LEABHARLANN CHOLÁISTE NA TRÍONÓIDE, BAILE ÁTHA CLIATH | TRINITY COLLEGE LIBRARY DUBLIN
Ollscoil Átha Cliath | The University of Dublin

Terms and Conditions of Use of Digitised Theses from Trinity College Library Dublin

Copyright statement

All material supplied by Trinity College Library is protected by copyright (under the Copyright and Related Rights Act, 2000 as amended) and other relevant Intellectual Property Rights. By accessing and using a Digitised Thesis from Trinity College Library you acknowledge that all Intellectual Property Rights in any Works supplied are the sole and exclusive property of the copyright and/or other IPR holder. Specific copyright holders may not be explicitly identified. Use of materials from other sources within a thesis should not be construed as a claim over them.

A non-exclusive, non-transferable licence is hereby granted to those using or reproducing, in whole or in part, the material for valid purposes, providing the copyright owners are acknowledged using the normal conventions. Where specific permission to use material is required, this is identified and such permission must be sought from the copyright holder or agency cited.

Liability statement

By using a Digitised Thesis, I accept that Trinity College Dublin bears no legal responsibility for the accuracy, legality or comprehensiveness of materials contained within the thesis, and that Trinity College Dublin accepts no liability for indirect, consequential, or incidental, damages or losses arising from use of the thesis for whatever reason. Information located in a thesis may be subject to specific use constraints, details of which may not be explicitly described. It is the responsibility of potential and actual users to be aware of such constraints and to abide by them. By making use of material from a digitised thesis, you accept these copyright and disclaimer provisions. Where it is brought to the attention of Trinity College Library that there may be a breach of copyright or other restraint, it is the policy to withdraw or take down access to a thesis while the issue is being resolved.

Access Agreement

By using a Digitised Thesis from Trinity College Library you are bound by the following Terms & Conditions. Please read them carefully.

I have read and I understand the following statement: All material supplied via a Digitised Thesis from Trinity College Library is protected by copyright and other intellectual property rights, and duplication or sale of all or part of any of a thesis is not permitted, except that material may be duplicated by you for your research use or for educational purposes in electronic or print form providing the copyright owners are acknowledged using the normal conventions. You must obtain permission for any other use. Electronic or print copies may not be offered, whether for sale or otherwise to anyone. This copy has been supplied on the understanding that it is copyright material and that no quotation from the thesis may be published without proper acknowledgement.

Falsificationism and Theory Adjudication

A Critical Rationalist Critique of Justificationist Theories of Science

By

Steven William Clarke

Submitted in fulfilment of the requirements for the degree of Doctor of Philosophy at University of Dublin, Trinity College, Department of Philosophy

2014

Supervisors: Professor Peter Simons and Dr Paul O'Grady

TRINITY COLLEGE
5 JAN 2015
LIBRARY DUBLIN
Thesis 10715

Declaration

This thesis has not been submitted as an exercise for a degree at this or any other University. This thesis is entirely my own work.

The librarian of Trinity College, Dublin may lend or copy the thesis upon request without reference to me on the understanding that such authority applies to the provision of single copies made for study purposes, subject to normal conditions of acknowledgement.

Signed ....Steven Clarke....

Date ....31/7/14....

"It is wrong always, everywhere, and for anyone, to believe anything upon insufficient evidence."

W. K. Clifford (1879, p. 186)

"It is a simple, though ancient, mistake in the theory of knowledge to think that justification, in any degree, is central to rationality, or even important to it."

David W. Miller (2011a, p. 1)

Summary

This study deals with what is perhaps the most fundamental aspect of Karl Popper's theory of scientific knowledge—that is, his treatment of what he called "the demarcation problem." This problem was concisely formulated by Popper, in *Realism and the Aim of Science* (1983, p. 19), as follows:

> The central problem of the philosophy of knowledge, at least since the Reformation, has been this. How can we adjudicate or evaluate the far-reaching claims of competing theories and beliefs?

My aim in this thesis is to present a critique of the currently dominant approach to this problem, the broad-ranging epistemological position described herein as "justificationism." This critique of justificationism—the multifaceted doctrine that holds that epistemological justification is a necessary condition for rationality—forms the main thrust of the study, but there are several important subsidiary themes.

1. A defence of Popper's thesis that the demarcation problem is the most fundamental problem in the theory of knowledge, "the central problem to which probably all other questions of the theory of knowledge, including the problem of induction, can be reduced" (Popper, 2009, p. 4), and the construal of justificationist philosophy as essentially a response to this problem.

2. The presentation of Popper's critical rationalism as the continuation and culmination of the sceptical tradition in epistemology.

3. A restatement and elaboration of the critical rationalist critique of inductive logic.

4. An analysis of epistemological relativism as an outcome of the failure of justificationism.

5. A restatement and defence of the critical rationalist solution to the demarcation problem, and a response to the major extant criticisms of this solution.

Contents

Acknowledgements

First, I would like to thank my supervisors, Professor Peter Simons and Dr. Paul O'Grady. I have benefitted immensely from their dedicated supervision, their generosity with their time, and, most especially, their patience over the last four years with my evolving research project.

In addition, I would also like to thank Professor James Levine and Professor Darrell Rowbottom, who, as examiners of my thesis, provided many valuable suggestions and corrections to this work, and greatly helped me to improve it in various aspects.

I also thank Dr. Maria Baghramian, Dr. David Berman, and Dr. Thomas McNally for invaluable encouragement, and everyone in the TCD philosophy department who offered helpful feedback and criticism.

On a personal note, I would like to especially thank my parents, John and Maria, for all their love and support.

Steven William Clarke,
University of Dublin, Trinity College, July 2014.

Foreword

> The central problem of the philosophy of knowledge, at least since the Reformation, has been this. How can we adjudicate or evaluate the far-reaching claims of competing theories and beliefs?
>
> (Karl Popper, *Realism and the Aim of Science*, p. 19)

The fundamental problem of the theory of knowledge is the problem of rationally demarcating truths from falsehoods.

This basic problem, of evaluating knowledge claims, has been formulated in numerous ways; as the problem of theory choice, for example, or the problem of hypothesis preference. I propose to call it the *demarcation* problem. Since it is the problem of *rational* adjudication or arbitration amongst rival or competing theories it may also be described, interchangeably, as the problem of *demarcation-cum-rationality*.[1] As such, it is the central methodological problem, not just of theoretical empirical science, but of all truth-seeking enterprises. In particular, it is the fundamental methodological problem for the "nomological" sciences[2], whose task it is to determine in which respects "nature continues always uniformly the same" (to borrow a phrase from Hume), and to separate those *genuine* patterns and regularities from the merely *apparent* ones.

My aim in this thesis is to present a critique of the currently dominant approach to this problem—*justificationism*. More specifically, my aim is to illustrate the main outlines of this prevalent justificationist theory of demarcation-cum-rationality, its inadequacies, and the consequences of its apparent shortcomings. Such an effort will, in the final chapter, clear the ground to advance in its place a defence of what I take to be the *critical rationalist* solution to the problem. This approach is associated most especially with the 20th-century Austrian philosopher of science, Karl Popper (1902-1994), but it has also been significantly extended and elaborated upon by subsequent philosophers; the most noteworthy of these, in my estimation, being W.W. Bartley (1934-1990) and David W. Miller (1942-). The "demarcation problem" primarily associated with Popper is the project within the philosophy of science of providing a demarcation between "the empirical sciences on the one hand, and mathematics and logic as well as "metaphysical" systems on the other" (1959, p. 34). The problem I am addressing in this thesis, very much in line with the work of Bartley (1962), is broader, applying to the rational adjudication of *any* set of competing factual hypotheses, be they empirical or metaphysical. However, the link to Popper's problem will be stressed, for the attempted solution is along essentially Popperian lines. Moreover, although my problem—of rational demarcation of theoretical knowledge claims—is completely general, my primary focus, in order not to get lost in generalities, will follow Popper in focusing on the empirical knowledge of the natural sciences.

The dominant justificationist response to this problem, to be opposed here, has enjoyed, and continues to enjoy something of a monopoly position amongst theoreticians of knowledge. As Bartley ([1962] 1984, p. 186) has asserted:

> We live in a world contaminated by a particular philosophical idea about how *any* such demarcation would have to be obtained. I call this "justificationism". In brief, it is the view that the way to criticize an idea is to see whether and how it can be justified. Justificationism deeply permeates all Western culture, and virtually controls all traditional, modern, *and* contemporary philosophy. This idea shapes the thinking of Plato and Aristotle, of Descartes, Spinoza, and Leibniz, of Locke, Berkeley, and Hume, of Kant and Hegel, of Whitehead and Russell—and also of Wittgenstein, Carnap, Ayer, Ryle, Austin, Quine, Husserl, Heidegger, Barth, Bultmann, Tillich, or almost any other philosopher one might want to name. It shapes phenomenology as much as it does the so-called analytical philosophy that is more characteristic of the English-speaking countries. All these periods, men, and movements participate in what I call the "justificationist metacontext".

What, then, are the central elements of this "justificationist metacontext"?

When employed primarily as a theory of *demarcation*, justificationism is the doctrine that in order to adjudicate between theories we must be able to justify[3] or validate that theory to which we give preference (or, at the very least, our *belief* in that theory).[4] Traditionally the demand was for sufficient or conclusive justification—that is to say, *proof*—but today *partial* justification is generally considered as much as can realistically be hoped for. Accordingly, when it is construed predominantly as a theory of *rationality*, justificationism asserts that the rational agent must be able to *justify,* or provide grounds or "warrant", for her beliefs and opinions. And when incorporated into a theory of science, justificationism holds that the aim of science is, ideally, conclusively verified theories, or, failing this, at least highly *probable* ones—ones which have overwhelming evidential *support*. For the justificationist, the aim of science, and of inquiry in general, is generally not merely *truth*, but the Platonic ideal[5] of *secure* knowledge—that is, true opinion with an *account*. When truth *is* retained as the primary aim of science by justificationists, epistemological justification is seen, at the very least, as the primary means to secure it. Genuine knowledge, so this theory asserts, must fulfil, or at the very least approximate, the ideal of knowledge as statements with truth-guarantee—in other words, statements which are *proven*. Together, these doctrines amount to the demand that theories are to be demarcated *epistemically*, in terms of support, or warrant, or grounds, or evidential backing. For, according to the justificationist, "preferences between theories, or our decisions about which theories should be accepted or believed... stand in need of justification if we are to escape promiscuous irrationalism" (Miller, 1994, p. 121).

As the dominant strategy in the history of epistemology, justificationism has had countless modifications and variations, but each can best be understood as a response to the underlying demarcation-cum-rationality problem. Popper (1983, p. 19) was quite explicit regarding this latent problem situation:

> The central problem of the philosophy of knowledge, at least since the Reformation, has been this. How can we adjudicate or evaluate the far-reaching claims of competing theories and beliefs? This problem has led, historically, to a second problem: How can we justify our theories or beliefs? And this second problem is, in turn, bound up with a number of other questions: What does a justification consist of? and, more especially: Is it possible to justify our theories or beliefs rationally: that is to say, by giving reasons—'positive reasons' (as I shall call them), such as an appeal to observation; reasons, that is, for holding them to be true, or to be at least 'probable' (in the sense of the probability calculus)? Clearly there is an unstated, and apparently innocuous, assumption which sponsors the transition from the first to the second question: namely, that one adjudicates among competing claims by determining which of them can be justified by positive reasons, and which cannot.

Although both popular and highly intuitive, I nevertheless contend that this justificationist response to the demarcation-cum-rationality problem has serious shortcomings. To put it as simply as possible, *it doesn't work.*

To be slightly more specific, *justification, in the epistemological sense, is logically impossible.*[6] This allegation is almost as old as justificationism itself; it is the central thesis of epistemological scepticism. In particular, it is the inevitable outcome of the trilemma of justification associated with the ancient Greek Pyrrhonnian Agrippa, *viz.,* the trilemma of:

a) infinite regress, or
b) vicious circle, or
c) the dogmatic (unjustified) stopping of the justification process.

This trilemma, which has also been referred to by Hans Albert (1985) as the *Münchhausen trilemma*, exposes a ruinous deficiency in the logical structure of any justificatory argument. As Duncan Pritchard (2009, p. 33) has stated, "[a]ll of these alternatives are unpalatable since they all seem to imply that we aren't really justified in holding our original belief." Quite so; no justificatory argument, either demonstrative or probabilistic, can achieve its aim. Justification, in the epistemological sense, is impossible. Such sceptical arguments completely scupper the justificationist project, as Robert Fogelin has noted (1994, p. 113 & p. 193):

> In the Pyrrhonist's hands, these [Agrippa's] modes are used (either singly or in concert) to show that any effort at justifying philosophical beliefs is bound to fail... no justificatory program seems to show any prospect of solving the Agrippa problem. The strength of each position seems to be wholly exhausted in the weaknesses of its competitors.

However, in addition to justification being *unattainable*, it is also, if less than conclusive, *useless* for purposes of objective adjudication between theories. This point, that *partial* justification is of no avail in theory choice, has been emphasised, in particular, by Miller (1994, Chapter 3; 1996). Thus, even *if* the sought for *inconclusive* reasons could be produced, they could serve no role in the search for truth. Fallibilist justificationism, even if possible, would be ineffectual for purposes of demarcation. While some prominent theorists of scientific knowledge have recognised the logical unassailability of such sceptical arguments, they have invariably retained the justificationist theory of demarcation-cum-rationality. It is the conjunction of these doctrines which is the main intellectual source of modern epistemological relativism—the thesis "that the choice between competing theories is arbitrary; since either, there is no such thing as objective truth; or, if there is, no such thing as a theory which is true or at any rate (though perhaps not true) nearer to the truth than another theory; or, if there are two or more theories, no ways or means of deciding whether one of them is better than another" (Popper, 1945, Vol. II, Addenda I).

Yet such a drastic reaction is not logically necessary. What has generally been overlooked by justificationist theorists of knowledge is that a solution to the demarcation problem is possible while eschewing justification, either complete or partial, altogether. Justification, on this second approach, is not *necessary* either for objective demarcation or rationality. This approach is essentially an expansion upon Popper's classical falsificationist solution to the demarcation problem, first elaborated in detail in *Logik der Forschung* (1934), and later generalised, from empirical science to the entirety of human knowledge, in the epistemology he named *critical rationalism* (introduced in the second volume of *The Open Society and its Enemies*, 1945).

Chapter Outline

This dissertation will be split into two unequal parts. The major theme of the majority of this work will be to document the failure of justificationism, with particular emphasis on the unsuccessful attempts to provide a justification for the theoretical content of empirical science. This will be followed by a sketch of the critical rationalist theory of demarcation-cum-rationality, with an emphasis on its advances in relation to justificationism. In particular, a survey of popular objections to this solution will be considered and rebutted.

The first three chapters expand upon the assertion that the demarcation problem occupies a central place in the history of epistemology. In chapter 1 I will introduce the demarcation problem, as addressed by Popper, and correct some misinterpretations that are common in the secondary literature. In Chapter 2 I will examine how justificationism as an epistemological position arose as an attempt to solve this problem, as well as explain how justificationism has functioned both as a purported solution to the demarcation problem, and as a theory of rationality more generally. In Chapter 3 I will introduce the traditional formal sceptical arguments against the logical possibility of justification, and survey some widely accepted, yet invalid, justificationist responses.

Moving more specifically to demarcation in empirical science, in chapter 4 I will examine Hume's problem of induction, historically the most famous stumbling block for the justificationist demarcational project. As Hume observed, since the natural laws of science are characteristically unrestrictedly universal statements, they cannot be derived from a finite number of observation statements, however large. They thus unavoidably transcend any feasible evidential basis—no amount of observational data can be sufficient to entail the truth of the universal hypotheses in question. In this chapter I will begin by looking at the classical exposition of the problem in Hume, before reformulating the problem in terms of *logical strength.* The major justificationist responses, in particular those that appeal to an inductive principle, will be examined. My conclusion to this chapter will be, in agreement with Hume and Popper, that there is no rational method to *certify* the truth of a hypothesis; there is no method of justifying an inductive principle. Hence the classical inductivist or Baconian attempt to solve the demarcation problem—what might be called "strong justificationism"—fails.

In chapter 5 I will examine responses to Hume which appeal to the probability calculus—no doubt the dominant justificationist strategy since the early 20th century. The impossibility of *conclusive* justification of universal scientific hypotheses is accepted on this account. However, it is maintained that scientific theories remain open to *partial* justification, or *confirmation* by empirical evidence; they can be rendered plausible, or reasonable, or probable (in the sense of the probability calculus). Such probabilist answers to Hume will be rejected. Drawing on various arguments by Popper and subsequent critical rationalists, and indeed by Hume himself, my conclusion will be that the resort to probability, as opposed to conclusive proof, is of no avail in response to the demarcation problem. Elaborating especially on the Popper and Miller theorem (published in *Nature,* 1983), I will argue against the possibility of any kind of ampliative inference- that is to say, so-called ampliative inferences are, in actuality, logically on par with guesses.

In Chapter 6, concluding the critique of justificationism, I will examine the relativistic consequences of the failure of the justificationist demarcation program, specifically in the philosophy of science. As a paradigm case of those theorists who have, recognising the unavailability of any objective justification, advanced dogmatic and relativistic theories of knowledge, I will scrutinise the theory of science proposed by Thomas Kuhn in his massively influential *The Structure of Scientific Revolutions* (1962). The general strategy employed in the *Structure* is exemplary of a much larger faction of disappointed justificationists—since our knowledge cannot be objectively justified, it must be justified relative to some arbitrary ultimate presuppositions, or paradigm, or form of life. Such approaches to science abandon both the demarcation problem, and rationality in general. This protean doctrine may perhaps best be labelled "epistemic relativism": "Epistemic relativists... maintain that not only what counts as a true or false belief may be relative, but more significantly, what counts as acceptable justification can and does vary from culture to culture and there is no neutral method or criterion for adjudicating between different justificatory schemes. Thus, relativism about both truth and rationality could be seen as variants of epistemic relativism" (Baghramian, 2004, p. 138).

Is there an alternative? In the final chapter of the dissertation I will propose that Popper's non-justificationist theory of demarcation is fit for the task of rationally adjudicating between competing theories. On this critical rationalist account, theories are demarcated not *epistemically,* or in terms of evidential support, but rather *realistically*—that is, in terms of what they say about the world, or in terms of how well they correspond to reality. Any successes in this regard remain, in perpetuity, conjectural. This is not to say, however, that they are not *actual*. For, although one can always deny that any theoretical progress, in terms of scientific realism or approximate truth, has been made, such a criticism will have little force if the charge is motivated *solely* by the fact that the theory in question is unjustified, for this property will equally apply to any theory which the critic advances in its place (antirealism, for instance). In other words, the charge of being unjustified loses its force since it applies trivially to all knowledge claims—more substantial criticism is needed in order to pose a serious threat to the realist outlook.

This critical rationalist response to the demarcation problem thus combines three central elements:

a) the *correspondence theory of truth* as an essential regulative ideal for epistemology and demarcation;

b) *scepticism* (or anti-justificationism): the doctrine that no part of human knowledge can achieve any positive degree of justification, and,

c) *radical deductivism*: the rejection of the assertion that there exists anything like inductive confirmation in the process of rational inquiry. The prospect of any type of inductive logic or ampliative inference is rejected as both *unfeasible,* and *unnecessary* for demarcation. Rational demarcation can proceed *entirely* by way of the *deductive testing* of hypotheses—most especially, by way of *empirical* (that is, *observational*) tests.[7]

In particular, I will respond in this chapter to critics who maintain that critical rationalism either a) covertly reinstates induction in theory selection (Salmon, 1966, 1968b; Good, 1975; Putnam, 1974; O'Hear, 1980; Bird, 1998; McGinn, 2002; Velupillai, 2008), or b) that it is indistinguishable from irrationalism (Lakatos 1974; O'Hear 1980; Newton-Smith, 1981; Stove, 1982). Only if these charges can be answered is it possible to consider the critical rationalist response to the demarcation problem an advance over the justificationist and relativist alternatives.

The picture we are left with, if the preceding analysis is correct, is one where rational theory adjudication is possible, and where the rationality of science is protected both from relativistic assaults and from the anti-theoretical and dogmatic tendencies of justificationism. Scepticism (understood as a purely epistemological thesis) is true—all human knowledge is conjectural, and no justificatory argument is possible which is not circular or merely authoritarian. Yet far from being the case that, to use Paul Feyerabend's phrase, "anything goes",[8] it is essential to rationality that knowledge claims be unsparingly subject to objective deductive error elimination. Reason and rationality are retained in this picture, but epistemological authorities and relativism discarded. Such a theory, perhaps, may make possible a rapprochement between those who rightly regard science as among mankind's greatest achievements, and those others who have correctly recognised the logical untenability of any claims to its justification.

---

[1] I use this latter label to emphasise that there is a distinction between "the demarcation problem" as understood here, and other, usually more specialised, senses of the term employed in the philosophical literature. Partly for reasons of brevity, but also to stress the generality of this particular demarcation problem, in later chapters I will use the two labels—demarcation problem and demarcation-cum-

rationality problem—interchangeably.

[2] That is, those sciences, such as physics, geology or microeconomics, that propose universal law statements, whether classically "deterministic" or statistical.

[3] To be clear, the word "justify" is not indispensable here, and various other words have been used to convey broadly the same idea: verify, probabilify, confirm, vindicate, prove, provide warrant for, and so forth.

[4] This modified version of justification is maintained by Alan Musgrave. See, for example, his (2004).

[5] This focus on *true belief with certification* can be found in Plato's *Meno,* where Socrates asserts that "the property distinctively possessed by knowledge is that of being 'tied-down' to the truth, like the mythical tethered statues of Daedalus which were so life-like that they were tied to the ground to ensure that they did not run away" (Pritchard and Turri, 2012, § 1).

[6] I am referring here to justification understood primarily in the *internalist* sense, as *externalist* theories, in my view, *do not even begin to address the problem of theory adjudication*. This distinction is expanded upon further in § 1.5 below.

[7] In this thesis, classical logic is *assumed*. The basic assumptions of this logic (most importantly the law of non-contradiction) are also regarded as *unjustifiable*—they too are subject to the trilemma of justification (see section 3.4 below). However, the crucial advantage of classical logic over so-called inductive logic, and the reason for its adoption here, is its property of truth preservation (and its corollary, *falsity re-transmission*), which allows stringent intersubjective criticism. It is for this reason that Popper dubbed classical logic *"the organon of criticism"* (1963, p. 64). Non-classical logics, of the kind currently championed by such logicians as Graham Priest, will not be considered further here (but for a relevant and insightful discussion of the status of the logic assumed in falsificationism, see Miller (2011a, § 4)).

[8] From his *Against Method: Outline of an Anarchist Theory of Knowledge* (1975a).

Chapter One: The Demarcation Problem

1.0 Introduction

The demarcation problem is the most fundamental problem in epistemology.

This opening thesis will be immediately recognisable to readers of Popper, especially those familiar with his earlier work. For example, in *Die beiden Grundprobleme der Erkenntnistheorie*[1] (*The Two Fundamental Problems of the Theory of Knowledge*), Popper (2009, p. 4, but written between 1930-1932) asserts:

> The problem of demarcation deserves our primary interest. It is by no means of only theoretical-philosophical significance. Rather, it is of the greatest relevance for the separate sciences, particularly for the research practices of the less highly developed ones. But even from a philosophical-epistemological point of view, it proves to be the central problem to which probably all other questions of the theory of knowledge, including the problem of induction, can be reduced.

As alluded to in this passage, the two fundamental problems of Popper's title are the problem of induction and the problem of the demarcation between "the empirical sciences on the one hand, and mathematics and logic as well as "metaphysical" systems on the other" (1959, p. 34). Popper again reaffirms the primacy of this latter problem in *Conjectures and Refutations* (1963, p. 42), describing it as "the key to most of the fundamental problems of the philosophy of science."

However, it must be admitted that these statements are in marked contrast to much of mainstream philosophical opinion, despite the fact that Popper's criterion of falsifiability is often championed by working scientists.[2] For while the problem of induction is, *pace* Popper, still commonly regarded as the central question in the philosophy of science, the demarcation problem has been repudiated by many of the leading practitioners in the field. The noted epistemologist and philosopher of logic Susan Haack (2009, § 3), to take one example, has opined that the demarcation problem looms much "larger than it should", and that "you can hardly avoid the conclusion that the apparently simple idea [Popper] started with has become something of an intellectual monster." Colin McGinn (2002, p. 50), the British philosopher of mind, has brushed the problem aside as merely verbal. Larry Laudan, one of the most influential contemporary philosophers of science, has also dismissed the problem as superfluous—the title of his widely republished essay, "The Demise of the Demarcation Problem" (1983), is indicative. Much the same can be said regarding Adolf Grünbaum's "The Degeneration of Popper's Theory of Demarcation" (1989). W. H. Newton-Smith (1992, p. 61) offers a similar negative opinion: he states that "[t]he project of developing a normative criterion of demarcation no longer commands great interest."

Despite these pronouncements, Popper's claim about the centrality of the demarcation problem is, I think, correct. Indeed, it is my contention that Popper's contribution to the theory of knowledge has been widely misunderstood within academic philosophy largely due to a misapprehension of the problem situation his theory was proposed to solve. However, I should say at once that even some of Popper's most famous statements of the problem are not ideal. The *demarcation-cum-rationality* problem,[3] as I understand it, is wider than how it is often characterised. How then is the demarcation problem to be understood? And why is it important? It will perhaps be easier to begin by stating how the demarcation problem is *not* to be understood.

1.1 How Not to Understand the Demarcation Problem

There are at least five popular misunderstandings in the secondary literature of what function Popper intended his demarcation criteria to serve. We may label these a) the logical positivist reading, b) the essentialist reading, c) the naturalistic reading, d) the justificationist reading and e) the anti-Freudian/Marxist reading.[4]

1.11 The Logical Positivist reading

According to this reading, Popper's falsifiability criterion, like the verification principle of his logical positivist contemporaries in the Vienna Circle, was intended as a criterion of *meaningfulness* or of "cognitive significance". Popper, in the Schlipp volume (Library of Living Philosophers), dubbed this the "Popper Legend", according to which "Popper was also in favour of a criterion of meaningfulness or literal significance, in order to exclude metaphysics as meaningless" (1974b, p. 963). However, Popper (ibid, pp. 963-4) had on numerous occasions dissented forcefully from this characteristic programme of the logical positivists:

> I always opposed those who declared that all metaphysics is meaningless pseudotalk, and I especially opposed the attempts of the Vienna Circle positivists who tried to back their views by developing a criterion of meaningfulness or literal significance... the meaningfulness of some metaphysical ideas (such as realism or atomism) can be shown by their historical influence on the growth of scientific theories... this whole enterprise was an attempt to solve a pseudoproblem (an attempt to kill rather than to recognize metaphysics), and that this pseudoproblem had usurped the logical place belonging to a serious and real problem...

Perhaps the most influential example of this confusion can be found in A.J. Ayer's *Language, Truth and Logic* (1936); an error which, unfortunately, remained through several reprints of Ayer's highly successful book. This error also appears in other reputable sources, such as Jorgen Jorgensen's "The Development of Logical Empiricism" in the *International Encyclopedia of Unified Science*[5] (1951). Also no doubt aiding this misinterpretation was the fact that Carnap, in his "Testability and Meaning" (1936) adopted testability, as a criterion of "cognitive meaningfulness", with specific reference to Popper. Popper, for his part, attributed this repeated distortion to the inability of the Vienna Circle to take seriously his defence of metaphysics (1974b, pp. 970-1), hostility to which was characteristic of their program.

This reading seems to be less common today than previously. However, it still turns up occasionally: Popper received a somewhat dubious recommendation from Stephen Hawking in his *The Universe in a Nutshell* (2001, p. 31), in which Hawking describes his own 'positivist' philosophy as an "approach put forward by Karl Popper and others." Since Popper clearly distanced himself from this project in his earliest publications (see his letter to the editors of *Erkenntnis* in *Erkenntnis,* 3 (1933), republished as Appendix *i in English in his (1959)), and continued to do so in numerous subsequent books (1963, Chapter 11), this notion need not detain us. Suffice to say, Popper's demarcation problem was *not* one of meaning, one designed to "eliminate metaphysics", nor one which identified the empirical with the verifiable.

1.12 Essentialist Misreadings

According to this reading, the demarcation problem is that of explicating the *definition* of the term "science" or of otherwise delineating the "essence" of science.[6] Okasha (2002, p. 16), to take one example, explicitly derides Popper for his supposed assumption that science has an "essential nature." Such a construal may possibly explain why the problem has been dismissed by Colin McGinn as entirely verbal (2002, p. 50), and by Susan Haack as "a distraction" (2005, § 69). Definitions are, after all, only abbreviations, and are hence theoretically dispensable—they are "mere typographical conveniences", to quote Whitehead and Russell in *Principia Mathematica* ([1910] 1925, p.11). Certainly, it is difficult to see how *this* problem could constitute one of the most fundamental of epistemology.

Yet this interpretation is obviously a distortion in regards to Popper's intentions; he was, after all, a strident opponent of philosophical essentialism (he even claimed to have invented the term while working on *The Poverty to Historicism* (1957)). Moreover, one of the central motifs throughout Popper's writings is his rejection of meaning analysis and his stress on the unimportance of definitions (see his 'A Long Digression Concerning Essentialism' in *Unended Quest* (1974a) and also the discussion of definitions in *The Open Society,* Book II, Chapter 11). The notion that we must first define "science" before we can say anything meaningful about it is entirely foreign to Popper's way of thinking; such programmes lead, in his view, to endless wrangling over preliminaries, with the frequent result that the problem at hand remains untouched. It is safe to say, therefore, that Popper's aim was not merely classificatory or definitional. Indeed, Popper was at pains to avoid this misunderstanding. In *Realism and The Aim of Science* (1983, p. 159), he writes:

> From the formulation given, it is hardly possible to gauge its significance. At first sight, it may even look more like a pedant's question than like a problem of real interest. For what is in a name, or in a distinction, or in a classification, or in a demarcation? If we are anxious to know, if it is our aim to learn about the world, we do not care much for the compartments or departments to which our prospective knowledge may have to be assigned... subject matters and other divisions of learning are fictitious and badly misleading, convenient though they may be as administrative units...

And later, in the Schlipp volume (1974b, p. 981), Popper wrote:

> ...[i]f I define "science" by my criterion of demarcation (I admit that this is more or less what I am doing) then anybody could propose another definition, such as "science is the sum total of true statements". A discussion of the merits of such definitions can be pretty pointless.

This reading, that the demarcation problem is concerned to produce the essential definition of science, may thus also be disregarded.

1.13 Naturalist Misreadings

Another reading, which can be found in Laudan (1983, p. 122), is that the demarcation problem was intended to "explicate the paradigmatic usages of the term 'scientific'". However, it is clear from Popper's discussion of the problem that his theory is designed to be neither sociological nor historical, but genuinely *normative*—that is to say, philosophical, methodological and logical, rather than itself empirical. In both *The Logic of Scientific Discovery* (§ 10)[7] and *Die beiden Grundprobleme der Erkenntnistheorie* (2009, pp. 433-434), Popper distanced himself from this naturalistic project, stressing that his criterion was not meant to empirically *describe* those theories which are called scientific, as it is often supposed.

Of course, there is a place for such sociological studies of science; as David Miller (2010, slide 8) points out in reference to Laudan's suggestion: "[f]or this naturalistic purpose, a description of the institutional working of science, such as Kuhn's, is doubtless to be preferred." Yet a sociological, psychological, or causal account of theory choice is unable to function as a substantive theory of *rational* demarcation. As Wesley Salmon has correctly noted (1966, p. 6), "[t]he fact that people do or do not use a certain type of inference is irrelevant to its justifiability. Whether people have confidence in the correctness of a certain type of inference has nothing to do with whether such confidence is justified. If we should adopt a logically incorrect method for inferring one fact from others, these facts would not actually constitute evidence for the conclusion we have drawn."

A somewhat similar criticism of naturalised epistemology was made by Jaegwon Kim (1988, p. 390) in his discussion of Quine's proposal to replace traditional normative epistemology with cognitive psychology (1969, p. 75). (Kim's suggestion to return to justification philosophy, is not, however, an advance relative to Quine's position). In other words, the demarcation problem is fundamentally *normative*—its solution calls for a *prescription*, not a mere empirical *description* of the usages of the word "science." (A corollary of this point is that it is not a valid criticism of Popper's methodology to point out examples of particular scientists having deviated from it. Indeed the reverse is true, as A.C. Grayling acknowledges in his *Ideas That Matter* (2008); there he singles out for praise "Popper's belief that non-scientists can legitimately criticise science for failing to abide by its own avowed standards.")

1.14 Justificationist Misreadings

Perhaps the most pertinent misreading of the demarcation problem for our purposes is the justificationist misreading. This reading is also quite apparent in Laudan, (1983, p. 118) where he characterises the problem as that of distinguishing "scientific and non-scientific matters in a way which exhibits a surer epistemic warrant or evidential ground for science than for non-science." Moreover, he cites this as an essential condition for "a philosophically significant demarcation." That this project cannot be attributed to Popper is made manifest by Popper's repeated and explicit rejection of "sure epistemic warrants" and "evidential grounds." Since Popper did not believe such things existed the point of a demarcation criterion, at least as Popper understood it, must be something else.

1.15 Partisan Readings

A final reading that must be rejected is that Popper's primary interest in proposing his demarcation criteria was to disqualify Freudian psychoanalysis and/or Marx's theory of "scientific socialism." This reading seems to be the view of Grünbaum in his "The Degeneration of Popper's Theory of Demarcation" (1989). There Grünbaum correctly recognises that Popper's aim was not to find a "criterion of evidential support", a project Popper repudiated. However, Grünbaum mistakenly assumes that Popper was *primarily* concerned to place Freudian psychology and Marxist historical materialism outside empirical science, much in the same way that the positivists sought to "eliminate" metaphysics.

There is, of course, some truth in this reading; after all, Popper *was* concerned to classify both these theories as nonempirical (1963, p. 34). But it is necessary to stress that Popper's demarcation criterion is not primarily preoccupied with the status of any *particular* theory. Thus, we may grant that Grünbaum is correct in his assertion that Freud's theory is falsifiable after all. Indeed, Popper had stated as much explicitly, citing an example by Bartley (1983, p. 169). Yet this fact would not, on that account, undermine "[the] central tenet of [Popper's] whole philosophy" (ibid, p. 155), as Grünbaum claims. (Indeed Freud's theory would remain suspect from a falsificationist perspective, due to its low *degree* of falsifiability). This point, that the correct determination of the empirical status of psychoanalysis was *not* of central importance, was made by Popper himself in his discussion of it in *Realism and The Aim of Science*. There he wrote (1983, p. 174), "it hardly matters whether or not I am right concerning the irrefutability of any of these three theories [those of Freud, Adler, and Marx]: here they serve merely as examples, as illustrations."

1.2 Popper's Problem: The Role of Experience in Rational Demarcation

How then *is* Popper's demarcation problem to be understood? His most revealing formulation is perhaps that be found in *Realism and The Aim of Science* (1983, p. 174). There he writes:

> ... my 'problem of demarcation' was from the beginning the practical problem of assessing theories, and of judging their claims. It certainly was not a problem of classifying or

> distinguishing some subject matters called 'science' and 'metaphysics'. It was, rather, an urgent practical problem: under what conditions is a critical appeal to experience possible—one that could bear some fruit?

This account makes it clear how far the problem Popper faced was from being one of defining science. Instead, Popper's problem was that of examining critically the role that experience plays in theory choice—of examining, that is, what empirical reports can *do epistemologically*, and what role they play in scientific investigation. In particular, it is the general problem of rational *adjudication* of competing theories, applied specifically to empirical science.

This problem has been called, by Jerzy Giedymin (Lakatos & Musgrave, 1970, p. 76), the *"original problem of empiricism...* the pragmatical problem of finding *criteria of empirical rationality* which might be used to criticise extravagant claims to knowledge based on mystical experiences, intuition, divine inspiration etc." Formulating the problem more colloquially, David Miller (2011a, p. 6) has described Popper's task as being that "of explaining how experience, or better, experiential reports, have a bearing on our knowledge; if you like, how we 'learn from experience'." The problem is thus central to both empiricist epistemology and to scientific methodology, which is, of course, taken to be fundamentally an empirical enterprise. As such, a solution to it is essential for any adequate theory of knowledge.

This problem, of what can we hope to gain from an empirical investigation, was earlier alluded to in § 10 of *The Logic of Scientific Discovery*. There Popper asserts (1959, p. 30), making plain his dissatisfaction with the justificationist response of traditional empiricism, "the main problem of philosophy is the critical analysis of the appeal to the authority of 'experience'—precisely that 'experience' which every latest discoverer of positivism is, as ever, artlessly taking for granted." His solution to this problem, which will be elaborated upon and defended in the second part of this dissertation, is simple: experiential reports can only bear *negatively* on the theoretical and universalistic theories of science. This view Popper summarised in *The Logic of Scientific Discovery*, § 1: *"a hypothesis can only be empirically tested—and only after it has been advanced."* This, essentially logical, point has far-ranging methodological consequences. It also has, as I will try to show in later chapters, a profoundly negative impact on traditional epistemological theories.

Admittedly, this is not how the demarcation problem is usually understood. A more common characterisation of the problem is that of finding a dividing line between science and *pseudoscience*, or sometimes, metaphysics. Peter Achinstein, for example, in his article on the "Demarcation Problem" (1998, § 1) in the *Routledge Encyclopaedia of Philosophy,* writes "[t]he problem of demarcation is to distinguish *science* from nonscientific disciplines", and in particular, from "metaphysics and pseudoscience". Indeed, Popper often emphasised (1963, p. 33; 1974b, p. 976) the usefulness of his falsifiability criterion in distinguishing between empirical science and pseudoscience. However, despite these formulations, the falsifiability criterion functions not *primarily* to demarcate science from non-science, or science from pseudoscience, or science from metaphysics, but rather, more accurately, to demarcate the *empirical* from the *non-empirical*. This point is important: the realm of the empirical is not co-extensive with "science" as usually understood. For example, as Joseph Agassi[8] (2006, p. 14) has pointed out, market research is certainly empirical in this sense, yet should not be regarded as bona fide "science". Miller (2006, Chapter 5, § 3), amongst many others, (Hatfield, 1948; Agassi, 1966b, 1985; Basalla, 1988; and Vincenti, 1990) also points to technology as a sphere which is indubitably empirical, yet is not to be identified with theoretical or basic science.

