrated in his first paper on the subject. Grosser remarks, however, that "one of the least expected consequences" of this paper was "the complete discrediting of Alexis Bouvard".)[54]

Secondly, if Musgrave does believe that Newton's theory was apparently refuted as often as he implies, he should also believe that there was at most only ever one promising line of inquiry in any such case. Checking or repeating calculations or tests, investigating presuppositions, or trying out novel auxiliary propositions would all have been *un*promising activities had Newton's theory ever in fact appeared to be responsible for a predictive failure. Modifications to this theory were sometimes tried, it is true, but as Hanson remarked - with obvious exaggeration to mark the point - to have done away with Newtonian mechanics in nineteenth century astronomy would have been "to refuse to think about the planets at all".[55]

Thirdly, no promising line of inquiry need be neglected if there is no diversity of response, for lines of inquiry can be worked *seriatim*. Suppose there are two promising lines but only three scientists available to work them, and that it requires at least two scientists per line to produce reliable results. Diversity of response would not be a rational policy in this case. It would be rational to work these lines, if need be, one after the other. Activities like managing an investment fund or gambling on a horse race are activities in which success depends upon *foregoing* some, usually many, possible lucrative courses of action, and hence there *is* a risk of failure to be distributed in such cases. But activities like repairing the tables of Uranus, or trying to find which key, if any, of a bundle of keys is the one to the garden shed, need not involve foregoing any such courses of action. Costs may attach to choosing unsuccessful courses of action in such activities, of course, but they need not, and often do not, jeopardise the primary goal of that activity. We can find the right key to the garden shed even if it is the last one we try.

Finally, if we take into account only how *promising* is a line of inquiry in deciding whether or not to follow it, the pragmatic reasons for (or against) any such course of action are not counted.

On a later occasion, Musgrave makes a similar recommendation of diversity, this time in relation to *research programmes*. Again he offers the justifications of wanting no promising line of inquiry to be neglected and to distribute the risk of failure.[56] Once again, however, it needs to be pointed out that there may be no good reason to have rival research programmes. The history of celestial mechanics in the eighteenth and nineteenth centuries, for example, might have alerted Musgrave to the point that a research programme with a monopoly need not be a bad thing in science.

While Musgrave can at least see *some* merit in the suggestion that it can be more rational to first tackle an anomaly one way rather than another, John Worrall, for example, in a criticism of Noretta Koertge, gives the impression of being thoroughly skeptical of it. Worrall's criticism of Koertge, however, rests on conflating attempting to solve a problem with solving a problem - a conflation he does not notice in Koertge, who would otherwise be on the right track. Let us begin with Koertge to see how this confusion arises.

In "Towards a New Theory of Scientific Inquiry", Koertge claims to have solved the Duhem (or Duhem-Quine) problem, which she simplifies as, "In the case of a prediction failure, when is the theory itself (as opposed to auxiliary hypotheses) refuted?"[57] To illustrate her solution, Koertge considers two kinds of anomalies which confronted Mendeleev in the construction of the Periodic Table. One of these was the "problem of reversed pairs".[58] An instance of this problem, Koertge says, is that iodine should have been located after tellurium in the table "because of its valence and other chemical and physical properties". However, "this order did not correspond to that given by the best available atomic weight data".[59] As Koertge describes Mendeleev's problem, then, he could reject the Periodic Law of chemical elements, T, (option #1). Or he could reject the assumption that in the experimental determination of the atomic weight of iodine, the gaseous iodine was impure, A, (option #2). Of these two options, Koertge says:

> Option #1 is undesirable because it would necessitate the very laborious task of trying to invent a replacement for *T*. It would be a shame to go through that process if there's any decent chance that *T* might in fact be true and in fact *T* has had a large number of empirical successes.
>
> Option #2 is more desirable because it is generally a fairly easy and routine matter to check on the purity of materials. Furthermore, although we have as yet no direct evidence that the iodine is contaminated, this conjecture does have a certain prior plausibility, given our past experience concerning the difficulty of purifying chemicals, especially gases. On balance, option 2 [sic] is definitely indicated.[60]

But indicated for what? For belief or acceptance *instead* of option #1? Or for investigation *ahead* of option #1? Clearly, an epistemic reason such as that there is a "certain prior plausibility" though "as yet no direct evidence" that the iodine gas was impure would be a poor reason for *believing* (or accepting) option #2, and the pragmatic reason that it was "a fairly easy and routine matter to check on the purity of materials" would be a bizarre reason for *believing* that some material was impure. The fact that it is easy to check whether or not there is any sugar in my tea, for example, is not a reason for believing that there is. Equally clearly, however, both the epistemic and pragmatic reasons Koertge evinces for option #2 would be good reasons for *investigating* that option first. But that is not the *Duhem* problem. The Duhem problem is which option should we believe or accept.

Koertge concludes:

> It is probably fairly obvious by now that the underlying structure of my rather informal analysis of Mendeleev's two problem situations is a decision-theoretic one. What we have done is to lay out the possible options and then try to estimate the expected scientific utility of each. Thus for each option, we have asked two questions:
>
> > (i) How scientifically *desirable* would its outcome be if it were successful?

(ii) How *likely* is this option to be successful?

Put more precisely, the two basic appraisals we have tried to make are these:

(i) How interesting or informative or explanatory would X be if it were true?

and

(ii) What is the probability that X is true?

I believe that most theories of scientific inquiry proposed by philosophers of science so far have either conflated these two appraisals or ignored one of them.[61]

Clearly, however, if one's concern *were* the Duhem problem, the question to be faced would be, 'Which option is the successful one?' or 'Is X true?' So the fact that, for example, it would indeed be more interesting or informative to learn that there was a planet beyond Uranus rather than merely that some telescope was incorrectly mounted has no bearing on whether or not one should believe or accept the former possibility rather than the latter, though it may induce some to investigate the former ahead of the latter.

Even once we recognize what problem Koertge would solve, however, there are at least a couple of things wrong with her questionnaire. Firstly, the question,

What is the probability that X is true?

is not, as she asserts, a more precise formulation of

How likely is this option ['X is true'] to be successful?

The probability of cognitive success in respect of 'X is true' is a function of *more* than the probability of 'X is true'. For example, suppose that in explaining some event, E, historian A presupposes that two commodities, X and Y, were in long supply at the time. Historian B points out that if either X or Y, were in short supply then some other explanation of E is called for. Now it may be equiprobable, for all anyone knows, that X and Y *were* both in long supply at that time but there may be a much more reliable means of determining whether or not X was than Y, for example, by checking shipping office records rather than by garnering chance remarks from letters or diaries. Secondly, Koertge's questionnaire does not elicit any of the pragmatic reasons for courses of action, though reasons of this sort do figure prominently in her case study, as we have seen.

Turning now to Worrall, he begins his criticism of Koertge, under the sub-heading "The Duhem Problem", thus:

> Noretta Koertge hopes to find a methodology which will demarcate those situations in which the better or more promising or more rational solution of the inconsistency [between prediction and observation] is for scientists to look for replacements for T, from those situations in which the better solution is for scientists to look for replacements for A. I wish her luck, but I do not think she will succeed.[62]

Worrall conflates 'the better solution' here with 'the better way of looking for the better solution', which in all probability explains his skepticism, for the most promising line of inquiry need not be the one that will succeed in removing the anomaly. But this fact is no objection to anyone who supposes that some lines of inquiry *are* more promising than others or that, other things being equal, it is better or more rational to pursue those lines first.

4.4 *Ad Hoc* Hypotheses, Circularity, and Avoidance: A Further Defence of the Rationality of the Trans-Uranian Planet Conjecture

The climate of suspicion or hostility which in certain quarters surrounds the introduction of auxiliary hypotheses, such as those that we examined in 2.3, also extends to hypotheses such as the trans-Uranian planet hypothesis. Popper is responsible for much of this unhealthy climate. As we have seen, although he asserts that the trans-Uranian planet hypothesis was *not ad* hoc_p, his account of it is characterised by a strong suggestion of guilt by association. The hypothesis is branded like so-called *ad hoc* hypotheses as a means of avoiding a refutation and is discussed side-by-side with such hypotheses. It is variously spoken of as one of those "*ad hoc* auxiliary hypotheses", as having been introduced *ad hoc*, as having had "only . . . one *ad hoc* reason" going for it, and as having been (if not *un*testable) at least "difficult to test", and so on.[63] If one were to believe Popper, the only saving grace of this hypothesis at the time it was introduced would seem to have been that it was independently testable - a logical property it shared with each of the other putative explanations for Uranus's residuals.