Hence, Popper's theory of falsifiability does *not* provide necessary and sufficient conditions for what is to count as *scientific*, but instead as to what can usefully be considered *empirical*. Yet the empirical status of a theory is significant—the criterion *is* of use in identifying empirical science, but only *negatively*—it alerts us to the fact that some theories are beyond empirical test. Popper's criterion thus provides a principled way of demarcating bona fide from counterfeit science. It is thus not merely of theoretical interest, but has far-reaching practical implications. It tells us what theories are non-empirical, and hence *not* part of empirical science.[9]

By the same token, the falsifiability criterion does not distinguish between true and false theories, but rather only between empirical and non-empirical ones. Given my more general characterisation of the demarcation problem, there is a lacuna here. As stated earlier, metaphysics, for Popper, in opposition to the Vienna Circle, was not meaningless or otherwise taboo. On the contrary, Popper argued that metaphysical speculation has historically been indispensable to theoretical science, and, indeed, remains so. In *Realism and The Aim of Science* (1983, pp. 159-160), for example, Popper asserted:

> Science has at all times been profoundly influenced by metaphysical ideas; certain metaphysical ideas and problems (such as the problem of change, or the Cartesian programme of explaining all change by action at vanishing distances) have dominated the development of science for centuries, as regulative ideas; while others (such as atomism, another attempt to solve the problem of change) have by degrees turned into scientific theories.

Such metaphysical theories undoubtedly make truth-evaluable assertions about the world. How are competing metaphysical theories to be demarcated? This question leads us to the demarcation-cum-rationality problem in its more general sense—that is, the problem of rationally adjudicating between competing descriptive theories, *whatever* their empirical status. *Must all criticism be empirical?*

1.3 Bartley's Generalisation of the Demarcation Problem

As stated in the foreword, the most fundamental demarcation problem is the problem of adjudicating between competing hypotheses with reference to their *truth or falsity*, understood as correspondence (or lack thereof) to reality. Whether these hypotheses are to be classed as empirical-scientific or metaphysical is not of *immediate* concern (although, of course, the question of their empirical arguability is indeed an important desiderata in theory choice). The primary problem, however, is simply that of separating true from false theories in a rational manner.

The problem arises from the following, relatively uncontroversial, propositions, which describe the fundamental problem situation in the theory of knowledge:

1) There exist mutually exclusive competing explanatory theories and hypotheses.

2) These mutually exclusive theories and hypotheses compete precisely because they purport to be about the same aspects of the world; they are offered as a response to a shared problem situation. (This may be considered roughly a formulation of common-sense realism).

This generalisation of the demarcation problem is most strongly associated with the work of W.W. Bartley, although it is also quite explicit in at least *some* of the formulations given by Popper. For example, as quoted earlier, in *Realism and the Aim of Science,* Popper writes (1983, p. 19):

> The central problem of the philosophy of knowledge, at least since the Reformation, has been this. How can we adjudicate or evaluate the far-reaching claims of competing theories and beliefs?

Moreover, it could be argued that this generalisation of Popper's problem of demarcation *beyond* the specific realm of empirical science was always implicit in his epistemological writings, even those focussed exclusively on scientific method. For even in *The Logic of Scientific Discovery* we find the claims, "scientific knowledge is merely a development of ordinary knowledge or common-sense knowledge" (1959, p. 18), and "scientific knowledge can be more easily studied than common-sense knowledge. For it is *common-sense knowledge writ large*" (1959, pp. xxv-xxvi).

Nevertheless, Popper was not always consistent on this point, as Bartley ([1962] 1984) and Popper himself (1972) have indicated. As Bartley recounts, in "his early, but not later, writings, Popper... implicitly tends to identify the demarcation between science and nonscience with the demarcation between good and bad... this... implicit identification between testable and good theory will not do, as Popper himself has long since recognized" ([1962] 1984, p. 205). Likewise, Popper in *Objective Knowledge* (1972, p. 40) states about his earliest writings: "In those days I identified wrongly the limits of science with those of arguability. I later changed my mind and argued that non-testable (i.e., irrefutable) metaphysical theories may be rationally arguable."

This point, regarding rational arguability, is of cardinal importance in assessing the merit of the demarcation project. Again, it is *not* the problem of giving a satisfactory *definition* of science. Rather, it is the problem of *adjudicating* between competing theories. Understood as the problem of rational adjudication between theories, the demarcation problem indeed applies to *all* knowledge claims, not just those that are purportedly empirical or scientific. Thus it cannot be equated with the narrower problem of demarcating empirical-scientific theories from non-empirical theories (and certainly not with the positivist project of demarcating meaningful from meaningless utterances).

The key to a generalisation of Popper's falsificationist *methodology* is just that—to bear in mind that it is *a methodology,* and not merely a device to *classify* hypotheses as empirical.[10] And as a methodology, its key features are that it is entirely *critical* and *selective*. As it happens, perhaps the most important means we have at our disposal in scientific investigation is the possibility of exposing our theories to empirical criticism. It is *only* such theories which can be so criticised that we may usefully call "empirical". This, in essentials, is Popper's falsifiability criterion between empirical-scientific theories and those which are immune from empirical criticism. This demarcation criterion for the *empirical character* of a theory is, in my opinion, still valid, but it must be seen as applicable to only a *special* case (albeit still the *paradigmatic* case—that is, empirical science) of human knowledge.

Yet not *all* criticism need be *empirical* criticism. The truth value of metaphysical theories, despite being non-empirical, may still be rationally *arguable*—that is, they may still be critically discussed in an objective (or better, *intersubjective*)[11] manner, not only with reference to their objective problem-solving ability or explanatory power, but also in regards to their *consistency* (both internally, and in relation to other accepted theories). No claim is made that such properties are truth conducive; instead, greater explanatory power, ceteris paribus, is simply proposed as a desirable trait in explanatory theories (this proposal is itself open to criticism). Likewise, mere consistency is not regarded as truth conducive; *inconsistency,* however, is not to be tolerated.[12]

This problem—of the arguability of metaphysics—is an interesting one, and was addressed by Popper in his "On the Status of Science and Metaphysics", first published in 1958 and now in *Conjectures and Refutations*, 1963), as well as in Popper's "Metaphysical Epilogue" in *Quantum Theory and the Schism in Physics*, where Popper discusses 'metaphysical research programs' and their vital importance in the development of empirical science. There he writes (1982b, pp. 200-211):

> But is it possible rationally to appraise or evaluate an irrefutable theory? What is the point of criticizing a theory rationally if we know from the start that it is neither refutable by pure reason, nor testable by experience?

> My answer is this. If a metaphysical theory is a more or less isolated assertion, no more than the product of an intuition or an insight flung at us with an implied 'take it or leave it', then it may well be impossible to discuss it rationally. But the same would be true of a 'scientific' theory... any rational theory, no matter whether scientific or metaphysical, is rational only because it ties up with something else—because it is an attempt to solve certain problems; and it can be rationally discussed only in relation to the problem situation with which it is tied up. Any critical discussion of it will consist, in the main, in considering how well it solves its problems; how much better it does so than various competing theories; whether it does not create greater difficulties than those which it set out to dispel; whether the solution is simple; how fruitful it is in suggesting new problems and new solutions; and whether we cannot, perhaps, refute it by empirical tests.

Metaphysical theories are thus tentative explanatory hypotheses, like those of empirical science, which may, again like those of empirical science, be susceptible to objective rational criticism. It should be clear from this discussion, however, that though the same *principles* apply, the problem of adjudicating between metaphysical theories is more difficult than that of empirical theories. Indeed, some metaphysical theories may not be rationally arguable at all, especially if they are logically weak—that is, deficient in *logical* content, in addition to being devoid of any *empirical* content.[13] The only characteristic difference in their critical discussion, however, is that the possibility of empirical criticism is blocked.

My point in this brief discussion of metaphysics is to make explicit the generality, both of the problem of rational adjudication, and of critical rationalism as a comprehensive theory of knowledge. This remark is necessary due to the fact the mainstream epistemological tradition seems more or less insensitive to Popper's contribution to epistemology proper; critical rationalism rarely appears in the index of introductions and anthologies of the field.[14] Vickers (2006, §4.2), writes that "Popper's epistemology is almost exclusively the epistemology of scientific knowledge", but this view, though commonly held, is erroneous.[15] Critical rationalism is a comprehensive theory of knowledge; applicable not merely to scientific knowledge but to *all* human knowledge. Thus, the problem I am addressing in this thesis is completely general, applying to the rational adjudication of *any* set of competing factual hypotheses, be they empirical or metaphysical. However, although the *problem* of rational demarcation of theoretical knowledge claims is universal, in order not to get lost in generalities my primary focus will be on the empirical knowledge of the natural sciences; the so-called "nomological" sciences rather than metaphysics or technological application.[16] There are three main reasons for this.

First, scientific knowledge is, in my view, by far the most significant and philosophically consequential knowledge we have, and hence the problem of rational demarcation is especially salient in this sphere. Second, given its open, public, and objective nature, it is the variant of knowledge which is most susceptible to *logical* analysis. Third, the question of what can be gained from *empirical* investigation is not just crucial to an adequate theory of science, but to *any* theory of knowledge which calls itself empirical. Thus, the empirical theories of natural science will be the main point of focus in what follows, as they remained for Popper, even when he broadened his philosophical concern to address explicitly metaphysical issues. As Hans Reichenbach remarked in *The Rise of Scientific Philosophy,* I think accurately, "An analysis of natural science is the only path to the central problems of epistemology" (1951, p. 110).

1.4 Demarcation and the Nomological Sciences

Focusing then on the theories of empirical science, here I wish to make explicit some terminological conventions.

The first regards the term "knowledge." In the epistemological tradition "knowledge" has often been used not descriptively, but normatively; and specifically to denote *justified* knowledge, or "justified true belief". That is, knowledge has been used almost exclusively in the Greek sense of *epistêmê,* as opposed to *doxa* (opinion or seeming; roughly, *conjectural* knowledge). In other words, knowledge is used in the epistemological tradition as a "success-word", to use Gilbert Ryle's terminology (1949, p. 130f).

This is especially evident in the protracted twentieth century attempt to analyse the concept of knowledge, which John Greco (2007, p. 172) has called "the project of explanation."[17] This project centred around the so-called tripartite conception of knowledge, and attempts to delineate necessary and sufficient conditions for the correct application of the term. This tripartite account is usually schematised as follows, where "S" is a subject, and "p" stands for an arbitrary proposition:

S knows that *p* iff

1. *p* is true;
2. *S* believes that *p*;
3. *S* is justified in believing that *p*.

This "justified true belief" analysis is defective in more ways than one,[18] and "the project of explanation" has largely been abandoned.[19] However, the assumption that knowledge, and especially scientific knowledge, is necessarily justified remains.

This assumption is uncritical, especially since no philosopher has ever been able to provide a generally accepted theory of justification. Admittedly, there is a pragmatic implication in everyday assertions of knowledge. In this sense "to *know* that p" entails certainty or sureness of p. But this conversational fact, which is not confined to the English language, should not be elevated to a theory of scientific knowledge, especially if it is contradicted by decisive logical arguments. Moreover, there are perfectly acceptable usages of "knowledge" which do not make any epistemological assumptions about justification. Thus, as Watkins (1984, p. 11) has noted "many philosophers treat ['knowledge'] as a success-word, so that one can no more speak of 'erroneous knowledge' than of 'invalid deduction'. But in everyday speech it is often used just to refer to some organised body of learning." So as not to prejudge the issue, I will use this latter sense of the word. In *this* sense, there may be knowledge which is nevertheless conjectural, and even (sometimes) incorrect.

Understood in this latter way, the *reference* of "scientific knowledge", that is, empirical scientific knowledge as an organised body of learning, is, despite some borderline cases, less problematic than its *sense*. To give a few examples of what I mean by scientific knowledge, take, for instance: Archimedes' buoyancy principle, Kepler's laws of planetary motion, Newton's universal law of gravitation and laws of motion, Einstein's theories of special and general relativity, Hubble's law of cosmic expansion, the laws of thermodynamics, Darwin's theory of evolution and natural selection, Wegener's theory of continental drift, the germ theory of disease and the cell theory in biology. As these examples illustrate, the *reference* of "scientific knowledge", at least in a great deal of cases, is uncontroversial—it is its *sense* which is open to debate.

This distinction needs to be made, as one criticism of critical rationalism rests on the equivocation between the two senses of the word "knowledge". David Stove, for instance, in his *Popper and After: Four Modern Irrationalists* (1982, p. 3) makes the claim that it is a "well-known fact... [that]...[m]uch more is known now than was known fifty years ago, and much more was known then than in 1580. So there has been a great accumulation or growth of knowledge in the last four hundred years." Of course, this claim is undoubtedly true in the second, non-justificationist sense of the word "knowledge", as "an organised body of learning", which may or may not entail truth. However, Stove alleges that Popper is inclined to deny the proposition in this *second* sense, and is hence "uncommonly ignorant". Yet Popper's rejection is only of the assertion understood in the first, *epistemic* sense of the word "knowledge"—that is, knowledge as *justified* true belief.

Stove (ibid, p. 14), and others (Haack, 1979) also charge Popper with covertly changing the essential meaning of the word "knowledge", despite the fact, as we have seen, that Popper's usage conforms to at least one of the common dictionary definitions. However this may be, Stove's preferred usage does have a rather unfortunate catch—it clearly disqualifies much of what is quite uncontroversially held to be *scientific* "knowledge". This is especially evident of those scientific theories that are currently considered superseded or refuted—Newtonian mechanics, for instance. Miller makes this point in his (1994, pp. 52-3); Stove and others who complain that the term "knowledge" is being abused have, "...in order to accommodate awkward discoveries... been forced to change its reference instead. This is incontestably the case with regard to the term 'scientific knowledge', whose extension once encompassed theories like Kepler's, Newton's, and Dalton's. Now, as linguistic purists are pleased to wield the term, it refers to nothing at all."

Despite these qualms over the "correct" usage of the term, it is plain that it is better to upset philosophical usage rather than the popular extra-philosophical convention of referring to scientific theories as instances of "knowledge". Thus, with this distinction made, there is no contradiction in *denying* the equation of "scientific knowledge" with "knowledge in the traditional philosophical sense." Indeed, Popper repeatedly denied this equation in *The Logic of Scientific Discovery*: "our science is not knowledge (*epistêmê*)... we do not know; we can only guess" (1959, p. 278); and "the old scientific ideal of *epistêmê*—of absolutely certain, demonstrable knowledge—has proved to be an idol" (ibid, p. 280). Even more pertinently for Stove's criticism, Popper distinguished the two senses of knowledge as follows (ibid, p. 381 fn):

> Empirical knowledge *in some sense* of the word "knowledge", exists. But in other senses—for example in the sense of *certain* knowledge, or of *demonstrable* knowledge—it does not. And we must not assume, uncritically, that we have 'probable' knowledge—knowledge that is probable in the sense of the calculus of probability. It is indeed my contention that we do not have probable knowledge in this sense. For I believe that what we may call 'empirical knowledge', including 'scientific knowledge', consists of guesses, and that many of these guesses are not probable (or have a probability zero) even though they may be very well corroborated.

And later, in *The Open Society and its Enemies,* Popper wrote (1945, Ch. 11):

> ... in science there is no 'knowledge', in the sense in which Plato and Aristotle understood the word, in the sense which implies finality; in science, we never have sufficient reason for the belief that we have attained the truth. What we usually call 'scientific knowledge' is, as a rule, not knowledge in this sense, but rather information regarding the various competing hypotheses and the way in which they have stood up to various tests; it is, using the language of Plato and Aristotle, information concerning the latest, and the best tested, scientific 'opinion'.

As Robert Fogelin (1994, pp. 193-195) has noted, the "Pyrrhonist is not concerned with the everyday use of words like "know," "certain," and "justified," which are not freighted with extreme philosophical commitments. He can use them as freely as anyone... the Pyrrhonist is under no constraint to conform his activities—including his linguistic activities—to philosophical standards."

A second terminological point regards my preferred parlance of empirical *theories*, or *hypotheses*. In the philosophical literature there have been many debates on the nature of 'truth bearers'—that is, those objects which may be classified as either true or false. What exactly fulfils this role? A non-exhaustive list of proposed candidates include: beliefs, propositions, judgments, assertions, statements, theories, remarks, ideas, acts of thought, utterances, sentence tokens, sentence types, and speech acts. It is not my intention to take a position on this debate, if only because there have been so many conflicting usages of each of these terms that to untangle them would involve a lengthy digression which would have little impact on the problem at hand.[20]

Nevertheless, I wish to draw attention to the fact that for the purposes of the demarcation problem, it is the *content* of the theory which is of primary importance. Since Susan Haack (1978b, pp. 75-76) gives a definition of a *"statement"* as "what is said when a declarative sentence is uttered or inscribed", whilst Platts (1979, p. 38), speaks of a *"proposition"* being "the content of a saying", I will accordingly use "theories", "propositions", "hypotheses" and "statements" (and sometimes also "beliefs") *interchangeably*, as bearers of intersubjective informative content, and which are capable of being true or false. Again, the important point is that what is to be demarcated are linguistically formulated knowledge claims, which have some determinate objective (or at the very least intersubjective), public content.

This is not to deny the existence of pre-linguistic or biological knowledge, either in humans or in non-human animals. Nor is it to deny the important continuities between animal and human knowledge. (Indeed, if I am correct, animal knowledge is notably comparable to human knowledge in at least one crucial respect—both are completely epistemically unjustified.) My point here is merely to stress that linguistic knowledge is the only form of knowledge amenable to intersubjective discussion and criticism, as it is the only form of knowledge which can stand in *logical* relations, as opposed to merely *psychological* relations, to other items of knowledge. This externalisable, or public, or exosomatic knowledge is plainly not to be equated with such psychological-subjective concepts as belief-acts.[21] To stress this point again, it is the *content* of beliefs that is of primary importance, as opposed to any subjective qualities. (Indeed, the content of a statement—that is, its logical consequence class—may not even be conscious (Popper 1972, Chapters 3 and 4). Actually, unless we assume logical omniscience, the exhaustive logical consequences of any interesting scientific theory *cannot* be conscious—the logical content of statements generally outruns our subjective or psychological grasp).

Thus, my concern is with the *propositional* knowledge of the nomological sciences, and it is the justificatory status of such propositional knowledge[22] which is the primary subject of sceptical misgivings. In other words, it is the *truth* of such statements that are to be evaluated, not the praiseworthiness or culpability of any subjective act of belief.

1.5 A Note on Externalism

Before continuing, however, a final preliminary point must be addressed: given the common practice of dividing contemporary epistemologies into either internalist or externalist positions, something must be said about this distinction and its relevance for this project. The Blackwell *Companion to Epistemology* (2nd edition), edited by Dancy, Sosa, and Steup, explicates the distinction as follows (2010, p. 364):

> ... a theory of justification is *internalist* if and only if it requires that all of the factors needed for a belief to be epistemically justified for a given person be *cognitively accessible* to that person, *internal* to his cognitive perspective; and *externalist*, if it allows that at least some of the justifying factors need not be thus accessible, so that they can be *external* to the believer's cognitive perspective, beyond his ken.

The authors further divide internalism into two variants, strong and weak, depending upon whether *actual* conscious awareness of justifying reasons is required for justification, or merely the *capacity* to *become* aware of them; that is, that such reasons be "cognitively accessible". In contrast to this, the best known externalist epistemology, reliabilism, is described as follows (ibid, p. 365):

> ... [Its] main requirement for justification is roughly that the belief be produced in a way or via a process that makes it objectively likely that the belief is true... What makes such a view externalist is the absence of any requirement that the person for whom the belief is justified have any sort of cognitive access to the relation of reliability in question. Lacking such access, such a person will in general have no reason for thinking that the belief is true or likely to be true, but will, on such an account, none the less be epistemically justified in accepting it.

What significance does such a position have to the problem of theory choice as described above? In brief, very little. That is, although externalism is often *presented* as a theory of epistemic justification, I do not designate it as such as it fails to provide even a minimal response to the problem of theory adjudication. For, even should there exist reliable faculties (in the sense of ones which tend to produce true beliefs in a large proportion of cases), this by itself would not provide a solution to the problem of theory adjudication as addressed in this work. The reason for this is that the purported reliability of such faculties—the paradigmatic cases generally employed by externalists are the noninferential judgments of perception and memory—are *themselves* open to sceptical doubt. As Darrell Rowbottom, a philosopher sympathetic to critical rationalism, has written regarding the epistemological status of perception (2011, pp. 10-11):

> … why not say that some statements count as items of basic (and perhaps even unassailable) knowledge because of the unintentional processes by which they are classified as true? Why not say that if my true belief that there is a table in front of me is generated by an appropriate causal process, say, then it counts as basic knowledge? *The answer is not so much that we should not say this, but rather that doing so does not appear to solve the present problem satisfactorily…* In short, the objection here is the classic one that the internalist (who thinks that if there is to be any form of justification, we must have access to it) makes against externalism. Imagine we miraculously knew beyond all doubt that either science or Bible studies (but not both) provided a reliable means by which to generate true beliefs (and eliminate false beliefs). Which should we choose? Externalism, at least when understood in the relatively crude fashion outlined earlier, appears to be silent.[23]

In other words, externalism fails to address the same issues as traditional (internalist) justificationism—*especially* the problem of theory adjudication. When a champion of externalism claims that factual propositions can be "justified", but in a way that is inaccessible to any critical evaluation, he effectively abandons the problem of demarcation completely. Clearly, it is only intersubjectively *accessible* considerations which can influence *rational deliberations* upon theory choice. Thus, although it *may* be the case that perception (for instance) is *generally* reliable, we simply cannot have any cognitively *accessible* reason that is immune from sceptical doubts that the faculty of perception is *in fact reliable* (or, moreover, that it will *remain so*, if it is, in fact, currently reliable).

Thus, I am in agreement with Robert Fogelin (1994, p. 65), who notes that externalism has "no force against most forms of traditional philosophical skepticism." This, however, raises the question of what problems externalism *is* designed to solve, if it neither addresses the demarcation problem, nor responds to sceptical criticisms of (traditional) justificationism. I believe there are two major motivations underlying externalism, both of which can be discerned in Alvin Goldman's seminal "A Causal

Theory of Knowing" (1967). The more prominent of these motivations is the desire to address so-called Gettier problems, which are primarily concerned with formulating a satisfactory *definition* of knowledge, rather than any practical problem of theory choice. Secondly, Goldman draws an explicit distinction between his externalist theory and *Cartesian* epistemologies, which, by granting subjective mental contents a privileged epistemic role, are open to radical skeptical scenarios (e.g. the "brain in a vat" thought experiment). Yet, as Fogelin notes (1994, p. 121), "the Agrippa problem... is not the same problem as the Cartesian skeptical problem... [a] theory of epistemic justification, as I understand it, is an attempt to solve the Agrippa problem." This problem, to be treated in detail in section 3.4 below, is a direct consequence of justificationist responses to the problem of theory adjudication—the problem that arises from "the fact of widespread and seemingly endless disagreement regarding issues of fundamental importance" (Thorsrud, 2004, Introduction). Externalism does not provide any solution to this problem, as Rowbottom's example quoted above makes clear: "Imagine we miraculously knew beyond all doubt that either science or Bible studies (but not both) provided a reliable means by which to generate true beliefs (and eliminate false beliefs). Which should we choose? Externalism... appears to be silent" (2011, pp. 10-11).[24] It is precisely such disputes that are central to the demarcation problem as addressed in this work, and which externalism leaves entirely untouched. As Colin Howson has also noted (2000, p. 34), externalism does not "... assist you in telling, *from the information that you have available to you*, that you are on the right track, even if as a matter of fact you are." Indeed, by relinquishing even the pretence that cognisant factors play a role in epistemic justification, externalism seems, if anything, *more* likely to lapse into relativistic dogmatism than internalist theories of justificationism (This tendency of justificationism to degenerate into relativism will be explored further in chapter 6).

To summarise, externalism explicitly *abandons* the problem of rational theory adjudication in so far as it claims that the purported justificatory grounds are not cognitively accessible. If, on the other hand, the externalist *does* engage in a critical discussion (in the case of a theoretical dispute, for example) wherein they claim that some proposition is justified, then their position will be just as susceptible to Agrippa's trilemma as any other variant of justificationism, and hence just as susceptible to the general objections that I will raise against justificationist epistemologies in the following chapters. As a consequence, since internalist variants of justificationism, in contrast to externalist theories, are *explicitly* pertinent to the demarcation problem as defined above, they will be the major focus in what follows.[25]

Having established these conventions, and the scope of the demarcation problem, I will now turn to the historically dominant response to the problem, justificationism, in greater detail.

---

[1] Written between 1930 and 1932, and containing a series of drafts and preliminary work to what would become *Logik der Forschung, Die beiden Grundprobleme der Erkenntnistheorie* remained unpublished until 1979, after several years of work by Troels Eggers Hansen to prepare it for publication. Originally there were two volumes, but only the first, *The Problem of Induction,* could be completely reconstructed. As for the second, *The Problem of Demarcation,* much of the original material had been lost. The title is an allusion to Arthur Schopenhauer's *Die beiden Grundprobleme der Ethik* (*The Two Fundamental Problems of Ethics*).

[2] A notable example is the appraisal of Sir Hermann Bondi, who has stated, "There is no more to science than its method, and there is no more to its method than Popper has said..." (Magee, 1973, p. 2) Similar sentiments have been expressed by various other outstanding scientists: Sir Peter Medawar, a winner of the Nobel Prize for Medicine, Jacques Monod, Sir John Eccles, and, most recently, David Deutsch.

[3] Perhaps the first to state the demarcation-cum-rationality problem explicitly in the sense I use it here was Popper's one-time student W.W. Bartley (See his 1962, 2nd revised edition 1984).

[4] This selection of misreadings is influenced by a discussion in Miller (2011b, § 1), with some modification and augmentation.

[5] Jorgensen writes, "Popper in his *Logik der Forschung*... [proposes]... as the criterion of the meaningfulness of a sentence... not the verifiability but the falsifiability of the sentence." (From the "The Development of Logical Empiricism" in the *International Encyclopedia of Unified Science* (Vol. II, No.9 (1951), p. 72), quoted in Popper, 1974b).

[6] These two tasks are closely related. As G. Aldo Antonelli writes in his article on 'Definition' for the Routledge Encyclopaedia of Philosophy (1998): "Classical theory maintains that a good definition captures the *'real nature'* of what is defined: *'A "definition" is a phrase signifying a thing's essence'* (Aristotle). Historically, philosophers have come to distinguish these *'real'* definitions from *'nominal'* definitions that specify the meaning of a linguistic expression rather than signify the essential nature of an object."

[7] In Section 10 of *The Logic*, for instance, Popper writes:
"This view, according to which methodology is an empirical science in its turn—a study of the actual behaviour of scientists, or of the actual procedure of 'science'—may be described as 'naturalistic'. A

naturalistic methodology... has its value, no doubt. A student of the logic of science may well take an interest in it, and learn from it. But what I call 'methodology' should not be taken for an empirical science. I do not believe that it is possible to decide, by using the methods of an empirical science, such controversial questions as whether science actually uses a principle of induction or not. And my doubts increase when I remember that what is to be called a 'science' and who is to be called a 'scientist' must always remain a matter of convention or decision.

I believe that questions of this kind should be treated in a different way. For example, we may consider and compare two different systems of methodological rules; one with, and one without, a principle of induction. And we may then examine whether such a principle, once introduced, can be applied without giving rise to inconsistencies; whether it helps us; and whether we really need it. It is this type of inquiry which leads me to dispense with the principle of induction: not because such a principle is as a matter of fact never used in science, but because I think that it is not needed; that it does not help us; and that it even gives rise to inconsistencies."

[8] "In my view some empirical research is not science—such as market research—and some science is not empirical—any scientific ethos, egalitarianism, and metaphysics." Agassi (2006, p. 14)

[9] Difficulties in *determining* the falsifiability status of given theories associated with Duhem's thesis will be postponed until chapter 7, section 3 below.

[10] Popper's emphasis on methodology is quite plain in his assertion that "[t]he theory of knowledge, whose task is the analysis of the method or procedure peculiar to empirical science, may accordingly be described as a theory of the empirical method" (Popper 1959, § 5).

[11] Popper, in *The Logic of Scientific Discovery* (1959, p. 22), attributes this conception of objectivity to Kant: "My use of the terms 'objective' and 'subjective' is not unlike Kant's. He uses the word 'objective' to indicate that scientific knowledge should be *justifiable*, independently of anybody's whim: a justification is 'objective' if in principle it can be tested and understood by anybody. Now I hold that scientific theories are never fully justifiable or verifiable, but that they are nevertheless testable. I shall therefore say that the *objectivity* of scientific statements lies in the fact that they can be *inter-subjectively tested."*

This Kantian influence can also be seen in Popper's interesting remarks in *The Two Fundamental Problems of the Theory of Knowledge* about a *Kantian* conception of knowledge, deriving from the "transcendental deduction" whereby knowledge consists of "bringing to light some kind of order", or "discovering a law" (2009, pp. 84-5), suggesting the superiority of this definition over the *justified true belief* account. Such a definition seems fruitful, especially given its neutrality between inductivist and deductivist conceptions of knowledge: "inductivism and deductivism agree on the fact that it is the aim of knowledge to discover law-like regularities, with the help of which we can explain and understand natural events" (2009, p. xxxvi).

[12] Here, as mentioned previously, classical logic is assumed.

[13] Purely existential statements, for example, often face this problem. (See Popper 1959, § 15)

[14] The reason for this lack of engagement is the traditional assumption, disputed by Popper, that human

knowledge is to be equated with some variation of "justified true belief." By making this assumption, these epistemologists beg the question. See section 1.4, below.

[15] Miller alludes to this remark by Vickers in his (2007).

[16] Of course, the nomological sciences are crucial for technological application, but only in a *negative* sense—they rule out logical possibilities as impracticable. This point is made by Popper (1959 § 6), by Miller (2006, Ch.5), and by Albert (1968, § 28—"The foremost function of the nomological sciences, under practical aspects, is to point out limits of realizability.")

[17] Incidentally, the demarcation problem can also be discerned as the implicit motivation of this project of supplying necessary and sufficient conditions for knowledge claims. This can be seen in the consternation caused by so-called Gettier cases. Gettier cases, the first of which was advanced by Edmund Gettier (1963), are generally scenarios in which a subject's belief *does* fulfil the condition of the JTB schema, yet does *not* constitute genuine knowledge, at least not according to the intuitions of many philosophers. Thus, replies to Gettier have attempted to give definitions of knowledge that would better distinguish *legitimate* and *illegitimate* knowledge claims. As Greco states: "... 'the project of explanation' is the project of explaining what knowledge is. Put another way: it is the project of *explaining the difference between knowing and not knowing*." (2007, p. 176 emphasis added)

[18] Miller provocatively claims that "scientific knowledge is usually unjustified untrue unbelief, all that the official view... says that it ought not to be" (1994, p. 54).

[19] As Greco writes, "During the years between 1963 and 1983, over 100 papers were published on the Gettier problem. However, it is fair to say that epistemologists reached no consensus regarding how the problem ought to be solved (Shope 1983). By the end of the twentieth century, discussion of the Gettier problem considerably waned" (2007, pp. 178-9).

[20] Richard Kirkham (1995, p.55) has called the conflicting usages of basic terms in this debate a "conceptual mess."

[21] Something like this distinction, between the logical content of a belief and an agent's psychological act or state of believing it, was previously stressed by Frege, Bolzano, and Plato (see Popper, 1972, p. 73).

[22] I readily admit that there are other forms of knowledge, such as "knowledge by acquaintance" (Russell, 1912), or "knowledge-how" (Ryle, 1949), which may or may not be reducible or expressible in propositional form.

[23] Here, as elsewhere unless indicated, emphasis is in the original.

[24] I am also in agreement with a later passage (2011, pp. 144-5) by Rowbottom regarding externalism: "But why do critical rationalists not turn to externalism? Perhaps this is because the externalist programme appears to be altogether too descriptive to address the kinds of problems with which they are concerned. Critical rationalists are worried about our individual and group dilemmas, when it comes to finding out the truth... So even if they accept that there is such a thing as external justification, they will want to know *how we can identify the means by which to achieve it*. The mindset of the critical

rationalist still has an internalist flavour—the critical rationalist is concerned with what (critical) reasons are *accessible* to him or the group of inquirers that he is a part of—even though internal justification is rejected. The critical rationalist is worried about *how* he should inquire, and what accessible reasons he can (and does) have to prefer information from one source over another (say). So even if he accepts that reliable sources provide justification, he still persists in asking how, exactly, he can work out which sources are reliable (or at least which sources are not reliable).

In short, saying that what differentiates a knower of *p* from a mere believer in *p* is the reliability of the means by which the former came to believe *p* is fine as far as it goes, but it does nothing to help us to inquire more effectively (beyond informing us that we should seek such a reliable means). It does not, in short, tell us which means are reliable although we should dearly like to employ such means, e.g. to arrive at basic statements with which to criticise our theories."

[25] It should be noted in passing, however, that the area in which externalist reliabilism is most plausible —as an account of perception—is also the area in which it has most affinity with falsificationism, which relies predominantly (but not exclusively) on observation statements as a means to test more theoretical statements. Indeed, Rowbottom cites (ibid, p. 22) a passage by Popper which seems to be in broad agreement with this particular tenet of reliabilism (Popper, 1974b, p. 1114):

"Our experiences are not only motives for accepting or rejecting an observational statement, but they may even be described as *inconclusive reasons*. They are reasons because of the generally reliable character of our observations; they are inconclusive because of our fallibility."

In addition, I also regard externalism to be correct in so far as it asserts "that knowledge can be unproblematically ascribed to relatively unsophisticated adults, to young children, and even to higher animals" (Dancy, Sosa, and Steup, 2010, p. 365), but what I believe this commonsensical assertion underlines is the need for "knowledge" to be completely divorced from "justification" (indeed, this is precisely the motivation behind Popper's introduction (1972, chapter 1) of the term "*conjectural* knowledge").

Chapter Two: Justificationism and its Relation to the Demarcation Problem

2.0 Introduction

The orthodox response to the demarcation problem, virtually co-extensive with epistemology itself, is the theory I have called "justificationism". As stated in the foreword, justificationism, or *begrundungsphilosophie* (Radnitzky, 1987), is at once a theory of *demarcation*, a theory of *rationality* and the role of argument, and a theory of the *aim* and *methodology* of the empirical sciences. In this chapter I will elaborate upon the rationale and central assumptions underlying the justificationist approach, as well as make some historical remarks on how justificationism has been applied to each of these overlapping areas.

2.1 Justificationism as a theory of *Demarcation-cum-Rationality*

Justificationism is *primarily* a response to what I have called the demarcation-cum-rationality problem. That is, it assumes that in order to *adjudicate* between competing theories we must be able to *justify*—either conclusively or inconclusively—that theory to which we give preference. As Gerard Radnitzky (1987, p. 282) has stated:

> Within the context of justification philosophy the main problem of epistemology (or, more accurately, what in this context is perceived as the central problem of epistemology) may be

> formulated: "When is it rational or, so to speak, consistent with one's intellectual integrity, to *accept* a particular position"? The formulation suggests the direction in which the answer is to be sought: "When concerned with a statement, a theory, etc., accept those and only those statements, theories, etc., which not only are true but whose truth has been established."

Indeed, the philosophical consensus, at least since Plato,[1] has been that the demarcation problem is to be solved by some justificationist method or other. The common assumption has been that a belief or theory, to be rationally held, must be *positively justified* or *vindicated*. Given the centrality of the demarcation problem, and the ubiquity of this general response, the project of vindication or justification has often been regarded as *co-extensive* with epistemology itself. Yet it should not be thought that justification is prized for its own sake—rather, the central problem in epistemology has been to specify when, and under what conditions, we are justified in holding a proposition to be *true*.

This elemental concern with *truth* is evident in all varieties of traditional justificationism. For instance, Paul K. Moser, a justificationist of the foundationalist variety,[2] writes (1985, p. 4), "[t]o accept a proposition in the absence of good reason is to neglect the cognitive goal of truth. Such acceptance, according to the present normative conception of justification, is epistemically irresponsible." In a similar vein, Laurence BonJour, an (erstwhile) coherentist justificationist, declares, (1985, p. 8) "[t]o accept a belief in the absence of... a [good] reason, however appealing or even mandatory such acceptance might be from some other standpoint, is to neglect the pursuit of truth; such acceptance is, one might say, *epistemically irresponsible*... being epistemically responsible in one's believing, is the core of the notion of epistemic justification."[3] In other words, justification is only of *instrumental* value; it is, ultimately, the pursuit of *truth* which motivates justification philosophy.[4]

The over-arching assumption implicit here is indeed so prevalent that it has been dubbed, by W.W. Bartley ([1962] 1984, pp. 172-3), "*the justificationist metacontext.*" Bartley writes:

> ...most Western philosophies—philosophies of science and epistemologies as much as philosophies of religion—are justificationist. That is, *they sponsor justificationist metacontexts of true belief...* Such philosophies are concerned with how to justify, verify, confirm, make firmer, strengthen, validate, vindicate, make certain, show to be certain, make acceptable, probabilify,

> cause to survive, defend particular contexts and positions.

And again, with reference specifically to the demarcation problem, Bartley writes (ibid, p. 186):

> We live in a world contaminated by a particular philosophical idea about how *any* such demarcation would have to be obtained. I call this "justificationism". In brief, it is the view that the way to criticize an idea is to see whether and how it can be justified. Justificationism deeply permeates all Western culture, and virtually controls all traditional, modern, *and* contemporary philosophy.

This is a sweeping and somewhat audacious claim, but it is, I think, corroborated by a perusal of the history of epistemology. To take an example, we may consider John Greco's essay, "Epistemology", in *The Edinburgh Companion to Twentieth-Century Philosophy* (2007). The author of this expository piece, naturally enough, begins his overview with a discussion of "the perennial questions of epistemology, going back at least as far as Plato's *Theaetetus*" (p. 172). What are these questions? The first question Greco poses is a straightforward (albeit justificationist) variant of the demarcation problem, in the sense given in the previous chapter. Greco asks: "assuming that knowledge is superior to mere opinion, what is it that *distinguishes* the two?" (ibid, emphasis added). Although already question-begging in that it uses "knowledge" as a success-word, it is clearly motivated by the pursuit of a *method* of adjudication between competing truth claims; what is sought is a method of *demarcation*. (Recall Popper's assertion in *Realism and The Aim of Science* (1956, p. 19) that the fundamental problem of the theory of knowledge is the problem of adjudication or evaluation of "the far-reaching claims of competing theories and beliefs.") In addition, it is quite clear from Greco's entry that the *justificationist* program of traditional (and much contemporary) epistemology is a direct response to this demarcation problem. For immediately after posing this question about demarcation Greco raises the issue of justification (ibid): "What makes knowledge 'justified' or 'warranted'?" What Greco goes on to call "the project of vindication" is, it seems clear, motivated by what Popper (ibid) had diagnosed as the "unstated, and apparently innocuous, assumption… that one adjudicates among competing claims by determining which of them can be justified by positive reasons, and which cannot." This transition from the fundamental demarcation problem to subsequent justificationist responses is typical, and usually unexamined.

The attractions of this justificationist theory of rational adjudication are immediately apparent, especially when, as was traditional, the justification demanded was *conclusive*. After all, if our aim is truth, a theory that has been conclusively *certified* or established as true is, by definition, true, and hence is sufficient to meet our aim. That is to say, a certified truth is, tautologically, a truth; certified truth entails truth (a proof of $P \vdash P$). Thus, if achievable, *conclusive* justification would be invaluable for purposes of theory preference; it would solve the demarcation problem.

A further chief attraction of justificationism, as a response to the demarcation problem, is that it promises to avoid cognitive *relativism*. Relativism is, of course, a perennial epistemological concern, and it can best be viewed as a nihilistic response to the demarcation problem. That is, relativism amounts to the assertion that *in addition* to their being no objective method for *justifying* truth claims (scepticism), there is also no objective method for *adjudicating* between them. It is precisely the justificationist theory of demarcation-cum-rationality that licenses this progression from scepticism to relativism. Popper, in the first Addendum to *The Open Society*, is quite explicit on this link between relativism and the demarcation problem (1945, Vol. II, p. 419):

> The main philosophical malady of our time is... relativism... By relativism... I mean here, briefly, the theory that the choice between competing theories is arbitrary; since either, there is no such thing as objective truth; or, if there is, no such thing as a theory which is true or at any rate (though perhaps not true) nearer to the truth than another theory; or, *if there are two or more theories, no ways or means of deciding whether one of them is better than another* (emphasis added).

This hypothesis, that relativism is a nihilistic response to the demarcation problem, can also, perhaps, be discerned in Paul O'Grady's discussion of paradigms and *incommensurability* in his 2002 monograph *Relativism* (p. 18, emphasis added). There he writes: "[T]he crucial issue for relativism is the thought that paradigms are incommensurable... [that it]... is *not possible to adjudicate rationally* between competing paradigms." This notion of paradigm is crucial to twentieth-century relativistic thought, and takes various forms. It is essentially an extension of traditional Protagorean relativism or *subjectivism*, which made truth relative to the individual.

However, modern relativists tend to emphasise larger social groups—social classes in the case of (vulgar) Marxists, cultures for cultural relativists, "forms of life" for Wittgensteinians, and for those influenced by Thomas Kuhn, "scientific paradigms." Justification, for these theorists, is only possible *within* a particular paradigm, and hence rational adjudication cannot transcend specific, (and arbitrary) locales.

With reference to scientific knowledge in particular, a successful theory of objective justification would also avoid the associated relativistic doctrine of social constructivism, exemplified most notably by the Edinburgh School sociologists of science (see Bloor, 1992) and by Harry Collins' "empirical programme of relativism" (Collins and Pinch, 1993). These theorists assert that since scientific theories are underdetermined by evidence, theory adjudication proceeds not through objective and rational means, but rather through 'negotiation' and other noncognitive social processes. As Stephen M. Downes (1998, § 1) explains, social constructivists "maintain that social interests... determine which beliefs are held to be true or false... the truth or falsehood of scientific beliefs derives not from their relation to the world but from the social arrangements of scientists." The resolution of scientific controversies depends, on this view, not on objective evidence or data, but on (essentially sociological) "other factors". Justificationists, to their credit, seek to deny the truth of this (essentially irrationalist) outcome.

Thus, the major appeal of the justificationist response to the demarcation problem is that it promises to avoid dogmatic subjectivism and relativism.[5] In other words, it is intended to be a theory of *objective demarcation;* justificationism "recognizes that something more than and something different from blind belief must be demanded" (Radnitzky, ibid, p. 282). As Mark Notturno (2000, pp. xix-xx), writes "the whole point of a justification is to give evidence that is as compelling for others as it is for ourselves." Indeed, what is common to the various forms of justificationism is that the justification is meant to be *objective*; it is meant to provide evidence for a proposition that is acceptable to any rational enquirer. As such, the justification is purported to be both normative and impersonal, in order for theory adjudication to be intersubjectively acceptable. Robert Audi (1998 § 0-1) expresses this requirement of objectivity well in his article "Reasons for Belief" in the *Routledge Encyclopaedia of Philosophy*. "Reasons for believing something are one or another kind of ground for believing it," Audi states, and these evidential reasons must be "[i]mpersonal normative reasons... a normative reason...[is]... a reason... to believe the proposition in question." The important point here is that this sort of epistemically justifying reason should not be confused with subjective "certainty", or with a mere *psychological* explanation of why a particular agent has a specific belief. As Peter Klein (1998, Introduction) writes:

> Psychological certainty is an attitude that persons can have towards a proposition, and propositional certainty is a measure of the epistemic warrant for a proposition. Thus, it is clear that psychological certainty and propositional certainty are logically independent... In general, psychological certainty has not been a topic which philosophers have found problematic.

Thus it is *propositional* certainty or the justification of *propositions* which is the focus of the justificationist project (Klein calls this type of justification "warrant"). Admittedly, it is possible to formulate the demarcation problem subjectively, as the problem of which *beliefs* we should hold. And as Klein writes (ibid, § 1), "[p]resumably a person would want the degree of belief in a proposition to parallel the degree of epistemic warrant for it." Despite this, the justification itself, to avoid relativism, must be objective. Audi again (§ 4): "[i]mpersonal normative reasons need not be internal. Perhaps, however, they must be accessible, in the sense that someone could, by suitable investigation, become aware of them..." Indeed, it is precisely such *publicly accessible* and *objectivist* aspirations that help explain why justificationism has been the standard response to the demarcation problem.

However, justificationism is not *solely* a theory of demarcation; it is also the notion of *rationality* underlying the whole Western tradition, in both philosophy and science. When so construed, as a theory of rationality, the justificationist holds that the rational agent must be able to justify, or provide grounds or warrant for, her beliefs and opinions. Justification is central to rationality in this view, and reason and argument, it is held, are used *primarily* to provide justification and support of the theory or course of action being examined.

Common to all forms of this justificationist theory of rationality are the following two theses:

1) In order for a theory to be *rationally* held as true, there must be supporting

evidence for it; in other words, there must exist some positive justification for it. 2) Failing the provision of such a justification, an agent is irrational to hold such beliefs or theories, or even to provisionally rule any out.

Although neither of these statements are, I think, true, they do explain why the provision of justification for theories has been regarded by epistemologists as such an important and pressing task—indeed, *the* central task for any theory of knowledge.

This doctrine, that rational agents hold *only* those theories that they can justify, is traditional. Descartes, for instance, in his *Regulae ad directionem ingenii* (*Rules for the Direction of the Mind*) (1620) states that "we must occupy ourselves only with those objects that our intellectual powers appear competent to know certainly and indubitably." Later, in his *Meditations on First Philosophy* (1641, p. 12), he asserts a similar rule for rational belief: "I should hold back my assent from opinions which are not completely certain and indubitable just as carefully as I do from those which are patently false." This equation of rationality with justification can also be discerned in Hume's remark, in *An Enquiry Concerning Human Understanding* (1748, § 10), that "[a] wise man proportions his belief to the evidence." It is also conspicuous in Russell's writings, for instance (1912, ch. 11), "[i]t is felt by many that a belief for which no reason can be given is an unreasonable belief. In the main, this view is just." In the same vein, A.J. Ayer, in *Probability and Evidence* (1972, p. 3), has stated, "A rational man is one who makes a proper use of reason: and this implies, among other things, that he correctly estimates the strength of evidence." It is also apparent in *The Web of Belief* by Quine and Ullian (1978, p. 16): "[i]nsofar as we are rational in our beliefs, the intensity of belief will tend to correspond to the firmness of the available evidence. Insofar as we are rational, we will drop a belief when we have tried in vain to find evidence for it."[6]

This justificationist explication of rationality—the doctrine that our knowledge must be justified in order to be deemed rational—is also predominant in contemporary philosophy of science, as Miller (1994, p. 54) notes:

> ... Stove and many others (for example, Salmon 1981; Newton-Smith 1981, Chapter 3; Lieberson 1982, 1983; Black 1983, 20; Zahar 1983, 168; Watkins 1984, 58f; and Musgrave 1991, 26f)... [restrict]... the word 'rationalist' to those who think that good reasons of some kind

or other need to be supplied if we are to have knowledge or science, or to make rational decisions.

Closely linked to this justificationist conception of rationality is the justificationist account of the role of logic and argumentation. This theory holds that the "primary purpose of argument is to justify or prove or establish or consolidate or support or to provide good reasons for propositions that we are interested in" (Miller, 2006a, p. 65). This is the view of many writers, and is well expressed by Toulmin (1958, p. 12): "[Justification is]... the *primary* function of arguments... the other functions which arguments have for us, are in a sense secondary, and parasitic upon this primary justificatory use." To illustrate the prevalence of this thesis Miller also gives a representative list of books in the "critical thinking" genre where this doctrine—that both deductive and inductive arguments are used primarily for justification—is advanced: Bowell & Kemp (2002, p. 1); Fisher (1988, p. 16); Fisher (2001, p. 3); Govier (2001, p. 11); Jones (1997, p. 3); Shand (2000, pp. 2f., 38); Thomson (2002, p. 6). This view is also apparent even amongst some self-described Popperians. John Watkins, for instance, suggests that the denial of "good reasons" is tantamount to what he calls "rationality-skepticism"—the doctrine that "we never have any good cognitive reason to adopt a hypothesis about the external world" (1984, p. 58).[7]

Thus, we may distinguish two approaches to theories, and hence two explications of *rationality per se*: the justificationist approach, and the non-justificationist or critical approach. The untenability of the justificationist approach will be explicated in the following three chapters, using both classical sceptical arguments, and more modern ones aimed especially at probabilistic versions of justificationism. A rehabilitation of Popper's critical approach to demarcation will then be canvassed in the concluding chapter.

2.2 The Justificational Schema

Within the overarching "justificationist metacontext", to use Bartley's term, there have been many variants of the justificationist response to the demarcation problem. Nonetheless, Bartley (1984, pp. 186-7) identifies a common structure that justificationist theories of demarcation typically exhibit:

> (1) an authority (or authorities), or authoritatively good trait, in terms of which final evaluation (i.e., demarcation of the good from the bad) is to be made;
> (2) the idea that the goodness or badness of any idea or policy is to be determined by reducing it to (i.e., deriving it from or combining it out of) the authority (or authorities), or to statements possessing the authoritatively good trait. That which can so be reduced is justified; that which cannot is to be rejected.

Let us take each of these conditions in turn. Regarding the first condition, determining the authoritatively good trait has proved contentious. Indeed, a major traditional debate in epistemology concerns precisely the identity of the justifying authority; this is the debate between the classical schools of British empiricism and continental rationalism (or better, *intellectualism*). The dispute, essentially, pertained to what authority was to provide the justification for truth claims, and hence play the crucial role in theory choice.

For example, Descartes' answer to the demarcation problem, in his *Discourse on the Method for Properly Conducting Reason and Searching for Truth in the Sciences* (1637), was that competing beliefs and theories were to be adjudicated based upon which could be *justified* by, or derived from, the "natural light" of reason, or equivalently, by determining which could be reduced to "clear and distinct" ideas. The most fundamental principles of science were to be derived *a priori* by the use of rational intuitions and deductions from them. Other apriorists, most notably Kant (1781), have appealed to so-called "transcendental logic" to justify fundamental scientific principles involving space, matter, and motion.[8] Such a logic would, in contrast to (informatively empty) classical logic, allow the apodictic justification of synthetic judgements—informative statements about the world.

Conversely, the classical *empiricist* program for demarcation entailed adjudicating theories upon the basis of their alleged derivability from (and hence justification by) *experience*. Thus the problem of theory choice is automatically solved for the (classical) empiricist—only those propositions sanctioned by immediate experience, and those which can be deductively or inductively derived from them are candidates for rational belief. As my focus here is on empirical science, this empiricist theory of demarcation is especially relevant, as justificationist theorists of science have also customarily taken the epistemic authority to be *sense-observation* or *experience*.[9]

This empiricist variant of justificationism was memorably applied to the adjudication of competing theories by David Hume in his *An Enquiry Concerning Human Understanding* (1748, ch. 12):

> *When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask,* Does it contain any abstract reasoning concerning quantity or number? *No.* Does it contain any experimental reasoning concerning matter of fact and existence? *No. Commit it then to the flames: for it can contain nothing but sophistry and illusion.*

This justificationist criterion of demarcation—that genuine (i.e. scientific) knowledge is what is reducible to sense experience—was also a central tenet of (at least some of) the philosophers of the Vienna Circle. Like Hume, they recognised only two varieties of knowledge as legitimate: analytic knowledge, justified by formal proof, and scientific knowledge, justified by empirical verification. This criterion, it was hoped, would have the desired consequence of eliminating traditional metaphysics and theology as neither formally demonstrable nor empirically verifiable.[10]

This brings us to Bartley's second standard component of the justificationist theory of demarcation (1984, pp. 186-7):

> ...the idea that the goodness or badness of any idea or policy is to be determined by reducing it to (i.e., deriving it from or combining it out of) the authority (or authorities), or to statements possessing the authoritatively good trait. That which can so be reduced is justified; that which cannot is to be rejected.

This second condition is more nuanced than the first. It may be alternatively formulated as the question of how theories are to be inferred or justified relative to the evidential or empirical base. Must this relationship be exclusively deductive—that is, using only strictly valid inferences? Or might it employ some form, yet to be elucidated, of inductive, or ampliative, or probabilistic logic? How, in other words, does justification transmit from foundational beliefs to inferentially justified, non-foundational beliefs? Bartley later (1984, p. 261) refers to this as "the transmissibility assumption"—however it is elucidated, the assumption is that higher-level beliefs somehow inherit the justificatory status of the basic beliefs or the justifying authority. Hence *"[m]ost philosophical views take for granted that all properties, measures, and tokens of intellectual value or merit are transmitted from premises to conclusion, in the same manner as truth, through the relationship of logical derivability or deducibility."* Justificationists have invariably found it necessary to weaken this relationship, however, in order to avoid demarcational absurdities.

This latter issue raises another major debate in traditional epistemology—the controversy, originating in Aristotle, between inductivism and deductivism. Given my focus on nomological science, Aristotle's version of justificationism is especially relevant. For not only is Aristotle one of the earliest theorists of justificationist empiricism, he is also the inventor of induction, the ever popular and protean theory of scientific justification.

2.3 A Historical Example of Justificationist Demarcation–Aristotle

Indeed, Aristotle is undoubtedly the most influential ancient epistemologist and theorist of science to explicitly exhibit Bartley's schema. Although both Plato and Aristotle held that theories are to be adjudicated based upon which can be justified, this idea is only a vague suggestion in Plato; in Aristotle it becomes much more systematic. For instance, the idea that genuine knowledge is *justified* true belief, can be discerned in the *Meno,* (97d- 98b) where Plato suggests that *knowledge (epistêmê),* as opposed to mere *true belief* (doxa), must be "tied down" or "tethered". Plato's justificationism is also apparent in the *Theaetetus* (201d), where it is suggested that genuine knowledge is true belief plus an account or *logos,* and, later, in Book V of the *Republic* Plato goes as far as to characterise knowledge as both certain and *infallible* (477e). However, Plato's position is not entirely unambiguous. According to Popper (1998, pp. 1-5):

> Plato, though a political dogmatist, was not a dogmatist in epistemology... Concerning the field

> that we now describe as natural science, Plato says explicitly (in the *Timaeus,* for example, but also in other places) that all he can tell us is at best only 'truthlike' and *not* the truth: it is, at best, *like* the truth...

Moreover, Plato gives few clues on how this justified knowledge is to be achieved. Aristotle is much more forthcoming. Indeed, Popper opines that "Aristotle was the first real dogmatist...what to Plato is a scientific *hypothesis* becomes with Aristotle *episteme,* demonstrable knowledge. And for most epistemologists of the West, it has remained so ever since" (ibid).

So let us examine Aristotle's views on demarcation. Aristotle indeed took the justificationist response further than Plato; he may be regarded as the first empiricist *foundationalist.* That is, in terms of Bartley's two conditions for justification, for Aristotle the final authority by which knowledge claims are to be adjudicated are the fundamental principles of the specific sciences, which in turn are to be deduced from sense-perception via inductive inference. Moreover, the subsequent theorems of the sciences are to be justified by reducing them, via deductive syllogisms, to these essentialist definitions.

Aristotle lays out his foundationalist theory of scientific knowledge in the *Posterior Analytics*. As Louis F. Groarke (2011, § 12) explains, Aristotle "believes that knowledge, understood as justified true belief, is most perfectly expressed in a *scientific demonstration* (*apodeixis*), also known as an *apodeitic* or *scientific syllogism*." The basic foundational elements in Aristotle's system are the *archai;* "first principles" which are purportedly "true, primary, immediate, better known than, prior to, and causative of the conclusion" (*Posterior Analytics*, I.2.71b20ff.). From these first principles all other knowledge is to be derived syllogistically. As George Pappas (1998, § 1) states, Aristotelian science "will be the sum of all such theorems, demonstrated either from first principles or from already-demonstrated theorems in appropriate syllogisms."

Hence, once given the fundamental principles via induction, the structure of Aristotelian science is commendably deductive. According to Popper (1998, p. 5):

> ...the Aristotelian ideal of science is more or less an encyclopaedia full of concepts, the names of the essences. What is known about these essences defines the concepts, so that we can deduce' everything about the concepts from their various definitions and their interconnections. This is the structure of a deductive encyclopaedia with all its concepts obtained by inductive

procedures: the *archai* from which we can then derive everything else by means of logical deductions, the syllogisms.

However, a fundamental qualification is that in order to *secure* the demonstration, these premises must be *better* known than the demonstrated conclusion; indeed, it seems that they must *already* be demonstrated. In order to adjudicate amongst competing claims in this fashion, we need authorities that are not themselves in dispute. Yet, as Aristotle recognised, if the premises must in turn be demonstrated, this leads to an infinite regress. Rejecting both the possibility that human knowledge is conjectural and unjustified, and also the viability of circular demonstration, Aristotle instead posits an immediate, intuitive grasp of the first principles. Thus, in order to stem the regress, Aristotle advises the "inductive" grasp of the essential principles of the specific sciences. This is Aristotle's theory of induction or *epagoge*.[11] It is essential to his theory of demarcation.

However, this theory is not easy to formulate, and there appears to be a certain equivocation in Aristotle's presentation. On the one hand, there are some signs that Aristotle views induction "as a manifestation of immediate understanding and not as an argument form" (Groarke, 2011, § 10). This minority view (of McCaskey, 2007; Biondi 2004; and Rijk 2002) interprets Aristotelian induction *not* as an enumerative process, but instead as a "matter of intelligent insight into natures... [analogous] to modern notions of abduction or inference to the best explanation" (Groarke, 2011, § 10). However, this interpretation, if correct, would completely invalidate Aristotle's system as an objective method of theory adjudication; it would be "mere intuitionism dependent on an antiquated metaphysics" (ibid).

Moreover, this interpretation ignores the fact that Aristotle explicitly refers to *inductive*, as opposed to deductive, *syllogisms*, and even, in the *Prior Analytics* II.23 and 24, gives an example of the "syllogism that springs out of induction" (*ho ex epagoges sullogismos*). Indeed, it appears plain that Aristotle sought a more objectivist justification for his basic principles than mystical intuition, one that would secure all knowledge derived from them.

The specific example of an inductive syllogism that Aristotle gives concerns the lifespan of bileless animals. If we use the terminology of Aristotle's syllogistic, it goes:

Major Premise: All men, horses, mules, and so forth, are long-lived;
Minor Premise: All men, horses, mules, and so forth, are bileless animals;
Conclusion: Therefore, all bileless animals are long-lived.

In symbolism—with the subject term, S = men, horses, mules, and so forth; the predicate term, P = long-lived animals; and the middle term, M = bileless animals—we can formalise this inference as:

Major Premise: All S are P
Minor Premise: All S are M
Conclusion: Therefore, all M are P

Cast in syllogistic form, this inference is clearly invalid. How is this fact to be explained?

One view (McKirahan 1992; Peters 1967) interprets Aristotle as referring here to "perfect induction"—that is, a generalisation that is built up from a complete enumeration of particular cases. However, such a limiting case of purportedly inductive inference is, in fact, *deductive*. Moreover, given the practical impossibility of complete enumeration, this interpretation makes Aristotelian induction inapplicable to scientific knowledge, which clearly conflicts with Aristotle's stated aims. Given such weakness, this apologetic interpretation may thus be discarded.

According to Popper (1998, p. 3), we can better explain this oddity if we regard Aristotle as attempting to convert the Socratic *elenchus,* or *refutation by counterexample,* into a method of *proof* or *justification*. Indeed, Aristotle explicitly attributes his theory of induction to Socrates.[12] However, there is a definite inversion here. While the Socratic method involved the refutation of general claims by the use of concrete and experiential counterexamples, Aristotle's *epagoge* invokes the enumeration of concrete instances as a method of inducing knowledge of essences in the inquirer—in other words, as a *positive* method of justification.

To overcome this logical gap, Groarke suggests that Aristotle licensed the conversion of the minor premise in an inductive syllogism—All S are M—to its converse—All M are S—thereby transforming the syllogism into a valid deductive inference in the form *Barbara:*

Major Premise: All S are P

Minor Premise: All M are S

Conclusion: Therefore, all M are P

As Groarke (2011, § 11) states, the "logical form of the inductive syllogism, after the convertibility maneuver, is the same as the deductive syllogism". However, "in order to successfully perform an induction, one has to know that convertibility is possible, and this requires an act of intelligence which is able to discern the metaphysical realities between things out in the world." In other words, like all theorists of induction, Aristotle requires an inductive principle to secure his system. This strategy will be explored further in chapter 4. This, then, is the basic logic of Aristotelian science; it forms the blueprint for contemporary theories of inductivist demarcation.

2.4 Transition to Contemporary Justificationism in the Nomological Sciences

Since my concern is first and foremost with the demarcation problem as applied to theoretical science it will be worthwhile to look specifically at how the general doctrine of justificationism has had an influence in this context. Unsurprisingly, justificationism has had a significant impact upon modern methodologists of science. Indeed, in contemporary philosophy of science, the major justificationist concerns are analogous to Aristotle's. They primarily concern a) the justification of the *universal statements of scientific laws*, in addition to b) the status of the *observation reports* which figure in empirical investigations, and which purportedly provide justification for the truth claims of those aforementioned theories.

However, something must first be said about the redirection in the justificationist program from traditional Aristotelian or Cartesian[13] *conclusive* justificationism (what I will call *strong justificationism*), to modern *fallibilist* justificationism (*weak* *justificationism*). As Paul K. Moser, in the *Routledge History of Philosophy* (Volume 10, 1997, p. 151), states:

> Justification for a proposition, according to most twentieth-century epistemologists, need not logically entail the proposition justified: it need not be such that necessarily if the justifying proposition is true, then the justified proposition is true too. When justification does logically entail what it justifies, we have *deductive* justification. *Inductive* justification, in contrast, does not logically entail what it justifies; it rather is such that if the justifying proposition is true, then the justified proposition is, to some extent, *probably* true.

Indeed, conclusive justification—that is, *proof*—is now generally recognised as unobtainable, and certainly since the middle of the twentieth century, fallibilism has come to be nearly universally accepted.[14] As a result, *inconclusive* or *partial* support is now more commonly sought, rather than conclusive verification. Theories need no longer be *demonstrated*, but they are still to be demarcated *epistemically*, in terms of evidential *support*.[15] In the philosophy of science, this justificationist adoption of fallibilism is closely tied to modern attempts to formulate an inductive logic utilising the mathematical apparatus of the probability calculus. This modern variant of justificationism has come to be named "confirmation theory", which interprets the confirmation of a hypothesis by new evidence as increasing its probability. As Radnitzky (1987, p. 284) notes:

> In the methodology of scientific research the very idea of a confirmation theory is governed by the justificationist style of philosophizing. That absolute verificationism has been replaced by probabilistic verificationism or "soft inductivism" does not change the basic orientation.

As this narrative suggests, a great deal of twentieth-century epistemologists and philosophers of science have held that non-deductive, purportedly "probabilistic" connections could transfer justification from empirical premises to theoretical conclusions, although they have not reached agreement on the exact nature of such connections. (This, of course, should not be surprising if no such probabilistic induction exists). This project will be criticised in chapter five. Suffice to say here that, on this account, justification, albeit *partial,* remains essential for rational demarcation or theory choice.

Justificationism, or confirmationism, also has methodological implications. Since justificationism holds that a theory can become justified (though not generally conclusively justified) by the accumulation of evidence, this has naturally led to the methodological recommendation that scientists should seek evidence to *confirm* their theories. As Miller (2006a, p. 53) states:

> What is expected from an observational or experimental project is positive evidence; evidence, that is, that illustrates the scope and breadth of the theory, that enables us to make a case in the theory's favour... although our theories usually cannot be verified they can be very well—perhaps overwhelmingly—confirmed.

Thus, science seeks *confirmations* on this theory; the purpose of experiments is primarily to *confirm* theories. This view is evident in Howson (2000, p. 101), where the author commends "the fundamental Bayesian principle... that experiments are well designed to the extent that they bring the posterior probabilities as close to the extreme value of 1 as possible." A weaker justificationist position, held by Carnap and others, asserts that scientists "should aim at providing empirical evidence for a theory that increases its probability without necessarily rendering it high" (Achinstein, 1998, § 3). (Clearly, if taken seriously, such advice has implications for experiment design.) Common to both versions is the assumption that the demarcation problem is to be solved with reference to the degree of support, or degree of confirmation, that the empirical evidence provides.

Thus, in addition to the (exclusively) hypothetico-deductivist method that will be championed by this author, confirmationists advocate an extra *inductive* step; some "inductive argument giving independent support to the hypothesis or theory" (Achinstein, 1998, § 4). What is required, in addition to an unembellished hypothetico-deductive method, which can only eliminate theories and not recommend a particular theory over unfalsified competitors, is "independent inductive support" (ibid). This requirement is stated quite clearly by Salmon (1966, p. 21):

> ...the hypothetico-deductive method, since it is ampliative and nondemonstrative, is not strictly deductive; it is, in fact, inductive in the relevant sense. As long as the hypothetico-deductive method is regarded as a method for supporting scientific hypotheses, it cannot succeed in making science thoroughly deductive. Popper realizes this, so in arguing that deduction is the

sole mode of inference in science he rejects the hypothetico-deductive method as a means for confirming scientific hypotheses.

Indeed, the hypothetico-deductive method favoured by contemporary justificationist philosophers of science is not *truly* hypothetico-deductive—it differs from simple induction by enumeration accounts, but it still assumes some inductive inference which uniquely justifies a particular hypothesis. Accordingly, Salmon states, explicitly with reference to theory choice, that (1966, p. 19):

> Deduction is an indispensible part of the logic of the hypothetico-deductive method, but it is not the only part... science aims at establishing general statements about the world. Scientific prediction and explanation require such generalizations. While we are concerned with the status of the general hypothesis—whether we should accept it or reject it—the hypothesis must be treated as a conclusion to be supported by evidence, not as a premise lending support to other conclusions. The inference *from* observational evidence *to* hypothesis is surely not deductive. If this point is not already obvious it becomes clear the moment we recall that for any given body of observational data there is, in general, more than one hypothesis compatible with it. These alternative hypotheses differ in factual content and are incompatible with each other. Therefore, they cannot be deductive consequences of any consistent body of observational evidence.

In the same vein as Salmon, Achinstein asserts that one of the goals of scientific practitioners "is to achieve certainty, at least as much as possible, by... carrying out... empirical testing" (ibid, § 7). This echoes his earlier reference to Newton who, we are told, recommended that "scientists should always aim at the highest certainty possible in an empirical endeavour" (§ 3)[16].

Moreover, this justificationist assumption has also come to infect the *goal* of inquiry itself—that is, the idea that *truth* is the aim of science. Concerning the goal of inquiry, justificationism holds that the aim of science is, ideally, conclusively *verified* theories, or, failing this, at least highly *probable* ones—ones which have overwhelming evidential *support*. Thus, following Plato, it has been common to regard justified theories—knowledge in the traditional philosophical sense—as the goal of inquiry, instead of truth *simpliciter*. Thus, justificationism leads to the doctrine that the aim of science is not truth, but rather *probable* truth. The famous game theorist and Nobel Memorial Prize winner in economics, John Harsanyi (1985), is quite explicit about this point:

> Probably most people will agree that ideally the task of science is to come up with true theories that provide truthful explanations for the empirical facts. Yet, we can never be quite sure whether any given scientific theory is in fact completely true in all its specific details or not. *Therefore, more realistically, we may say that the task of science is to identify, among all alternative theories existing in a given field, that particular theory that can be reasonably judged to have the highest probability of being true* and of providing a correct explanation for the facts. A further task of science is to propose new theories whose probability of being true and of providing correct explanations is higher than that of any existing alternative theory (emphasis added).

The reasoning here is that probability, or high probability, could serve as a guide to truth. This position will be criticised in chapter 5. In the extreme, justificationism has led some probabilists to renounce the demarcation problem altogether: subjective Bayesians, according to Miller (2006, p.55), "deny that truth is a value at all, or that we are ever interested in the truth values of the theories that come before us... they repudiate entirely the traditional interest in the truth of what they investigate, and affect to be interested only in its (subjective) probability." This completely jettisons what BonJour accurately stresses is an essential component of any theory of justification—that is, "an appropriate connection between justification and truth" (1985, p. 25).

To conclude this chapter, the justificationist response to the demarcation problem is ever-present in the central debates in the theory of knowledge—the major disputes have centred upon *how* this justification is to be achieved, as well as, of course, attempts to respond to the ever-present sceptic who has asserted, time and again, that any objective justification is impossible. It is this sceptical challenge that will be my focus in the following chapter.

---

[1] Plato's distinction between justified knowledge and true belief, and his recommendation of the former, can be found in the *Meno*. This distinction between genuine knowledge *(saphes, aletheia;* later: *episteme),* which is certain, and mere belief or opinion (*doxa*) is also traditional amongst the Presocratics. It is sometimes equated with the theological distinction between divine knowledge and merely human knowledge (especially in Parmenides and Xenophanes).

[2] Foundationalism will be explored further in sections 2.3 and 3.7; coherentism in section 3.6.

[3] Both authors are quoted in Fogelin (1994, p. 114- 115).

[4] This is not always explicit however. To take an example, Matthias Steup, in his *Stanford Encyclopaedia of Philosophy* entry on "Epistemology" (2005), makes no mention of truth in his (otherwise quite orthodox) justificationist assertion that "epistemology is the study of knowledge and justified belief."

[5] This has also been noted by Radnitzky (1987, p. 282): justificationism, "[s]ince it implicitly acknowledges that not all positions are equally good or bad... avoids relativism."

[6] Yet another example is Wesley Salmon in his *The Foundations of Scientific Inference* (1966, p. 6): "One of the basic differences between knowledge and belief is that knowledge must be founded upon evidence—i.e., it must be belief founded upon some rational justification."

[7] This is a misnomer however, for this doctrine merely constitutes *justification*-skepticism, not *rationality*-skepticism. In the second part of this dissertation I will dispute the validity of this equation, which only the justificationist theory of rationality sanctions.

[8] Kant's justificationist transcendental argument will be discussed further in chapter 4.

[9] As Achinstein (1998) notes, "Newton... claimed to derive his three laws of motion and law of gravity from observed phenomena using induction and deduction... Similar views have been held by many empiricists, including J.S. Mill and the logical positivists."

[10] This programme is especially prominent in Carnap's *Pseudo-problems in Philosophy* (*Scheinprobleme in der Philosophie,* 1928), his essay, "The Overthrow of Metaphysics through the Logical Analysis of Language" ("Überwindung der Metaphysik durch logische Analyse der Sprache", 1932) and in *The Logical Syntax of Language (Logische Syntax der Sprache,* 1934). Being neither formally demonstrable nor empirically verifiable, metaphysical statements were deemed as lacking any empirical significance—they were regarded as being absolutely meaningless. Only propositions which could be justified in one of the aforementioned ways were to be admitted to the corpus of scientific knowledge.

[11] As Salmon (1966, p. 2) notes, "Aristotle regarded scientific reasoning as strictly syllogistic in character; the only nonsyllogistic part is the establishment of first principles, and this is accomplished by intuitive induction. Intuitive induction is very different from inductive generalization as we think of it nowadays; it is, instead, a kind of rational insight." However, contra Salmon, this need for some kind of *a priori* rationalist insight is *also* characteristic (explicitly or implicitly) of more modern theories of induction.

[12] This is despite the fact that Socrates famously claims, in the *Apology, not* to know in Aristotle's sense—motivating Aristotle to claim that Socrates' profession of ignorance is merely ironical.

[13] In his *Regulae ad directionem ingenii (Rules for the Direction of the Mind) (*1620 ch.9) Descartes' second rule is that "we must occupy ourselves only with those objects that our intellectual powers appear competent to know *certainly* and *indubitably*."

[14] There are exceptions of course. It is seemingly disputed, for instance, by some prominent contemporary epistemologists—notably University of Pittsburgh philosopher John McDowell, who speaks of "*indefeasible* warrant for perceptual beliefs". See his *Perception as a Capacity for Knowledge,*

2011.

[15] This justificationist conception of demarcation is also closely linked to the position dubbed, in recent epistemological literature, *evidentialism*. This position, as *The Blackwell Dictionary of Western Philosophy* (2004, p. 234) states, "claims that a belief... toward proposition P is epistemologically justified... if and only if this belief fits the evidence... and the evidence... is certainly well supported epistemologically and is properly arrived at." Although explicitly christened only relatively recently by Feldman and Conee (1985), "evidentialism" is long established in the epistemological tradition and especially in the epistemology aimed at the study of science, where the demand for objectivity has meant that the justification of a theory should depend exclusively on the publicly available evidence for it. Thus Feldman and Conee write in their paper "Evidentialism": "What we call evidentialism is the view that the epistemic justification of a belief is determined by the quality of the believer's evidence for the belief" (ibid).

[16] Interestingly, Achinstein characterises "philosophers, theologians and mathematicians" as *also* aiming, primarily, to achieve certainty, but, in contrast to empirical scientists, through "a priori argumentation" (1998, § 7). Justificationism, that is, extends far beyond empirical knowledge.

Chapter Three: The Sceptical Challenge to Justificationism

3.0 Introduction

Justificationism, although the dominant response to the demarcation problem in the history of epistemology, has not gone unchallenged. In fact, the majority of justification philosophy is little more than a series of defence works against scepticism (Hamlyn 1970, p. 9), the doctrine, in its most general form, that no part of human knowledge can achieve any positive degree of justification. As such, scepticism necessarily entails the rejection of what I have called the justificationist theory of demarcation-cum-rationality. Moreover, since justificationists regard scientific knowledge to be *rational* only to the extent to which it is justified, scepticism has frequently been equated with irrationalism or relativism. In this chapter I will disentangle scepticism from these conflations, before introducing the most general of the sceptical arguments against justificationism—the logical trilemma associated with the Pyrrhonian sceptic Agrippa. As Robert Fogelin has noted, "the philosophical problem of epistemic justification [is] the attempt to solve the Agrippa problem, that is... the attempt to meet the skeptical challenge presented by Agrippa's Five Modes Leading to the Suspension of Belief" (1994, p. 114).

3.1 Scepticism and Justificationism

The term "scepticism" is diversely applied. Etymologically, the word is derived from the Greek *skeptikos*, meaning "inquirer", but that, by itself, does not get us very far.[1] What I have in mind by "scepticism" is the broad movement in ancient Greek philosophy, comprising the Academic and the Pyrrhonian schools—and in particular, their assertion that epistemic justification is impossible. That is, my concern is with scepticism as a purely *epistemological* doctrine, which says nothing about either *psychological* doubt or about the *ontological* nature of the world. In other words, although the philosophers customarily grouped together as sceptics have heterodox views in many areas of philosophy,[2] my concern is primarily with their shared negative program—that is, their critique of justificationism.

Academic scepticism, which flourished from the fifth to second centuries B.C.E. Is the earlier of the two major schools of ancient scepticism. It derives its name from Plato's Academy in Athens, and is attributed in particular to Plato's successors Arcesilas (c. 315-241 B.C.) and Carneades (c. 213-129 B.C.). Their major inspiration seems to have been Socrates, and in particular his profession of ignorance in the *Apology* (22d) — "I know that I know nothing". Indeed, this is often said to have been their central doctrine: there is only one thing one can know, namely that one can know nothing else. As Katja Vogt notes (2010, § 3.1), with reference to Cicero, "Socrates' commitment to investigation, to the testing and exploring of one's own and others' beliefs, and his passion for weeding out falsehoods, are the starting-points of Academic skepticism." Both Arcesilaus and Carneades also adopted a dialectical method reminiscent of that employed by Socrates (Couissin 1929 [1983]), and both, like Socrates, refrained from producing written works. Indeed, what we know of them is largely through secondary sources—in particular Cicero[3] and Augustine.

Yet even Socrates' seemingly modest claim was rejected as dogmatic by the second major school of Greek scepticism, the Pyrrhonian school (c. 365–275 B.C.E.), who labelled the Academics "negative dogmatists." This later school was named after its founder Pyrrho of Elis (c. 360-275 B.C), and was continued by Pyrrho's student Timon (325/20–235/30 BCE). Another important figure was Aenesidemus (c. 100-40 B.C.), who revived Pyrrho's philosophy, and his followers ensured Pyrrhonianism still survived centuries later to be documented by Sextus Empiricus around 200 A.D. Sextus' writings are traditionally divided into the three books of the *Outlines of Pyrrhonism*, and another collection titled *Against the Mathematicians*. It was through the rediscovery of Sextus' work by Henri Etienne that scepticism had a significant intellectual impact in Europe in the 16th Century (Annas-Barnes 1985, p. 5-7; Bailey 2002, p. 1-20), particularly on such figures as Michel de Montaigne and Descartes.