It is thus perhaps not altogether surprising to find that several of Popper's commentators assert or imply, if sometimes unintentionally, that there *is* something fishy about this hypothesis, or about the reasoning of Adams or Leverrier in formulating one or other version of it. It will be clear from what I have said earlier on these matters that I reject these claims. But let us inspect the arguments of three such commentators. Firstly, I consider Newton-Smith, who approves of this hypothesis, but does not rule out the possibility that it is *ad* hoc_p. His concern is, apparently, with whether or not the hypothesis was open only to a circular defence and, if so, does this matter. Secondly, I consider Adolf Grünbaum and Jarrett Leplin, both of whom disapprove of this hypothesis, though to varying degrees, but who disagree about whether or not it was in some sense *ad hoc*.

Newton-Smith says:

> One of Popper's strategies . . . involves forbidding us to make *ad hoc* moves in the face of an anomaly. If we can only preserve our theory by making an *ad hoc* move, out it goes . . . . When is a move *ad hoc*? It is clear from his examples that the intuitive content of saying that a move is *ad hoc* is that it involves a justification which runs in a circle. To explain the storm at sea by appeal to Neptune's anger is *ad hoc* if the justification for the claim that Neptune is angry is that there is a storm at sea. Take my simple theory that all swans are white. Suppose the scientific elite is inclined to assent to the basic sentence that there is a black swan on the Cherwell. If I defend my theory by claiming that some things that look like swans (identical up to colour) are not in fact swans my move is *ad hoc* if my only justification is the theory that all swans are white. While this seems reasonable in the abstract, attention to actual scientific practice shows that it is not. For instance, consider the apparent anomaly for Newtonian mechanics due to the observed motion of Uranus. The scientific community did not give up Newtonian mechanics. Instead they posited the existence of Neptune. The only justification for making this move at the time was the fact that the theory was pretty good.[64]

I have argued in relation to Popper's case of the storm at sea that someone who asserts that p is an explanation, or a probable explanation, of q is not embroiled in any "justification which runs in a circle" if that person has no justification for p other than q. If I return home to find our front door wide open and our video recorder missing, I would conclude that we had been burgled. There is no circularity in my asserting or believing this explanation even if I have as yet no independent evidence that we have been burgled. My reason for believing that our front door is wide open and that our video recorder is missing is not that we have been burgled.

There may be, however, a circular argument in the second case Newton-Smith considers - that of the black swan on the Cherwell - which he models, if unsuccessfully, on Popper's case of the storm at sea. I shall set this case out as a dialogue, which is the form Newton-Smith presumably intends:

(1) Newton-Smith:	All swans are white.
(2) Scientific elite:	There is a black swan on the Cherwell.
(3) Newton-Smith:	Some things that look like swans (identical up to colour) are not in fact swans [and this is one of them].
(4) Scientific elite:	What justification can you give for this?

(5) Newton-Smith: All swans are white [and those who claim to have seen a black swan are good observers, so what they saw must have been a simulacrum].

If this dialogue is what Newton-Smith intends, then he commits the fallacy of begging the question or *petitio principii* at (5) for 'All swans are white' is still under challenge. The *basis* of his defence of 'All swans are white' is, in part, that all swans are white. So his argument is circular. But is it a "justification which runs in a circle"? If by this expression Newton-Smith has in mind an argument of the sort in which, put simply, p is used to justify q when q has already been used to justify p then this is not such an argument. Newton-Smith uses 'All swans are white' as a (partial) justification of the simulacrum hypothesis which in turn justifies his rejection of the alleged sighting of a black swan. But there is no suggestion that he uses this rejection to try to justify the theory that all swans are white. (He *could* do so, however, for 'There is no black swan on the Cherwell' is *entailed* by this theory.)

Newton-Smith's talk of an *ad hoc* move above is inspired, as with Musgrave for example (p. 71 above), by the fact that the simulacrum hypothesis is designed to protect the belief that all swans are white, and that its designer resists the call at (4) for some independent reason to believe this hypothesis. The simulacrum hypothesis is not of course *ad hoc* in Popper's sense, that is, *ad hoc*$_p$, though this point eludes Newton-Smith.

Although Newton-Smith seems set to reject the pattern of reasoning he does or would illustrate in this case (even if he is unclear or does not know why he should do so), he then convinces himself that this pattern is the same as that which the scientific community displayed in dealing with Uranus's residuals, and thus that it is perfectly acceptable. A fallacy, it would seem, is to receive the *imprimatur* of science. This is a familiar ploy. The irrational is held to be rational because of how *science* is thought to work. We have previously been invited to believe that certain theories require minders, that dogmatism is good, and that men of practical wisdom are licensed to ignore contradictions. Are we now being invited to add circular reasoning to this list? That is certainly the *impression* one gets from this passage.

I have constructed the dialogue above to bring out the fallacy of begging the question because it seems clear that it is this fallacy Newton-Smith is alluding to with his imprecise remark about a justification running in a circle. There is some room for doubt here, however, as to whether or not he does commit this fallacy.

Firstly, *Newton-Smith* seems to describe a dialogue which goes only as far as (3). He does assert that the only justification he has for the simulacrum hypothesis is that all swans are white, but he does not say either that he has *offered* this justification to the scientific elite, or even that they have *requested* any justification for this hypothesis. On the first point, if Newton-Smith understands begging the question he would refrain from the would-be justification he is held to offer at (5). A rational response to:

(4) Scientific elite:	What justification do you have for this [the simulacrum hypothes]?

would be

(5)' Newton-Smith :	Well, none that will satisfy you (or me), so I'm off to the Cherwell to try to get some.

As to the second point, the scientific elite may not even *bother* with the justification request I have them making at (4). If they have a low tolerance of eccentric philosophers they may simply stop talking to Newton-Smith once they have heard his response to their claim that a black swan had been sighted.

Secondly, what Newton-Smith has in mind to justify is, in any case, not a *belief* but a *move*. He is confused about this point because he conflates propositions, which one may wish to justify *believing*, with moves, which one can only justify *making*. A striking instance of this conflation is the following remark by him, made shortly after the above passage: "The criterion Popper employs to distinguish between good and bad auxiliary hypotheses is that of independent testability. A move is not *ad hoc* if it is independently testable."[65] Moves are such things, however, as 'accepting *p* ' and 'justifying *q* '. They are *acts*; it does not make sense to ask whether or not 'accepting *p* ' is independently testable. Since the sort of moves Newton-Smith principally has in mind, so I will argue, are those of entertaining propositions, his conflation of justifying believing a proposition with justifying making a move cashes out as the familiar conflation of justifying beliefs with justifying suppositions.

Now there is no rational objection to a move of the sort which results in (3) above, and which Newton-Smith singles out as *ad hoc*. This is a corollary of the point I reiterated above in the case of the storm at sea. There is no fallacy that Newton-Smith commits in formulating or entertaining the simulacrum hypothesis though his only justification for doing so is that 'All swans are white' has been (let us allow) hitherto well supported - *and* that certain members of the scientific elite are good observers. (He denies that any reason other than the former is involved; but if he did not think that those observers were good he would not suggest they had seen a simulacrum, but perhaps a duck, or that the light had played tricks on them.) If we deny all moves like (3) we stifle inquiry. What if the alleged swan *is* a simulacrum? What if our house *has* been burgled? By all means point out to Newton-Smith that he is probably clutching at straws, and that if he wants anyone to *believe* his hypothesis he will need to do better than merely the reasons he so far has for *entertaining* it. Moreover, as with the case of Popper's biologist that we examined in 2.2 above (p. 57), we need to be assured that it is not a tautology that Newton-Smith is asserting when he utters the sentence 'All swans are white' in the dialogue above.

When Newton-Smith finally arrives at the case of Uranus's residuals, it is clear that what he thinks needs to be justified is, as he puts it, the *positing* of a suitable exterior planet, not the *belief* that such a planet exists. Moreover, he is quite correct in thinking that the fact that Newton's theory was "pretty good" is a reason for entertaining that conjecture, for making that move. But he is quite wrong in supposing that, as we have seen, it was the *only* reason for doing so. Unlike the case of the 'theory' that all swans are white, the counter-argument for Newton's theory in the case of Uranus's residuals was not strong, and there were good epistemic and pragmatic reasons for preferring the exterior planet hypothesis to the other possibilities suggested.

By overlooking such reasons and comparing this case with others that are unscientific or merely bizarre curiousities, Newton-Smith does little for our understanding of the rationality of entertaining novel auxiliary hypotheses in science. Similarly, there is nothing remotely resembling a circular justification, or any other kind of circular reasoning, in the rational conclusion of the scientific community that the exterior planet hypothesis was the one to explore. And if Adams or Leverrier would justify some measure of *belief* in this hypothesis, he can do so by pointing to the fact that it would remove the anomaly of Uranus's residuals from a hitherto well supported theory and that this hypothesis is consistent with existing knowledge of the trans-Uranian region. He need not then justify his belief in that theory by pointing to the repairs he has just made with this hypothesis to the prediction of the motion of Uranus; it is highly implausible to imagine or suggest that either would have done so.

Turning now to Grünbaum and Leplin, I shall first examine their opinions about the logical status of the trans-Uranian planet hypothesis - was it in some sense *ad hoc*? - before considering their objections, real or imagined, to this hypothesis.

Grünbaum distinguishes three senses of '*ad hoc* hypothesis'. Put simply, a hypothesis is "*ad* hoc_a" at time *t* for Grünbaum only if it has no independently testable consequence for which there is either "empirical sanction or disapprobation" at *t*. More strongly, a hypothesis is "*ah* hoc_b" for him only if no such consequence is known at *t*. More strongly still, it is "*ad* hoc_c" only if there is no such consequence at all.[66] An *ad* hoc_c hypothesis is thus the same, in this important respect, as what I have called an *ad* hoc_p hypothesis. By constructing a hierarchy of types of hypothesis in this manner, however, all gathered under the general classification '*ad hoc*', Grünbaum gives a gloss of logical respectability to Popper's purely rhetorical attack on reasonable auxiliary hypotheses as '*ad hoc*'.

Now according to Grünbaum, the trans-Uranian planet hypothesis was *ad* hoc_a. This is false. It is a consequence of any of the hypotheses of trans-Uranian planets that Leverrier or Adams formulated, for example, that none of these planets would cause any significant perturbations in Saturn's orbit, a consequence which had the "empirical sanction" of the fact that the tables for Saturn were in good agreement with observation at the time.[67] This same fact of course constituted an "empirical . . . disapprobation" of the hypothesis of an *intra*-Uranian planet, because of the perturbations that such a planet *would* cause in Saturn. The *trans*-Uranian planet hypothesis was of course designed not only to remove one anomaly, but to avoid generating others. Another, if perhaps weaker, empirical sanction of this hypothesis, and one that it was not designed to secure, was the fact that such a distant planet would likely be so inconspicuous as to have gone thus far undetected, which of course was the case. Had someone suggested, however implausibly, a planet with an angular diameter equal to that of, say, Saturn, one of the first questions that person would have faced would have been: why have we not noticed this planet before?

Grünbaum contends that his notion of an *ad* hoc_a hypothesis explicates Popper's remark that the trans-Uranian planet hypothesis was "introduced *ad hoc*".[68] On syntactical grounds alone, however, this is implausible. It is much more plausible to suppose that '*ad hoc*' is being used by Popper here simply in its ordinary English sense.

Leplin sensibly rejects Grünbaum's classification of the trans-Uranian planet hypothesis as even weakly *ad hoc*, and he thinks that Grünbaum is on the wrong track in regarding *ad hoc*ness as an "epistemic concept" rather than a "methodological concept".[69] *Ad hoc*ness, Leplin claims, is "used not to assess the extant or prospective empirical support of a hypothesis, but to assail the type of response the hypothesis represents" to some problem.[70] But *ad hoc*ness *is* an epistemic or a purely logical concept. Leplin fails to distinguish the logical or epistemic basis or criterion for classifying a hypothesis as *ad hoc* from the methodological rule proscribing or tut-tutting any hypothesis so classified. To illustrate this point, take 'analyticity', which is clearly a logical notion. Suppose I point out to someone whose advice about tomorrow's weather is that it may or may not rain that it is no business of any such adviser to dispense analyticities. This does not make analyticity a methodological concept. The concept of drag, for example, is a fluid mechanical concept; the concept of cross-infection is a medical concept. Neither is a methodological concept because we advise aircraft designers and nurses to take steps to minimize the incidence of drag and cross-infection, respectively.

Leplin would be at least less likely to make this mistake if he and Grünbaum were arguing in a similar vein about analyticity rather than *adhoc*ness. This would be so, I conjecture, largely because 'analyticity' and its cognates are not used in ordinary English, and only occasionally or rarely in philosophical and scientific discourse, to qualify methodological terms. Not so, of course, '*ad hoc*'. We speak of a move, or a procedure, or a strategy as *ad hoc*. In the unlikely event that it was equally common or natural in all of the above discourses to speak of analyticity moves (or analytic moves) when assailing the response of, say, my would-be adviser on the weather, Leplin would probably repeat his mistake with *ad hoc*ness and describe analyticity as a methodological concept.

Leplin's conditions for a hypothesis to be *ad hoc* are many, though all of them are, as one would expect, either logical or epistemic. The key condition he now thinks is this: a hypothesis, h, is *ad hoc* relative to some theory, T, and some "empirical result", e, only if "there are problems for T other than e which there is reason to require that a solution to e solve or help to solve as well."[71] Clearly, this is an epistemic condition; it is a condition that h *explain* certain empirical results besides e. Since there were few, if any, such results confronting Newtonian mechanics at the time, apart from Uranus's residuals, Leplin does not count the trans-Uranian planet hypothesis as *ad hoc*. Even so, he is no friend of this hypothesis, as we shall see.

Leplin does seem to be closer here than Popper ever comes, however, to giving an account of what scientists characteristically or often mean when they describe a hypothesis as *ad hoc*. But we need to bear in mind that any judgement about whether or not the various anomalies he refers to are so related may itself be mistaken. For example, had there been problems with residuals in several other planets of the same order as those in Uranus, perhaps amongst other anomalies in Newtonian mechanics, I take it Leplin would think the above condition of *ad hoc*ness satisfied. Nonetheless, Uranus's residuals could still have been caused by Neptune, and other novel auxiliary hypotheses found to explain the other anomalies. I agree that there would have been considerably less reason for believing or supposing that the trans-Uranian planet hypothesis would succeed in this case, which is a function of the fact that the support for Newtonian mechanics would have been correspondingly weaker. But this only shows that we can distil talk of *ad hoc*ness from such analyses, leaving the familiar concern of asking for the reasons or evidence for a proposition.

Grünbaum and Leplin both presuppose WPF. Both assert that the function of hypotheses like the trans-Uranian planet hypothesis is to protect or rescue theories like the law of gravitation from refutation. Leplin regards such hypotheses as a "threat to a falsificationist methodology".[72] This is false. They are a threat to *Popper's* falsificationist methodology, or to one like it, but that is all. Anyone who believes that Neptune has been discovered should believe that the assumption that the solar system ends at Uranus has been falsified. Leplin says, forcefully, that there were "substantive bases for impugning" the trans-Uranian planet hypothesis.[73] Grünbaum, however, is equivocal. At first, he lumps this hypothesis together with other so-called *ad hoc* hypotheses describing them all as "cognitively a *deus ex machina* and hence methodologically somewhat suspect or even quite illegitimate".[74] He then claims, however, that this hypothesis is amongst those *ad hoc* hypotheses that are "not . . . even peccadilloes";[75] next that it "may call [only] for . . . a non-pejorative *caveat*";[76] but finally, stiffening his resolve, he says that it "twice evoked an attitude of epistemic *caveat* from no less a nineteenth century astronomer than the Astronomer Royal [Airy]",[77] who was "rightly uneasy" about it.[78] Leplin also relies heavily on certain of Airy's alleged beliefs or reasons, and I shall begin my criticism of their attack on this hypothesis by examining aspects of Airy's role in events prior to the discovery of Neptune.

Let us begin with the two 'epistemic *caveats*' Grünbaum refers to above. In 1834, Airy (who was not then Astronomer Royal) wrote to the Reverend T.J. Hussey:

> I have often thought of the irregularity of *Uranus*, and since the receipt of your letter have looked more carefully to it. It is a puzzling subject, but I give it as my opinion, without hesitation, that it is not yet in such a state as to give *the smallest hope* [emphasis mine] of making out the nature of any external action on the planet.[79]

And in 1837, writing to Bouvard's nephew, Eugene Bouvard, Airy said:

> I cannot conjecture what is the cause of these errors [in Bouvard's tables], but I am inclined, in the first instance, to ascribe them to some error in the perturbations. . . If it be the effect of any unseen body, it will be nearly impossible to *ever* find out its place. (Emphasis mine.)[80]

Since the evidence is, firstly, that Airy made these remarks when no one had conducted any analysis of this problem; secondly, that both Adams and Leverrier *were* able to make out the nature of the "external action" on Uranus and to locate Neptune from their respective analyses; and, thirdly, that modern opinion is that this problem was or could then have been solved by their methods or by others, the onus is on Grünbaum to *defend* Airy.[81] He makes no attempt to do so, however, merely quoting the above opinions. For Grünbaum, apparently, it is sufficient that Airy became Astronomer Royal in 1835.