What both schools of scepticism had in common, and which marks their distinctive contribution to epistemological thought, is the rejection of the pretensions of justification philosophy. Specifically, their targets were the ancient Stoics, Epicureans, Cynics, and Megarian logicians who made claims to justified knowledge—*epistēmē*—and even claimed to have a criterion of truth. Against the"'dogmatic philosophers", as the sceptics referred to them, the sceptics advanced two broad types of arguments. First they disputed the sufficiency of the justifying authority—be it reason or the senses—to provide a secure basis for knowledge claims. Second, and more fundamentally, they criticised the very logical structure of justificationist arguments, and asserted that they inevitably led to paradoxical results. This distinction largely overlaps with the differing arguments presented by Sextus, in the Ten and Five Modes respectively.[4]

Accordingly, within the "justificationist metacontext", scepticism is often regarded as a nihilistic response to the demarcation problem, and indeed, *within* this context, it is. This is evident in the first Mode of Agrippa:

> The mode that argues from disagreement. With respect to some matter that presents itself, there is undecided (*anepikriton*) conflict, both among the views of ordinary life and the views held by philosophers. Due to this, we are unable to choose or reject one thing, and must fall back on suspension (Vogt, 2010, § 4.3).

Further corroborating my earlier thesis of demarcation's centrality to epistemology, Harald Thorsrud (2004, Introduction) writes, in connection with ancient skepticism, "What leads most skeptics to begin to examine and then eventually to be at a loss as to what one should believe, if anything, is the fact of widespread and seemingly endless disagreement regarding issues of fundamental importance". And as R.J. Hankinson, in his article on *Pyrrhonism*, writes, "Aenesidemean Scepticism took the form of emphasizing the disagreement among both lay people and theoreticians as to the nature of things, and the fact that things appear differently under different circumstances... [f]aced with endemic dispute, Sceptics reserve judgment." Again, as Vogt (2010, § 4.4) writes, "[t]he starting-point (*archê*) of skepticism is divergency—*anômalia*."

Thus, the basic problem situation of both schools of ancient scepticism, that is, the Academic and the Pyrrhonian schools, is the same as that of justificationism; all are primarily concerned with the problem of theory adjudication. This concern with demarcation is not confined only to ancient scepticism however. It is quite apparent in its 16th Century revival as well, as can be seen in the writings of Montaigne—Richard H. Popkin (1998a, § 3) cites Montaigne's sceptical philosophy to be a response to the "deplorable history of dogmatic philosophers whose endless disputes and heterodox views exhibit nothing but human stupidity and credulity." Popkin continues:

> In the best tradition of Renaissance humanists, Montaigne cited the vast range of opinions of ancient thinkers from Greece and Rome that had recently been discovered... In view of the enormous diversity of points of view, can one really determine what to believe or to accept as true? In all fields of inquiry the dogmatists have finally had to confess their ignorance, and their inability to come to definitive and unquestionable conclusions.

Montaigne is also quite emphatic about his concern with the demarcation problem in the following quotation (1933, p. 544):

> To adjudicate [between the true and the false] among the appearances of things, we need to have a distinguishing method; to validate this method, we need to have a justifying argument; but to validate this justifying argument, we need the very method at issue. And there we are, going round on the wheel.

Thus the demarcation problem is what underlies sceptical philosophy, just as it underlies justificationism. Yet as Montaigne makes clear, all is not right with the justificationist response. This last quotation reveals the key (formal) sceptical argument against the justificationist: justificationism, so the sceptics claim, is by its own lights logically impossible. This argument, which might be called the Agrippa problem, will be surveyed in section 3.4; as Robert Fogelin (1994, p. 195) has stated, if "the Agrippa problem cannot be solved, there is no reason to suppose that knowledge of the kind sought by justificationalist philosophers exists."

However, sceptical arguments have often been simply dismissed rather than seriously addressed. This is due to some popular, yet invalid, justificationist objections. It is thus necessary to clear up these objections in order to make any progress in adequately assessing the sceptical position. To these I turn now.

3.2 Is Scepticism Coherent?

Let us examine these preliminary justificationist replies to scepticism. Harald Thorsrud (2004, Introduction) documents what he describes as the "two most frequently made objections" to skepticism. The first, and perhaps most popular, response to the sceptic is the assertion that scepticism is incoherent:

> ...the skeptic's commitment to our epistemic limitations is inconsistent. He cannot consistently claim to know, for example, that knowledge is not possible; neither can he consistently claim that we should suspend judgment regarding all matters insofar as this claim is itself a judgment. Either such claims will refute themselves, since they fall under their own scope, or the skeptic will have to make an apparently arbitrary exemption.

In other words, it is claimed that scepticism cannot be stated consistently—it is self-contradictory, or "a self-nullifying paradox", to quote the biologist Paul Odgren (in Miller, 2006a, p.152). This criticism, which was originally levelled at the ancient Greek sceptics, is also prevalent in contemporary philosophy. For example, Ludwig Wittgenstein, in the *Tractatus Logico-Philosophicus* (1918/1922, Proposition 6.51) asserts that scepticism is "obviously nonsensical", while P.F. Strawson claims that the sceptic sets a standard for knowledge that is "self-contradictorily high" (1959, p. 34).

Admittedly, if scepticism is so formulated as to assert that "no proof or justification is possible", whilst also claiming that *that* statement is proven or justified, then this justificationist criticism would have much force. However, it is simplicity itself to formulate scepticism so as to avoid formal inconsistency.[5] This can be done in two ways.

The most simple device, which we have seen to be more often associated with Academic scepticism, is simply to make an exception for the sceptical doctrine itself, so that it can be expressed as "nothing can be proved nor justified *except* the fact that nothing can be proved nor justified", or something along similar lines.[6] Clearly, this succeeds in avoiding the inconsistency. Nor need this strategy be completely arbitrary, or a case of special pleading. For instance, Humean scepticism is of this type—Hume asserts that knowledge is possible of both immediate sensory impressions and of logical truths, yet maintains scepticism regarding knowledge of the external world. Thus, even if a comprehensive scepticism *was* self-contradictory, this would not invalidate more restricted formulations, where it could be argued that scepticism is *known* upon the basis of unquestioned logical or egocentric knowledge. Exceptions to the scope of scepticism, therefore, need not *necessarily* be arbitrary.

However, the more thorough-going form of ancient scepticism, usually, but not exclusively, associated with Pyrrho, is *not,* in fact, self-contradictory either. This form rejects "negative dogmatism"—it claims that not even the sceptical doctrine can be proven or 'known' in the sense of being *justified* knowledge. Indeed, this actually seems to be the more common variant of ancient scepticism. Cicero reports concerning the Academic Arcesilaus, for example, (*Academics* I 45): "he denied that there is anything which can be known - not even the one thing which Socrates allowed himself, that he knew that he knew nothing." This self-referential form of scepticism was also expressed by Democritus student Metrodorus of Chios: "None of us knows anything, not even this, whether we know or we do not know; nor do we know what 'to not know' or 'to know' are, nor on the whole, whether anything is or is not" (Cicero, *Acad.* 2.73).

Again, there is no logical inconsistency in this assertion. The only thing this anti-sceptical argument makes plain is that *if* scepticism is true, then it is unjustified. Yet, unless truth is conflated with *justified truth* a statement may be true without being in any way supported. That is, a conjecture may have a truth value—be either true or false —without evidence either way. Indeed, if we assume the truth of the principle of the excluded middle, and take care to eliminate from consideration such ambiguous terms as indexicals, *every* propositional statement in a language will either be true or else its negation will be true.[7] Thus, the truth or falsity of a statement is completely independent of whether it is justified or not.

A variation of this incoherency argument runs as follows (Foley, 1998, § 6):

> ...in raising radical worries, would-be sceptics inevitably make use of the very intellectual faculties and methods about which they are raising doubts. In doing so, they are presupposing the general reliability of these faculties and methods. Hence, it is incoherent for them to entertain the idea that they might be unreliable.

That is, since the skeptic asserts that empirical knowledge, or even logical inference itself, cannot be justified, it is therefore illegitimate for her to use them to criticise the justificationist. Yet as Foley notes (ibid):

> ...this anti-sceptical argument... fails to appreciate that the argumentative strategy of sceptics can be entirely negative. Their aim need not be to establish any positive thesis, not even the thesis that our faculties and methods are untrustworthy. Rather, their strategy can be to assume for the sake of argument that our favourite faculties, procedures and methods are reliable, and then to illustrate that these faculties, procedures and methods, if applied rigorously, will generate evidence that undermines their pretence of reliability.

Indeed, this form of argument is entirely legitimate—it is simply a form of the dialectical method of Socrates, which is itself just a variant of the *reductio ad absurdum*. Vogt (2010, § 3.1) recounts how Arcesilaus employed this method: "[i]t proceeds by asking one's real or imaginary interlocutor what they think about a given question, then plunging into an examination of their views, employing their premises. Can they explain their position without running into inconsistencies, and without having to accept implications that they want to resist?" The sceptic is not thereby committed to knowledge of their opponents' premises. As a practical matter, it may be necessary to first assume, *for the sake of argument*, the truth of a proposition in order to derive a contradiction from it, but this can hardly be said to negate the criticism. As Musgrave (1993, pp. 20-21) states along the same lines, the "traditional sceptic response... runs in terms of using the weapons of your opponent to beat your opponent, without believing that these weapons have any special potency."[8]

Yet, the justificationist might respond, if a knowledge claim is unjustified, why need we even consider it? Or, in other words, if it is unjustified, then why assert it? Yet this is question-begging—it assumes justificationism is true. Here again it seems that there is a conflation between truth and justified truth. Put simply, if truth is the goal of inquiry, then scepticism (non-justificationism) deserves serious consideration simply because it stands up better to criticism than its competitor (justificationism). Indeed, scepticism is so perennial a philosophical doctrine precisely because of its logical *strength*. Far from being self-contradictory, scepticism is, as Russell once stated, "logically impeccable" (1948, p. 9). A.J. Ayer has made similar concessions: "[n]ot that the sceptic's argument is fallacious; as usual his logic is impeccable" (1956, p. 68), and "[h]ere again the sceptic makes his point. There is no flaw in his logic" (ibid, p. 75).

Why, then, does such a weak argument against scepticism enjoy such wide currency? The likely explanation seems to be that these critics confuse scepticism with *relativism*,[9] which is indeed, in most formulations, inconsistent. Although relativism is a protean doctrine it may be said to, characteristically, assert that there is no *absolute* truth. Hence, it is either self-contradictory if it is itself taken to be an absolutely true statement, or has no hold on our interest, since it is, by its own lights, just one of several irreconcilable and arbitrary opinions. Yet relativism is a completely distinct position from scepticism.[10] Katja Vogt (2010, § 4.2) is quite emphatic about this distinction between scepticism and relativism:

> Relativism, as envisaged in Plato's *Theaetetus*, looks at a similar range of phenomena. Things appear different to different kinds of animals; to different people; and so on... In the *Theaetetus*, the world dissolves into radical flux: there are no stable items with stable properties that we both refer to...
> The Ten Modes... implicitly rely on the intuition that there are stable items with stable properties. Of course, the skeptic is not committed to the thesis that opposites cannot hold of the same thing, and that therefore no two conflicting appearances can be true. However, the modes presuppose a common sense metaphysics that does not accommodate faultless disagreement. In all cases of disagreement, only one of us can be right.

Thus, skepticism is a thesis concerning the impossibility of *justification*; it does not deny absolute truth, or realism, or the possibility of rationality, or the validity of the principle of non-contradiction. Indeed, it doesn't even go as far as to assert the impossibility of (unjustified, conjectural) knowledge, but only the impossibility of *epistêmê*. As Miller (2006a, p. 134) states, scepticism asserts simply that "nothing that we call knowledge, science included, is known with any authority... we cannot know anything in the traditional sense of the word (very crudely: knowledge = justified true belief)." This does not rule out the possibility of objective demarcation amongst knowledge claims, or the possibility of progress in the sciences. Indeed, these interpretations seem inconsistent with what the original sceptics explicitly propounded; that is, rational inquiry in the search for truth. As Vogt (ibid, § 1) asserts:

> "*Skepsis*" means investigation, and ancient skepticism is perhaps best described as a deep and persistent commitment to investigation... The skeptic is committed to a search for the truth, on virtually all questions...aiming at the truth includes two aims: to accept truths, and to avoid

falsehoods. The Modes are tailored to keep us from assenting to something that could be false. Insofar as the skeptic's effort to avoid falsehoods expresses a valuation for the truth, the skeptic might be a genuine investigator.

And again (§ 3.1):

To Arcesilaus, the skeptical life is a life lived following reason, a life based *on* reason...

Despite these facts, scepticism is often taken to entail relativism or irrationalism.[11] This seemingly invalid inference may perhaps, since it is so commonly made, be better understood as an instance of a deductive enthymeme.[12] What premise has been elided? The unstated assumption is, I suggest, some variant of justificationism—the ideal that genuine knowledge is *secure* knowledge. Indeed, justificationist assumptions are, in any case, sufficient to allow the derivations. If we take scepticism to assert that justification is impossible, and conjoin this with some typical justificationist equations (see chapter 2) we obtain the following table:

Table of Assumptions:

Scepticism ('No justification is possible') and...	Entails...
Objective or absolute truth = justified truth	No objective truth
Rational opinion = justified opinion	No rational opinion
Objective knowledge = justified knowledge	No objective knowledge
Objective theory adjudication = justificationist adjudication	No objective theory adjudication

Thus, it is only the conjunction of the two doctrines—scepticism and justificationism—which has such disastrous results for the demarcation program and rationality in general. Yet, given that scepticism is "logically impeccable", may we not reject justificationism instead? In the next two chapters I will try to further illustrate the logical strength of scepticism *vis*-à-*vis* justificationism. The final chapter of the dissertation will aim to erect a better theory of demarcation in its place.

3.3 Is Scepticism Possible?

Returning to Harald Thorsrud's survey (2004, Introduction) of the most widespread objections to scepticism, the second major objection cited concerns the thesis common, though not universal, among the classical sceptics—the commendation of the suspension of judgment (*epochê*):

> The second sort of objection is that the alleged epistemic limitations and/or the suggestion that we should suspend judgment would make life unlivable. For, the business of day-to-day life requires that we make choices and this requires making judgments. Similarly, one might point out that our apparent success in interacting with the world and each other entails that we must know some things.

There seem to be several distinct criticisms to be distinguished here. One argument seems to be that belief is necessary if practical action is to be undertaken at all. As such, this is close to Hume's assertion that the adoption of Pyrrhonianism would have the effect that, "[a]ll discourse, all action would… cease; and men remain in a total lethargy, till the necessities of nature, unsatisfied, put an end to their miserable existence" (1748, p. 160). Christopher Cherniak (1998, § 2) deduces similar consequences of a universal suspension of judgement:

> Descartes recommends a method of universal doubt, reconstructing our entire belief system from a blank slate… However, history's majority verdict seems to be that universal doubt is not in fact advisable, that it would leave us in an epistemological tragicomedy of total, irreversible cognitive paralysis, which is rarely a rational option.

A closely related criticism was made by Russell: "Scepticism, while logically impeccable, is psychologically impossible, and there is an element of frivolous insincerity in any philosophy which pretends to accept it" (1948, p. 9).[13] Similar remarks have been made by Hamlyn (1970, p. 9)—"our skeptic is a hypothetical animal"—and by David Armstrong (1973, p. 218)—"It is hard to take such a problem seriously"; "the sceptic's position is even more arbitrary than this…" It seems that something like this criticism was also made by the Stoics, on the basis of their presumption that there can be no action without (positive) assent (Inwood,1985). Jonathan Barnes (1998, § 2) rehearses this argument colourfully:

> One argument which ancient dogmatists deployed against their sceptical colleagues was the following. Human actions are characteristically explained in terms of the beliefs (and the desires) of their agents. But sceptics have no beliefs. Hence sceptics cannot act. Hence sceptics cannot live - the only good sceptic is a dead sceptic.

My reply to these criticisms is simple. As stated earlier, scepticism as understood here is a purely *epistemological* thesis which asserts only that none of our beliefs are *justified*. It says nothing about psychological doubt and has no logical connection to a blanket suspension of belief. Scepticism, as an epistemological thesis, should not be confused with any psychological assertion about doubt. Thus, this criticism is largely irrelevant.[14] As Miller (2006, p. 150) points out, scepticism does not recommend "universal suspension of judgement, unless it is joined to the ruinous doctrine that all rational opinion is justified opinion." And as Watkins notes (1984, pp. 11-12), the denial of epistemological justification is not the same as a recommendation to purge yourself of all judgements:

> ...a sceptic is not obliged to try to anaesthetise his personal beliefs... but he is obliged to declare that he cannot justify them... [scepticism is] ... analogous to a moral philosophy that says that there can be no objective justification for any moral system. A moral philosopher who holds this view is not thereby obliged to purge himself of his own moral preferences, though he is obliged to admit that he cannot offer any rational defence of them.

Thus, *unjustified belief* is a perfectly viable psychological state. Watkins quotes George Santayana's essay on scepticism in this regard, and his endorsement of "the animal faith I live by from day to day" (1923, p. 308). Epistemological justification is not *necessary* for assent—indeed, assent without justification seems to be the usual case. As such, it seems that Sextus' response (*PH*, I, 23-24) to this charge of "apraxia" is completely adequate:

> Thus, attending to what is apparent, we live in accordance with everyday observances, without holding opinions—for we are not able to be utterly inactive. These everyday observances seem to be fourfold, and to consist in guidance by nature, necessitation by feelings, handing down of

laws and customs, and teaching of kinds of expertise. By nature's guidance we are naturally capable of perceiving and thinking. By the necessitation of feelings, hunger conducts us to food and thirst to drink. By the handing down of customs and laws, we accept, from an everyday point of view, that piety is good and impiety bad. By teaching of kinds of expertise we are not inactive in those which we accept. And we say all this without holding any opinions.

Fogelin also satisfactorily rebuts this line of anti-sceptical criticism. It is (1994, p. 7):

...not only wrong, but deeply wrong... [it] simply misrepresents the dialectical character of the Pyrrhonian attack on the dogmatists. The Pyrrhonist does *not* hold the view that judgments may not be made in the absence of a criterion of truth. That is a view held by the dogmatists, for example, the Stoic epistemologists who attempted to formulate such a criterion of truth. If the argument from the criterion is correct, it will have as a consequence that the *dogmatist* ought to suspend judgment on her dogmatic philosophical beliefs, and also on her ordinary beliefs, for, as Barnes rightly notes, the argument applies equally to both. But this leaves the Pyrrhonist untouched, for it is no part of his position to suppose that judgments may only be made on the basis of a criterion of truth. Not to see this is not to see what Pyrrhonian skepticism (whether it is right or wrong—persuasive or unpersuasive) is all about.

However, there also seems to be the suggestion in this criticism that not only is universal *doubt* impossible or impractical, but also the distinct assumption that unless knowledge claims are justified, then any *practical success* resulting from them would be inexplicable. Justification may not be needed for action, but might it be necessary for *successful* action? As Thorsrud (ibid.) writes "one might point out that our apparent success in interacting with the world and each other entails that we must know some things." The argument thus appears to be that unless a statement is justified, which the sceptic denies, it is useless for practical purposes—a theory must be justified to be successfully applied.

This, however, is obviously false. Indeed, a statement need not even be true to be successfully applied in practical action—false theories generally have a number of true consequences. A noteworthy example is Newtonian mechanics, which, at least to a certain degree of precision, made numerous true predictions regarding the movement of both heavenly and terrestrial bodies. Moreover, a completely unjustified, albeit true, theory will be *just as useful* as one that is *proven* true. It is a fact that theoretical knowledge is necessary to make predictions about the future, but the usefulness of any theoretical claim does emphatically *not* depend on its evidential *support* or *justification*. Instead what matters is its *truth-content*, understood either in terms of verisimilitude, or, more sceptically and perhaps more feasibly, in terms of empirical adequacy. That is, what matters is whether, and to what extent, the theory corresponds to reality.[15] Miller (2006a, p. 148) goes as far as to call this the "most mischievous error in the theory of knowledge since Plato, this presumption that unjustified opinions are rationally worthless." He goes on to note:

> a true hypothesis, or one that is a good approximation to the truth, even if not in the least justified, is valuable, though of course we may not know that it is true, or how valuable it is. The entire non-human animal kingdom depends for its survival on genetically encoded truths that they have no ground whatever for supposing to be true.

Indeed, it is *true* knowledge that we seek, and a conjectural truth will serve just as well—that is, will be just as reliable, in the sense of not entailing falsehoods—as one that is purportedly justified. As long as the spatio-temporally universal generalisations of science are *true*, then the predictions derived from them (when supplemented with true auxiliary assumptions) will also be true. No additional epistemological support is necessary. Thus, a denial of *justified* knowledge is not the denial of verisimilitude, or of reliability in the non-epistemological sense. Only such a denial would allow this last criticism of scepticism some plausibility.

To conclude this discussion of these preliminary objections to scepticism, I will repeat that although sceptics, such as Pyrrho, are also sometimes associated with such *psychological* doctrines as the suspension of belief (not to mention such ethical doctrines such as the attainment of the state of mind known to Greek philosophers as *ataraxia* (tranquility), or such metaphysical doctrines as the "metaphysics of indeterminacy" (Bett, 2000)), these details are not logically connected to the key sceptical criticisms of justificationism. Nor should the suspension of (dogmatic) *judgement* be taken to advocate the suspension of *inquiry* or a disinterest in truth or theory evaluation. The denial of justified knowledge—*epistêmê*—is not the denial of conjectural knowledge (*doxa*). Some opinions may be better than others—more truth-like—despite the fact that none are in any way justified.

3.4 The Trilemma of Justification

I now turn to what I consider to be the most significant sceptical argument against the possibility of a justificationist solution to the demarcation problem—namely, the claim that any attempt to do so will inevitably meet with the following trilemma: either infinite regress, circular reasoning, or dogmatic (unjustified) commitment. That is, any attempt to give reasons sufficient to justify a proposition necessarily leads to:

1. Further demands for justification and hence an infinite regress (*regress ad infinitum*), or
2. A viciously circular justification which assumes what is to be justified (*circular reasoning*), or
3. An attempt to cut short the infinite regress and avoid circularity by "grounding" our knowledge upon an unjustified authority (*mere assumption*).

This trilemma is most often associated with the obscure[16] 1st/ 2nd A.D. Pyrrhonian Agrippa (Barnes 1990, Hankinson 1994)—the horns constitute three of the five "Modes" attributed to him by Sextus Empiricus. Thus, in the *Outlines of Pyrrhonism*, Sextus gives the following account of the Five Modes (*PH*, I, XV):

> The more recent Sceptics offer the following five modes of suspension of judgement: first, the mode deriving from dispute; second, the mode throwing one back *ad infinitum;* third, the mode deriving from relativity; fourth, the hypothetical mode; fifth, the reciprocal mode. According to the mode deriving from dispute, we find that undecidable dissension about the matter proposed has come about both in ordinary life and among philosophers. Because of this we are not able either to choose or to rule out anything, and we end up with suspension of judgement. In the mode deriving from infinite regress, we say that what is brought forward as a source of conviction for the matter proposed itself needs another such source, which itself needs another, and so *ad infinitum,* so that we have no point from which to begin to establish anything, and suspension of judgement follows. In the mode deriving from relativity, as we said above, the existing object appears to be such-and-such relative to the subject judging and to the things observed together with it, but we suspend judgement on what it is like in its nature. We have the mode from hypothesis when the Dogmatists, being thrown back *ad infinitum,* begin from something which they do not establish but claim to assume simply and without proof in virtue of

> a concession. The reciprocal mode occurs when what ought to be confirmatory of the object under investigation needs to be made convincing by the object under investigation; then, being unable to take either in order to establish the other, we suspend judgement about both.

Thus, in the terminology of Sextus, we have the trilemma as: a). infinite regress (second mode), b). circularity (fifth mode), or c). hypothesis (fourth mode). Vogt (2010, § 4.3) summarised the modes of the trilemma as follows:

Second Mode: *Eis apeiron ekballonta*

> Arguments that throw one into an infinite *regress*. That which is brought forward to make a given matter credible needs yet something else to make *it* credible, and so on *ad infinitum*. Since we thus have no starting point for our argument, suspension of judgment follows.

Fourth Mode: Hypothesis

> Someone makes an assumption without providing argument. A dogmatist, if thrown back into an infinite regress of arguments, just assumes something as a starting-point, without providing an argument (*anapodeiktôs*). We suspend over mere hypotheses—they could be false, opposite hypotheses could be formulated, and so on.

Fifth Mode: *Ton diallêlon*

> Arguments that disclose a *circularity*. This mode is used when that which ought to confirm a given investigated matter requires confirmation (*pistis*—credibility) from that matter. We are unable to assume either in order to establish the other. We suspend judgment on both.

Notably, and in contrast with the earlier Ten Modes presented in Sextus, which sought to show the *contingent* unreliability of knowledge claims, these modes are formal, and concerned primarily with a *logical* shortcoming of the justificationist program. That these modes were put forth in reference to the demarcation problem is also made plain by Harald Thorsrud (2004, c, iii) in his *Internet Encyclopaedia of Philosophy* entry on "Ancient Greek Skepticism":

> Agrippa's Five Modes relies [sic] on the prevalence of dispute and repeats the main theme of Aenesidemus' Modes: *we are frequently faced with dissenting opinions regarding the same matter and yet we have no adequate grounds on which to prefer one view over another.* (Emphasis added).

And as R. J. Hankinson (1998b) writes in an article on Agrippa:

> ...the views of positive theorists are subject to endemic disagreement due to the relativity of appearances, and adjudication cannot succeed, since it will either be mere assertion (and hence will not command assent) or appeal to further considerations, which process will either be infinitely regressive or circular, or terminate in unfounded assumption.

Indeed, the Five Modes of Agrippa are pleasingly complete, for two of the modes —discrepancy and relativity—*raise* the demarcation problem; they are challenging modes, in Fogelin's phrase (1994, p. 116). The two modes "trigger a demand for justification by revealing that there are competing claims concerning the nature of the world we perceive. Given this competition, it would be epistemically irresponsible for the [justificationist] to choose *without argument* one of these competing claims over the others. Thus the modes of discrepancy and relativity force anyone who makes claims beyond the modest expression of opinion to give reasons in support of these claims" (ibid). On the other hand, the remaining modes—"those based on regress *ad infinitum*, circularity, and (arbitrary) hypothesis—show that it is impossible to complete this reason giving process in a satisfactory way. If the Pyrrhonists are right, no argument, once started, can avoid falling into one of the traps of circularity, infinite regress, or arbitrary assumption" (ibid).

A structurally identical trilemma has also been dubbed, by Hans Albert (1968/1985, p.16ff), the *Münchhausen trilemma.* The name is inspired by the story of a German nobleman, Baron Münchhausen, who claimed to have extracted both himself and his horse from a swamp by pulling on his own hair. Another variation—*Fries's Trilemma*—was presented by J. F. Fries in 1828-31 in relation to the status of observation statements. As Gerhard Vollmer states, this trilemma is "all-pervasive in justificational contexts. We may spot it in traditional epistemologies (except, of course, skepticism and agnosticism). It obtains for the definition of concepts, for the derivation of statements, for the validation of norms, for the justification of values" (1987, p. 175). Albert is also quite emphatic that this trilemma applies, not only to deductive or sufficient justifications, but also to inductive, causal, and transcendental justificatory arguments—indeed, to any justificatory argument whatsoever, and both for factual statements and for value judgements. That is, the target of the trilemma is not only *proof*, as is sometimes supposed, but also partial support. As Vogt (ibid) notes:

> ...the so-called formal modes... might not be narrowly concerned with proof, but rather with everything that can lend credibility to something else. Consider Regress... the first of the formal modes. The text does not actually speak of proof (*apodeixis*)... 5-4 is the only place in Sextus' report of the Five Modes that uses a cognate of *apodeixis*... Sextus' language is wider: the mode deals with everything that can make something else credible... their target might include what we would call inductive reasoning and causal explanations...Taken together, the Five Modes deny all "proof, criterion, sign, cause, movement, learning, coming into being, and that there is anything by nature good or bad." (DL 9.90). This is notably more than just proof.

Of course, this trilemma does not exhaust the arguments offered by sceptics, but it is, I think, the most important because of its absolute *generality.* As Hankinson (1998b) asserts, "the whole subsequent history of the epistemology of justification may be seen as a series of attempts to evade [the trilemma's] purportedly all embracing grasp." Accordingly, the three main approaches to justification—foundationalism, coherentism, and infinitism—each correspond roughly to a horn of the trilemma. All three share the justificationist approach to the demarcation problem; they all assume that in order to adjudicate between theories we must be able to *justify,* to some degree —either partially or completely—that theory to which we give preference.

Let us examine each of the horns in turn.

3.5 Justificationist responses to the Trilemma I- Infinitism

Of the three basic approaches, foundationalism and coherentism are by far the most popular. Indeed, until quite recently it seems that no one, except, perhaps, C. S. Pierce (Aikin 2011, pp. 80–90), had seriously considered that an infinite chain of justificatory reasons was even worth exploring. *Infinitism*, as this position has been called, is not so much as mentioned in passing by Greco, for instance, in his discussion of the "project of vindication" (2007, p. 175): "Foundationalism and coherentism are historically tied to the project of vindication – both theories arise in response to Pyrrhonnian concerns about a regress of justification, and about the possibility of demonstrating that one has knowledge." However, this binary opposition between foundationalism and coherentism is becoming increasingly outdated,with infinitism attracting considerable attention in contemporary scholarly debates.

To repeat, the infinite regress argument relies on the fact that every proof relies upon an inference, and an inference requires premises. Yet if these premises are not proved, then neither is the conclusion. Any attempt to prove the premises initiates an infinite regress of justification. Of course, for the conclusion of a valid argument to be true it is sufficient that the premises are, in fact, true. But in order for the conclusion to be *proven or demonstrated to be true*, the premises themselves need to be proven or demonstrated. (This is the difference between a derivation and a demonstration). Should a dogmatist offer an account of such grounds, the sceptic may then request further justification, making a regress unavoidable. The inevitable result is that there can be no *justified* knowledge, for *the justifying grounds themselves need grounds*, the provision of which is impossible.

This regress seems to be unacceptable for any justificationist attempt to provide a demarcation criterion, which clearly requires a *practical* solution in order to effectively adjudicate between theories. Aristotle, to take an example, rejected the possibility of infinite chains of reasoning emphatically, stating in the *Posterior Analytics that* "one cannot traverse an infinite series" (72b5–14). Hume, too, rejected summarily the possibility of such reasoning:

> If I ask you why you believe a particular matter of fact which you relate, you must tell me some reason; and this reason will be some other fact connected with it. But as you cannot proceed after this manner *in infinitum*, you must at last terminate with some fact which is present to your memory or senses or must allow that your belief is entirely without foundation. (1748 , Section V, Part I)

However, despite such pronouncements, it has more recently been suggested (in, for example, Peter Klein's "Human Knowledge and the Infinite Regress of Reasons," 1999), that an infinite series of reasons can, despite appearances, provide an acceptable justification for knowledge claims. Klein is no doubt the most prominent defender of this theory, although John Turri has also published an impressive number of articles (see, for instance his, 2009 and 2012) elaborating the position. Infinitism shares with foundationalism and coherentism the tenet that knowledge requires a justification condition. Like these more established positions infinitists "agree that knowledge or justification requires an appropriately structured chain of reasons" (Turri and Klein, 2014, p. v). In contrast to those other positions however, infinitists construe the structure of an adequate justificatory chain to be a) *non-repeating* (in contrast to coherentism), and b) *infinite* (in contrast to foundationalism). Knowledge claims, in other words, can purportedly be justified by an non-repeating, infinite chain of reasons (ibid).

Klein describes infinitism as follows (2010, p. 162):

> I have developed and defended infinitism as the solution to the epistemic regress problem. Infinitism is the view that there is no last member in the set of justificatory reasons for our beliefs. Of course, in any actual reason-giving session, we do stop giving reasons. So the claim is not that we don't stop. Of course we do… we justify a belief by providing reasons for it. But justification comes in degrees, and the infinitist will suggest that the further along the path of reasons we have traveled, the more justified the proposition becomes. An infinitist can argue that on some occasions we shall have traced the path of reasons far enough to reach the threshold of the strength of justification required to satisfy… the definition of knowledge.

There are, I think, numerous problems with this theory if it is taken to be pertinent to theory adjudication. An immediate problem infinitists face is that the main argumentative strategy employed to endorse infinitism simply *assumes* that a justificationist response to the demarcation problem is both possible and necessary. Turri and Klein, for instance, begin their recent collection of essays on the subject by stating that "one of the goals of reasoning is to enhance the justification of a belief" (2014, p.1), and then proceed to outline their basic "*argument pattern* for infinitism" (ibid, p.3):

> 1. There are three possible, non-skeptical solutions to the regress problem: foundationalism, coherentism and infinitism.
> 2. There are insurmountable difficulties with two of the solutions (in this case, foundationalism and coherentism).
> 3. The third view (in this case, infinitism) faces no insurmountable difficulties.
> 4. Therefore, the third view (in this case, infinitism) is the best non-skeptical solution to the regress problem.

Such an argument for infinitism will only have force for a reader who has already rejected scepticism- that is, to a reader *within* the justificationist context. For the sceptic, this argument assumes the main point at issue. However, the sceptic can at least take some satisfaction that the strongest premise, (premise 2- that both foundationalism and coherentism are faced with insurmountable difficulties), is essentially an employment of two of the horns of Agrippa's trilemma. That is, coherentism is rejected on the grounds of circularity (ibid, p. 1): "No reason [for a belief, Q] can be Q itself, or equivalent to a conjunction containing Q as a conjunct. That is, circular reasoning is excluded." Such reasoning, Turri and Klein assert, in agreement with the sceptics, cannot "improve the justificatory status of a belief... circular reasoning begs the question by positing the very propositional content of the belief whose justificatory status the reasoning is designed to enhance" (ibid). Infinitism likewise adopts the traditional sceptical argument against foundationalism; that is, foundationalist accounts are rejected due to "the specter of arbitrariness... infinitists deny that there is any reason which is immune to further legitimate challenge. And once a reason is challenged, then on pain of arbitrariness, a further reason must be produced in order for the challenged reason to serve as a good reason for a belief" (ibid, p. 2).

How, then, do infinitists envision the purported justification of beliefs, having rejected both coherentism and foundationalism as justification enhancing? The answer lies in the reason giving process itself (ibid, p. 2):

> ...infinitism takes reasoning to be a process that generates an important type of justification—call it "reason-enhanced justification." In opposition to foundationalism, reasoning is not depicted as merely a tool for transferring justification from the reasons to the beliefs. Instead, a belief's justification is enhanced when sufficiently good reasons are offered on its behalf. Such enhancement can occur even when the reasons offered have not yet been reason-enhanced themselves... reasoning can generate epistemic justification.

In other words, infinitists view epistemic justification to be "a property of entire sets of beliefs... the inferential relationships among beliefs in a set of propositions generates a justified set of beliefs; individual beliefs are justified merely in virtue of being members of such a set" (ibid). This position is, as Turri and Klein note, shared by holistic coherentism, and it is open to similar objections. For instance, Scott Aikin

(2005, pp. 198–9; 2008 pp. 182–3) has put forward an argument quite similar to the "alternative systems" objection to coherentism (see § 3.6 below) that has been dubbed "the AC/DC objection". Essentially, this argument asserts that for any proposition, both an infinite affirmation chain (AC) of reasons, and an infinite denial chain (DC) of reasons can be (potentially) constructed, leading to paradoxical results. Such cases show the inadequacy of infinitism, at least as currently understood, to address problems of theory adjudication, and has led Aikin, in his (2008), to reinstate some components of foundationalism.

Turri and Klein go on (2004, pp. 12-16) to list some further common criticisms of infinitism, in addition to Aikin's AC/DC objection. The first, which they attribute to Aristotle, simply cites the finiteness of human minds and critical discourse as a barrier to any infinitist theory of justification—an infinite chain of reasons is (seemingly at least) psychologically impossible. The infinitist response to this "finite mind" objection is to refrain from claiming the existence of *actual* infinite chains of reasons; what will suffice, it is claimed, is "that we must have an appropriately structured, infinite set of reasons available to us" (ibid, p.13). This reply, however, is open to the "proof of concept" objection (ibid)—no really plausible candidate for the kind of infinite chain of reasons that infinitists posit has been produced.

Further technical objections may be raised against infinitism. One problem is specifying, in a non-arbitrary fashion, just how long a justificatory chain, which is the key determinate of a theory's acceptability for the infinitist, is needed to achieve "adequate" justification or "knowledge" (a term which, as we have seen, has no uncontroversial definition). Related to this, there is also the problem of measuring the *length* of the justificatory chain in a way that is objective and language independent, given that a justificatory chain with the same content can be formulated in various different ways. More fundamentally, the doctrine that inferential procedures can, by themselves, confer epistemic justification on a proposition is suspect, and has been rejected both by sceptics and by their foundationalist rivals. Jonathan Dancy, for instance (1985, p. 55), writes that:

> Suppose that all justification is inferential. When we justify belief A by appeal to belief B and C, we have not yet shown A to be justified. We have only shown that it is justified if B and C are. Justification by inference is conditional justification only; A's justification is conditional upon the

> justification of B and C. But if all justification is conditional in this sense, then nothing can be shown to be actually non-conditionally justified.

This objection, dubbed by Turri and Klein the *"unexplained origin"* objection, is also attributed to Aristotle (2014, p. 5). Finally, however, and most importantly, there does not seem to be any plausible connection between how *many* reasons are produced in a justificatory chain and the truth-value of the original hypothesis. Infinitism is thus, I think, unable to provide any assistance in rational theory adjudication. Either the chain of reasons is stopped arbitrarily, when the infinitist deems that "knowledge" has been achieved, or objective adjudication is infinitely deferred. Yet a purported justification, to have any demarcational force, must be actually *produced*; it must be *accessible*.