But there is more. Firstly, whatever his other or later opinions, Airy does not express any uneasiness or misgiving above about the trans-Uranian planet hypothesis, that is, about the hypothesis that, as Grünbaum oversimplifies it, "there is an extra-Uranian planet".[82] Airy indicated to Bouvard's nephew a tentative preference for another explanation, but his skepticism above is reserved for the proposition that an exterior planet could be found by analytical means. One could always have attempted, though it would have been laborious, to find the would-be planet by sitting at the telescope, as the Lilienthal Detectives had planned to do with the alleged planet between Mars and Jupiter.

Secondly, Airy did an about face on the thorough-going conviction he expresses above that the problem was virtually insoluble, though Grünbaum does not mention this fact. The first sign of this about face came at the end of 1845 when Airy remarked of Leverrier's first paper on Uranus that "the theory of *Uranus* was now, for the first time, placed on a satisfactory foundation".[83] In this paper, Leverrier concluded that the irregularity in Uranus's motion was the result of "*outside causes*".[84] (Emphasis mine.) In his second paper, Leverrier assigned a longitude to the exterior planet whose existence he had earlier hinted at, and Airy said of this paper soon after sighting it, "I cannot sufficiently express the feeling of delight and satisfaction which I received from it."[85] What Airy could not have been but struck by in this paper was the near coincidence of the longitude Leverrier had assigned the unknown planet with that which Adams had done, a difference of little more than one degree. (Adams had left a brief statement of the results of his investigation with the Astronomer Royal several months earlier.)[86] Less than a week after he received Leverrier's second paper on Uranus, Airy announced to a meeting of the Board of Visitors at the Greenwich Observatory that there was now an "extreme probability" of "discovering a new planet in a very short time".[87]

So why does Grünbaum prefer Airy's *earlier* opinion, one which was formed without the benefit of his later acquaintance with these two investigations - the only two ever completed before the discovery of Neptune? If Airy had been *rightly* uneasy about such investigations only a few years earlier, what has changed to make such unease apparently no longer justified? Grünbaum does not venture to say.

Thirdly, there is, in any case, good reason to doubt that Airy is a reliable source, or at the very least that he is one to be taken at face value, in this whole affair. It is difficult to escape the conclusion that Airy's scientific opinions and behaviour in this case were often shaped at least as much by political considerations and personal prejudice or other idiosyncracies as by the science of the matter. To illustrate this point, let me flesh out the circumstances that surrounded his avowed sea change on the possibility of determining the location of an exterior planet, circumstances that might easily have led to the discovery of Neptune but that did not.

Adams had been unable to see Airy in October 1845 when he left the statement of his results I mentioned above.[88] In this statement, Adams set down the mass and orbital elements of the hypothetical planet along with the remaining Uranian residuals, the mean of which was just a few seconds of arc.[89] Although habitually prompt as a correspondent, Airy did not reply to Adams for a fortnight, a delay which Grosser puts down to his doubts about not only such investigations but also "the abilities of younger scientists".[90] In his reply, Airy referred to the elements of Adams's planet as "assumed", which suggests that he thought Adams had simply made an educated guess at all the relevant properties of the hypothetical body, and then calculated the effect of such a body on Uranus. Airy also asked whether or not his solution would remove the discrepancies in the radius vector of Uranus (the distance of the planet from the Sun at any instant).[91]

Adams was put off by this response of the Astronomer Royal, not least because he thought the inquiry about the radius vector trivial, as others were to do.[92] It is also plain that he would have interpreted Airy's remark about 'assumed' orbital elements as indicating that Airy either did not understand or would not acknowledge the problem he was trying to solve.[93] In any event he did not reply to Airy.[94] Thereafter, Airy made little mention of Adams's investigation or his proposed solution, and even avoided opportunities for doing so, in the remaining months before Neptune's discovery.[95] When Leverrier's proposed solution landed on Airy's desk in June the following year, he shot off the same question about the radius vector to him.[96] Leverrier, however, replied, speedily and firmly, pointing out that this discrepancy was taken care of along with that in the planet's longitude.[97] Airy was apparently wholly reassured by this reply, saying he had "no longer any doubt upon the reality and general exactness of the [exterior] planet's place".[98]

Now according to Airy the radius vector query was a serious one. More than that, it was crucial. In an account of the circumstances that might have led to the discovery of Neptune, which Airy gave to The Royal Astronomical Society shortly after Neptune was discovered, he said that he had thought the question, "Whether the error of the radius vector would be explained by the same theory which explained the error of longitude, would be truly an *experimentum crucis*."[99] Writing to Challis a month later, Airy said, "On this question, therefore, turned the continuance or fall of the law of gravitation."[100]

Taking Airy to be rational in this regard, some modern commentators date his apparent commitment to what we may call the Adams-Leverrier solution to the problem of Uranus's orbit from his receipt of Leverrier's assurance that the errors in the radius vector of the planet had also been removed.[101] On the other hand, no other modern commentator, with the probable exception of W.M. Smart, seems to have grasped the significance of the dates of this correspondence between Airy and Leverrier.[102] Airy wrote to Leverrier with the radius vector query on June 26, 1846.[103] He received Leverrier's reply five days later, on July 1.[104] The meeting at Greenwich at which Airy announced the "extreme probability" of soon finding a new planet, and which was attended by some important astronomers, took place *two days earlier*, on June 29.[105]

This is by no means the only puzzling or recondite feature of Airy's behaviour. For example, at this stage he had seen only abstracts or conclusions of the investigations of both Leverrier and Adams. The full mathematical treatments did not appear until much later.[106] Yet for someone who could say, "I have always considered the correctness of a distant mathematical result to be a subject rather of moral than mathematical evidence", his doubts were seemingly easily allayed.[107] Furthermore, Leverrier had not yet supplied, unlike Adams, the mass or any orbital element of the hypothetical planet save its longitude.[108] But Airy showed no concern to obtain this information before making the above pronouncements, and no interest in doing so once he had.[109] Thus Smart is led to ask, "If there had been wild disagreement between the two sets of elements, would Airy have been so confident in the hypothesis of an exterior planet from the remarkable agreement alone in the planet's longitude . . . ?"[110]

Leverrier asked Airy to look for the hypothetical exterior planet in his reply of July 1, and offered to send its "exact position" as soon as his calculations were done.[111] Airy did not take up this offer, on the excuse that he was leaving soon for Europe. He was not due to leave, however, for almost six weeks, on August 10.[112] Nor did he inform Leverrier of Adams's investigation, notwithstanding the striking agreement in their respective conclusions as to the planet's longitude.[113] The day after he received Leverrier's letter of July 1, another revealing incident occurred. Grosser takes up the story:

> Since June 10, Airy had been host to Peter Andreas Hansen, director of the Seeberg Observatory. Hansen was deeply interested in perturbation theory, and . . . had corresponded with Alexis Bouvard in the early 1830's [sic] about the problem of Uranus. It is very probable that Airy and Hansen discussed Leverrier's paper, but an encounter that sharply delineated Airy's attitude toward Adams indicated that Hansen was not told about the coincidence of Adams's and Leverrier's results. On July 2, 1846, Airy and Hansen visited Cambridge University. Before this time, Airy had met John Adams only once, with Challis; the meeting must have taken place between December 4 and 6, 1845, but Airy "totally forgot where". Now, on St. John's bridge, Airy and Hansen met Adams by chance. One might have expected the Astronomer Royal to take advantage of this golden opportunity to publicize Adam's work, perhaps to arrange a meeting. Airy did neither of these things [nor presumably did he mention anything of Leverrier to Adams]. He was preoccupied and cool; the only thing he later remembered about the interview was that it "might [have lasted] two minutes." His otherwise excellent memory invariably failed him in matters involving Adams.[114]

Grosser continues:

> It is possible that Airy's close brush with his conscience may have been partly responsible for his actions a week later. On July 6, he went to visit George Peacock, the Lowndean professor of geometry and astronomy at Cambridge; Peacock's questions about the problem of Uranus, following close on Airy's chance encounter with Adams, overcome the Astronomer Royal's last resistance to the step he had been avoiding so determinedly. On July 9, 1846, Airy wrote to Challis, asking him to begin a search for the hypothetical planet. The Astronomer Royal's letter began with a phrase that was flatly unbelievable to anyone familiar with his past actions:
>
>> You know that I attach importance to the examination of that part of the heavens in which there is . . . reason for suspecting the existence of a planet exterior to *Uranus*.[115]

Airy went on to say that the situation was now "almost desperate";[116] nonetheless, the search he proposed to Challis was a decidedly conservative one, belying the confidence he professed to have in the Adams-Leverrier solution. Airy urged Challis to search not, say, five, ten, or even fifteen degrees along the ecliptic, and perhaps a few degrees either side, but an area thirty degrees long and ten degrees wide (centred on the ecliptic); and Challis agreed to do so.[117] Even though an unenthusiastic conscript in this exercise, Challis's conservatism led him to map stars down to the *eleventh* magnitude in this area, despite the advice he received from Adams that the planet would be no duller than the ninth.[118] Had he acted on this advice, Challis would have had in the order of eight times fewer stars to bother with, and the head start he had on Galle of almost two months might well have paid off.[119]

It is always possible, of course, that further historical or scientific research will cast Airy's actions or judgements in a more favourable light, though it is a remote possibility that any such revisions would be sufficient to justify Grünbaum's position (and there is more concerning Airy that would need to be explained away than I have revealed above).[120] Even so, such considerations are not to the point. The point is that it is the job of Grünbaum, and as we shall see Leplin, to address that research which *has* been done.

Leplin in fact chides Grünbaum for failing to do just that, though his criticism is muddled. He points out that the very historian from whom Grünbaum draws his evidence for Airy's uneasiness, Morton Grosser, holds that Airy's attitude to Adams's hypothesis is "psychosociologically explained" - his prejudice against young astronomers.[121] Grünbaum, however, could reply that Adams was in short pants at the time Airy expressed the uneasiness *he* cites, and that it had nothing to do with the age of the person conducting the investigation.[122] More importantly, Leplin proceeds to ignore the very point he tries to make stick against Grünbaum, as I shall now explain.

Grosser's view that Airy was, amongst other things, ageist is unmistakable. He says, in part:

> Airy . . . divided the people around him into two groups: those who had succeeded and were worthy of cultivation, and those who had not succeeded and were beneath consideration. Young people fell almost automatically into the latter group. Airy affected no confidence in their abilities, but in fact he determinedly blocked every opportunity for his own young assistants to demonstrate their talents.[123]

Leplin simply ignores this judgement of Grosser's. When he turns his attention to Airy's requirement that an exterior planet should also remove the radius vector error, Leplin says that Airy "made an issue of this requirement because he thought it possible to explain one type of error without explaining the other, and thought that Adams had done so".[124] So much for 'psychosociological' explanations. What is Grosser's view of this matter? He believes that such an explanation *is* called for. He takes the view that Airy was merely trying to rationalise his "negative feelings" about Adams's solution.[125] Writing of Airy's uncharacteristic delay in replying to Adams, Grosser says:

> Perhaps it took that long for him to find some suitable scientific basis for his rejection. He found one, characteristically, in his own past work. In 1838 he had reported to the *Astronomische Nachrichten* that the tabular radius vector of Uranus was too small.[126]

Whatever the truth of Grosser's conjecture about Airy's mental goings on, Leplin at least cannot afford to ignore or dismiss it. Both he and Grünbaum make extensive use of Grosser, who would seem to be almost their only source of information about this whole episode. Of all the modern commentators on the discovery of Neptune or of Airy's role in it, however, none would be more severe on Airy than Grosser. Yet this fact causes not the slightest perturbation in the account that either Leplin or Grünbaum provides. Why is this?

Grünbaum and Leplin together have a battery of objections to the trans-Uranian planet hypothesis, other than the one-time skepticism of the Astronomer Royal about complex or extensive mathematical investigations. I shall consider these other objections briefly, beginning with Grünbaum, and we can then return to answer the question I have raised above.

Having cited Airy's remark that it would be "nearly impossible to ever find" the place of an exterior planet, Grünbaum says:

> *In the same vein* [emphasis mine], not only Bessel (Newcomb [1911a], p.227) but of course Leverrier were all too aware of the fallibility of the assumptions made - *e.g.* about the *masses* of perturbing *intra*-Uranian planets - in the calculation of the Uranian orbit which is theoretically expected in the *absence* of any exterior planet. Indeed, Grant ([1966], Appendix, p.609) provides a table, constructed by Benjamin Peirce, which contains 'the results of Le Verrier's attempt to account for the irregularities of the planet without supposing it to be influenced by any foreign cause of disturbance'.[127]

Taking Grünbaum's first point, if one supposes that the calculation of the mass of, say, Saturn is significantly in error, how is one to explain the accuracy of the tables for Jupiter, and *vice versa*?[128] Increasing the estimate of the mass of Saturn to remove Uranus's residuals would generate undetected residuals in the motions of at least Jupiter and Mars. Simon Newcomb, *Grünbaum's source*, points out that when Bessel tried this approach he found that he could succeed with Uranus "only by assigning a mass not otherwise admissible".[129]

As to Grünbaum's second point, since he does not report the *contents* of Peirce's table - both Peirce and Grant were astronomers, contemporaries of Leverrier and Adams - what is his point? Is it merely that Leverrier *did* consider other options? If so, why is *that* remarkable? Summarising the portion of the table Grünbaum refers to, namely, Leverrier's no-planet hypothesis, the mean residual for the modern observations on this hypothesis is 6".1, and for the ancient observations 230".7.[130] The table also shows the residuals for the exterior planet hypotheses of Leverrier and Adams. The corresponding mean residuals for these hypotheses are a healthier 1".4 and 8".4, and 1".1 and 15".6, respectively.[131] Assuming this table is correct, what use, if any, would Grünbaum make of this data? He does not say.

Turning to Leplin, his major objection to the hypotheses of Adams and Leverrier is the idle falsehood that "there was a strong conviction that the postulation of a new planet was premature".[132] Does Leplin really believe this? A remark he makes soon after would not persuade one that he does. Leplin says, "It is ironic that astronomers were more reluctant to accord Uranus planetary status than they were the cause of its perturbations, as they could see Uranus".[133] For all that, he continues:

> But they [astronomers] might well have considered it illegitimate to introduce a new planet as a solution to the problem of Uranus so long as the possibility of solving the problem by reference to known planets was not exhausted. Adams was more subject than Leverrier to this criticism, as Leverrier turned to *UH* [the trans-Uranian planet hypothesis] . . . only after attempting solutions based on revisions, within existing margins of error, of estimates of the masses of known bodies. That *UH* for Adams was a 'mere assumption', insufficiently motivated by the evidential situation and unacceptably extreme among the options yet open, is probably an accurate description of Airy's opinion. These options, moreover, still included a Uranian encounter with an unknown comet and an unknown Uranian satellite, if the most conservative approach via known bodies proved untenable.[134]

But as we have seen (3.3, 3.5, and immediately above), all of the *other* options Leplin canvasses here had been rejected or passed over by astronomers for the hypothesis of an exterior planet, and with good reasons. Secondly, Airy's suggestion that Adams had merely assumed the orbital elements of the planet he postulated was itself a mere assumption. He knew nothing of Adams's method.[135] And Leplin seems once again unconcerned by the very objection he raises to Grünbaum's historiography where Airy's motives or reasons are concerned. In a footnote to the penultimate sentence of the passage quoted above, Leplin even invites us to compare Grosser's account with that of N.R. Hanson, to whom he seems to look for support. Hanson says:

> Although Adams knew (in 1843) that no undisturbed orbit would fit the observations of Uranus he *assumed* that the disturbance was due to an unknown body. Leverrier's first move was to *establish* this by careful analysis of all available observations.[136]

But it is difficult to see what can be Hanson's point about Adams here. If Adams knew that no "undisturbed orbit" would do, then he knew that there was *some* unknown body that disturbed Uranus (or that the law of gravitation was false). Moreover, how can he believe that Leverrier could have established that it was the former possibility from an analysis of Uranus's residuals? In any event, Hanson's principal source for this criticism of Adams would appear to be Adams's famous diary note of July 3, 1841,[137] which reads:

> Formed a design, in the beginning of this week, of investigating, as soon as possible after taking my degree, the irregularities in the motion of Uranus which are yet unaccounted for; *in order to find whether they may be attributed to the action of an undiscovered planet beyond it*; and if possible thence to determine the elements of its orbit etc. approximately, which would probably lead to its discovery. (Emphasis mine.)[138]

In this note (as elsewhere), Adams does *not* merely assume that an unknown planet is the cause of Uranus's irregularities. He asserts that he intends to determine whether or not this is so.