3.6 Justificationist responses to the Trilemma II- Coherentism

Might, in that case, circular reasoning allow us to adjudicate in a disputed case? *Prima facie*, it seems not. The vast majority of philosophers since Aristotle (*Prior Analytics* II, 16) have maintained that circularity, or "begging the question", is indeed a problem, and constitutes fallacious reasoning (cf. Walton, 1991). As noted by Aristotle, this is a "simple way of proving anything" (*Posterior Analytics*, I, iii, 73a5)—any conclusion can be "justified" if it is itself allowed to function as a premise in the justificatory argument. No doubt the most notorious example of a circular "justification", or *petitio principii* in the history of epistemology appears in Descartes's Meditations. There Descartes attempts to establish that his "clear and distinct" criterion of truth is reliable by appeal to the "clear and distinct", and hence veridical conception of a non-deceiving God. The vicious circularity of this argument was quickly pointed out by Antoine Arnauld (1641, p. 32):

> I have one further worry, namely how the author avoids reasoning in a circle when he says that we are sure that what we clearly and distinctly perceive is true only because God exists.
> But we can be sure that God exists only because we clearly and distinctly perceive this. Hence,

> before we can be sure that God exists, we ought to be able to be sure that whatever we perceive clearly and evidently is true.

Yet Descartes is not alone in his resort to circularity; as Daniel Garber (1998, § 7) states, the "problem of circularity... is not a superficial oversight on Descartes' part. It is a deep philosophical problem that will arise in some form or another whenever one attempts a rational defence of reason." (Another prominent example of the resort to circular justification, concerning the problem of induction, will be addressed in the next chapter.)

Descartes did not dispute that circular reasoning was problematic. However, there is a significant group of justificationist epistemologists (BonJour, 1985; Davidson, 1990; Lehrer, 1974; Elgin, 2005; Kvanvig, 2003) who, in some form or other, maintain that a circular series of reasons *is* legitimate. This subset of justificationism is called *coherentism*. Michael Huemer (2010, pp. 22-23) gives the following general characterisation of the coherentist position:

> The coherence theory of justification locates the source of all justification for belief in the relation of *coherence*. Typically, a system of beliefs is said to cohere well when it is consistent, many of the beliefs in the system are mutually supporting (that is, entail each other or render each other probable), and the system contains few or no anomalies (claims that have no explanation within the system)... While foundationalists may grant that coherence plays a role in enhancing the justification of some beliefs, coherentists hold the stronger thesis that coherence can *by itself* provide justification for belief.

Thus, according to coherentism, "a belief or set of beliefs is justified, or justifiably held, just in case the belief coheres with a set of beliefs, the set forms a coherent system or some variation on these themes" (Olsson, 2012, Introduction). To use the usual metaphors, the structure of justification in this scheme is similar to a "web of belief" (Quine and Ullian, 1970) or like that of a raft which may have to be rebuilt on the open sea (Neurath 1983/1932). Crucially, in contrast to the foundationalist picture, no particular plank or strand of webbing is privileged—each has an equal epistemic standing. Justification then arises holistically—"what justifies our beliefs is ultimately the way in which they hang together or dovetail so as to produce a coherent set" (Olsson, ibid, § 1).

However, such an approach does not, I think, constitute an adequate answer to the demarcation problem—it fails for much the same reasons as infinitism. That is to say, 1). coherentism does not generally allow objective adjudication amongst consistent competing theories, and 2). coherence does not, in any event, have any logical connection to truth.

To examine the first objection a little more closely, we may distinguish, with Peter Klein (2010b, § 10), between two seemingly exhaustive varieties of coherentism —"warrant-transfer" coherentism and "warrant-emergent" coherentism. Of the former, which Klein attributes to Sosa (1980), and BonJour (1978), Klein is quite explicit that it fails to address the demarcation problem. He writes (ibid):

> The propositions in the circle might be mutually probability enhancing, but the point is that we could just as well have circular reasoning to the [opposite] conclusion... In this fashion anything could be justified—too simply! It is ultimately arbitrary which set of mutually probability enhancing propositions we believe because there is no basis for preferring one over the other.

Of the latter variety, in which "warrant for each proposition in the circle obtains because it is a member of a set of mutually probability enhancing propositions... [w]arrant emerges all at once, so to speak, from the web-like structure of the set of propositions", there are, once again, "too many competing circles that are coherent." To solve this problem, and to allow adjudication, Klein claims that both varieties of coherentism are forced to embrace foundationalism. Coherentism, then, is by itself incapable of fulfilling its task of indicating which "one of the two competing circles... is worthy of assent" (ibid). In the literature this has come to be known as the *alternative systems* objection (Olsson 2005, Ch. 10). Undoubtedly, knowledge claims must minimally be *consistent* with other propositions held to be true, and *in*consistency certainly calls for a revision *somewhere* in the system. But since there are alternative consistent systems (and alternative *coherent* systems, however this stronger relation is explicated), coherentism fails as a positive *justificatory* response to the demarcation problem.[17] In cases of competing explanations, both contestants may, with equal validity, use this strategy, and hence no objective demarcation is achieved.[18] As Olsson (2012, § 1) writes:

> For each coherent system of beliefs there exist, conceivably, other systems that are equally coherent yet incompatible with the first system. If coherence is sufficient for justification, then all these incompatible systems will be justified. But this observation, of course, thoroughly undermines any claim suggesting that coherence is indicative of truth.

Indeed, one of the most influential expositions of coherentism—that of Laurence BonJour's *The Structure of Empirical Knowledge* (1985)—explicitly moves away from the traditional assumption that it is *individual* hypotheses that are to be justified, but rather proposes that it is entire belief *systems*. Evidently, this problem shift to a more vague and abstract plane is not conducive to theory demarcation amongst competing hypotheses. Such "holistic justification", even if workable, would do little to solve the fundamental problem underlying justificationism—that is, theory adjudication. Moreover, BonJour's system, because it defines coherence in terms of a range of different aspects or "coherence criteria" (1985, pp. 97–99),[19] is operationally opaque on how one is even to adjudicate between *systems*, assuming that this concept can be adequately defined. This is because systems may rate differently depending on which specific criteria are employed, and Bonjour gives no indication on how to resolve this conflict to produce an overall coherence measure.[20]

Indeed, some recent theories that *purport* to be coherence theories of justification seem to have *de facto* given up the theory altogether. Keith Lehrer's system (e.g., Lehrer 2000 and 2003), for instance, while clearly motivated by the demarcation problem—his concern is with the acceptance of individual propositions, and he posits an "acceptance system" (later a more complex "evaluation system")—seems to abandon justification altogether, and instead settles for permitting acceptance of a theory just in case it has failed to be falsified or otherwise decisively criticised. As Olsson remarks (2012, § 4), "A critic may wonder what reasons there are for calling the relation of meeting objections to a given claim relative to an evaluation system a relation of coherence." In this vein, BonJour (1985, p. 101) had, quite plausibly, written that "nonlinear justification and the concept of coherence" were "arguably essential to a viable coherence theory." Lehrer's system thus seems to be a coherence theory in name only.

My second objection—that coherentism does not have any logical connection to truth—runs counter to coherentist claims that coherence can function as a criterion of truth. As Olsson asserts, coherence theorists typically claim that coherence is *truth* *conducive*, and so is highly relevant to theory adjudication. He writes (2012, Introduction- § 1):

> Modern coherence theorists... typically subscribe to a coherence theory of justification without advocating a coherence theory of truth. Rather, they either favor a correspondence theory of truth or take the notion of truth for granted... [T]his does not prevent many authors from claiming that coherence justification is an indication or "criterion" of truth...The fact that our beliefs cohere can establish their truth, even though each individual belief may lack justification entirely if considered in splendid isolation, or so it is thought.

Such an approach is quite evident in BonJour's exposition—he explicitly asserts (1985, p. 88) that "our concern is with coherence theories of *empirical justification* and not with coherence theories of truth", and further adds, "I am concerned here only with coherence theories that purport to provide a response to skepticism." This approach is also particularly conspicuous in Nicolas Rescher's *A Coherence Theory of Truth* (1973), in which his stated goal was to find a "criterial route" for evaluating theories; a "test", or "authorizing criterion" for truth (1973, pp. 1, 9-10), which was to provide "rational warrant" (p. 4) for truth claims. That is, he sought a justificatory procedure for theory selection from a set of competing "truth-candidates", to use Rescher's terminology. Coherence is meant to provide this. Yet this claim of truth conduciveness is made problematic by the argument in the literature known as the *isolation objection*.[21] The question, that is, is (Olsson, 2012, § 1):

> ...how can the mere fact that a system is coherent, if the latter is understood as a purely system-internal matter, provide any guidance whatsoever to truth and reality? Since the theory does not assign any essential role to experience, there is little reason to think that a coherent system of belief will accurately reflect the external world.

In other words, a pure coherence theory is problematic in that coherence doesn't seem to be logically connected to truth. This criticism has traditionally been hard to clinch, simply because of the vagueness and metaphorical character of many of the early theories of coherence justificationism. However, coherentism has recently been much more systematically formulated, building on the pioneering work of C. I. Lewis (1946). This work has, since the mid-1990's, succeeded in defining coherence quite precisely in terms of the well-established mathematical calculus of probability. Such research has, however, led to impossibility results that undermine the claims of coherentists that coherence is indicative of truth. Specifically, results by Peter Klein and Ted Warfield (1994), by Luc Bovens and Stephan Hartmann (2003), and by Erik Olsson (Olsson 2005, appendix B) have, on widely accepted and relatively uncontroversial assumptions, proved that no coherence measure can be truth conducive. As Olsson explains (2012, § 7):

> According to Klein and Warfield, just because one set of beliefs is more coherent than another set, this does not mean that the first set is more likely to be true. On the contrary, a higher degree of coherence can... be associated with a lower probability of the whole set. The idea behind their reasoning is simple: We can often raise the coherence of an informational set by adding more information that explains the information already in the set. But as more genuinely new information is added, the probability that all the elements of the set are true is correspondingly diminished. This... follows from the well-known inverse relationship between probability and informational content.

Thus, coherence is not truth conducive—a greater degree of coherence does not have any necessary connection to truth (to the extent that likelihood of truth can be measured by the probability calculus). BonJour, before the discovery of these results, had written concerning this "*problem of truth*... It must be somehow shown that justification as conceived by the [coherence] theory is *truth-conducive*, that one who seeks justified beliefs is at least likely to find true ones. The objection is simply that a coherence theory will be unable to accomplish this part of the epistemological task unless it also adopts a coherence theory of truth and the idealistic metaphysics which goes along with it" (1985, pp. 108-9). Given these results, it appears that this classic objection has been vindicated.

Finally, convincing proofs by Huemer (1997), Olsson (2002), and Van Cleve (2011) have also gone some way to establishing that coherence by itself cannot provide any justificatory support on the assumption that the *individual beliefs are themselves unjustified*. As such, coherentism must necessarily resort to some appeal to statements which have some probability *independent* of any coherence considerations. Whatever these are called—"supposed facts asserted" (Lewis, 1946), "truth-candidates" (Rescher, 1973), "cognitively spontaneous beliefs" (BonJour, 1985)—this amounts to a return to a weak foundationalism. In sum, it seems that not only can coherence not produce justification from scratch, as a pure coherentism would require, but increasing coherence also does not necessarily have a positive relationship with truth. Such criticisms have led to a return to the final horn of the trilemma—is foundationalism an adequate response to the demarcation problem?

3.7 Justificationist responses to the Trilemma III- Foundationalism

The most popular response to the regress argument, both for empiricists and rationalists, and which will be the focus of the following two chapters, has been *foundationalism*. Indeed, foundationalism has long been *the* de facto theory of justification, with alternatives only arising relatively recently. As BonJour admits (1985, p. 149) "the main motivation for a coherence theory is not any independent plausibility attaching to the idea that coherence is the sole basis for justification, but rather t*he untenability of foundationalism in all its forms*" (emphasis added). Foundationalism is a hierarchical-linear theory of justification that asserts that theories are to be adjudicated with reference to some objective, rational, and compelling foundation, which provides a secure basis to guarantee inferentially derived knowledge claims. The basic idea is that these foundations can stem the regress as their truth or reliability needs no justification—they are "indubitable", or "self-evident", or "clear and distinct", or "infallibly true", or "intrinsically credible", or otherwise self-justifying. Knowledge claims are justified relative to these secure authorities, and only such beliefs that are so sanctioned constitute genuine knowledge. "The foundationalist's task," Fogelin tells us "is... to identify some class of justified beliefs that are not justified by other beliefs, explain how they acquire their justified status, and then show how these beliefs can provide a foundation for at least a tolerably large portion of those other beliefs we count as knowledge" (1994, p. 117).

As noted in the last chapter, perhaps the clearest example of an empiricist foundationalist philosopher is Aristotle. For Aristotle, at least in the *Posterior Analytics,* scientific knowledge—*epistēmē*—is demonstrable knowledge. In order for a deduction to achieve the status of a demonstration, the premises must be necessarily true; they must be prior to and *better known* than the conclusions, and explanatory of the *derived* conclusions. Having rejected both infinite chains of reasons and circular justifications, Aristotle held that an adequate justification must appeal to such basic, foundational propositions. These primary principles of a science form the basis of all demonstrations, and cannot be themselves demonstrated. Hence Aristotle posits a non-demonstrative understanding (*nous*: *Posterior Analytics* II, 19) by which these ultimate principles are grasped. This is the original theory of induction.

This foundationalist account has been taken up by modern empiricist theorists of science, particularly under the influence of Francis Bacon. Sensory perception, on this narrative, is a source of immediate knowledge of the truth of observation statements, and on this basis other true beliefs can be established or justified inferentially. Indeed, it has often been held that the regress argument is an argument *for* foundationalism (rather than a *problem* for foundationalism)—that is, it is argued that, since knowledge-as-*epistēmē* is *obviously* possible, we can infer from the regress argument that some propositions are known, not by demonstration from further propositions, but in some non-inferential fashion. Fogelin (1994, p. 114) has noted the special pleading in this line of reasoning:

> In recent literature, what I am calling the Agrippa problem is often referred to as the infinite regress problem. I find this characterization too narrow, for the problem that presents itself is not simply that of avoiding a bad infinite regress; the challenge is to avoid this regress without falling into a bad form of circularity or a bad form of unjustified acceptance. Indeed, I think speaking of the infinite regress problem exerts a subtle, but strong, pressure on the discussion. If we think the threat of an infinite regress of reasons as the central challenge to justified belief, then theories, despite their own difficulties, may lay claim to our acceptance just because they seem to deal with this single aspect of the Agrippa problem... It is important, then, not to grant unwarranted dialectical advantages, but to insist, instead, that a philosophical theory of justification must simultaneously avoid involvement in a bad infinite regress, in a bad form of circularity, and in a bad appeal to unwarranted assumption. The Agrippa problem poses these challenges in an evenhanded way.

The key metaphor for foundationalism, as the name suggests, is that of knowledge as a building or edifice, but the justificatory structure is most helpfully viewed as modelled on (a particular theory of) mathematical proof. Specifically, it is modelled on the classical conception of Euclidean geometry—demonstrable knowledge based upon self-evident axioms. In order for a demonstration to succeed on this model, what is required is:

1. Self-evident premises.
2. Valid or truth-preserving inference.

Foundationalism faces two main sceptical objections. The first concerns the purportedly indubitable status of the premises and their non-inferential justification. Sceptics had denied the existence of any such basic beliefs—the senses are demonstrably subject to illusion, and hence cannot provide a secure basis for knowledge. Moreover, any attempt to provide a rationalistic criterion of right reasoning or truth will itself need to be validated by a further criterion, so initiating an infinite regress. The second problem concerns the *logical strength* of the premises and whether they are sufficient to guarantee even the most intuitively unproblematic of knowledge claims. That is, sceptics had disputed the legitimacy and justification of the methods that are supposed to *transfer* justification from the basic to the derived beliefs; even granting that the premises are secure it is dubious that they provide a sufficiently *broad* basis for scientific knowledge.[22]

The most famous objection of this second variety—the problem of induction—was posed by David Hume, who saw himself as a student of Pyrrhonian scepticism (Ainslie, 2003). Humean scepticism, particularly relevant to justificationist accounts of the nomological sciences, is not directed at the status of perceptual experiences, which Hume regarded as non-problematic. Rather, Hume's challenge is that, *even if* we accept reports of perceptual experience as needing no further demonstration, no logical inference about the unobserved may be made on this basis. That is, no universal statement or even singular prediction can be validly derived from experiential reports alone, and the attempt to supplement these premises to validate such an inference leads to an infinite regress. According to Hume, both conclusive and probable justification is on a par—all knowledge is equally unjustified. From observation reports accepted as true, we can validly derive *nothing* about the unobserved. There is no form of logical reasoning that allows the progression from perceptual experience to any genuine knowledge of an external world. No factual statement can be proved to even the slightest degree. It is to this Humean argument which we now turn in the next two chapters.

---

[1] There may also be a connection with the Greek word *ephektikos*, meaning "one who suspends judgment", but as I shall argue later in this chapter, this *psychological* recommendation is not logically connected to the *epistemological* thesis that justification is impossible.

[2] Thorsrud (2004, Introduction): "Although all skeptics in some way cast doubt on our ability to gain knowledge of the world, the term "skeptic" actually covers a wide range of attitudes and positions... "

[3] Cicero's *Academics* (45 BC) is one of the most important single sources on the sceptical or 'New' Academy, along with Sextus' later comparisons between Pyrrhonian and Academic scepticism in his *Outlines*.

[4] As Vogt (2010 § 4.3) notes, "It is a commonplace to say that, while the Ten Modes, as presented in Sextus, are concerned with conflicting appearances, the Five Modes are about argument or proof."

[5] Moreover, criticisms which rely for their cogency upon very strict formulations of the position they are attacking are usually weak. As Popper points out (2009, p. xix), regarding "the classic formulation of scepticism, "There is no universal criterion of truth", is far from being nonsensical: indeed, scepticism in this sense is a true theory." This thesis concerning a criterion of truth will be discussed further in chapter 7.

[6] See also Popper's 1978 introduction to *The Two Fundamental Problems of the Theory of Knowledge* (2009), where Popper expresses Socratic scepticism as "I know that I know (almost) nothing."

[7] Cf. Popper (1994, p.175): "If we eliminate from language ambiguous terms like 'yesterday', a term which today means something different from what it will mean tomorrow, and if we take some further similar precautions, then it follows from Tarski's theory that every statement in this purified language will be either true or false, with no third possibility. Moreover, we can have an operation of negation in our language such that if a proposition is not true, then its negation is true."

[8] David Hume also defended scepticism from this objection in a similar manner:
"I ...cannot approve of that expeditious way, which some take with the sceptics, to reject at once all their arguments without enquiry or examination. If the sceptical reasoning be strong, say they, 'tis a proof, that reason may have some force and authority: if weak, they can never be sufficient to invalidate all the conclusions of our understanding. This argument is not just; because the sceptical reasonings... wou'd be successively both strong and weak, according to the successive dispositions of the mind. Reason first appears in possession of the throne... Her enemy, therefore, is oblig'd to take shelter under her protection, and by making use of rational arguments... prove the fallaciousness and imbecility of reason. This... gradually diminishes the force of that governing power [reason], and its own at the same time; till at last they both vanish away into nothing... "(*Treatise*, i, iv, i; pp. 186-7)

[9] Paul O'Grady puts it concisely (2002, p. 97): "Scepticism and relativism differ. Relativism accepts that

alternative accounts of knowledge are legitimate. Scepticism holds that the existence of alternatives blocks the possibility of knowledge." Here "knowledge" is understood in the philosophical sense—that is, as epistemically justified.

[10] Although *Pyrrhonian scepticism* is distinct from relativism, some accounts of *Pyrrho* himself suggest that he may have been a relativist, or at least a Heraclitean—if the reports of Timon are correct he held the (dogmatic) metaphysical position that reality itself is indeterminate. Yet, it must be stressed that this position is completely inconsistent with mainstream scepticism, leading some scholars (Bett 2000) to argue that Pyrrho himself was *not,* ironically, a Pyrrhonian skeptic

[11] An example of the tendency to conflate scepticism and relativism can be found in Sokal & Bricmont's *Intellectual Impostures* (1998).

[12] Agassi and Meidan make a similar point regarding the widespread antipathy to scepticism: "Philosophers invest much effort in attempts to refute skepticism because it strikes them as contrary to common sense, but they have met with no shred of success. The reason is simple: common sense actually sides with skepticism rather than against it. But, philosophers see things differently because they derive from skepticism unreasonable corollaries. These corollaries are indeed unreasonable, yet *their derivations are all invalid*." (2008, p.i, emphasis added)

[13] Hume also seems to, sometimes, take scepticism to be frivolous: "Tis evident, that so extravagant a Doubt as that which Scepticism may seem to recommend, by destroying *every Thing,* really affects *nothing,* and was never intended to be understood *seriously,* but was meant as a *mere* Philosophical Amusement" (1745, p. 20). On the other hand, he also asserts that sceptical arguments are logically unanswerable, and that the only possible response is "[c]arelessness and inattention" (1739, Part IV, the end of §ii).

[14] Indeed, it may be an empirical fact of human (or animal) psychology that universal doubt—the abandonment of all beliefs—is impossible. I do not mean to take a position on this question either way. However, whether doubt is or is not psychologically possible concerning any particular proposition, that would not thereby have any bearing on the *truth value* of the proposition. Statements about psychological conviction are epistemologically irrelevant; even species-wide beliefs may be mistaken.

[15] Incidentally, this is also the view of the subjective Bayesian Colin Howson, who fully accepts Hume's sceptical argument (2000, p. 2)—itself just a variant of Agrippa's trilemma: "Hume's argument gives us no reason to suppose that relying on our scientific knowledge is in any way misguided; it does not tell us we are *wrong* to do so. It merely says that the attempt to show that there is any sound inductive reasoning to that knowledge from observation alone will fail." In other words, the claim that scientific knowledge is *unjustified* is not the same as the claim that scientific knowledge is false or worthless; far from it.

[16] Robert Fogelin asserts that "most recent writers on this subject seem never to have heard of Agrippa and his Five Modes" (1994, p. 114).

[17] As mentioned before, classical logic is here (provisionally) assumed; I consider the interesting systems of paraconsistent logicians to be of greater formal interest than practical utility.

[18] BonJour (1985, p. 107) had called this the *"alternative coherent system objection...* An appeal to coherence will never even begin to pick out one uniquely justified system of beliefs, since on any plausible conception of coherence, there will always be many, probably infinitely many, different and incompatible systems of belief which are equally coherent."

[19] These include (BonJour, 1985, pp. 97-99):

1. A system of beliefs is coherent only if it is logically consistent.
2. A system of beliefs is coherent in proportion to its degree of probabilistic consistency.
3. The coherence of a system of beliefs is increased by the presence of inferential connections between its component beliefs and increased in proportion to the number and strength of such connections.
4. The coherence of a system of beliefs is diminished to the extent to which it is divided into subsystems of beliefs which are relatively unconnected to each other by inferential connections.
5. The coherence of a system of beliefs is decreased in proportion to the presence of unexplained anomalies in the believed content of the system.

[20] As Olsson (2012, § 3) notes: "A difficulty pertaining to theories of coherence that construe coherence as a multidimensional concept is to specify how the different dimensions are to be amalgamated so as to produce an overall coherence judgment. It could well happen that one system S is more coherent than another system T in one respect, whereas T is more coherent than S in another. Perhaps S contains more inferential connections than T, but T is less anomalous than S. If so, which system is more coherent in an overall sense? Bonjour's theory is largely silent on this point."

In addition, one of Bonjour's criteria—that the coherence of a system of beliefs is increased by the presence of inferential connections between its component beliefs and increased in proportion to the number and strength of such connections—has the unintended result of being biased towards bigger systems, simply in virtue of their size. This suggests that what advice it does offer regarding theory adjudication is not impartial, but instead incorporates arbitrary biases.

[21] BonJour (1985, p. 108) had called this *"the input objection...* If coherence is the sole basis for empirical justification, it follows that a system of empirical beliefs might be adequately justified, indeed might constitute empirical knowledge, in spite of being utterly out of contact with the world it purports to describe."

Paul O'Grady (2002, p. 100-101) also notes this difficulty for coherentism, and suggests that it has, unless it is somehow addressed, radically relativistic consequences: "Now, a standard objection to coherentist accounts of justification is germane to questions about relativism. If membership of a coherent system suffices to justify a belief, how do you cope with different such systems? There seems to be an endless supply of hypothetical coherent systems that supply justification to potentially contradictory statements. Such a vista is usually seen as a major problem for coherentists, since it leads to radical relativism. This is due to the lack of any principled way of distinguishing systems because coherence is an internal feature of belief systems."

[22] Fogelin (1994, p. 123) formulates these objections in a critique of the foundationalism of Roderick

Chisholm as follows: "Such a theory faces a double task: the first is to find suitable starting points that do not themselves stand in need of justification—Chisholm appeals to what he calls self-presenting properties to do this.The second is to show that from these starting points, a suitably large domain of those knowledge claims that we take to be justified is justified."

Chapter Four: Induction as Demarcational Method

4.0 Introduction

The strong justificationist response to the problem of theory adjudication asserts that science pursues certainty; conclusively justified knowledge that is grounded on indubitable premises and secured by valid inference. This idea underlies traditional empiricist foundationalism—genuine knowledge is inductively inferred from the firm foundation of immediate experience. This approach to the demarcation problem faces two difficulties however—the observation statements which form the evidential base are not certain, but even assuming they were, they would still be insufficient to justify scientific knowledge. The first problem is the problem of the empirical basis; the second problem is the problem of induction—"the question concerning the validity or justification, of universal propositions of the empirical sciences" (Popper, 2009, p. 3).

My focus in this chapter will be on the latter problem for strong justificationism, which I present as a problem of logical strength. In particular, I will focus on justificationist responses to Hume that appeal to a formal or topic-neutral inductive principle to justify empirical knowledge. I will argue for the following thesis:

If the thrust of Hume's result is correct, any attempt to certify a principle of induction by empirical means leads to either an infinite regress or circularity, and there is no known method by which an a priori certification could be achieved which is not itself merely dogmatic. In consequence, no inductive principle is possible which does not have the status of an unjustified postulate, and hence is inadmissible if theory adjudication is to be both non-authoritarian and non-question-begging.

4.1 Induction as Demarcational Methodology

Inductive logic, at least since Francis Bacon's *Novum Organum* (1620), has been employed by empiricists primarily as a method of theory adjudication—for the empiricist, demarcation and inductive logic are inseparable. Popper was quite emphatic about this assertion in *The Logic of Scientific Discovery* (1959, p. 11): "...the main reason why epistemologists with empiricist leanings tend to pin their faith to the 'method of induction' seems to be their belief that this method alone can provide a suitable criterion of demarcation." This point may be illustrated with some historical remarks, specifically on the tremendously influential inductivism of Bacon.

A central idea of inductivism is that science begins with careful and extensive observation, and proceeds, via the "inductive method" to *generalisations* (i.e., laws and theories) and singular *predictions*. This theory is often referred to as *enumerative* induction. In contrast to this, *eliminative* induction regards the role of observation as being primarily negative, but still justificatory—*exhaustive* error elimination is possible. Both variants of induction can be discerned to some extent in Bacon's theory of science; its major stimulus is the demarcation problem. Indeed, Bacon's central problem in writing the *Novum Organum*[1] was the problem of theory adjudication—his primary motivation was his dissatisfaction with the then popular theories of the natural world.[2] To explain the prevalence of dispute in theoretical matters, Bacon developed a theory to explain the origins of such competing and (presumably) false theories, the four classes of "idols" he identifies in Book 1 of *Novum Organum*. These idols are the "idols of the tribe" (that is, those distortions which arise from human nature), the "idols of the cave" (individual biases), the "idols of the marketplace" (distortions which arise from language), and "the idols of the theatre", prejudices stemming from dogmatic philosophies. Regarding this latter idol, Bacon's view was that the "...rival philosophies were like stage-plays, with different casts and different plots, but all equally fictitious" (J.R. Milton, 1998, § 5):

> Bacon distinguished three main types... The natural philosophy of Aristotle and his followers was corrupted partly by logic, and partly by a reliance on common notions... The empirical school (exemplified by the alchemists, but also including William Gilbert who investigated

> magnetism) was misled by too narrow a line of experimental enquiry: restricted ranges of data fill the imagination and lead to one-sided accounts of the world in chemical or magnetic terms. Platonism… was worst affected of all, being corrupted by theology and superstition.

The problem with these theories, according to Bacon, was that they were not justifiable by experience. As is typical of inductivism, Bacon was dismissive of what he called *Anticipations of Nature,* that is, speculation in the absence of data (1620, p. 262):

> The conclusions of human reason as ordinarily applied in matter of nature, I call for the sake of distinction *Anticipations of Nature* (as a thing rash or premature). That reason which is elicited from facts by a just and methodical process, I call *Interpretation of Nature…*

To more adequately adjudicate between such theoretical knowledge claims, Bacon asserted that a new investigatory method was necessary—one which started from observation, from "senses and particulars", and hence proceeded to "the highest generalities" (1620, I, xxii). The aim of this method was to discover certainly true universal statements about the natural world, secure theoretical knowledge about "the inner and further recesses of nature" (I, xviii).

Bacon's solution to the demarcation problem was a variation on eliminative induction (J.R. Milton, 1998, § 0):

> The route to success lay in a new method, one based not on deductive logic or mathematics, but on eliminative induction… allowing conclusions to be established with certainty, and thus enabling a firm and lasting structure of knowledge to be built.

An unfinished account of this method was set forth in the *Novum Organum.* Although not worked out in detail, Book I gives a schematic outline of the proposed method. The first step is simply to attend to the justifying authority—that is, experience—which Bacon, sharing a traditional empiricist assumption, takes to be intrinsically and non-inferentially justified. This is evident in the first three aphorisms of Book I (1620, I, 1-3):

> Man, being the servant and interpreter of nature, can only do and understand so much… as he

has observed in fact or in thought of the order of nature: beyond this he neither knows anything nor can do anything.

Further methodological steps are outlined in Book II, where Bacon proposed the systematic assembly of such unquestioned empirical premises into a "natural history" consisting of various tables—a "Table of Essence and Presence", a "Table of Deviation or Absence in Proximity", and a "Table of Degrees or Comparison". From an examination of such tables the inductive grasp of the true essence of the phenomena under investigation—what Bacon called the "nature" in question—would be possible, after an initial eliminative process (1620, II, 16):

> The first work therefore of true induction... is the rejection or exclusion of the several natures which are not found in some instance when the given nature is present, or are found in some instance where the given nature is absent, or are found to increase in some instance where the given nature decreases, or to decrease where the given nature increases.

This process would allow the derivation of "grounded conclusions", as Bacon wrote in a letter of 1592 to Lord Burghley (cited in Milton § 2). Although Peter Urbach (1987, § 2), has disputed the standard attribution to Bacon of the doctrine that induction delivers incorrigible certainty, Bacon certainly believed that his method could deliver *moral* certainty or *justification* for particular theories, and it is just such a claim that Hume's result calls into question.[3] As J.R. Milton writes (1998, § 4):

> Bacon was no fallibilist, prepared to settle for a natural philosophy of conjectures and merely provisional conclusions. Certainty was quite as important for him as it would be for Descartes, but what he was looking for was certainty of a very different kind - not immunity from sceptical doubt, but complete reliability. This could be furnished by induction...

Watkins (1984, p. 129) also emphasises this justificationist strand of Baconian inductivism:

> We may regard Bacon and Descartes as philosophical spokesmen for the high expectations that science excited in the early seventeenth century... Bacon's method was intended 'to penetrate into the inner and further recesses of nature' (1620, I, xviii) and discover the ultimate Forms, the very alphabet of nature... Baconian induction, like Cartesian deduction, was supposed to lead

> to conclusions whose truth is guaranteed by the premises from which they are induced; it was supposed to be *truth-preserving*.

And Colin Howson adds that (2000, p. 119) "[a]n assumption… that Bacon and all his successors accepted, is that the endorsement of scientific procedure must also extend to the endorsement of truth-claims made on behalf of the appropriate scientific theories: that for the principles of scientific reasoning to be correct means that they should lead in some guaranteed way to truth, or to some surrogate, like 'approximate truth' or probable truth." It thus seems that Bacon's pronounced justificationism can hardly be denied.

Although Bacon was harshly critical of a priori elements in other philosophies, his method, as he was aware, required a metaphysical *"principle of limited variety"*—his theory of induction presupposes that there is a finite variety of simple "natures". Indeed, Bacon assumed this number to be quite small, and asserted that using this method a complete inventory of all the various natures or essences could be discovered within just a few years. His method, despite being quite baroque, is also frustratingly vague regarding key details. For instance, he draws an analogy between his inductive method and wine-making (1620, I, 73). We first diligently gather a "great number of grapes… ripe [and] ready for the vintage" (that is, we make careful observations), and from these the wine (a scientific theory) is "then crushed in the winepress." How exactly theories are to be inferred beyond this metaphorical account is not divulged.

Despite such theoretical problems and unfulfilled promises, Bacon's influence as a methodologist was immense.[4] During the course of the rest of the 17th and 18th centuries, Bacon's became the official philosophy of the Royal Society (as lionised in Thomas Sprat's semi-official *History* (1667)), and he found advocates in such illustrious scientific figures and natural philosophers as Robert Boyle, Robert Hooke, John Locke, Voltaire and the other French encyclopedists, and Thomas Reid. Bacon's reputation continued to grow in the 19th century, as J.R. Milton (ibid, § 8) notes:

> The Baconian revival reached its climax in the second quarter of the nineteenth century. Sir John Herschel's Preliminary Discourse on the Study of Natural Philosophy (1830) was a thorough attempt to recast Baconianism in a form compatible with contemporary science. John Stuart Mill and William Whewell, though disagreeing about almost everything, both acknowledged a deep

debt to Bacon, and to the inductive method of science.

Indeed, it is difficult to underestimate the impact of Bacon's program, especially upon the theory of demarcation. Like all verificationist approaches to scientific knowledge, Bacon's theory accepts only those hypotheses that are verified or confirmed by inductive inference as genuine knowledge. Such an empiricist theory of demarcation remained, in essentials, unchanged right into the 20th century, when Popper (preceded in some aspects by Pierre Duhem's *The Aim and Structure of Physical Theory* (1906)) produced his alternative demarcational theory. To take an example from that period, Hans Reichenbach, in his sponsorship of induction, is evidently primarily motivated by the problem of *demarcation*. A principle of induction was necessary, Reichenbach argued, to determine (1930, p. 186):

> ...the truth of scientific theories. To eliminate it from science would mean nothing less than to deprive science of the power to decide the truth or falsity of its theories. Without it, clearly, science would no longer have the right to distinguish its theories from the fanciful and arbitrary creations of the poet's mind.

Wesley Salmon (1998, § 0) also notes Reichenbach's interest in demarcation:

> [Reichenbach] unconditionally rejected speculative metaphysics and theology because their claims could not be substantiated either a priori, on the basis of logic and mathematics, or a posteriori, on the basis of sense-experience.

To give another example from the same milieu, Rudolf Carnap, in the following passage from *The Logical Structure of the World,* explicitly links his inductivist program with a response to the demarcation problem (1967, Preface):

> This requirement for justification and conclusive foundation of each thesis will eliminate all speculative and poetic work from philosophy... It must be possible to give a rational foundation for each scientific thesis... the physicist does not cite irrational factors, but gives a purely empirical-rational justification.

However, it is just such conclusive foundations, and by extension the justificationist demarcation program in the nomological sciences, that Hume brought into question with his critique of Baconian inductivism. Hume, no doubt the greatest sceptic of the modern era, noted that there is no valid way to derive from empirical reports, which he granted the status of certain and immediate knowledge, any general claim. On the basis of past evidence logic does not sanction even a solitary prediction about the future, much less a universal law statement of the type so characteristic of theoretical science. An immediate casualty of this result is the empiricist theory of demarcation, which holds that empirical generalisations are to be graded according to their empirical justification. Any such justification would require, in addition to the empirical evidence, an inductive principle which cannot itself be justified by experience, and should hence be rejected according to the empiricist demarcational criteria.

4.2 Hume's Infinite Regress Argument

Hume's philosophy can be seen as the product of two distinct traditions—the empiricist tradition of Locke and Berkeley, and the sceptical tradition, especially as presented by Sextus Empiricus. Indeed, to the end of his life Hume considered himself a sceptic, and this sceptical influence is nowhere more apparent than in his first and now most famous work, *A Treatise on Human Nature* (1739/1740). It is here we find his critique of induction. Although Hume formulates the critique as one of "causal inference", this has been commonly generalised, at least since Kant[5], to include *any* inference from the observed to the unobserved (see, for instance, Keynes, 1921, p. 302).

Hume's argument (Book I, Part III, Section VI) is this. There can be no valid demonstrative argument which would allow us to show *"[t]hat those instances, of which we have had no experience, resemble those, of which we have had experience."* As a consequence, *"even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience."* For, Hume continues, "should it be said that we have experience"—experience teaching us that objects constantly conjoined with certain other objects continue to be so conjoined—then, Hume says, "I wou'd renew my question, *why from this experience we form any conclusion beyond those past instances, of which we have had experience."* In other words, an attempt to logically justify the practice of inductive logic is untenable, and the attempt to justify it by an appeal to experience must lead to either circularity or an infinite regress. In either event, Hume's basic idea is that "in analysing any inductive generalisation, one will necessarily encounter impermissible logical operations" (Popper, 2009, p. 35).

The upshot of this argument is that we cannot *validly* generalise from a sequence of observations in which a certain regularity holds without exception, no matter how large the sequence. No strictly universal statement or natural law can be deduced from this summary report of particular events. Since the explanatory statements of science are, for the most part, law statements, (whether statistical or deterministic), Hume's result poses a significant problem for justificationist theories of demarcation. As Popper explains in his restatement of Hume's argument in *The Two Fundamental Problems of the Theory of Knowledge* (ibid, pp. 36-7):

> The observation material that furnished us with a basis for the summary report is, by itself, certainly not capable of providing a sufficient basis for this strictly universal proposition. For in the latter, we are asserting more than we are able to justify by those experiences. From a logical point of view, whenever we make an induction, we (tacitly or explicitly) make certain assumptions that are not justified by the observation material on which the generalisation is directly based.

According to Hume, the additional assumption that is tacitly invoked in inductive inference is that "the future is conformable to the past", or that "nature is uniform." In modern jargon, what is presupposed in an inductive inference is an assumption that the world is so constructed that all observed samples are representative of the populations from which they are drawn. Might this assumption be justifiable by previous observations? Not, according to Hume, without embarking upon an infinite regress. As Popper states (ibid, p. 40)

> The inference from our observations as to the validity of the principle of induction in turn represents a generalisation, an inductive inference. And in this generalisation again we (tacitly or explicitly) make an assumption analogous to the earlier assumption formulated in the principle of induction.