In his *History of Physical Astronomy*, written shortly after the discovery of Neptune, the astronomer *cum* historian, Robert Grant, discusses the problem-solving aspects of this case at length. Of this aspect of Adams's investigation, he says:

> Mr Adams first proceeded to examine the perturbations produced in the motion of Uranus by the other planets, in order to assure himself beyond doubt that the errors of Bouvard's tables did not proceed from an erroneous application of the existing theory. For this purpose he recomputed the principal perturbations due to Jupiter and Saturn, and introduced some new inequalities which had been first pointed out by Hansen. He also took into account the correction to Jupiter's mass, to which recent researchers had conducted Astronomers. Notwithstanding these improvements, the theory still failed to represent the motion of the planet. Two important advantages were, however, gained by these preliminary labours. In the first place, it was clearly established that the cause of the irregularities must be sought elsewhere than in the development of the actual theory. In the second place the application of the improvements had the effect of exhibiting the errors of the tables as residual facts wholly dependent on some extraneous influence, and consequently they now assumed a more precise and definite character than they had previously done.[139]

What other direct, or indirect, evidence there is on how Adams tackled this problem supports the view that, whatever mistakes he may have made, he was not negligent or headstrong in the way Leplin and Hanson imply, or indeed in any other.

Leplin continues:

> A different reason for considering *UH* premature was the belief that data on Uranus, which only fifteen years earlier [c. 1830] had begun to depart markedly from Bouvard's tables, were insufficient to determine elements of orbit for a perturbing planet. Leverrier's 1846 proof that the perturbations of Uranus could not be explained by influences of known bodies pertained to a Uranian orbit which itself was an uncertain projection from the data, and definitive tables for Uranus became available only later in the century. Thus it would have been perfectly sensible to 'dismiss' *UH* or to discourage others from pursuing it even for one who did not at all presume that the final solution lay elsewhere. In fact, Airy was not pursuing alternative solutions to the problem of Uranus while discounting Adams's; he was keeping careful data on Uranus while working on other problems altogether. The rejection of an hypothesis as speculative is common. It reflects the capacities of the discipline at a certain time, not an intrinsic implausibility of the hypothesis.[140]

There are several points to be made about the above passage. Firstly, it is Airy's belief alone about the insufficiency of the data on Uranus that Leplin has evidence for above. In a footnote to this passage, he says that Airy thought that "it might require several revolutions of Uranus" to provide sufficient data.[141] Airy, however, said he was *sure* of this requirement. Leplin waters down Airy's opinion, which has not been a popular one, for it would be sometime *next century* at the earliest before such data would become available.[142] (Grünbaum chooses not to mention this opinion of Airy's, which was expressed in the letter to Hussey from which he quotes.)[143] Later in the above passage of Leplin's, it is Airy, once again, who supposedly speaks for the discipline as a whole as to its capacity, or lack thereof, to locate an exterior planet. But what of the fact that many disagreed with Airy, or of Airy's own about face on this matter shortly after? Leplin is silent.

Secondly, Leplin's central criticism above shows that he fails to understand the nature of the problem Leverrier and Adams tackled, and this failure vitiates his criticism. Uranus's residuals had *two* components: the errors in the orbital elements of the planet and the perturbations caused by Neptune.[144] The former would remain uncertain whilever the latter was not taken into account. Even if someone *had* hit upon the correct orbital elements for Uranus, that person would have had no justification for believing that he or she had done so for Uranus would still have been plagued by residuals, those caused by Neptune. Thus the uncertainty that so concerns Leplin would remain, and such efforts as Adams and Leverrier expended would therefore *always* have been premature on his account of the matter. When Leplin adds that definitive tables for Uranus only became available much later in the nineteenth century, this confirms his mistake. There would have been no such tables *unless*

UH had been entertained (apart from the fact that there is no *need* for *UH* once such tables are available). Accurate tables for a planet depend upon knowing, or at least having a true description of, the relevant properties of *all* the planets which sensibly perturb the planet for which those tables are being constructed. Leplin falsely implies that astronomers could, and presumably should, have simply waited for such tables to become available before entertaining *UH*. Moreover, one does not need to know what are the elements of a planet's orbit in order to show or have good reason to believe that it has residual or unexplained perturbations. If the perturbations due to Jupiter and Saturn are removed from the observed longitudes of Uranus, and yet no (elliptical) orbit will satisfy these supposedly unperturbed longitudes, there is good reason to believe that the inventory of perturbing forces on Uranus is incomplete. All that Leverrier and Adams needed to do was to satisfy themselves that no member of the family of possible (unperturbed) orbits of Uranus would satisfy this data.[145] They did not need to know what was the planet's true orbit.

Leplin has a final objection:

> A third reason for opposition both to *UH* and *MH* [the hypothesis of an intra-Mercurial planet, later introduced to explain the advance of Mercury's perihelion] concerns the observability of postulated planets . . . . That such planets were unobserved, or, in the case of Neptune, unrecognised, was only part of the problem. The other part was that they were theoretically observable. Were they unobservable, had it been possible to argue that their properties precluded observation, the legitimacy of *UH* and *MH* would thereby have been *enhanced*. This is particularly clear in the case of *MH* on behalf of which astronomers attempted persistently if unsuccessfully to contrive suitably perturbing matter so positioned . . . as systematically to elude detection. And when a trans-Uranian planet was first suggested [in 1835] to provide additional forces on Halley's comet, the assumption that Bode's law would prevent the planet from ever being observed made astronomers consider the possibility of theoretically inferring it all the more significant an accomplishment, attesting to the maturation of their science.[146]

This is a curious objection, to say the least. How does someone come to believe that it is preferable to postulate *un*observable planets? Leplin later asserts that propositions about unobservables are subject to an "epistemic liability" from which propositions about observables are free.[147] Quite so. Alleged observables are liable to be shown not to exist *by observation* - a point in favour, one would think, of the postulation of the corresponding propositions. And anyone who finds virtue in a falsificationist methodology, as Leplin does, should not need reminding of this point. It is true, as he points out, that scientists do postulate unobservables. But that is not to say that, other things being equal, such postulates are preferable to postulates about observables, or that scientists regard the latter as somehow less legitimate. If Leverrier had surmised that there was little or no prospect of observing the planet he thought responsible for Uranus's residual perturbations, would that have *enhanced* his conclusions? Would that have given him an advantage over Adams?

As to Leplin's two examples, in the case of *MH*, astronomers generally turned to hypotheses involving matter that would be difficult to observe or unobservable, such as a ring of small asteroids or mere "planetary dust", only once it was realised that one body of the required mass ought already to have been observed or could not be found.[148] In the case of Halley's comet, the one astronomer, Jean Valz, that Leplin does have evidence for (again he generalises wildly) said, "Would it not be admirable . . . to ascertain the existence of a body which we cannot even observe?"[149] Valz does not say here that it would be *more* admirable than if this body were merely as yet unobserved. Nor does he say that it would be *more* admirable if this body were such that we could never observe it. To see that astronomers are more impressed by theoretical inferences which then gain observational support one need look no further than the discovery of Neptune. By and large, Newtonian mechanics was eulogized, Leverrier and (eventually) Adams showered with praise and honours only *after* the planet was discovered.[150] "There were some dignified huzzahs for another analytical 'triumph'" when Leverrier's prediction was first published, says Grosser, but that was all; and the more detailed calculation which appeared in his third paper on Uranus fared no better.[151] Had Leverrier or Adams postulated an unobservable planet as the cause of Uranus's troubles, it is difficult to believe that the relative indifference astronomers *had* shown to their theoretical demonstrations would suddenly have given way to enthusiastic talk about a "significant accomplishment" and "the maturation of their science".

To sum up Leplin, he has no grounds for his belief either that by the 1840s *UH* was premature or that one reason for opposing this hypothesis was that the planet it postulated was observable. But even if he had such grounds, it would be *philosophically* small beer. If instead the evidence were that some *other* hypothesis - a massive Uranian satellite, a dark exterior planet, a much heavier Saturn, or merely some errors in Bouvard's original analysis - *should* have been considered before *UH*, what would that show? It would show only that astronomers were misinformed as to how best to set about protecting their core beliefs, not that there was anything untoward in their attempting to do so.