Thus, to justify a principle of induction, or to adjudicate between two incompatible principles, a higher-level principle is needed, analogous to the first-order principle (ibid, pp. 41-2):

> Now everything depends on the validity of the second-order principle of induction. It requires no further explanation that considerations analogous to those concerning the validity of a first-order principle may be brought to bear on the validity of the second-order principle of induction. If its validity is to be based on an induction, a third-order principle of induction would have to be presupposed, which would then be a statement about statements of the type of the second-order principle of induction... Every universal empirical statement requires a principle of induction of a higher type than the inductum... Therein consists the infinite regression.

This, then, is the basic argument against the admissibility of induction.[6] It is, evidently, simply a variation on the traditional trilemma of justification: if induction is to provide any justification for knowledge claims, even on the favourable assumption that the observational basis is completely secure, the principle which licences such inductive inferences must itself be certified true. However, the attempt to justify any such principle by empirical argument requires either an infinite series of higher-order principles, or the admission of circular justification.[7] The final option is to abandon empiricism[8] and appeal to the self-evident (dogmatic) truth of some a priori principle. Any empiricist demarcation program is seriously suspect in the light of this result, as is especially apparent in the exposition of the problem of induction by Russell in his *History of Western Philosophy* (1945, p. 699):

> It is... important to discover whether there is any answer to Hume within the framework of a philosophy that is wholly or mainly empirical. If not, there is no intellectual difference between sanity and insanity. The lunatic who believes that he is a poached egg is to be condemned solely on the ground that he is in a minority, or rather--since we must not assume democracy—on the ground that the government does not agree with him. This is a desperate point of view, and it must be hoped that there is some way of escaping from it.

Reichenbach took a similar stance—Hume's result had "tragic consequences" for the empiricist demarcational project; it "was the heaviest blow against empiricism" (1938, pp. 345- 347). And as Miller glosses Hume's result (2006a, p. 138), "reports of experience, of observation and experiment, do not conclusively justify any prediction concerning the future (or, more generally, the unobserved), even if they are held to be so solidly based as to need no justification themselves." Thus, even under the (generous) assumption that empirical observation reports are unassailable, Hume's result, to all appearances, revealed the empiricist demarcation project to be quite unachievable.

4.3 Induction as a Problem of Logical Strength

When construed in light of the problem of demarcation, the problem of induction can best be interpreted as a problem of logical strength; it is a problem that arises with the attempt to derive a stronger conclusion than the accepted premises allow. As Bartley explains (1984, pp. 188-9):

> The problem of logical strength arises when the statement or policy under evaluation, although not in conflict with the authorities, has a logical strength greater than that of any authority or combination of authorities, which hence cannot be reduced to or derived from the authorities, and which must therefore be rejected as not sanctioned by the authorities.

In this case, scientific hypotheses (or hypotheses about the unobserved more generally), cannot be reduced to truth functions of basic observation statements—no finite class of observation statements is sufficiently strong, logically, to derive a universal scientific hypothesis.[9] Yet such hypotheses seem obviously legitimate, despite not being justifiable by any empirical authority. Moreover, this problem obtains even when the justifying authorities, whatever their identity, are taken as absolutely secure and self-evident. The problem then, for a purely empiricist demarcation criterion, is that it leads to the rejection of hypotheses which seem obviously legitimate. Such an account may easily be seen to apply to inductive reasoning (ibid, p. 191):

> In... inductive reasoning... we have statements the merits of which must be decided... these statements being scientific projections about the future (or "universal statements")... The problem... is to "justify" such statements, taken as the conclusions of arguments of justification, when it can be shown that the available justifiers, or statements which might be used as

premises in such a justifying argument, are not sufficiently strong to entail the statements in question.

Thus, the problem of induction can be reduced to the problem of demarcation—without a valid theory of induction the empiricist justificationist is restricted to an extreme positivism and the disavowal of all theoretical knowledge which transcends immediate experience. Since the empirical evidence is always consistent with an infinite number of hypotheses—"every proposition may be generalised in an infinite number of ways" (Poincaré, 1902, p. 130)—justificationist empiricism cannot adjudicate between them. The result is the rejection of hypotheses that should obviously be retained.

Since, for the rationalist at least, scientific laws should clearly be retained, Bartley (ibid, pp. 193-4) describes two strategies open to those who hold justificationist theories of demarcation. The first option is to:

> a) *strengthen* the authorities by supplementing them with *a priori* or other principles—as in Bertrand Russell's *a priori* principle of induction—so as to permit a deduction or reduction in terms of this principle.

The second option is to:

> b) *weaken* the requirement that the justified statements be logically reducible to the authorities. For example, the justified statements might only be "inductively" related to the justifiers—thus once again making use of some principle of induction.

In fact, both strategies often appear concurrently.[10] In the rest of this chapter I will explore the first option, postponing until the next chapter the strategy of abandoning sufficient justification in favour of partial support. In particular my aim in this chapter is to explore responses to Hume that stay within the remit of classical logic; responses that appeal to probability logic will be addressed in the next chapter. On either account, however, inductive inferences can best be construed as enthymemes—that is, as deductive arguments with a missing or "suppressed" premise.[11] In the case of inductive inferences, the missing premise is generally taken to be a formal or topic-neutral *principle of induction*, thus solving the problem of logical strength by transforming the purportedly inductive inference into a robustly deductive one.[12] As Russell stated (1946, p. 699):

> Hume's scepticism rests entirely upon his rejection of the principle of induction... If this principle is not true, every attempt to arrive at general scientific laws from particular observations is fallacious, and Hume's scepticism is inescapable for an empiricist. The principle itself cannot, of course, without circularity, be inferred from observed uniformities, since it is required to justify any such inferences. It must therefore be... an independent principle not based on experience...
>
> But if this one principle is admitted, everything else can proceed in accordance with the theory that all our knowledge is based on experience.

An immediate problem with this approach is *identifying* the suppressed premise, or, in other words, explicitly *formulating* the principle of induction. What is clear, though, is that *some* such principle is necessary if the reasoning from premises to conclusion is to constitute *reasoning* at all, rather than simply a subjective, uncontrolled leap from one proposition to another.

Before turning to this problem however, it is worth noting that problems of logical strength, exactly analogous to the problem of induction, also appear at various other points in the justificationist theory of scientific knowledge. For instance, although the problem of induction is usually presented using non-quantitative empirical generalisations—the classic 'all swans are white' example in the literature is of this type—further aspects of the problem can be distinguished. Moving beyond simple empirical generalisations about observable events or objects, even more characteristic of the nomological sciences are *exact* experimental laws concerning *measurable* physical magnitudes. More abstractly, and more characteristic still, are scientific theories that are not only universal and exact but which postulate *unobservable* entities. Each level of this hierarchy introduces new difficulties for the justificationist.

Concerning exact experimental laws, a problem for the justificationist is that the precision of the law generally goes beyond what may be justified by observation reports. As C.S. Peirce put it: "An opinion that something is *universally* true clearly goes further than experience can warrant... I may add that whatever is held to be precisely true goes further than experience can possibly warrant" (1893, pp. 239-240). Popper also made the same point: "Now it is incredible that... the absolutely precise statements of [Newtonian theory] could be logically derived from less exact or inexact [observation statements]" (1963, p. 186). This raises the curve-fitting problem of induction, which particularly troubled Poincaré (1902, p. 146): "Why do we avoid angular points and inflexions that are too sharp? Why do we not make our curve describe the most capricious zigzags?" Just like the non-quantitative problem, this problem also appears to necessitate an a priori principle of discrimination (usually a principle of simplicity) to allow the confirming force of the evidence to amass upon a particular exact curve hypothesis from amongst the infinity of possible curves that can be fitted over a finite number of isolated points. The difficulty is then that of justifying the adoption of any such principle of selection, which would unavoidably amount to a very strong synthetic assumption, and one that could not itself be confirmed empirically without circularity.

Problems of logical strength also occur concerning the theoretical ontology of science. Most fundamentally for traditional empiricists is the difficulty that their premises—first person perceptual reports—do not seem to warrant the postulation of an external world of intersubjective material objects. That is, the problem for the empiricist justificationist is that there seems to be no grounds, on empiricist assumptions, to retain common-sense realism. This problem is perhaps most strikingly addressed in Bertrand Russell's *Our Knowledge of the External World* (1914), where he asks (1914, p. 80), "Can the existence of anything other than our own hard data be inferred from the existence of those data?" This problem of the external world has led many empiricists, from Berkeley onwards, to attempt to reduce physical objects to clusters of actual or possible perceptions. This approach—usually called phenomenalism when applied to scientific theories—is especially associated with Ernst Mach (1897), Russell (1914), and Carnap (1928).[13] As Russell wrote in his (1914, p. 116): "If physics is to consist wholly of propositions known to be true, or at least capable of being proved or disproved... hypothetical entities... must all be capable of being exhibited as logical functions of sense-data." Less radical than this all-encompassing phenomenalism, in which all statements about the external world are to be reduced completely to statements about subjective experience, are those empiricists who cite justificationist qualms to reject *unobservable* theoretical entities, such as electrons, genes, Higgs particles etc. This reductive empiricism directly contradicts scientific realism, and is again primarily a problem of logical strength—statements about unobservable entities cannot be derived from premises that are restricted to observation reports.

Let us now turn to the most common way to strengthen the premises so as to avoid disastrous demarcational results—the introduction of an inductive principle.

4.4 The Epistemological Status of the Principle of Induction

Justificationists have had little success in formulating an inductive principle that would be sufficient to perform the task allotted to it. Is it to be empirical? Metaphysical? Epistemic? A purely formal rule of inference?

Hume's own, highly influential, suggestion was that the suppressed premise in inductive inferences should assert what has been called a principle of "the uniformity of nature". He wrote that "all our experimental conclusions proceed upon the supposition that the future will be conformable to the past" (1748, § IV, Part II, p. 35), and "all reasonings from experience *are founded on the supposition* that the course of nature will continue uniformly the same" (1740, p. 651).[14] Yet such a principle licences too much; *any* generalisation can appeal to it for justification. It is only in *certain* respects (if at all) that "the course of nature continues always uniformly the same", and the demarcation problem is precisely to separate these *actual* regularities from the merely *apparent* ones. As Russell reminds us in *The Problems of Philosophy* (1912, p. 98):

> We know that all these rather crude expectations of uniformity are liable to be misleading. The man who has fed the chicken every day throughout its life at last wrings its neck instead, showing that more refined views as to the uniformity of nature would have been useful to the chicken.

As this example illustrates, observed samples are not *always* representative of the populations from which they are drawn. Clearly, such an indiscriminate principle is useless for demarcation.

Indeed, it is a major difficulty for inductivists to formulate a plausible principle of induction which is sufficient for their purposes that is not, at the same time, blatantly false. To my knowledge no such principle has yet been discovered. However, many have repeated Hume's claim that inductive inference presupposes some such principle. The question thus arises (Miller, 2006a, p.159), since "no single inductive inference need presuppose anything like as much as the whole uniformity of nature, how much uniformity must a typical inference presuppose?"[15]

One (quite conservative) option is to choose the inductive principle that is *itself* most probable—that is, the most probable statement that will allow, in conjunction with some set of (observational, singular) evidential statements *e*, the derivation of the universal hypothesis *h* that is the desired conclusion of the specific inductive inference at hand. This approach is inadequate, however. This is due to the fact that for any particular inductive generalisation, the logically weakest, and hence *most* probable, statement that is sufficient to allow such a generalisation is simply the material conditional, "if e, then *h*" ($e \rightarrow h$) . The inference would then run as follows: $e, e \rightarrow h \models h$. The conditional in this inference, as Popper and Miller note (1987, § 3), is the very *least* a (specific) inductive principle must assert; it is the ampliative content that goes beyond the evidential statements (which in this context are taken to be uncontroversial).[16] Of course, such a conditional statement, adopted as an ad hoc principle of induction, would be of no *general* use, and would more sensibly be replaced with the universal scientific hypothesis itself—that is, since such a principle fails to be topic-neutral it would hence be of no interest to a theory of methodology, and would certainly be of no value in theory adjudication more generally. Another traditional strategy, popular before the advent of quantum physics—that is, an appeal to an all-embracing deterministic "principle of causality"—faces an opposite problem; such a principle is obviously too strong.[17]

Moreover, inductive principles are not always advertised as such. For instance, the debate centring around "projectible" predicates, for instance, is simply a rebranding of the problem, as can be seen by the fact that a criterion of projectibility would *de facto* function as an inductive principle of discrimination.[18] As Mark Kaplan notes however (1998, § 1), no principled demarcation between projectible and nonprojectible predicates has yet been proposed. It should be added also that no such criterion could guarantee or validate inductive inference unless it was *itself* guaranteed, thus initiating an infinite regress.

Some authors have even gone so far as to propose that the exact details of inductive procedures are *unspecifiable*. Strawson, for instance, in his (1952), asserts the existence of conclusive inductive proofs (pp. 237-38), yet also admits that "no precise rules of general application can be formulated for the assessment of evidence" (p. 248). A similar approach is taken by Wittgenstein (1953, § 480-483). Clearly such unspecified rules of inference cannot aid in theory evaluation—without objective rules nothing is to stop any invalid inference whatsoever being proposed as an "inductive proof".

Since the variety of possible inductive principles is infinite, and since attempts to formulate one continue,[19] it will be perhaps more fruitful to look at the general epistemological or logical status that such a principle may possess. The different possible answers to this question will be discussed, as far as possible, systematically below.[20]

These are displayed in the following table, which is much influenced by John Watkins' presentation in *Science and Scepticism* (1984, Ch. 3).

The Status of The Inductive Principle

Logical Status	Method of Justification
Analytic	Logic
Synthetic	Empirically Justified
Synthetic	A priori
Synthetic	Transcendental Argument
Synthetic	Pragmatically Vindicated
Synthetic	Self-Justification

4.5 The Inductive Principle as Analytic

One suggestion is that, contra Hume, a principle of induction might be analytic, and hence a priori or logically true. This was proposed by Carnap in his (1950). There he suggested an inductive logic that involved no "synthetic presuppositions like the much debated principle of the uniformity of the world" (1950, p. v). Despite being completely analytic, this system would, according to Carnap, nevertheless allow us to assign predictions about the future a high probability solely on the basis of past evidence. Referring to Hume's and Russell's assertion that a principle of induction can be justified only a priori, Carnap countered that his solution was purely logical (1950, p. 181): "According to our conception, the theory of induction is inductive *logic*. Any inductive statement (that is, not the hypothesis involved, but the statement of the inductive relation between the hypothesis and the evidence) is purely logical."

Yet Carnap seems to have shifted his position in his (1952), here apparently asserting that the confirmation function is *not* purely logical, but is rather to be determined by experience, and is hence, to all appearances, synthetic (1952, p. 55). An inductive methodologist "may not be quite satisfied... [with]... a particular inductive method", and hence may "therefore look around for another method. He will take into consideration the performance of a method, that is, the values it supplies and their relation to later empirical results, e.g., the truth-frequency of predictions and the error of estimates... Here, as anywhere else, life is a process of never ending adjustments."

Such a position raises serious difficulties. The criterion for the analytic-synthetic distinction, deriving most influentially from Kant (Prolegomena (1783), Paul Carus translation § 2, pp. 12- 14), is purely logical—analytic statements depend solely on the principle of contradiction, and are hence tautological—their denial or their negation is a contradiction. Salmon, (1966, p. 31), gives the following definition:

> [W]e may define an *analytic statement* as one that is a logical truth or can be transformed into a logical truth by definitional substitution of *definiens* for *definiendum*. The negation of an analytic truth is a *self-contradiction*. Any statement that is neither analytic nor self-contradictory is *synthetic*. More technically, we may define an *analytic statement* as one whose truth can be established solely by reference to the syntactic and semantic rules of the language, a *self-*

> *contradictory statement* as one whose falsity can be established solely by reference to the syntactic and semantic rules of the language, and a *synthetic statement* as one whose truth value is, in relation to the syntactic and semantic rules alone, indeterminate.

With this in mind, Carnap's appeal to experience is perplexing. After all, an analytic or logically true statement is said to hold in *every* possible world, so the suggestion that such a principle may prove *empirically* unsatisfactory seems to make little sense. Analytic statements cannot be negated without arriving at an internally inconsistent statement or a contradiction, and consequently say nothing about reality. As Salmon asked, (1966, p. 76) *"How can statements that say nothing about any matters of fact serve as 'a guide of life?'"*

Indeed, Carnap's position has been championed by some authors as explicitly non-analytic—John Graves (1974, p. 316), for instance, has interpreted Carnap's confirmation function as "a precise, quantitative, numerical, scalar measure of the degree of uniformity of nature. The value we choose represents our estimate of how uniform nature is, at least in the area under investigation. We may make a poor choice, but the ontologically correct value determines the best rule to use. This value can be discovered only inductively, for it reflects our actual world and would be different in other possible worlds" (quoted in Watkins, 1984, p. 102).

This last point of Graves' seems correct. Arguably, all statements about reality must be synthetic statements whose truth or falsity cannot be decided by logic alone; only such a *synthetic* principle could be genuinely informative about the world. Despite this, Carnap still maintained in his (1968b, pp. 264-265) that:

> ...in principle it is never necessary to refer to experiences in order to judge the rationality of a C-function. Think of the situation in arithmetic. You can show to a child that three times five is fifteen by counting five apples in each of three baskets and then counting the total. But... this is not necessary... [iv] We regard arithmetic as a field of *a priori* knowledge. And I believe that the same holds for inductive logic.

However Carnap's system is to be interpreted, it is plain that, if it is to be adequate to assign hypotheses inductive confirmation values on past evidence, it *cannot* be analytic or tautologous. That is, if a confirmation function $c(h, e) = r$ is analytic, it cannot tell us anything about a predictive hypotheses h beyond what is already asserted by the evidence *e*. Any inductive principle, to be strong enough to aid in demarcation, must therefore be synthetic. Watkins bolsters this point with reference to Nelson Goodman's technique (1947) of manufacturing alternative hypotheses—*h1*, *h2*, *h3*, etc,—that all stand in the same relationship to the observational evidence, and yet are mutually incompatible. It is plain that a purely logical confirmation function must be impartial between these alternatives. As Watkins (1984, p. 92) explains, "in the absence of some extralogical principle of discrimination, there is no reason why e should favour one of these extensions more than others. But if it is impartial, it can confirm only their disjunction; but this is equivalent to e itself." Such a result makes it quite plain that no such *analytic* principle allows theory adjudication amongst competing hypotheses that each account for the evidence.

Accordingly, Carnap's inductive calculus, if it is to serve the purpose it is designed for, must be extra-logical and synthetic—it must employ a synthetic inductive principle. This point is sponsored even by inductivist critics of Carnap, such as Nagel (1963), and Salmon (1968), and was made much earlier by Wittgenstein (1918/1922, proposition 6.31):

> 6.31 The so-called law of induction cannot possibly be a law of logic, since it is obviously a proposition with sense.—Nor, therefore, can it be an a priori law.

More generally, any inductive principle in order to be useful for demarcation will necessarily have to make some very strong claims about the world, and will hence not be an analytic principle, which is, by definition, compatible with any experience whatsoever. Thus, whatever the formulation of the inductive principle adopted, since it necessarily must state something about the law-like character or uniformity of nature—of reality—it must be synthetic. Indeed, as Popper (1983, p. 330) noted, Carnap's system of 1950 does, in fact, posit a rule establishing the mutual dependence, or relevance, of any two instances of the same property, and such a principle is clearly synthetic. This point is also made in Appendix *vii of the *Logic* (1959, p. 379):

> in the absence of any (other than tautological) information, we must consider all these singular statements as mutually *independent* of one another... Every other assumption would amount to postulating *ad hoc* a kind of after-effect; or in other words, to postulating that there is something like a causal connection between [one instance and the next]. But this would obviously be a

non-logical, a synthetic assumption, to be formulated as a hypothesis. It thus cannot form part of [a] purely logical theory of probability.

4.6 The Inductive Principle as Synthetic and Empirically Justified

How then can such a, necessarily *synthetic,* principle be justified? The empiricist hope is that, despite Hume, an empirical justification may yet be possible. This was Mill's approach (1843, III, iii, 1), and also that of J.M. Keynes. According to Keynes (1921, p. 302), "Hume showed, not that inductive methods were false, but that their validity had never been established and that all possible lines of proof seemed equally unpromising." Keynes solution is to posit as an inductive principle (*H*), "the postulate of limited independent variety." This is used by Keynes to restrict the number of possible empirical hypotheses, and hence to allow those remaining to obtain some positive degree of probability upon favourable evidence. Such a synthetic principle, Keynes claims, could then be non-circularly justified by experience, using the inductive method. He writes (1921, pp. 259-260):

> [I]t is not circular to use the inductive method to strengthen the inductive hypothesis itself, relative to some more primitive and less far-reaching assumption... [W]e can support the Inductive Hypothesis by experience. In dealing with any particular question we can take the Inductive Hypothesis, not at its *a priori* value, but at the value to which experience in general has raised it: *(h, H).*

That is, Keynes thinks his inductive principle *H* has a certain a priori probability owing to the postulate of limited independent variety, and this probability can then be raised by using the inductive method, which is, in turn, sanctioned by "some more primitive and less far-reaching assumption" of the basic form of his inductive principle *H*. How such a manoeuvre avoids circularity or regress is unclear, for, according to Keynes, the inductive hypothesis is neither a self-evident logical axiom (ibid, p. 291) nor an object of direct acquaintance (p. 294). It seems that Keynes was aware of the logical vulnerability of his argument, admitting that he had failed to give a "perfectly adequate reason for accepting" his inductive principle. Accordingly, he later appears to appeal to a transcendental argument—the postulate of limited independent variety must be assumed, as "the inductive method can only be based on it or on something like it" (p. 264). This will be examined in § 4.8 below.

More recently Max Black (1954, 1958, 1966) has also claimed that induction can be non-circularly justified, on the basis of having worked well in the past. Yet this argument is invalidated by Salmon's (1957) and Stegmuller's (1977, ii, p. 75) demonstrations that a counter-inductive principle—one that assumes the non-uniformity of nature, or that the future will not be like the past—could be justified in the same fashion on exactly the same evidence.[21] Nola and Sankey (2007, pp.156-8) make a similar point:

> There is a further problem about the inductive justification of induction. Such an approach can be used to justify some weird inductive rules since it uses a style of justification that is too profligate in what it will allow to be justified... can counter-inductivists also use a similar rule-circular argument to justify counter-induction? Unfortunately, yes, so the style of justification is too permissive in what it will admit... rule-circular arguments can justify two different systems of inductive rules, one of standard induction and the other of counter-induction. Both cannot be correct... Although the counter-inductivist system of rules seems weird to us, nonetheless such rules can receive the same kind of justification that our normal inductive rules do.

Thus, the evidence cannot support an inductive principle without first presupposing it—by itself the evidence is compatible with a completely contradictory principle. Any such principle can only be derived from the evidence with its own help. Thus Hume and Russell are correct—justificationist empiricism cannot be consistently maintained because induction presupposes some principle or postulates which cannot be based upon experience. Any strictly empirical justification of an inductive principle will involve circularity. That is, upon justificationist assumptions an inductive principle is badly in need of justification, but, given strict empiricism, it is incapable of receiving any. As Salmon (1966, p. 13) has stated regarding Black's attempt, "[t]he trouble with circular arguments is obvious: with an appropriate circular argument you can prove anything."[22]

4.7 The Inductive Principle as Synthetic and A priori

According to empiricist assumptions about justification then, a factual statement that goes *beyond* past experience can be established as true *only* by reference to past experience. Yet if Hume is correct, past experience is completely uninformative about any future occurrence. That is, we cannot progress by logical reasoning from perceptual experience to any genuine knowledge about the unobserved.

As mentioned above in the quote from Russell,[23] this situation has led many justificationists to suggest that a synthetic inductive principle may—indeed *must*—be justifiable a priori. Yet it is decidedly unclear how such an a priori certification for a synthetic statement is achievable. After all, the terms "a priori" and "a posteriori" are not fully symmetrical. While "a posteriori" refers to justification that is dependent upon experience—that is, empirical verification—"a priori" only indicates that experience *isn't* necessary. How the justification *is* to be achieved isn't specified. A relatively uncontroversial sphere of a priori knowledge is that of logic and mathematics. However, as we have seen, a sufficient principle of induction cannot be analytic. Thus what we need is some method to attain *synthetic* a priori justification.

Galen Strawson (2003) gives the following formulation—an *a priori* argument is one in which "you can see that it is true just lying on your couch. You don't have to get up off your couch and go outside and examine the way things are in the physical world. You don't have to do any science." Accordingly, the usual interpretation is that an *a priori* argument or statement entails that a proposition is known to be true "on grounds of reason"—that is, by being immediately plausible or rationally self-evident. We may thus call this position "the doctrine of self-evidence."

The question whether any such propositions exist was one of the main points of contentions between classical rationalism and classical empiricism, as Popper notes in his (2009, p. 8):

> Thus, classical rationalism (Descartes, Spinoza), for example, has a strictly deductivist orientation (its model is geometrical deduction [Euclid]), As also emphasised by Kant, Euclidean geometry is the model of classical rationalism (Kant speaks about "dogmatism"). In the past, the major premises of geometry (the "axioms" or "postulates") used to be characterised as "immediately plausible". At any rate, they stand at the top of the system, without either proof or inductive justification, and all other statements are deduced from them in a purely logical fashion (axiomatic-deductive method)... Rationalism, which postulates the most fundamental principles of its system a priori (in the manner of geometrical axioms), also obtains the entire

> scientific structure in an axiomatic-deductive fashion, purely by way of logical deduction... the most general statements (axioms) of natural science are adopted without logical or empirical justification and a priori assumed to be true (in view of their self-evidence),

However, such apriorism has lost all its intuitive appeal with subsequent developments in mathematics, and in geometry in particular (ibid, p. 17):

> Before the discovery of non-Euclidean geometry, Euclid's axioms could well be regarded as the only possible ones, as "immediately plausible" and "a priori true". Modern developments have shown, however, that Euclid's geometry represents only one possibility among many and that other, a priori equally warranted systems can be developed in the same non-contradictory and compelling fashion as Euclid's system. The different systems are to be understood as freely postulated (freely chosen within the confines of logic), and none of them should be given a priori preference... The question of which system best corresponds to real space can only be decided by experience: by deducing consequences that can be empirically tested ("predictions").

Thus, the primary example appealed to by rationalist philosophers as propositions that made statements about reality and yet were known a priori "on grounds of reason", are not only not *necessarily* true, they are seemingly not even true. The lesson, it seems, is that even the most plausible synthetic judgements may turn out to be false.

Can the doctrine that we can, without resorting to experience, ascertain the truth of synthetic statements be salvaged? Can the claim that some propositions are "immediately evident to the understanding", "true on rational grounds", or "intuitively grasped" provide any justification for an inductive principle? This position seems to have been adopted by Russell, (although he also makes a transcendental argument, to be discussed in the next section). In his 1912, Russell concludes that an inductive principle must be accepted *a priori,* or, as he put it, "on the ground of its intrinsic evidence" (1912, p. 106): "We can never use experience to prove the inductive principle without begging the question. Thus we must either accept the inductive principle on the ground of its intrinsic evidence, or forgo all justification of our expectations about the future."

Yet as Hume noted (1748, p. 164), any internally consistent synthetic proposition may be denied without contradiction:

> ...enquiries concerning only matter of fact and existence are evidently incapable of demonstration. Whatever *is* may *not be*. No negation of a fact can involve a contradiction. The non-existence of any being, without exception, is as clear and distinct an idea as its existence. The proposition, which affirms it not to be, however false, is no less conceivable and intelligible, than that which affirms it to be.

This point is significant, for it has been shown by Carnap (1950, p. x) that for any two inductive or predictive methods, no matter how inadequate or unreasonable the first might seem in comparison with the second, there will be possible worlds in which the first performs better than the second. This, according to Watkins (1984, p. 94), "knocks out the possibility that [an inductive principle] could be both synthetic and true a priori." Thus, the possibility of demonstrating a synthetic principle of induction by this method is nixed. As Watkins goes on to comment, it "would be nice if there were a pre-established harmony between certain inborn ideas in our minds and certain structural features of the world, but there is no a priori reason to suppose that this is so" (ibid). And as Salmon notes, (1966, p. 17), "it is extremely difficult, psychologically speaking, to shake the view that past success of the inductive method constitutes a genuine justification of induction. Nevertheless, the basic fact remains: Hume showed that inductive justifications of induction are fallacious, and no one has since proved him wrong."

Perhaps one of the most eloquent critics of apriorism, or "intuitionism," as he also called it, was J. S. Mill. This "German, or *a priori* view of human knowledge" (CW, I.233),[24] Mill asserted, holds that "the constitution of the mind is the key to the constitution of external nature—that the laws of the human intellect have a necessary correspondence with the objective laws of the universe, such that these may be inferred from those" (*CW*, XI.343). Mill's criticism was the same as the sceptics—apriorism is simply dogmatic, and, as he notes in his autobiography, its dangers extend far beyond epistemology:

> The notion that truths external to the mind may be known by intuition or consciousness, independently of observation and experience, is, I am persuaded, in these times, the great intellectual support of false doctrines and bad institutions. By the aid of this theory, every inveterate belief and every intense feeling, of which the origin is not remembered, is enabled to dispense with the obligation of justifying itself by reason, and is erected into its own all-sufficient voucher and justification. There never was such an instrument devised for consecrating

> all deep-seated prejudices. And the chief strength of this false philosophy in morals, politics, and religion, lies in the appeal which it is accustomed to make to the evidence of mathematics and of the cognate branches of physical science. To expel it from these, is to drive it from its stronghold. (*CW*, I.233)

That is, Mill's claim is that the appeal to apriorism is inherently authoritarian. A similar point is made by Bentham, in his 1789 *An Introduction to the Principles of Morals and Legislation*:

> [Apriorists] consist all of them in so many contrivances for avoiding the obligation of appealing to any external standard, and for prevailing upon the reader to accept of the author's sentiment or opinion as a... sufficient [reason] for itself. (1789, II.14).[25]

Indeed, if there really were some clear and distinct, self-evident, and indubitable principle by which we could recognise genuine knowledge the demarcation problem wouldn't exist, and the dispute between rationalist and empiricist epistemologists would never have occurred. Yet it did (as far as I'm aware). The doctrine of self-evidence cannot aid in genuine disputes; the truth is not manifest. The amount of false doctrines that have been deemed "immediately plausible" surely demonstrates that.

4.8 The Inductive Principle as Transcendentally Justified

A more plausible apriorist strategy than the doctrine of self-evidence of traditional rationalism is the employment of a so-called transcendental argument. This approach is most famously associated with Immanuel Kant, but it is actually quite popular even amongst self-described empiricists.[26]

Kant's response to Hume, and the problem of induction, can be found in the "transcendental analytic" of the *Critique of Pure Reason*. Indeed, Kant regarded his philosophy as, to a large extent, a response to Hume. (In the Preface to the *Prolegomena* (1783, p. 9) Kant wrote, "I freely admit: it was David Hume's remark that first, many years ago, interrupted my dogmatic slumber and gave a completely different direction to my enquiries.") Kant too, like Hume, was critical of apriorist rationalism—it was this critique which informs the title of the book—yet he found Hume's scepticism unacceptable. His solution was to accept Hume's result that experience alone cannot justify scientific knowledge, but to add that there are certain non-analytic principles which are nevertheless necessarily true. For, according to Kant, experience is structured by a framework of synthetic categories whose truth can be known a priori, and it is such synthetic a priori categories, including a formal law of causality, which guarantee the validity of inductive inference and permits the most general scientific laws to be inferred from empirical evidence (whilst rejecting the material a priori synthetic judgements of traditional rationalism). However, critical of the "doctrine of self evidence", which Kant correctly dismissed as "dogmatic", he demanded that any synthetic a priori judgements that are to be accepted as valid must be given a more rigorous form of proof.

This proof is his so-called "transcendental deduction." He writes (1787, p. 56): "Since these sciences actually exist, it is quite proper to ask how they are possible; for that they must be possible is proved by the fact that they exist." Thus Kant, accepting both the existence of demonstrable science (*epistêmê*), and also the validity of Hume's proof that a principle of induction cannot be empirically justified, was obliged to show how synthetic a priori judgements are possible which aren't merely dogmatic. Taking a more responsible stance than traditional rationalism, which, in practice, put no limits on arbitrary speculation, Kant sought to demonstrate only what he saw as the presuppositions of science as it actually existed. In addition, Kant sought an *objective* justification; in contrast to the "audacious pretensions... [which] demand to be accepted as actual axioms", Kant asserts that "it is indispensable that, if not a proof, at least a deduction of the legitimacy of such an assertion should be supplied... for if, in dealing with synthetic propositions, we are to recognise them as possessing unconditioned validity independently of any deduction... then, no matter how evident they may be, all critique of understanding is given up."[27]

Kant's argumentation is difficult, and I risk mangling his views to some extent here.[28] Yet I think it is not too much of a distortion to say that Kant's solution was to look to the *fact* of experience, unquestioned as a source of knowledge by Hume, for its formal presuppositions. Among these, Kant thought, was a formal principle of the type of inductive rule. If valid, such a general principle could then be shown to have the same security as any singular empirical report. As Popper summarises Kant's reasoning (2009, p. 68):

> With his own empiricist presuppositions, Hume would unconsciously have presupposed those very principles he doubts; indeed, he would virtually have made them into the foundations of all validity, since they would ultimately form the basis for the validity of all experience - and experience for Hume represents the highest authority in questions of validity. Hume's scepticism would have proved contradictory, and Hume's problem would be solved... This may be put more simply: it must be shown that all knowledge of nature, even every singular empirical statement, presupposes the existence of law-like regularities.

Let us take a simplification of one of Kant's transcendental arguments:

(i) Perceptual experience is impossible unless it obeys the axioms of Euclidian geometry.

(ii) Perceptual experience exists;

(iii) therefore, the axioms of Euclidian geometry are (*a priori*) true

The general form of such an argument runs like this:

(i) q is impossible unless p

(ii) q

(iii) therefore p

Something like this argument can be found in several modern defences of induction, as Popper noted (1983, p. 331): "...the transcendental argument... is now most fashionable among all kinds of inductive logicians who scorn Kant as an *apriorist* and transcendentalist... The argument may be found in connection with various probabilistic theories of induction in Russell, Jeffreys, and Reichenbach."

Accordingly, we find in Russell (1912 p. 107): "The general principles of science, such as the belief in the reign of law, and the belief that every event must have a cause, are as completely dependent upon the inductive principle as are the beliefs of daily life." And later, in *Human Knowledge, Its Scope and Its Limits (1948,* p. 524), Russell asserts that an inductive principle "certainly cannot be logically deduced from the facts of experience. Either, therefore, we know something independently of experience, or science is moonshine." This transcendental appeal is also made by Jeffreys in his

Theory of Probability—we must accept his (inductive) simplicity-postulate[29] "without hesitation" (1957, p. 35; 1961, p. 48) because scientific laws can gain no confirmations without it. The transcendental argument can also be found, I think, in Keynes, who asserts that "[i]nductive processes have formed, of course, at all times a vital, habitual part of the mind's machinery. Whenever we learn by experience, we are using them" (1921, p. 217). He urges that his inductive principle be accepted, despite its apparent circularity, because "the inductive method can only be based on it or on something like it" (p. 264). Carnap too, but reluctantly, writes in his *Logical Foundations of Probability,* "[t]he preceding considerations show that the following argument, admittedly not a strong one, can be offered in favour of m* [Carnap's version of the simple inductive rule]... Of the two m-functions which are most simple and suggest themselves as the most natural ones, m* is the only one which is not entirely inadequate" (1950, p. 565). And, slightly more recently, Salmon (1966, p. 11), makes a transcendental appeal: "'Hume's paradox,'... although ingeniously argued, is utterly repugnant to common sense and our deepest convictions. We *know* ("in our hearts") that we have knowledge of unobserved fact. The challenge is to show how this is possible."

Although not quite explicit, a plausible reconstruction of this line of argument, as a justification of induction, runs as follows:

(i) Science (or learning from experience) depends on an inductive principle.
(ii) Science (or learning from experience) exists;
(iii) therefore induction exists (and can be justified independently of experience).

The problem with arguments of this form is simply that they are circular. They all assume, in the first premise, what they set out to demonstrate in the conclusion. Consider the Kantian example. An adequate transcendental proof obviously requires more than the mere *assertion* that perceptual experiences requires some presupposition, it requires demonstrating that this presupposition is *the only possible one* allowing perceptual experience. Kant had simply asserted that no experience is possible without his presuppositions. This is not a proof. All such arguments are circular.

As Popper commented (1983, p. 319), a transcendental argument... "should not be used, as it so often is, to argue in favour of any particular theory of learning; for it is an argument which always amounts to the more or less explicit claim that the theory in question is the only possible one. (It is surprising to find how many incompatible theories have been claimed to be the only possible ones)."[30] In the case of an inductive principle, the argument generally assumes that theoretical science, which surely exists, is justified or probable knowledge (in the sense of the logical calculus of probability). Or it is assumed that knowledge, as it exists, is possible *only* on the condition that induction exists. But that is just the point at issue. If an inductivist assumes that theoretical knowledge is *epistêmê*, he (or she) has begged the question. Yet if he (or she) refers to knowledge or science only *descriptively*, in the sense of an organised body of knowledge, then it still remains to be proved that this sense of "scientific knowledge" cannot exist *except* on the assumption of a valid theory of induction. In either case, the argument is circular.[31]

4.9 The Inductive Principle as Pragmatically Vindicated

Yet another strategy to consider is *vindicationism*, a novel approach developed by Hans Reichenbach (1938, §§ 38-41), and later taken up by Herbert Feigl (1950) and Wesley Salmon (e.g. 1961, 1963, 1963). This approach is also often dubbed the *pragmatist* response to the problem of induction (Kaplan, 1998) This approach holds that although an inductive principle can neither be justified a posteriori (that is, empirically), or a priori (that is, analytically—on the basis of logic or mathematics), it may nevertheless be *vindicated*.