In conclusion, I have analysed the accounts of Grünbaum and Leplin at some length in order to show the *resilience* of Popper's false doctrine that the so-called practice of avoiding refutations, with its dark suggestions of *ad hoc* hypotheses, is a rationally undesirable practice. No other explanation of Grünbaum's and Leplin's treatment of the historical evidence or of the scientific arguments seems to me at all plausible than that both were in the grip of this doctrine when they came to consider this case.

Notes for Chapter Four

1. Lakatos, "Falsification", pp. 100-102.

2. Ibid., p. 133.

3. Ibid.

4. This apt description of Lakatos's "protective belt" as a 'minder' is due to Eddie Hughes.

5. Mason, *Main Currents*, p. 233.

6. Ibid., pp. 233-34. See also Rene Taton, ed., *History of Science: The Beginnings of Modern Science from 1450-1800*, trans. A.J. Pomerans (New York: Basic Books, 1964), pp. 449-50.

7. Musgrave, "Evidential Support", p. 188; John Worrall, "Research Programmes, Empirical Support, and the Duhem Problem: Replies to Criticism", in *Progress and Rationality in Science*, vol. 58, eds. Gerard Radnitzky and Gunnar Andersson, Boston Studies in the Philosophy of Science (Dordrecht: D. Reidel Publishing Co., 1978), p. 334. Adolf Grünbaum, in "*Ad Hoc* Auxiliary Hypotheses and Falsificationism", *British Journal for the Philosophy of Science* 27 (1976): 360, goes only so far as to falsely claim that "the arrow of *modus tollens*" in one case was "already aiming at the heart" of *T & A* (when some auxiliary hypothesis was introduced).

8. Worrall, "Research Programmes", p. 333.

9. Ibid, pp. 333-34.

10. Mason, *Main Currents*, p. 234; Taton, *History*, p. 450.

11. Anthony O'Hear, *Karl Popper*, (London: Routledge and Kegan Paul, 1980), p. 101.

12. Ibid., p. 107.

13. Ibid., p. 106.

14. Popper, *Unended Quest*, p. 42.

15. O'Hear, *Popper*, p. 100.

16. Ibid., p. 100.

17. Ibid., p. 104.

18. Ibid., p. 109.

19. Ibid., p. 111.

20. Ibid., p. 109.

21. Thomas S. Kuhn, "The Function of Dogma in Scientific Research", in *Scientific Change: Historical Studies in the Intellectual, Social and Technical Conditions for Scientific Discovery and Technical Invention, from Antiquity to the Present*, ed. A.C. Crombie (London: Heinemann, 1963), pp. 347-69.

22. Ibid., pp. 365-66.

23. Newton-Smith, *Rationality*, p. 81. Two pages later he contradicts himself: "However, if the [research] programme develops and has success, the scientist will come thereby to have reasons for his belief in hard-core."

24. Paul K. Feyerabend, "Consolations for the Specialist", in *Criticism and the Growth of Knowledge*, eds. Imre Lakatos and Alan E. Musgrave (Cambridge: Cambridge University Press, 1970), p. 205.

25. Popper, *Unended Quest*, p. 42.

26. John W.N. Watkins, "Against 'Normal Science'", in *Criticism and the Growth of Knowledge*, eds. Imre Lakatos and Alan E. Musgrave (Cambridge: Cambridge University Press, 1970), p. 28.

27. Kuhn, *Scientific Revolutions*, p. 151.

28. Kuhn, "Dogma", p. 347 (n. 1).

29. Thomas S. Kuhn, "Reflections on My Critics", in *Criticism and the Growth of Knowledge*, eds. Imre Lakatos and Alan E. Musgrave (Cambridge: Cambridge University Press, 1970), p. 244.

30. Ibid., p. 247.

31. Ibid.

32. Kuhn, "Logic of Discovery?", pp. 4-10.

33. Kuhn, *Scientific Revolutions*, p. 34. For discussion, see pp. 25-29 and pp. 30-31.

34. Nevil Maskelyne, "The Mountain Method of Measuring the Earth's Density", in *A Source Book in Astronomy*, eds. Harlow Shapely and Helen E. Howarth, Source Books in the History of the Sciences (New York: McGraw Hill Book Co., 1929), pp. 133-39. See also Mason, *Main Currents*, pp. 237-38; A. Wolf, *A History of Science, Technology, and Philosophy in the Eighteenth Century*, 2d ed. (London: George Allen & Unwin Ltd., 1952), pp. 111-12; and H.S. Harrison and Ralph D. Wyckoff, s.v. "Gravitation", *Encyclopedia Britannica*, 1951 ed.

35. Maskelyne, "Mountain Method", pp. 137-38.

36. Wolf, *History*, pp. 112-13; Mason, *Main Currents*, p. 238; and Harrison and Wyckoff, s.v. "Gravitation".

37. Kuhn, *Scientific Revolutions*, p. 27.

38. Putnam, "Corroboration", p. 228.

39. Popper, "Replies", bk. 2: 984.

40 Chris Mortensen and Tim Burgess, "On Logical Strength and Weakness", *History and Philosophy of Logic* 10 (1989): 48.

41. Popper, *Unended Quest*, p. 45.

42. Musgrave, "Method", p. 466. Chalmers, in *What is Science?*, pp. 74-75, for example, discusses the development of "Copernican theory" and says "Early formulations of the new theory, involving imperfectly formulated novel conceptions, were persevered with and developed in spite of apparent falsifications."

43. Musgrave, "Method", p. 466.

44. Brown, *Perception*, p. 10.

45. Ibid., p. 149; for discussion, see pp. 145-51.

46. Ibid., p. 149.

47. Ibid., p. 97.

48. Harvey Siegel, "Brown on Epistemology and the New Philosophy of Science", *Synthese* 56 (July 1983): 80.

49. Ibid.

50. Ibid.

51. Harold I. Brown, "Response to Siegel", *Synthese* 56 (July 1983): 102.

52. Musgrave, "Falsification", p. 405.

53. Ibid.

54. Grosser, *Neptune*, p. 99.

55. N.R. Hanson, *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science* (Cambridge: Cambridge University Press, 1958), pp. 103-4.

56. Musgrave, "Method", p. 479.

57. Koertge, "New Theory", p. 255.

58. Ibid., p. 256.

59. Ibid.

60. Ibid., p. 262.

61. Ibid., p. 263.

62. Worrall, "Research Programmes", pp. 332-33.

63. Popper, *Unended Quest*, p. 42; and "Replies", p. 986.

64. Newton-Smith, *Rationality*, p. 73.

65. Ibid.

66. Grünbaum, "*Ad Hoc*", p. 354; for discussion, see pp. 331-37.

67. Ibid., pp. 332-36.

68. Ibid., p. 332.

69. Jarrett Leplin, "The Assessment of Auxiliary Hypotheses", *British Journal for the Philosophy of Science* 33 (1982): 236 and 240-41.

70. Ibid., p. 236.

71. Ibid., p. 237. See also Leplin, "*Ad Hoc* Hypothesis", pp. 331-32.

72. Leplin, "Assessment", p. 235.

73. Ibid., p. 242. Elsewhere (p. 237), he says there were "serious objections" to this hypothesis.

74. Grünbaum, "*Ad Hoc*", pp. 329-30.

75. Ibid., p. 338.

76. Ibid., p. 355.

77. Ibid., p. 357.

78. Ibid., p. 355.

79. Quoted in Grünbaum, "*Ad Hoc*", p. 355; see also Grosser, *Neptune*, p. 93.

80. Ibid,

81. Grosser, *Neptune*, pp. 46-57, and chap. 5. See also note 119, p. 148 above.

82. Grünbaum, "*Ad Hoc*", p. 355. To Grünbaum's inadequate description of this hypothesis should be added "and which is the cause of Uranus's residuals".

83. Quoted in Grosser, *Neptune*, p. 97.

84. Ibid.

85. Ibid., p. 102.

86. Grosser, *Neptune*, pp. 88-89.

87. Quoted in Grosser, *Neptune*, p. 103.

88. Grosser, *Neptune*, pp. 87-88.

89. Ibid., pp. 88-89.

90. Ibid., p. 92.

91. Ibid., p. 94.

92. Ibid., pp. 94-95, and 104.

93. See Hanson, "Retroductive Inference", pp. 35-36; Lyttleton, *Mysteries*, p. 221; Smart, *Occasional Notes*, pp. 23 and 37; and W.M. Smart, "John Couch Adams and the Discovery of Neptune" *Nature* 158 (November 9, 1946): 650.