To explain vindicationism, something must first be said about Reichenbach's general theory of knowledge. In Reichenbach's view, all of our knowledge—both scientific and commonsensical—is inherently probabilistic, and the only acceptable interpretation of probability is the frequentist theory, in which probability is understood as the limit of a sequence of relative frequencies.[32] Such limiting frequencies are, according to Reichenbach, the basis of all knowledge claims. To apply these doctrines to the theory of induction, we may consider a series of experimental reports which record the frequency of which observed *A*s are *B*. From these experimental observations we can generate a sequence of fractions m/n representing this frequency. If this sequence of fractions has a limit then the members of this sequence will become and remain arbitrarily close to some value, and as the sequence is extended indefinitely, then that limit constitutes the correct ratio of *A*s which are *B*. It is Reichenbach's claim that we are justified in positing that the observed frequency in an initial section of the sequence, does, in fact, approximate the limiting frequency. Thus Reichenbach's version of the principle of induction takes the form of a rule of acceptance based upon his theory of limiting frequencies—it allows us to assert, if n per cent of observed *A*s have been *B*, that n per cent of the remaining *A*s are also *B*. As such, it is essentially a *probabilistic* version of the traditional inductive principle that nature is uniform or that the future will be like the past.[33] This "straight rule" may be expressed thus:

Straight rule of induction: If $F^n(A, B) = m/n$, then in the limit as $n \rightarrow \infty$, $F^n(A, B) = m/n$.

Of course, we cannot *know* that such a limit exists—there may be no statistical regularity to converge to, and hence no correct probability to discover. However, the employment of Reichenbach's inductive method, he claims, "must lead to the true value, if there is a limit at all." (1938, p. 353).

How Reichenbach justifies this principle is innovative. As Wesley Salmon (1998) states:

> The justification is pragmatic. First, if no limit exists, we will be wrong in our posits, but no other method would succeed, for there is no limit to be discovered. Second, there may be a limit, but it may be quite different from the frequency in the observed initial portion of the sequence. It follows from the definition of 'limit', however, that persistent use of this method, 'the rule of induction', will eventually lead to posits that are accurate to any desired degree of approximation. In ascertaining probabilities (that is, limiting frequencies) we have everything to gain and nothing to lose by using the inductive method.

In other words, Reichenbach admitted that it is not possible to demonstrate that the inductive method *will* work reliably, but he asserted that all that is necessary to answer Hume is to show that *if* any method for predicting the future (ascertaining probabilities) *will* be successful, then an inductive method will be *at least as successful.* On the condition that *any* method will work, then no alternative method will *outperform* an inductive method. As Reichenbach stated (1935/1971, p. 475), "the rule of induction is justified as an instrument of positing because it is a method of which we know that if it is possible to make statements about the future we shall find them by means of this method."

Watkins compares Reichenbach's vindicationism to Pascal's wager (1984, p. 98):

> if the world is fundamentally lawless and unpredictable (if God does not exist), then we lose nothing by applying an inductive method to it, for no noninductive method would succeed any better; but if the world is, at least to some degree and in some domains, lawful and predictable (if God does exist), then an inductive method will succeed at least as well as any other. An inductive strategy, we might say, is a dominant strategy in the game-theoretical sense: it may well serve you better and cannot serve you worse than any noninductive strategy.

However, although this approach is much weaker than any of the previous ones surveyed, it is still too strong. For it can be demonstrated that an infinite class of incompatible asymptotic rules or principles of induction can be equally vindicated by Reichenbach's method, and there is no principled way to choose between these incompatible principles. As a method of demarcation, the vindicationist approach is empty—it justifies an infinity of inductive principles of the following argument form: $m/n + c_n$, where c_n is a "corrective term" that approaches zero as n approaches infinity. Each of these variants will find the true relative frequency if any will, yet among them are principles that licence inferences that are intuitively counter-inductive and unsupported by the evidence. Moreover, it is possible to construct possible worlds in which even intuitively highly unreasonable counter-inductive principles *will* perform better than Reichenbach's inductive principle (See Watkin's 1984, pp. 99-100 for an example). Watkins also notes that Reichenbach's vindicationism conflicts with Carnap's claim (in his 1950) that for any two prediction methods, one of which seems extremely illogical compared to a more intuitively "inductive" rival, there will be possible worlds in which the former works better.

Thus Reichenbach's inductive method is ineffective for demarcation—the claim that an inductive method *will* deliver better predictions than some noninductive method cannot be defended either a priori or a posteriori, and nor can it be vindicated.

4.10 The Inductive Principle as Self-Justifying

A final approach is to claim, quite frankly, that an inductive principle needs no external validation because it is self-justifying. One way to do this is to claim that following inductive procedures is just what "being rational" *means*. Ayer took this position in his *The Problem of Knowledge (*1956), and it can also be discerned in Strawson (1952, p. 261).[34] Admitting the cogency of sceptical criticisms of induction, Ayer wrote that these were to some extent irrelevant, for induction "could be irrational only if there were a standard of rationality which it failed to meet; whereas in fact it goes to set the standard: arguments are judged to be rational or irrational by reference to it" (1956, p. 75). Accordingly, a philosopher must merely *describe* such procedures; no justification is necessary: "When it is understood that there logically could be no court of superior jurisdiction, it hardly seems troubling that inductive reasoning should be left, as it were, to act as judge in its own cause. The sceptic's merit is that he forces us to see that this must be so" (ibid, p. 81).

Colin Howson (2000, p. 17), echoing Popper's account in his (1974, pp. 168-9), notes that Nelson Goodman's "reflective equilibrium" approach is a "slightly more sophisticated" variant of this response, whereby inductive inferences are purportedly justified:

> ...by whether or not they conform with accepted canons as expressed in the judgements people actually make... Principles of deductive inference are justified by their conformity with accepted deductive practice. Their validity depends upon accordance with the particular deductive inferences we actually make and sanction. If a rule yields unacceptable inferences, we drop it as invalid... All this applies equally well to induction (Goodman, 1946, pp. 62–5).

Putting aside the difficulty of even formulating a plausible inductive principle, this argument is obviously question-begging. As Bartley writes in response to Ayer, "Ayer's argument... would be valid only *if* the standards to which he is committed are assumed to be the correct ones. *Yet that is just what is at issue"* (1962, pp. 129-130). Such an approach is clearly useless for demarcation, since questions of truth and falsity cannot be solved by merely postulating a fact (if it *is* a fact) about linguistic usage.[35] The question remains open *whether* such procedures are correct. Howson (2000, p. 17) further adds that the attempted analogy with deductive arguments fails, for a "rule of deductive inference is not judged valid according to the standard of whether or not it conforms with practice; it is defined to be valid... if no counterexample exists, and it is judged valid if it can be shown that this condition is satisfied. In other words, deductive rules are justified only if it can be shown that they satisfy appropriate semantic criteria." This is essentially in agreement with Popper's point in *Unended Quest* (1974a, pp. 168-9):

> ...a deductive inference is *valid if no counterexample exists*. Thus we have a method of objective critical testing at our disposal: to any proposed rule of deduction, we can try to construct a counterexample. If we succeed, then the inference, or the rule of inference, is invalid, whether or not it is held to be intuitively valid by some people or even by everybody... As we have objective tests and in many cases even objective proofs at our disposal, psychological considerations, subjective convictions, habits, and conventions become completely irrelevant to the issue.

In short, this response to Hume is nothing more than an endorsement of the dogmatic horn of Agrippa's trilemma. It does nothing to rationally justify inductive inference or aid in objective theory choice.

4.11 Conclusion

If the arguments in this chapter are valid, there is no possibility of deducing a genuinely universal statement from a finite conjunction of singular statements without the adoption of some form of inductive principle. Yet if the principle is to be strong enough to deductively entail its conclusion it will necessarily be a strong synthetic statement that cannot itself be justified without begging the question. There is thus no rational method to certify or demonstrate the truth of a universal hypothesis; as a consequence the classical inductivist or Baconian attempt to solve the demarcation problem fails.

Yet might the evidence nonetheless raise the probability of a universal statement? Might, that is, inductive inference *partially* justify the truth of a scientific hypothesis? Might scientific hypotheses be more or less probable, in the sense of the mathematical calculus of probability? These questions, and the popular appeal to probability to aid in theory choice, will be explored in the following chapter.

---

[1] As the title suggests, Bacon saw his method as a successor to that of Aristotle's *Organum,* which was, he felt, inadequate to separate truths from falsehoods.

[2] Colin Howson (2000, p. 8) characterises Bacon as seeking a "third way" between "the still-dominant philosophical apriorism deriving from Aristotle and the unstructured empiricism of the alchemists and others: that of systematic, carefully designed observation, designed to elicit the hidden causal springs underlying the appearances..."

[3] Howson (2000, p. 8 fn3), citing Peter Urbach's (1987), criticises Popper's presentation of Bacon as propounding the view that science is "a system of certain, or well-established, statements", which "starts from observation and experiment and then proceeds to theories" (1959, pp. 278-279). Yet Howson also seems to attribute these doctrines to Bacon—he wrote earlier in the same text that "the Baconian ideal of a secure pathway from experience to truth dies hard" (p. 4) and that: "Bacon famously dismissed [Aristotle's logic] as quite useless for deciding what are the sound inferences from observation. In other words, Aristotle's logic was not a logic of induction, and this Bacon set out to provide... deductive inferences cannot 'discover' facts not already implicit in what is premissed. Such a feature is now recognized to be a necessary condition of the correctness of any deductive inference, but it is clearly of no use to anyone who, like Bacon, wishes to conclude generalizations from premisses about particular instances" (p. 7 & fn2). Moreover, while Howson is correct that Bacon should not be caricatured as a primitive "induction by enumeration" theorist, in the passage that he cites Popper does not in fact make this claim—instead Popper refers to Bacon as proposing a form of *eliminative* induction (1959, p. 279).

[4] Howson (2000, p. 9) writes of the scientific revolution that "there has been an almost universal perception of Bacon as its spiritual leader and chief propagandist. Most revolutions acquire a defining manifesto, then or later. Later commentators have seen as the manifesto of this one the *Novum Organum*. The early members of the Royal Society certainly did, and Sprat's *History* of the Society (1667) cites Bacon's work as 'the best... that can be produced for the defence of Experimental Philosophy; and the best directions, that are needful to promote it'. Cowley contributed an Ode to that History which declared that 'Bacon, like Moses, led us forth at last...'" Despite this, Bacon's methodology was "[m]uch criticized as scientifically useless in the nineteenth and twentieth centuries, and never seriously employed" (ibid).

In a similar, but more critical vein, Wesley Salmon writes (1966, p. 3): "Bacon realized that scientific knowledge must somehow be built upon inductive generalization from experience, and he tried to formulate the principles of this new logic—"a true induction." He confidently predicted that the assiduous application of this method would answer all important scientific questions. Looking back, we must regard his characterization as extremely primitive and wholly inadequate to the complexity of scientific method. His optimism for the future of science was charmingly naive. He was, nevertheless, the enthusiastic herald of the new inductive method of science, and this in itself is an important contribution."

[5] It was precisely this argument which famously awoke Kant from his *"dogmatic slumber"* (1783, p. 9).

[6] Popper addressed Hume's argument again in *The Logic of Scientific Discovery* (1959, pp. 5-6): "That inconsistencies may easily arise in connection with the principle of induction should have been clear from the work of Hume; also, that they can be avoided, if at all, only with difficulty. For the principle of induction must be a universal statement in its turn. Thus if we try to regard its truth as known from experience, then the very same problems which occasioned its introduction will arise all over again. To justify it, we should have to employ inductive inferences; and to justify these we should have to assume an inductive principle of a higher order; and so on. Thus the attempt to base the principle of induction on experience breaks down, since it must lead to an infinite regress.

Kant tried to force his way out of this difficulty by taking the principle of induction (which he formulated as the 'principle of universal causation') to be '*a priori* valid'. But I do not think that his ingenious attempt to provide an *a priori* justification for synthetic statements was successful.

My own view is that the various difficulties of inductive logic here sketched are insurmountable."

[7] Colin Howson, the prominent Bayesian theorist of science, refers to Hume's result as his "circularity thesis... that all inferences from experience suppose, as their foundation, that the future will resemble the past—in some way or other." He goes on to note that Hume's " argument has stood since it was first presented, a philosophical classic, not really believed but withstanding all attempts to overturn it. The continuing failure suggests that it might actually be correct. I believe that, for all its apparent absurdity, it is" (2000, pp. 10-11).

[8] "The doctrine that there are synthetic a priori statements is, I take it, the thesis of rationalism. It was maintained by Kant, as well as by many other philosophers both before and after him. The doctrine that all a priori statements are either analytic or self-contradictory is the thesis of empiricism as I understand it" (Salmon, 1966, p. 34).

[9] Bartley notes that a variation of this problem of logical strength underlaid the positivist problem of meaning (1984, p. 189): "Another way of putting this, by Herbert Feigl, is to say that such statements "possess a surplus meaning over against their evidential basis; they are not equivalent with or reducible to... any set of actual or possible confirming statements.""

[10] An example is Russell's account of induction in his *The Problems of Philosophy* (1912), in which he appeals to both an inductive principle *and* to probabilistic inference in his attempted solution to Hume's problem.

[11] This enthymematic approach is applied by some authors to the analysis of *all* invalid arguments—see, for instance Musgrave 1989a, § 1.

[12] Colin Howson seems to agree with this construal of the problem of induction as one of logical strength; he writes (2000, p. 2), "Hume [showed], in general terms, that a sound inductive inference must possess, in addition to whatever observational or experimental data is specified, at least one

independent assumption (an inductive assumption) that in effect weights some of the possibilities consistent with that evidence more than others. I take this to be a great *logical* discovery, comparable to that of deductive inference itself..." He later notes (2000, p. 19), "[o]ne of the commonest of responses to Hume is to concede the insolubility of the problem as it is stated, and say that what it reveals is the need for an 'inductive principle', that is to say an additional premiss permitting the passage, deductively, from appropriate observation-reports to conclusions asserting either the truth or the high probability of some prediction or even general theory."

[13] It should be noted, however, that the claim that Carnap's phenomenalism was primarily epistemologically motivated is controversial.

[14] Salmon, 1966, p. 10, explains: "Hume and many of his successors noticed that typical inductive inferences... would seem perfectly sound if we could have recourse to some sort of principle of uniformity of nature. If we could only prove that the course of nature is uniform, that the future will be like the past, or that uniformities that have existed thus far will continue to hold in the future, then we would seem to be justified in generalizing from past cases to future cases—from the observed to the unobserved."

[15] Miller (ibid) also notes that Howson & Urbach (1993, Chapter 1), pose a closely analogous question: "Suppose that a certain number of metals have on a certain number of occasions been reliably observed to expand shortly after being heated. If we wished to combine this information with a Uniformity of Nature principle, in order to derive the conclusion that *all* metals expand *whenever* they are heated, what form would that principle have to take?"

[16] As Popper and Miller (1987, p. 44) assert, summarising a result originally published in *Nature* (1983): "...relative to evidence e, the content of any hypothesis *h* may be split into two parts, the disjunction *h* v e (read *h* or *e)* and the material conditional $h \leftarrow e$ (read *h* if *e);* and the 'ampliative' part of *h* relative to e was identified with this conditional $h \leftarrow e$; that is, with the deductively weakest proposition that is sufficient, in the presence of e, to yield *h."*

This result will be further discussed in section 5.4 below.

[17] The following principle of Russell's seems to be of this kind, "The belief in the uniformity of nature is the belief that everything that has happened or will happen is an instance of some general law to which there are no exceptions" (1912, p. 99).

[18] That is, it would presumably take the form, approximately, of some principle like the following: "Where *F* and *G* are projectible, whenever at least some *Fs* are observed and *n* per cent of all the *Fs* observed are *G*, *n* per cent of the remaining *Fs* are *G*."

[19] Howson (2000, p. 19) notes that a "recent advocate seems to be Maxwell (1998), who calls it a 'principle of the comprehensibility of the universe'. However it is precisely characterized, such an inductive principle... enjoys the status of a postulate whose justification is its indispensability for factual knowledge."

[20] Robert Fogelin employs a similar strategy in his (1994 p. 194): "I have not attempted to survey every

theory of empirical justification and to show that each of them is unsatisfactory. I have not, in lieu of this, attempted to produce a systematic and exhaustive classification of every possible theory of empirical justification and then, on the basis of this classification, argued that every possible type of theory of justification must be inadequate... It is possible that someone will produce a wholly new sort of theory of empirical justification that will provide a satisfactory solution to the Agrippa problem, or perhaps someone will accomplish this through hitting upon an utterly novel way of developing one of the traditional theories of empirical justification. It would be an unseemly dogmatism to rule these possibilities out in advance. What I have tried to show, using a number of exemplary cases, is that Pyrrhonian skepticism, when taken seriously and made a party to the debate, is much more intractable than those who have produced theories of empirical justification have generally supposed. As far as I can see, the challenge of Pyrrhonian skepticism, once accepted, is unanswerable."

[21] An example of a so-called counter-inductive rule is the following: "Given m/n of observed A are B, infer that the "long run" relative frequency of B among A is $(n - m)/n$."

[22] Earlier, he had written in the same vein, (1966, p. 11): "we cannot justify any sort of ampliative inference *inductively*. To do so would require the use of some sort of nondemonstrative inference. But the question at issue is the justification of nondemonstrative inference, so the procedure would be question begging."

[23] See also Russell's "The Limits of Empiricism", *Proceedings of the Aristotelian Society* 36 (1936), p. 131.

[24] Mill was evidently referring primarily to Kant here, but of course, ideas respect no national boundaries, and Kant had many followers in 19th century England, most notably Whewell.

[25] Salmon (1966, p. 55) in like manner warns that, "we have enough painful experience to know that the appeal to obviousness is dangerously likely to be an appeal to prejudice and superstition. What is obvious to one age or culture may well turn out, on closer examination, to be just plain false."

[26] As Popper remarked (1983, p. 87), "The difference between Russell's *apriorism* and Kant's mainly lies in Russell's formulation of his inductive principle as a set of rules of *probable* inference."

[27] The translations here of Kant's *Critique* are from Popper's (2009, Ch. 4).

[28] Nimrod Bar-Am (2008, pp. 116-117) makes the following interesting comments on Kant's transcendental logic, which he regards as a successor to Aristotelian induction: "Aristotle's *nous* and Kant's transcendental proof play similar roles: indeed the latter is a refinement and an elaboration of the former. The two roughly share the following structure: the existence of perceptual experience is taken as evident; then some (supposedly) universal knowledge is declared its inevitable presuppositions. This is all there is to it. The (supposedly) universal knowledge is declared a necessary prerequisite to the very possibility of perceptual experience. We then conclude that it must be true. Aristotle used a rough version of this line of argument as a justification of theories that he deemed scientific. Kant refined it, developed it, and used it for the very same purpose."

[29] This asserts, in effect, that a simpler law has a higher initial probability than a less simple law.

[30] Popper had elsewhere alluded to Carnap's acceptance of the zero probability of universal laws as

being in direct conflict with Jeffrey's system whereby it was claimed that such a view would entail the impossibility of learning from experience. See Popper's (1959, Appendix *viii).

[31] As Howson notes (2000, p. 18), "the Kantian 'transcendental deduction' is unsound... even if there were ever a sound deduction, it would have to employ some non- tautological premisses, and we should then need to enquire how they were established (at this point Hume enters again). Either that or there is an infinite regress of justification, and nothing is achieved."

[32] Reichenbach's views on probability are more systematically presented in his *The Theory of Probability* (1949).

[33] Reichenbach asserts (1935/1971, p. 351) that this method is simply induction by enumeration; it is "based on counting the relative frequency [of a certain attribute] in an initial section of the sequence, and consists in the inference that the relative frequency observed will persist approximately for the rest of the sequence; or, in other words, that the observed value represents, within certain limits of exactness, the value of the limit for the whole sequence."

[34] Thus, Strawson (1952, ch. 9) writes that "[i]t is an analytic proposition that it is reasonable to have a degree of belief in a statement which is proportional to the strength of the evidence in its favour; and it is an analytic proposition, though not a proposition of mathematics, that, other things being equal, the evidence for a generalisation is strong in proportion as the number of favourable instances, and the variety of circumstances in which they have been found, is great. So to ask whether it is reasonable to place reliance on inductive procedures is like asking whether it is reasonable to proportion the degree of one's convictions to the strength of the evidence. Doing this is what 'being reasonable' *means* in such a context."

Howson makes the following apt remarks on this procedure (2000, p. 16): "This [approach] completely misses the point. Saying that *by definition* 'being reasonable' includes in its meaning an acceptance of inductive reasoning implies nothing at all about the non-linguistic world, and in particular nothing about its tendency to verify (or not) predictions based on such 'good reasons'."

[35] Wesley Salmon too rejects this strategy (1966, pp. 47-48): "If a philosopher embraces the postulational approach to induction, he must not boggle at frankly making factual assumptions without attempting any justification of them. This is clearly an admission of defeat regarding Hume's problem... admission of unjustified and unjustifiable postulates to deal with the problem is tantamount to making scientific method a matter of faith."

Chapter Five: Probabilism and Demarcation

5.0 Introduction

In the last chapter I argued for the untenability of strong or conclusive justificationism. Specifically, I argued with Hume that there is no possibility of establishing the truth of a strictly universal statement—an empirical generalisation—or indeed, *any* prediction concerning the unobserved. This result applies even on the assumption that the reports of experience which function as evidential premises are taken as secure and in need of no justification themselves. Might a more modest form of justificationism, which eschews conclusive support, fare better?

Such a fallibilist version of justificationism is the currently most popular response to Hume. It accepts that all factual hypotheses with unverified predictive implications are uncertain—conclusive justification is given up. However, it is maintained that *partial* justification is still very much attainable. Scientific knowledge, it is claimed, may nonetheless, through empirical confirmation, be made *probable*. The justificationist ideal of conclusive proof has been weakened to *probable* truth—what is sought is *probability* rather than certainty. As Nicholas Rescher (1998, § 1) writes:

> Fallibilism is a philosophical doctrine regarding natural science... which maintains that our scientific knowledge claims are invariably vulnerable and may turn out to be false. Scientific theories cannot be asserted as true categorically, but only as having some probability of being true...we can never achieve certain knowledge (*epistēmē*) in matters regarding the world's ways, but have to make do with what is merely probable or plausible.[1]

In this chapter I will examine this *weak* justificationist response to the demarcation problem. Despite the ostensible modesty of such a position, it is still vulnerable to sceptical arguments. In particular, I will argue that the popular justificationist appeal to the calculus of probability is of no avail in theory adjudication. Scientific theories cannot be rendered interestingly probable without the aid of an inductive principle because probabilistic support is not ampliative, and such an inductive principle could not aid in demarcation without itself being demonstrated as true. Thus, inconclusive or partial inductive support is as unobtainable as conclusive support.

Still more fundamentally, even if high probabilities were attainable, this would not thereby solve the demarcation problem. An assessment of probability cannot validly sanction the classification of a theory as true without introducing an (unjustified) extra-logical principle. As such, weak justificationism, or probabilism, cannot adjudicate in any serious theoretical dispute concerning the truth value of a hypothesis, and the demarcational advice it does offer ('Obtain high probabilities!') leads to results which conflict with scientific practice.

5.1 Weak Justificationism and Probabilism

As I quoted Rescher in the introduction, fallibilism holds that "we can never achieve certain knowledge (*epistēmē*) in matters regarding the world's ways, but have to make do with what is merely probable or plausible". This fallibilist epistemological position is more or less the orthodoxy amongst philosophers of science,[2] and its scope is comprehensive. Not only is conclusive or sufficient justification denied in the case of universal laws, but observational and evidential statements are also considered open to doubt. Indeed, even the laws of logic are sometimes held to be open to revision (Quine, 1951; 1960, p. 59).

However, although it is admitted that scientific knowledge cannot be established with absolute certainty, weak justificationism holds that it may nonetheless be established "beyond any reasonable doubt." As Sokal and Bricmont write in *Intellectual Impostures* (1998, pp. 57f.):

> ... there is no *general* justification of the principle of induction (another problem going back to Hume). Quite simply, some inductions are justified and others are not; or, to be more precise,

> some inductions are more reasonable and others are less so... In a sense, we always return to Hume's problem: No statement about the real world can ever literally be *proven;* but... it can sometimes be proven beyond any *reasonable* doubt.

That is, conclusive justification is too much to ask for, but laws may yet be rendered *probable* by experience. This is the fundamental point of disagreement between fallibilism and scepticism—whereas Hume had declared that his sceptical result entailed that he could "look upon no opinion *even as more probable* or likely than another" (1739-40, pp. 268-269, my italics), weak justificationism holds that knowledge claims may be more or less probable or secure, or reliable in an epistemological sense. Hume, on the weak justificationist or fallibilist account, merely proved "the platitude that induction is not deduction... [yet] the function of *deduction* [is] to prove the truth of conclusions, given true premises. Induction has a different function. An inductive inference with true premises establishes its conclusions as *probable"* (Salmon, 1966, p. 48).

We may represent these divergent epistemological positions in the following table:

	Certain Knowledge	**Probable Knowledge**
Strong Justificationism	Yes	Yes
Weak Justificationism (Fallibilism)	No	Yes
Scepticism	No	No

As we can see, Humean scepticism holds that no factual knowledge can be rendered probable—that *all* inference from the known into the unknown is unwarranted. That is, according to this sceptical position, no statement whatsoever that goes beyond past observation can be either established as true, *or even as probable*. Weak justificationism or fallibilism holds that not all hypotheses are equally uncertain —indeed, they may be differentiated in terms of their differing evidential support. Miller (2006a, pp. 71-2) quotes Peter Lipton (1995, p. 34) expressing precisely this view:

> ... we must not confuse scepticism with fallibilism. A fallibilist account of knowledge is the ground that we all wish to occupy: neither theory nor data are very certain. Hume, however, was no mere fallibilist about induction. He did not claim [only] that the conclusions of inductive inferences are uncertain: he claimed they were epistemically worthless.

Thus, Humean scepticism goes beyond fallibilism—it renounces not only the pursuit of certainty, but also partial justification. It asserts that not only can scientific laws not be verified, they can also not be rendered probable.[3]

This view is directly opposed to the weak justificationist demarcational program. Probability, for weak justificationism, is to function as the primary demarcational criteria. All knowledge is uncertain, yes, but we may grade hypotheses according to their degree of probability or empirical confirmation.[4] (As Mary Hesse (1974, p. 142) has noted, the reason why the so-called paradoxes of confirmation have been so troubling for justificationists is because they trivialise confirmation—that is, they violate "the tacit condition that confirmation must be a *selective* relation" (my emphasis) among propositions).[5] Indeed, as justificationism is also a theory of rationality, probability (or partial support), is also held to be central to *rational* acceptance—acceptance or non-acceptance of a proposition should be controlled by its degree of confirmation. That is, rational credence requires a hypothesis be empirically justified or confirmed to some degree—minimally, at least to a greater degree than its negation. Rationality consists in having, if not *sufficient*, at least *probable,* reasons for your opinions.

Weak justificationism also has a corresponding theory of the *aim* of science—the goal of science is probable truth, or theories that are highly probable on the evidence. Strong justificationism held that the aim of science was *certain* truth; that being unattainable, *probable* truth seems a plausible substitute. (A non-justificationist or sceptic would suggest that truth, without any epistemological qualification, is the proper aim). Methodologically, weak justificationism forms the epistemological backdrop to the search for inductive probabilities—the aim of science is to attain high probabilities for its theories. This intuitive progression was noted by Popper (1983, pp. 221-2):

> The view of science from which this belief springs is fundamentally the old view of Science with a capital 'S'. It is the view of science as *scientia* or *epistēmē*—as certain, demonstrable,

knowledge. No doubt, this view is now somewhat modified: everybody now realizes that full certainty is unattainable in the sciences which are called 'inductive'. But as induction is considered a kind of (weakened) generalization of deduction, the old ideal is only slightly modified.

By looking upon inductive probability as a measure of the reasonableness of our beliefs or the reliability of our knowledge, the devotee of probable induction makes it clear that he still clings, like Bacon, to a weakened ideal of *epistēmē*. He conceives his evidential statements e as playing a part analogous to that of the self-evident axioms supposed to 'prove' our theorems. And he conceives his hypothesis *h* as playing a part analogous to that of theorems whose truth is made certain by deduction from the axioms; only that, induction being weaker than deduction, we now get merely an *Ersatz* certainty: probability comes in as the substitute, or surrogate, of certainty—not quite the thing, but at least the next best thing, and at any rate approaching it.

Thus, weak justificationism maintains, in agreement with strong justificationism, that scientific knowledge is still both *secure* and *authoritative*, if not absolutely invulnerable. As Miller states (2006a, pp. 147-148):

...many defenders of science... think... that science verges on the indubitable. The results of science are not certain, it is agreed, but they come close; they are not irrevocably proved by observation and experiment—that asks too much—but they are overwhelmingly supported by observation and experiment. Scientific theories, it is maintained, are justified in a way that most other beliefs are not.

An immediate problem that weak justificationism faces is how exactly this partial justification is to be attained. As noted in the last chapter, an argument, to be conclusive, must possess demonstrable or sufficient premises, and the inference must be valid—that is, truth-preserving. Hence, it seems that an argument providing partial support would have to weaken one or both of these components. This is represented in the following table:

	Strong (Sufficient) Justificationism	Weak (Partial) Justificationism
Premises	Sufficient, Certain	Probable, Plausible
Inference	Classical Entailment	Partial Entailment

If the arguments in the last chapter are correct, at least the second source of insufficiency must obtain—there is no way to validly derive, using classical logic, a universal statement from a finite collection of singular ones. I will further argue in the next section that the premises reporting observational facts cannot be certain. Hence *both* uncertain premises and uncertain inferences must be admitted; that is, inferences that possess not the relation of logical implication or logical consequence, but instead a relation of *partial* implication or *partial* consequence. The usual candidate here, as already alluded to, is probabilistic support.[6] As Popper wrote: "Probability comes in as the substitute, or surrogate, of certainty—not quite the thing, but at least the next best thing, and at any rate approaching it" (1983, p. 222).

Salmon (1966, p. 8) describes the move to insufficient inference as follows:

> ... one distinction is fundamental, namely the distinction between demonstrative and nondemonstrative inference. A *demonstrative* inference is one whose premises necessitate its conclusion; the conclusion cannot be false if the premises are true. All valid deductions are demonstrative inferences. A *nondemonstrative* inference is simply one that fails to be demonstrative. Its conclusion is not necessitated by its premises; the conclusion could be false even if the premises are true. A demonstrative inference is *necessarily truth-preserving;* a nondemonstrative inference is not.
>
> The category of nondemonstrative inferences, as I have characterized it, contains, among other things perhaps, all kinds of fallacious inferences. If, however, there is any kind of inference whose premises, although not necessitating the conclusion, do lend it weight, support it, or make it probable, then such inferences possess a certain kind of logical rectitude. It is not deductive validity, but it is important anyway. Inferences possessing it are *correct inductive inferences.*

"Probability", however, is not a univocal term; that is, the formal mathematical calculus lends itself to many conflicting interpretations. We may first distinguish, with

Popper (1959, p. 135), between subjectivist and objectivist interpretations. Popper writes that whereas "the subjective theory of probability springs from the belief that we use probability only if we have insufficient knowledge" (1983, p. 281), objective theories "take probabilities as properties of certain physical systems—experimental set-ups, for example'"[7] (ibid, p. 295). Foremost among the objective interpretations are the frequency theory of Richard von Mises, Dörge, Kamke, Reichenbach and Tornier, and also the Popper of *The Logic of Scientific Discovery.* This interpretation "treats every numerical probability statement as a statement about the *relative frequency* with which an event of a certain kind occurs within a *sequence of occurrences*" (1934, p. 149). We may also classify the classical (Laplacean) theory of probability as objective—this defines probability, as in games of chance, as the number of favourable cases divided by the number of possible cases.[8] Popper's propensity interpretation, which he worked out in detail only in the 1950's, is also objective, and is perhaps most closely related to the classical interpretation—"the propensity interpretation is very closely related to the interpretation which takes probability as a measure of possibilities. All that it adds to this is a physical interpretation of the possibilities, which it takes to be not mere abstractions but physical tendencies or propensities to bring about the possible state of affairs" (Popper, 1983, p. 286).

Opposed to these are what Popper calls, more problematically, *subjective* interpretations of probability. In this group, uncontroversially, is the subjective theory proper, subjective Bayesianism or Personalism, which takes probabilities to be subjective degrees of belief, measuring actual feelings of certainty or uncertainty, of belief or doubt. This interpretation of probability is associated most closely with the names of Ramsey, de Finetti, Good, Savage, and successors such as Colin Howson and Peter Urbach.

However, in the *Logic* Popper also puts in this group the *logical* interpretation of probability. He writes (1959, p. 136):

> A newer variant of the subjective interpretation, however, deserves more serious consideration here. This interprets probability statements not psychologically but logically, as assertions about what may be called the 'logical proximity' of statements. Statements, as we all know, can stand in various logical relations to one another, like derivability, incompatibility, or mutual independence; and the logico-subjective theory, of which Keynes is the principal exponent, treats the probability relation as a special kind of logical relationship between two statements.

> The two extreme cases of this probability relation are derivability and contradiction: a statement *q* 'gives', it is said, to another statement *p* the probability 1 if *p* follows from *q*. In case *p* and *q* contradict each other the probability given by *q* to *p* is zero. Between these extremes lie other probability relations which, roughly speaking, may be interpreted in the following way: The numerical probability of a statement *p* (given *q*) is the greater the less its content goes beyond what is already contained in that statement *q* upon which the probability of *p* depends (and which 'gives' to *p* a probability).

This interpretation, as measuring logical proximity or partial entailment, is seemingly as objective and tautologous as classical logic. However, Popper nevertheless labels it subjective in this context because of the widespread identification made by inductivists, such as Keynes (1921), of the logical interpretation with the "degree of rational belief."[9] By this, Keynes meant "the amount of trust it is proper to accord to a statement *p* in the light of the information or knowledge which we get from that statement *q* which 'gives' probability to *p*" (ibid). That is, according to this view, *p(h, e)* measures the *rational*, as opposed to *actual*, degree of belief in *h* given *e*. It was in this sense that the logical interpretation of probability was championed by such theorists of induction as Keynes (1921), Hosiasson-Lindenbaum (1940), Jeffreys (1948), Reichenbach, Carnap (1950), and Hintikka, amongst others.

The fundamental idea of this interpretation is that probability logic is a genuine extension or generalisation of classical logic. In classical logic, the only relationship a universal statement can stand in relation to a conjunction of singular existential statements is inconsistency or logical independence. For, as Hume noted, a finite conjunction of singular existential statements cannot entail a universal statement, and further, universal statements have no "existential import"—a universal statement *h* will either be logically independent of the evidence *e*, or inconsistent with it. As such, *e* cannot provide any logical support for *h*. Even if we take *h* not as a universal law but instead as a prediction only about the next instance, *e* and *h* will still be logically independent. The inference to draw from this is that evidence cannot provide any support to knowledge which transcends it, which is exactly Hume's result.

Probability logic changes this picture. In addition to the classical relations between statements of entailment, independence, and inconsistency, it recognises partial entailment. In particular the conditional probability p(*h, e*) is taken to be a generalisation of the deducibility relation $\vdash$. With this apparatus, it is possible to determine for two statements e and h the logical proximity of the two statements. In particular, this measure of the logical probability of a hypothesis, denoted by p(h, e), was also taken by justificationists to be a measure of how well h is empirically confirmed or backed by evidence, and further, of the rational degree of belief an agent should accord to h. That is, for e and h there is a number r ($0 \leq r \leq 1$) for a confirmation function c such that c(h, e) = r (Carnap, 1950, Preface (3)). In other words, in the case of a hypothesis h that is not entailed by the evidence e, it was held to be possible to establish, with the help of probability logic, that h is more or less strongly confirmed by e. However, this function, p(h, e), has now been widely rejected as a suitable measure of the extent to which e confirms h (see also Salmon 1975 on this point). In its place, the support function s(h, e), also advocated by Carnap (1950, §86) and called "the degree of relevance", is nearly universally preferred.[10] As Maria Carla Galavotti writes (2007, p. 121), Carnap, and with him practically all subsequent confirmation theorists, regarded "relevance confirmation as the genuine sense in which a hypothesis can be said to be confirmed by the given evidence and calls it 'positive relevance'." This will be discussed further below.

Before discussing the definition of support used in confirmation theory, it will be useful to elucidate two very general necessary conditions for support. First, in order for a hypothesis h to receive positive support from another statement (perhaps a statement of evidence) e, e should:

(1) bear *favourably* on h

and also,

(2) be *less uncertain* than h.

The reasoning here is plain. Intuitively, e can give h no positive support if it is itself less certain or probable than h. That is, it must non only bear favourably on h (condition 1), but also be less uncertain than h; for if *any* uncertain hypothesis is permissible as providing support or justification, then condition 1 could be easily satisfied—indeed, it could be manufactured at will. It seems that the most obvious way to bear favourably on a hypothesis is to entail it. However, this would violate condition

2. For if e entailed h then e could not be less uncertain than h. In other words, if e is to have h as a consequence it must be at least as logically strong as h and hence will be equally uncertain, if not more so. It would in that case contain all the uncertain content of h, and perhaps more. Hence, to provide support for (that is, *raise the probability of*) a disputed hypothesis, the justifying argument must employ *less* disputed premises.

Empiricist justificationism claims that we adjudicate amongst theories on the basis of which are inductively grounded or supported—that is, rational belief is positively controlled by observational evidence. But what is the epistemological status of this empirical basis? I will now argue that the premises of empirical arguments are not certain. May they nonetheless provide justification for scientific laws despite this uncertainty? Can we assign an observation sentence any positive probability? Or must their probability remain indeterminate?

5.2 Problems of Uncertain Premises

Popper called the problem of the justificatory status of observation reports "the problem of the empirical basis" (1959, ch. 5). Traditional "strong" justificationism holds that perceptual reports form the certainly true premises of inductive arguments. Yet this raises the question whether such reports consist of descriptions of observable material objects "out there", or if they are to be restricted to accounts of immediately accessible subjective sensory experience. Here the justificationist faces a dilemma. The latter option may allow subjective certainty, but it is of little significance in objective theory adjudication, where any proof or justification must be intersubjective.