94. Ibid., p. 94.

95. Grosser, *Neptune*, pp. 92, 103, and 105.

96. Ibid., p. 103.

97. Ibid., p. 104.

98. Quoted in Grosser, *Neptune*, pp. 104-5.

99. Ibid., p. 94.

100. Ibid., p. 96.

101. Harold S. Jones, "G.B. Airy and the Discovery of Neptune" *Nature*, 158 (December 7, 1946): 829; and Lyttleton, *Mysteries*, pp. 224-25.

102. See Smart, *Occasional Notes*, p. 27. Smart *notes* that Airy had not received Leverrier's reply when he made his announcement of the probability of finding a new planet, but Smart typically spells out what he finds odd or mistaken in Airy's views. For his account of this episode, see *Occasional Notes*, pp. 26-27.

103. Grosser, *Neptune*, p. 103.

104. Ibid., p. 104.

105. Grant, *History*, p. 184; Grosser, *Neptune*, p. 103; Smart, *Occasional Notes*, p. 27.

106. Smart, *Occasional Notes*. p.35.

107. Quoted in Smart, *Occasional Notes*, p. 26.

108. Smart, *Occasional Notes*, pp. 25-26. See also Grosser, *Neptune*, pp. 100-102.

109. Grosser, *Neptune*, pp. 102-7.

110. Smart, *Occasional Notes*, p. 27.

111. Grosser, *Neptune*, p. 104.

112. Ibid., p. 105.

113. Three weeks after Neptune was discovered Airy wrote to Leverrier:

> I do not know whether you are aware that collateral researches had been going on in England and that they led to precisely the same results as yours. I think it probably that I shall be called on to give an account of these. If in this I shall give praise to others, I beg that you will not consider it as at all interfering with my acknowledgement of your claims. *You are to be recognized beyond doubt as the real predictor of the planet's place*. I may add that the English investigations, as I believe, were not quite so extensive as yours. They were known to me earlier than yours. (Emphasis mine)

Quoted in Grosser, *Neptune*, pp. 128-29.

114. Grosser, *Neptune*, p. 105.

115. Ibid., pp. 105-6. See also Smart, *Occasional Notes*, p. 34.

116. Ibid., p. 106.

117. Grosser, *Neptune*, p. 108.

118. Ibid.

119. Arthur S. Eddington and Harold S. Jones, s.v. "Star", in *Encyclopedia Britannica*, 1951 ed., supply the following data:

Visual Magnitude	Star Numbers
9th	117,000
11th	870,000

120. See for example, Smart, *Occasional Notes*, pp. 36-37, and note 113, p.197 above.

121. Leplin, "Assessment", p. 241.

122. Adams turned fifteen in 1834, the year Airy wrote to Hussey.

123. Grosser, *Neptune*, p. 91.

124. Leplin, "Assessment", p. 247.

125. Grosser, *Neptune*, p. 93.

126. Quoted in Grosser, *Neptune*, p. 94.

127. Grünbaum, "*Ad Hoc*", p. 355.

128. Grosser, *Neptune*, p. 46; Smart, *Occasional Notes*, p. 7; Flammarion, *Flammarion Astronomy*, p. 321.

129. Simon Newcomb, Theodore E.R. Phillips, and Seth B. Nicholson, s.v. "Gravitation", in *Encyclopedia Britannica*, 1951 ed.

130. See Grant, *History*, p. 609.

131. Ibid.

132. Leplin, "Assessment", p. 242.

133. Ibid.

134. Ibid.

135. Grosser, *Neptune*, chap. 5.

136. Hanson, "Leverrier", p. 360.

137. Ibid. (n. 1). See also Grosser, *Neptune*, pp. 78-79.

138. Quoted in Grosser, *Neptune*, pp. 75-76.

139. Grant, *History*, p. 170. George Forbes, in *History of Astronomy* (London: Watts and Co., 1909) says, "Adams first recalculated all known causes of disturbance using the latest determinations of the planetary masses. Still the errors [in Uranus] were nearly as great as ever "(p. 69).

140. Leplin, "Assessment", p. 242.

141. Ibid. (n. 2).

142. Quoted in Grosser, *Neptune*, p. 93.

143. Cf. Grünbaum, "*Ad Hoc*", p. 355; and Grosser, *Neptune*, p. 93.

144. Harold S. Jones, *John Couch Adams and the Discovery of Neptune* (Cambridge: Cambridge University Press, 1947), pp. 9-10; Pannekoek, "Neptune", p. 134.

145. Smart, *Occasional Notes*, pp. 20-21; Grant, *History*, p. 177.

146. Leplin, "Assessment", p. 243.

147. Ibid., p. 244.

148. Hanson, "Leverrier", pp. 368-76. See also Newcomb, "Discordances", pp. 335-36.

149. Quoted in Grosser, *Neptune*, p. 51.

150. Grosser, *Neptune*, chap. 7; Smart *Occasional Notes*, pp. 43-44.

151. Grosser, *Neptune*, p. 102.

Some Concluding Remarks

Popper is a profoundly inconsistent skeptic. He wants very badly what he cannot have, namely an account of science in which theories are refuted; yet he is not prepared to relinquish the skepticism which stands in the way of his obtaining it. If one cannot hold a position, however, one can always delude oneself that one is able to do so or cultivate the impression of doing so by the use of rhetoric. This method of dealing with a genuine problem infects Popper's thinking generally, as is illustrated, for example, by his use of rhetoric to convince us that the refutation or attempted refutation of theories is the core of scientific practice. Rhetoric aside, however, inconsistent skepticism is a seductive position, and we should not be surprised to find so many people occupying it. For this position seemingly allows one to cut the ground from under anyone who shows the slightest tendency of asserting anything positive that is not consistent with one's own positive assertions. It also plays on our natural tendency to be more charitable to our own views than to the views of others who disagree with us.

What, then, of the place of refutation, or attempted refutation, of major theories in science? Given the condition that one can have more or less reason for believing or accepting a proposition or that one can rationally prefer to believe or accept one proposition rather than another, it matters little to science whether or not the law of gravitation is or is not now refuted - because of the difficulties involved in, say, knowing all the forces at work in any case. There are many propositions none of us can easily refute, but this fact is not one which, given the above condition, hinders intellectual progress. I have not refuted the proposition that the probability of discovering a trans-Uranian planet prior to 1846 was not low, but I do not need to do so for philosophy of science to progress in this (small) area. All that I need to do is to provide good reason to believe that this proposition is false. When people believed that universal generalizations were knowable and that it was important that they were, it was important to point out that this belief was false. The importance of pointing out that such propositions are falsifiable is much less now that this false belief is much less common. As to the point that we should attempt to refute our theories, if this is taken to mean only that a conscious test of a theory ought to be designed such that we stand the best chance of obtaining refuring evidence or strong counter-evidence for that theory (if it is false), then I agree. This is only the other side of the coin, however, from designing a test to obtain the strongest possible confirmation possible (if the theory is true). As to Popper's objection that confirmations can be had for the asking, my reply is that weak confirmations can certainly be had for the asking - just like *refutations*$_p$.

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Some Concluding Remarks: Addendum - see *Note, p.i above

One final point: it might be thought that a study of Popper's account of scientific method, especially one as extended as the above, would be deficient or defective to the extent that it did not treat his rejection of inductive reasoning. A complete analysis of his account would certainly require such a treatment, which is not attempted above. But this analysis, with its focus on scientific *practice* and its concern for the nature and role of refutations and their 'avoidance', is not compromised by this boundary to the study. Even if Popper's attitude to induction were not atypical, the problems raised for his methodology above would remain. His insistence that corroboration is exclusively 'backward looking' and that contingent propositions can command no positive support, for example, does nothing to relieve him of the task of explaining how it is that *theories* rather than auxiliary propositions are refuted, or *prima facie* refuted, when predictions fail.

Were he to face the fact that refutation$_p$ is mere discorroboration then if T is well corroborated compared with A, assuming such comparisons can be made, A would be well *dis*corroborated compared with T by the discovery that P is false. That much a deductivist can have, that much a weak falsificationist such as Popper needs. The fact that such discorroborations are themselves also only backward looking is *another* matter (if another problem for Popper). The point is that no *epistemic* conclusions about *either* T *or* A follow from the discovery that P is false. (It does not follow, for example, from the fact that P is false, and hence that $T \& A$ is false, even that T and A are equally doubtful.) So there is simply no falsificationist (even if discorroborationist) methodology, which has as its object *individual theories*, but that gives consideration to the strength of the antecedent in

$$(\sim P \ \& \ A) \dashrightarrow \sim T$$

whether 'strength' is understood as (involving) inductive support *or* as corroboration.