However, it is quite plain that statements of the former kind cannot be demonstrably true. Popper argued for this position (1959, pp. 94-5) with the aid of a glass of water as an example:

> Every description uses *universal* names (or symbols, or ideas); every statement has the character of a theory, of a hypothesis. The statement, 'Here is a glass of water' cannot be verified by any observational experience. The reason is that the *universals* which appear in it cannot be correlated with any specific sense-experience. (An 'immediate experience' is *only once* 'immediately given'; it is unique.) By the word 'glass', for example, we denote physical bodies which exhibit a certain *law-like behaviour*, and the same holds for the word 'water'. Universals

> cannot be reduced to classes of experiences; they cannot be 'constituted'.[11]

Popper called this doctrine the "transcendence inherent in any description." It asserts that even singular descriptions go beyond what can be known with certainty "on the basis of immediate experience". Such descriptions, of a glass of water, for instance, attribute certain *dispositional* properties, which in turn give rise to an indefinite series of conditional predictions which have not been verified.[12] Thus, the only way to circumvent the result that all observation reports are necessarily uncertain is to deny, with J. L. Austin (1946, pp. 56-57), and Norman Malcolm (1952, pp. 65-69) that they have any predictive implications at all. Yet such interpretations of descriptive statements that excise them of any predictive implications render them worthless for any practical or theoretical function, since they then become consistent with any future occurrence whatsoever. As a consequence, it seems necessary to admit that all descriptive statements are thus conjectural. As Popper commented (1959, p. 80):

> I readily admit that only observation can give us 'knowledge concerning facts', and that we can (as Hahn says) 'become aware of facts only by observation'. But this awareness, this knowledge of ours, does not justify or establish the truth of any statement... our *knowledge*, which may be described vaguely as a system of *dispositions*, and which may be of concern to psychology, may be... linked with feelings of belief or of conviction... with the feeling of... 'perceptual assurance'. But all this interests only the psychologist. It does not even touch upon problems like those of the logical connections between scientific statements, which alone interest the epistemologist.

Thus, it seems that insufficiency must be accepted in the empirical basis too; even singular statements about objects or events cannot be certainly true in any objective sense, regardless of any subjective feelings of assurance. This fact is now generally accepted. As Paul O'Grady (2002, p. 92) notes, the "issue, which led many to reject [sceptic-proof perceptual knowledge], was the tension between giving genuine content to the data of sense and simultaneously keeping it immune from doubt. The reason sense-data were immune from doubt was because they were so primitive; they were unstructured and below the level of conceptualization. Once they were given structure and conceptualized, they were no longer safe from sceptical challenge."

However, this again raises problems. For there are various arguments which suggest that the inductive basis, to provide a satisfactory justification for further knowledge claims, *must* be secure. This, however, is not possible without assuming extra-logical (and empirically unjustifiable) synthetic principles. In the rest of this section I will rehearse some of those arguments.

To reiterate, weak justificationism holds that the evidential premises of justificatory arguments need not be absolutely certain, but may nonetheless possess a degree of probability. As Paul Moser (1998, § 2) states "Opposing the radical foundationalism of Descartes, contemporary foundationalists typically endorse modest foundationalism, implying that foundational beliefs need not possess or yield certainty and need not deductively support justified non-foundational beliefs." In the last section I stated that one of the conditions for support or justification is that the supporting premises be *less* uncertain than the conclusion. If their status is indeterminate, they will, therefore, obviously not do the job. Thus, observation statements must have some *positive* probability, at least higher than 1/2—that is, minimally, they must be more probable than their negation. How are observation statements to procure such probability values?

An immediate problem can be derived from an argument by C. I. Lewis (1946). According to Lewis, we can only be justified in ascribing a probability value to a statement if there is something beyond that statement to provide this support. Without this further reason, the initial probability assignment will be completely indeterminate. Moreover, this further reason must itself be more probable than the statement to be justified. It is clear that if we continue on this justificatory chain, and if the justification is to succeed at all, we must arrive at a statement which has unit probability—that is, which is itself certain. The regress would look something like this:

$$p(a) \leq 1/2 \leq p(b) \leq p(c) \leq p(d) \leq p(e)\ldots \leq p(n1) = 1$$

This argument led Lewis (1946, p. 186) to assert: "[i]f anything is to be probable, then something must be certain. The data which support a genuine probability must themselves be certainties." And as Watkins states, "the regress that is opening out here can be halted only if it terminates with 'ultimate premises', to use Keynes's term, that are *known* to be true... The last link in a chain of justifications must be *secure,* otherwise the chain is dangling in limbo" (1984, p. 60). Thus, the regress must end with secure premises, or else the chain of justification is wholly unsupported, and hence is not a chain of "justification" at all. Such an argument led both Lewis and Keynes to posit that there are indeed ultimate premises which are certainly true, that are "given" in sensory experience.

It seems the upshot of Lewis's argument is that probabilism needs *certain* premises to get off the ground—if no ultimate premises exist which are completely certain, then there can be no inferential probability transmitted to other items of knowledge beyond observation statements. That is, probabilistic support would be impossible without absolutely secure observation statements, and these, if my previous arguments are correct, are unobtainable. Even *partial* justification of scientific hypotheses would require certainly true premises.

Lewis's argument seems plausible, and it may perhaps be echoed in the following complaint by Popper (1983, p. 222):

> The evidential statements *e* are themselves far from certain... no inductivist has ever explained how to interpret 'p(*h,e*)' when *e* itself is uncertain and, presumably, 'only probable'... Nor are these evidential statements 'given' to us—by God, or by nature, or by our senses. Every observation and, to an even higher degree, every observation statement, is itself already an interpretation in the light of our theories.

However, not long after Popper was writing this (in the early 1950's) probabilist responses were forthcoming. I will consider two such responses.

The first is that of Richard Jeffrey (1965, Chapter 11; 1968), who produced perhaps the earliest Bayesian response to the problem of uncertain evidence. Jeffrey here holds that an evidential statement may have an intrinsic probability. That is, since it is non-inferential, the regress of Lewis does not begin—evidential statements can be probable on their own merit, not relative to some further evidential statement. Moreover, according to Jeffrey, these statements can have *determinate* probability values less than 1. If successful, the probabilist program can be maintained together with the apparent fact that evidential statements are never absolutely certain.

However, there are problems with the method by which these values are obtained. According to Jeffrey observation reports are empirical yet non-inferential—probability values for observation statements are measured by degrees of confidence in those statements, and these assessments are quite involuntary. In most normal situations a person's confidence level in their observation reports will approach 1; in other cases, say in poor lighting conditions, the degree of confidence will be significantly less (the example Jeffrey gives involves examining a piece of cloth by candlelight to determine the colour). The major problem with this response, I think, is the extreme psychologism or subjectivism inherent in taking personal feelings of confidence as objective justification for truth claims. In particular, it seems at odds both with the objectivism of science, and also with the objectivity needed to resolve problems of theory adjudication in a non-relativistic manner. Of course, there can be no doubt that the decision to accept a basic statement is causally linked to our perceptual experiences, but where this leaves objective *justification*, in the sense needed to address the demarcation problem is unclear.[13]

However, we may, for the sake of argument, assume that Jeffrey's kind of non-inferential probability for observation statements is viable. There is an additional problem. This arises from the "requirement of total evidence"—a fundamental principle of inductive logic which prohibits the rigging of posterior probability values by way of selective consideration of the evidence. As formulated by Carnap:

> *Requirement of total evidence:* in the application of inductive logic to a given knowledge situation, the total evidence available must be taken as basis for determining the degree of confirmation (1950, p. 211).

This principle means that in calculating probabilities all relevant evidence must be taken into account. Indeed, since we cannot determine a priori what evidence is relevant, *all* of the known evidence—and most especially all the accepted observation statements—will have to be employed. This has the result that the conjunction of observation statements that feature in the calculation will be huge. But, if the individual probability of these statements is less than one, and the probability of each conjunct is independent, then the probability of a large conjunction of them will tend to sink very quickly to a low, and perhaps negligible, value. This is an unavoidable consequence of the "rule of content" of the calculus of probability, referred to by Popper as the "axiom of monotony." That is, the probability of a conjunction of statements will in general be less than, and at most equal to, the probability of any of the conjuncts. And, of course, premises that are themselves of negligible probability can hardly be used to justify further knowledge claims.

Partly in response to this problem, Mary Hesse (1974), has suggested a less psychologistic solution than Jeffrey, appealing instead to a basis of observation reports, "which make up the data upon which the community of science works" (1974, p. 127) These "need not be regarded as absolutely incorrigible, but only as substantially less subject to correction than the observation statements themselves... [I]t is in fact much less likely that mistakes are made in describing reports than in asserting observation statements" (ibid). Hesse is still obliged, however, to introduce two extra-logical postulates to avoid the aforementioned problem. First she postulates that "the observation statement corresponding to a given observation report is true more often than not" (1974, p. 128). Second, she asserts that the observation reports that form the evidential basis will strongly reinforce each other probabilistically.

My objection to Hesse is not necessarily to dispute the truth of these claims, but rather to point out the vulnerability to Agrippa's trilemma if they are taken to provide a justification of any further knowledge claims. For both these principles are clearly synthetic, and are hence in need of justification themselves. Even if a plausible rationale or explanation for the truth of these principles could be produced that would placate charges of ad hoc expediency, they could still not be justified without circularity unless they are assumed to be *a priori* true. Nor is it clear, given the all-inclusive scope of the requirement of total evidence, that even the adoption of these principles will avoid the problem of the diminishing compound probability in calculations of probabilistic support. And, of course, the stronger the assumptions employed to secure the justification, the less plausible that justification will be.

However, in what follows I will assume, for the time being at least, that Hesse's program concerning the evidential basis can be carried out successfully—empirical reports, although not absolutely secure, may have a determinate probability value close to 1. On this assumption, can we go on to adjudicate between competing universal hypotheses with the aid of probability logic? In the following I will argue that we cannot—even if a basis of certainty, or near certainty, is attained, it cannot support empirical hypotheses without circularity or apriorism.

5.3 Probabilistic Support

Weak justificationism requires that e must, to bear favourably on *h*, raise its probability. How is this to be measured? As mentioned earlier, the most common definition of probabilistic support employed by theorists of induction is known as the measure of support or relevance:

Definition: $s(h, e) = p(h, e) - p(h)$

That is, the support of *h* by e is defined as the increase in probability of *h* in the light of the information e. In those instances where $p(h, e)$, i.e. the probability of *h*, given e, is greater than $p(h)$, e is taken to support *h*. Conversely, if $p(h, e)$ is smaller than $p(h)$, then *h* is undermined, or countersupported, or disconfirmed by e. In this case $s(h, e)$ is negative. Finally, if $p(h, e) = p(h)$, then $s(h, e) = 0$; in this case *h* and e are probabilistically independent of each other.

This measure satisfies many intuitive requirements for empirical confirmation.[14] For instance, the value of $s(h, e)$ increases along with $p(h, e)$, and the function *s* takes the maximum and minimum values under the same conditions as *p*, for any fixed *h*. Most importantly, the major attraction of the measure *s* is that, excluding cases where some initial probabilities take the extreme values 0 or 1, a hypothesis *h* is always supported by its logical consequences.[15] In other words, if *h* logically implies e, then $s(h, e)$ is positive: $p(h, e) \geq p(h)$ whenever e is a logical consequence of *h*. An immediate consequence is that when e partially (but not fully) implies *h* we have:

$$0 < p(h) < p(h, e) < 1$$

This may readily be transferred to the case of the empirical—purportedly inductive—support of a hypothesis *h* on the evidence e. To illustrate this, we may take e to denote an empirical test statement that follows from a hypothesis *h* in the presence of our background knowledge *b* (which, we assume, contains the initial conditions). Thus, we have (since *h* entails e in the presence of *b*):

(1) $p(he, b) = p(h, b)$

Using the multiplication theorem we can then derive:

(2) $p(he, b) = p(h, eb)\, p(e, b) = p(h, b)$

Together with a simple theorem of the probability calculus, $0 \leq p(e, b) \leq 1$, we can then conclude:

(3) $p(h, eb) \geq p(h, b)$.

Generalising our definition of support $s(a, b)$ to accommodate the third variable of background knowledge, we have:

Definition: $s(h, e, b) = p(h, eb) - p(h, b)$

This defines the support of *h* by *e* in the presence of *b*, the background knowledge. With this generalised definition we then derive:

(4) $s(h, e, b) \geq 0$.

Thus, if the test statement *e* follows from *h* in the presence of *b*, then *h* will be supported by *e*: $s(h, e, b)$ is always non-negative.

This plausible account certainly suggests evidence *can* support a logically stronger statement—that is, it seems manifest that evidential statements can raise the probability of statements that transcend them. It appears to be an instance, in probabilistic language, of what Aristotle called induction or *epagoge*—that is, the method of "learning from examples". This idea can be traced back at least as far as the French philosopher and mathematician Jean Nicod (1889–1924), who, in his 1923 essay "The Logical Problem of Induction", had argued that law-like generalisations could be established as probable by way of confirmation by their favourable or positive instances, and, conversely, could be disconfirmed by their unfavourable or negative instances.[16] Induction by enumeration had thus been refashioned as probabilistic confirmation by repetition of instances. In Carl Hempel's "Studies in the Logic of Confirmation" (1945) this property of confirmation was accordingly dubbed "Nicod's criterion." It is this intuitive, qualitative idea which underlies the quantitative support function, and the British mathematician I. J. Good even went so far as to call this result, along with others like it, *induction theorems* (1983, pp. 164-165). Moreover, all the measures in Carnap's λ-continuum (1952) satisfy these theorems, as long as the prior probability of *h* is greater than 0.

Indeed, a central idea of probabilist justificationism is that probability is in some sense ampliative—that is, the claim is that the evidence *e* can, with the aid of the probability calculus, confer support, or raise the probability of, a statement that goes beyond *e*. Justificationists had claimed that "[c]onfirming instances... tend to enhance the probability of the hypothesis or give it inductive support. With enough confirming instances of appropriate kinds, the probability of the hypothesis becomes great enough to warrant accepting it as true—not, of course, with finality and certainty, but provisionally. With sufficient inductive support of this kind we are justified in regarding it as well established" (Salmon, 1966, p. 23). And indeed, these considerations imply that evidence can indeed make things *beyond* what it actually asserts at least a little more probable. Certainly the considerations above suggest that the favourable evidence *e can* make *h* more probable,[17] even though *h* says more than *e,* and, moreover, the probability will continue to increase with increasing favourable evidence. That is, it will tend to 1 as the favourable instances of evidence *e* tend to infinity.

5.4 The Popper-Miller Theorem

However, although these derivations are indisputably valid, this fact may not be taken to be a vindication of inductivism. For it has been demonstrated by Popper and Miller that this probabilistic support is not ampliative. This proof was published in *Nature* (vol. 302, pp. 687f.) in April 1983, and has been expanded and simplified by both authors since then.[18] I will present here a simplification of the proof.

First, we may consider the two propositions *h-if-e* $(h \leftarrow e)$[19] and *h-or-e* $(h \vee e)$. In classical logic these two propositions have no shared logical consequences (apart from the tautological truths which trivially follow from all propositions). Moreover, $h \leftarrow e$ and $h \vee e$ are together just sufficient to imply h; $h \leftarrow e$ is the logically weakest (and therefore the absolutely most probable) statement, which is strong enough (in the presence of e) to have h as a consequence. In other words, if e is given, then $h \leftarrow e$ is necessary and sufficient for h. Accordingly, the *excess content* that h has over e—the content that is not already deductively entailed in e—may be identified with the conditional $h \leftarrow e$; this conditional is precisely what, in addition to e, we need in order to get h; any other statement which can extend e to give h is stronger.

Working with the two variable version of the definition of support, $s(h, e)$, we can derive the following theorems, which are valid for any given statement h and for any given statement e:

Theorem 1. $s(h,e) = s\,(h \vee e,\, e) + s\,(h \leftarrow e,\, e)$

$h \vee e$ has the following truth table; it is true whenever either h is true, or e is true, or when both are true. Thus we can derive $h \vee e$ from h as well as from e—it is a logical consequence of both disjuncts.

h	e	h v e
T	T	T
T	F	T
F	T	T
F	F	F

$h \leftarrow e$ has the following truth table; it is true if and only if h is true or e is false.

h	e	h ← e
T	T	T
T	F	T
F	T	F
F	F	T

If e is given as a premise we can derive from the conditional statement $h \leftarrow e$ the conjunction *he*. Accordingly, $p(h \leftarrow e, e) = p(he, e) = p(h, e)$. From this we can derive:

$s(h \vee e, e) = 1 - p(h \vee e) = p(\sim h, \sim e),$

And:

$s(h \leftarrow e, e) = - Exc\ (h,e) = (1 - p(h,e))\ (1 - p(h)) = ct(h, e)\ ct(h)$

This is based upon an algebraic transformation from the equation for the excess content *Exc(a, b):*

Definition: $Exc(a, b) = p(a \leftarrow b) - p(a, b)$

From these definitions we can derive:

Theorem 2. $s(h \vee e, e) \geq 0 \geq s(h \leftarrow e, e)$.

Which states that *the* first summand in Theorem 1, $s(h \vee e, e)$, is always either a zero or positive support, while the second summand $s(h \leftarrow e, e)$ is either zero or negative—i.e. it is a negative support or a countersupport.

Theorem 3. $s(h \vee e, e) \geq 0$ is always positive, as $h \vee e$ follows from *e*, and so $p(h \vee e, e) = 1$.

Thus, this positive support of $s(h \vee e, e)$ is explained entirely deductively—it is a consequence of the fact that $h \vee e$ follows deductively from e.

With this stated, we may now state the content of the proof, derived solely from the axioms of the calculus of probability together with the definition for support: for any hypotheses *h* and evidential statement e we may distinguish between a purely deductive and a non-purely-deductive component:

$h = (h \vee e)\ (h \leftarrow e).$

Accordingly, any support $s(h, e)$ can be described, by theorem 1, as the sum of these components. *By* theorems 2 and 3 we can conclude that this support will consist of a purely deductive support and a remaining negative support, and that the evidence e will, unless it is zero, generally countersupport the conditional $h \leftarrow e$. Moreover, it is only this component which can possibly be considered "inductive" or "ampliative". Any positive support $s(h, e)$ *is to be explained* by the support provided by e for its purely deductive component $h \vee e$. Hence,

Theorem 4. If there exists something like inductive support, then its contribution to the total support $s(h, e)$ is *always negative.*

An even simpler proof is given by Miller in his (1994, p. 73). It is derived from the following two propositions:

(1) If *h* implies e, then $P(h, taut) \leq P(h, e)$;

This is just to assert that if *h* implies e then $s(h, e)$ is non-negative, as was demonstrated above.

(2) if $P(x, z) < P(y, w)$, then $P(not\text{-}y, w) < P(not\text{-}x, z)$.

This is a a weak form of the law of complementation.

If in (1) we substitute *e-&-not-h* (this is equivalent to the negation of *h-if-e)* for *h, we get*

P(e-&-not-h, taut) $\leq$ *P(e-&-not-h, e),*

From (2) we can then derive

P(h-if-e, e) $\leq$ *P(h-if-e)*

The identity of these components will only obtain if *p(e)* = 1 (i.e. if *e* is a tautology), or if *p(h, e)* = 1 (if *h* is derivable from *e*). Hence we may disregard both these cases when considering empirical confirmation. In all cases relevant to empirical science then, the excess content *h-if-e* is undermined or countersupported by the evidence e. Thus, for any statement *h* and for any statement e that is implied by it, if there is anything like inductive (non-deductive) support by *e*, then it must always be less than zero. This result can also be generalised to hold between any two statements whatsoever, whether e follows from *h* or not.[20]

This result has, of course, not been entirely uncontroversial. Rochefort-Maranda (2004) has produced a survey of the literature; the majority of criticisms have focused around the claim that the excess content *h* has over e has been misidentified, although Popper and Miller replied, I think convincingly, to the majority of these objections in their (1987, § 3). Pending any new lines of criticism, it seems that the result must be accepted, and I will assume its validity in what follows.

5.5 Significance of the Theorem

What consequences may be drawn from Popper and Miller's proof?

I will discuss three results. First, *probability logic is not ampliative*—the excess content of *h* over e receives no inductive support. Second, what "support" is attainable is not support at all—rather it is simply deductive *entailment* and hence is completely *circular.* Third, this means that probability logic cannot advance the justificationist demarcational program—evidence supports equally *any* hypothesis from which it can be derived.

1. Probability is not ampliative.

We have seen that probability is a genuine generalisation of classical logic. But it is not an *inductive* logic; excess content is countersupported. As Popper wrote (1974a, p. 171):

> ... induction is a myth. No "inductive logic" exists. And although there exists a "logical" interpretation of the probability calculus, there is no good reason to assume that this "generalized logic" (as it *may* be called) is a system of "inductive logic".

Like all effective arguments, Popper and Miller's proof is a *reductio*—in this case of the assumption that there is something like inductive or ampliative support. On the assumption that inductive or ampliative logic exists, we derive the absurd conclusion that all inductive support is, in fact, countersupport. The obvious lesson, it seems to me, is not that ampliative countersupport exists, but that inductive inference *in toto* is impossible. This, seemingly, is also Miller's view (1994, p. 73): "To my mind the function *s* not only does not measure inductive support, it does not measure anything." In its place, Miller suggests the measure *q* of deductive dependence (Miller and Popper 1986). This measure of partial implication does not produce the same strange results as the measure *s*—that is, the deductive dependence of *h-if-e on e,* $q(h \leftarrow e, e)$ is zero—*e* is neither favourable nor unfavourable to $h \leftarrow e$. In other words, *for any h and e, where h and e* have no content in common beyond logical truths then $q(h, k) = 0$. There is no inductive or ampliative support, and there is no countersupport either.[21] Intuitively, this measure fulfils the intuition underlying probabilism—that the evidence may partially entail a hypothesis—but it is entirely non-inductive. More especially, since the measure of partial entailment $q(h, e)$ may still be positive even when *h* is contradicted by *e,* it cannot be identified with any measure of degree of rational belief.

It should be noted here that even Colin Howson, one of the foremost critics of the Popper-Miller characterisation of the excess content mentioned earlier,[22] seems to concur at least on this point—which is in any effect the crucial outcome of the theorem. For he writes (2000, p. 62): "Hume's own answer to this question [is] that probability theory is a piece of mathematics which by itself conveys no information on any matter of empirical fact. I myself believe Hume's position to be correct." That is, he agrees both with Hume, and with Popper and Miller that "there can be no probable argument from the data to anything beyond it" (p. 71). And again (pp. 176-177):

> Consistency with Humean inductive scepticism is maintained by noting that the inferences are sound, but also that they depend on a premisses describing suitable belief-distributions. The situation is, *mutatis mutandis*, the same as that in deductive logic: you only get out some transformation of what you put in. Bacon notoriously castigated the contemporary version of deductive logic, Aristotelian syllogistic, for what he perceived as a crucial failing: deductive inference does not enlarge the stock of factual knowledge. But Bacon's condemnation has with the passage of centuries become modulated into a recognition of what is now regarded as a, if not the, fundamental property of logically sound inferences which, as we see, probabilistic reasoning shares, and in virtue of which it is an authentic logic: *sound inference does not beget new factual content.*

But this was all that was to be proved. Wesley Salmon, in his (1966, pp. 10-11) had identified "an exhaustive trichotomy of inferences: valid deductive inference, correct inductive inference, and assorted fallacies. The... question is, however, whether the second category is empty or whether there are such things as correct inductive inferences." If the preceding result is correct, the answer to Salmon's question is the former disjunct—all valid inferences, including probabilistic inferences, are deductive and non-ampliative.

2. All probabilistic support is circular, and hence no probabilistic support exists.

As we have seen, *in one sense* there is such a thing as probabilistic support. When e follows from *h,* the support *s(h, e)* is always positive. However, this positive support is entirely to be explained by the fact that e is part of the content of *h*. Hence this "support" is entirely deductive, and like all deductive support, it is circular. As noted by Sextus Empiricus, and as stressed in particular by J.S. Mill, all deductively valid arguments are question-begging—that is, they *assume* what they purport to prove.

Consider, for example, the argument:

(1) If A then B.

(2) A

(3) Therefore B

(4) If B then A.

(5) Therefore A.

The circularity here is stark. A is validly derived from (3) and (4), but in order to secure the demonstration we had to first assume A in (2). Indeed, there is no *demonstration*. Rather what we have is merely a *derivation*—valid, yes, but providing no support. As Irving Copi (1990, p. 102) comments on such circular justificatory arguments, "[a] *petitio principii* is always valid—but always worthless, too." Worthless, at any rate, for *justification*.

More generally, *all* deductive arguments can be recast in the form:

(1) A
(2) Therefore A (or some part of A).

That is, if the conclusion is to be validly derived, its content must be contained in the premises. However, such arguments are clearly of no value if construed as justificatory. Thus, while A can be validly derived from A, A cannot establish, or prove, or justify A. Moreover, from "A implies A" we cannot derive "A proves A", or "A is a sufficient reason for A". A is not, an cannot be, a "good reason" for itself. In addition, unless the argument is simply $A \vdash A$, the premises will be logically stronger than the conclusion and hence *less* probable than the conclusion. This, however, violates our second condition for support, that the justifying premises be *less* uncertain than the conclusion to be justified. Salmon also notes this feature of truth-preserving inferences (1966, p. 8):

> Since demonstrative inferences have been characterized in terms of their basic property of necessary truth preservation, it is natural to ask how they achieve this very desirable trait. For a

large group of demonstrative inferences, including those discussed under "valid deduction" in most logic texts, the answer is rather easy. Inferences of this type purchase necessary truth preservation by sacrificing any extension of content. The conclusion of such an inference says no more than do the premises—often less The conclusion cannot be false if the premises are true *because* the conclusion says nothing that was not already stated in the premises. The conclusion is a mere reformulation of all or part of the content of the premises. In some cases the reformulation is unanticipated and therefore psychologically surprising, but the conclusion cannot augment the content of the premises. Such inferences are *nonampliative;* an ampliative inference, then, has a conclusion with content not present either explicitly or implicitly in the premises.

The circularity of *invalid* arguments—arguments that claim to provide inconclusive support—is not as blatant as in these examples, but it is nevertheless there. For the only part of the content of *h,* where e does not beg the question, is the material conditional *h-if-e* (or if *e-then-h*). This conditional has no factual content in common with e, and together with e it is exactly equivalent to *h*. But this excess content is countersupported. The part of the content of *h* that is supported, *h v e,* is a deductive consequence is e itself. "In the light of these results", writes Miller (1994, pp. 62-3):

...the doctrine that e can provide a good reason in favour of *h* when it offers it positive probabilistic support seems indefensible. The evidence e provides no reason at all for part of *h;* and the residue of *h* is countersupported. Hence what is promoted as a good reason for *h* is at the same time a good reason against some of the consequences of *h*. Where this leaves the connection between rationality and truth defeats me.

Quite generally, a proposition can provide positive support for another proposition only when their contents overlap, and this support is to be entirely explained by the deductive *petitio principii*. As such, it is better to refrain from calling this circular support "support" it all—it is simply logical entailment. Miller states the ramifications of this fact quite forcefully in his (1994, p. 63):

...good reasons do not exist, and indeed cannot do so. This applies just as much to reasons that do not aim to be sufficient as it does to those that do. As I have stressed, I cannot prove this thesis. But it is surely up to those who think that good reasons exist to explain what truth emerges from allowing any proposition to act as one of the judges in its own cause.

Again, in a remarkable passage which I can only interpret as being in essential agreement with critical rationalism, Colin Howson (2000, p. 83) lauds the "beautiful... strategy Hume would employ to refute the pretensions of mathematical probability to solve the induction problem: where it is pure mathematics, it can give no indication of how we ought to adjust our beliefs to evidence; where it does attempt to give such an indication, it ceases to be pure mathematics and will employ synthetic assumptions which effectively beg the question." (Howson's own subjective Bayesian position will be explored further in the next section).

Indeed, this result may be considered simply a probabilistic version of the sceptical arguments of Francisco Sanches (1551-1623), the physician and sceptical philosopher, and distant cousin of Michel de Montaigne. Popkin (1998b) reports that Sanches "advanced what was perhaps the strongest sceptical critique of Aristotelianism" and "perhaps the best technical exposition of philosophical scepticism produced during the sixteenth century" in his sceptical work *Quod nihil scitur* (*That Nothing is Known*), written between 1574 and its publication in 1581. The work contains a detailed critique of the justificationist Aristotelian theory of science—demonstrable knowledge derived by syllogistic demonstrations on the basis of true definitions. Sanches, in addition to questioning whether we in fact possessed any such true definitions, argued that (Popkin, 1998b, § 2):

> ...the syllogistic method of reasoning does not lead to any new knowledge. There is a vicious circularity in any knowledge claims based on demonstrative syllogisms, for the conclusion that is being proved actually constitutes part of the evidence for the premises. In order to demonstrate that Socrates is mortal, it is argued that all men are mortal and that Socrates is a man. However, the premises involve the conclusion, since the particular, Socrates, is needed in order to have a conception of man and of mortality.

Thus Sanches argued that neither definitions nor syllogistic reasoning could produce knowledge (*epistêmê*). We may add to this that neither can probability logic. Like all deductive reasoning it is a *petitio principii,* a begging of the question—it can produce no new knowledge, and it can produce no justification.

It may not be too much of a stretch to regard Pierre Gassendi (1592-1655) and J.S.

Mill as further forerunners to this critique of ampliative logic. Gassendi, another seventeenth century critic of the Aristotelian justificationist view of knowledge, argued, in his *Exercitationes Paradoxicae Adversus Aristoteleos* (1624), and later in his *Syntagma Philosophicum* (1658) that "since the conclusion of a syllogism contains no information not already present in the premises, syllogistic demonstration is incapable of producing new knowledge" (Osler, 1998, § 3). Mill makes a similar point in his *System of Logic* (1843), in his discussion of "merely apparent" inferences—an inference is "apparent, not real" when:

> ...the proposition ostensibly inferred from another, appears on analysis to be merely a repetition of the same, or part of the same, assertion, which was contained in the first... In such cases there is not really any inference,—there is in the conclusion no new truth, nothing but what was already asserted in the premises, and obvious to whoever apprehends them. The fact asserted in the conclusion is either the very same fact, or part of the fact, asserted in the original proposition. (*CW* VII: 158-160)

And specifically on the justificatory capacity of logic, Mill asserted:

> Logicians have persisted in representing the syllogism as a process of inference or proof; though none of them has cleared up the difficulty which arises from the inconsistency between that assertion, and the principle, that if there be anything in the conclusion which was not already in the premises, the argument is vicious... It is impossible to attach any serious scientific value to such a mere salvo, as the distinction drawn between being involved by *implication* in the premises and being directly asserted in them. (*CW* VII: 185)

As John Skorupski (1998, § 2) summarises Mill's position on arguments in which the conclusion is literally asserted in the premises, in such a case "there can be no epistemological problem about justifying the apparent inference—*there is nothing to justify*" (emphasis added). That is, circular inferences create no problems of justification, *because they can provide no justification.*

3. Probabilistic support cannot aid in theory adjudication.

Since support defined in terms of logical probability is non-ampliative, considerations of probabilistic support are of no avail in theory adjudication;

probability logic cannot aid in the classification of theories as unconditionally true or false. This can be seen clearly if we consider an argument Popper makes in the 1982 introduction to *Realism and the Aim of Science*, in response to claims that falsificationism is somehow vulnerable to problems raised by Goodman's paradox. As Popper asserts, "[t]he calculus of probability is incompatible with the conjecture that probability is ampliative (and therefore inductive)... This is not a paradox, to be formulated and dissolved by linguistic investigations, but it is a demonstrable theorem of the calculus of probability" (1983, p. xxxvii).

Popper's argument runs as follows:

Assume two explanatory hypotheses, h_1, and h_2, that are both supported by an evidential statement e in the presence of *b*, our background knowledge:

$$p(e, h_1b) = p(e, h_2b) = 1.$$

Also assume the probability of the evidence on the background knowledge is not 1, $p(e, b) \neq 1$.

Then we may denote the ratios of the probabilities of h_1 and h_2, (both prior and posterior) to the evidence e as follows:

$$R_{1,\ 2}\ (prior) = p(h_1b)\ /\ p(h_2b)$$

And:

$$R_{1,\ 2}\ (posterior) = p(h1,\ eb)\ /\ p(h_2,\ eb)$$

Quite generally, for any h_1, h_2, and e that satisfy the above conditions, we can derive, with the help of Bayes's theorem, $p(a,bc) = p(ab,c)/p(b,c)$:

$$R_{1,\ 2}\ (prior) = R_{1,\ 2}\ (posterior)$$

This result demonstrates that "the evidence does not change the ratio of the prior probabilities, whether we have calculated them or freely assumed them, provided the two hypotheses can both explain the evidence *e*" (1983, p. xxxviii). Thus, neither hypothesis receives any ampliative support. We may conclude from this, I think, that evidence cannot adjudicate between two explanatory hypotheses that both account for it, unless the evidence entails that one of the hypotheses is false. As Popper and Miller elaborate in their (1987, p. 572):

> There is no sense in which *e*, by raising the probability of a hypothesis *h* from which it follows, points beyond itself. This should be obvious when we consider that such an *e* raises the probability of every proposition *h* from which it follows in the presence *of b*... Supporting evidence points in all directions at once, and therefore points usefully in no direction.

Since "supporting evidence points in all directions at once, and therefore points usefully in no direction", it seems there is only one way to rehabilitate the justificationist demarcational program, and that is to reinstate a topic neutral principle of induction. In this way, the empirical confirmation could be concentrated onto a particular hypothesis. But, if the arguments in the last chapter are correct, the prospects in this direction are irredeemably bleak.

Whenever some evidence *e* confirms a hypothesis *h,* it will simultaneously confirm a countably infinite series of alternative hypotheses *h1, h2, h3*... all standing in precisely the same relation to e. The confirming force will thus be dissipated among all these alternatives. However much evidence *e* comes in and however favourable *e* may be to *h, e* would never raise the posterior probability of *h* above zero. The set of mutually exclusive and exhaustive alternatives to which *h* belongs is infinite, and so the initial probability of each of them will be (virtually) zero. As we saw in the last chapter, Keynes (1921) accordingly asserted that we must delimit the set of initial hypotheses, so that their probability is not zero. However, even if this procedure could be justified, new evidence cannot adjudicate between hypotheses that it is consistent with.

Probability logic does nothing to alter the Humean predicament. There is no possibility of deducing a genuinely universal statement from a finite conjunction of singular statements, and there is no possibility of that conjunction *e* raising the probability of *h* over and above its infinity of competitors without an unjustifiable inductive principle. The evidence can confirm only the disjunction of this infinitude of generalisations, but that is just to say it can only provide "support" for itself. There can be no legitimate justificationist demarcational classification with just the help of logic, whether classical or probability logic, alone.

We may restate our general argument against the possibility of a valid inductive principle with the aid of the conditions for support mentioned in section 1. That is, in order for a hypothesis *p* to receive positive support from another statement *q*, *q* should:

(1) *bear favourably* on *p*

and also,

(2) be *less uncertain* than *p*.

Yet it may easily be seen that from any objective standpoint, no inductive principle can meet these conditions. This is simply because no general or topic neutral inductive principle could be less uncertain than the particular hypothesis in question. The principle will violate requirement (2). Thus, even if a feasible principle were to be formulated, it could not provide even the slightest support for scientific hypotheses. "Even if some grander and more general maxim were available," writes Miller (1994, p. 48-49),

> ...it would not improve matters. There is no respect in which "the course of nature continues always uniformly the same" or "Actions that are ultimately guided by hypotheses that are *well corroborated* have the best chance of being successful" (Watkins 1984) or "currently accepted low-level generalisations will continue to hold tomorrow" (Worrall 1989) are easier to justify than the corroborated generalizations that they are supposedly guaranteeing.

That is, any inductive principle strong enough to provide support for scientific knowledge will not itself be justifiable; any such support is illusory. Wesley Salmon (1966, p. 56) had written that "a serious concern for the solution of Hume's problem cannot fail to deepen our understanding of the nature of scientific inference. This... is the ultimate goal of the whole enterprise." It seems, on this reckoning, that the lesson we have learned from an examination of Hume's problem is that the nature of scientific inference is exclusively *deductive*.

5.6 Subjective Bayesianism

These difficulties with both traditional and probabilistic theories of inductive support have led to the growing popularity of a related probabilistic response—personalistic or subjective Bayesianism. Traditional probabilism, as we have seen, sought to provide an objective, impersonal measure of the logical proximity of a hypothesis h to the available evidence e. Since the posterior probability of h on e, using Bayes's theorem, is purely a function of the *a priori* probabilities, it was thought necessary that the distribution of *a priori* probabilities be assigned in a nonarbitrary fashion. Despite much technical work in this regard, such as Carnap's (1950) attempt to determine the *a priori* probabilities of all hypotheses by reference to the syntactic structure of rudimentary artificial languages,[23] the current consensus seems to agree with Ramsey (see Gillies, 2000, p. 53), and with Popper (1957a, point 3) that, in general, "there cannot be a satisfactory... [measure] of logical probability which is based upon purely logical considerations."[24]

The successors of Ramsey thus reject the objectivist strictures entirely, and require only that an agent's probability distribution is "coherent"—that is, satisfies the axioms of the probability calculus. The standard argument that degrees of certainty should obey the axioms of the calculus of probability is the Dutch Book argument. An explicit proof of such a result was first produced by De Finetti in 1937, but it had earlier been alluded to by Ramsey, who commented that a bookmaker who violated probabilistic consistency (1931, p. 3): "could have a book made against him by a cunning bettor and would stand to lose in any event." However, since the probability axioms do not themselves determine values for the initial probabilities, each agent is free to adopt any values they please, provided only that these values together obey the axioms. Thus the initial probabilities are chosen subjectively, and reflect personal expectations or degrees of belief.

This is the first characteristic element of personalist Bayesianism.[25] The second characteristic doctrine is that rationality is almost exclusively equated with the use of Bayes's theorem; the theorem is interpreted[26] as an algorithm for updating the probability of a hypothesis h in the face of new evidence e. Thus the posterior probability $P(h|e)$ is equal to $P(h)\ P(e\backslash h)\ /\ P(e)$. This is referred to as conditionalisation, and is equated with the process of learning from experience: "learning from experience... is just the process of revising probability assignments in the light of additional information" (Rosenkrantz 1977, p. 48).

Bayesians thus deny that "there is such a thing as *the* rational degree of belief in the truth of a proposition. Each individual is taken to (or allowed to) have her own subjective degree of belief in the truth of a certain proposition. Given that the probability calculus does not establish any (prior) probability values, subjectivists argue that it is up to the agent to supply the probabilities" (Psillos, 2007, pp. 198-9). It may seem that this strategy would, necessarily, make the evaluation of hypotheses an entirely arbitrary affair. After all, it is entirely possible to rig or fix the initial probabilities in such a way that a preferred *h* gets a high posterior probability on the evidence, whilst, on a different initial probability assignment *h* might get a low, or even zero, probability.

The personalist response to this objection is to assert that in the long run differences of subjective opinion are typically "swamped" by growing evidence, so that widely different probability distributions are almost bound to converge. Eventually, the posterior probabilities of people who started with very different initial probabilities will coincide, and thus there will be, in the long run, intersubjective agreement. As Dorling (1981, p. 120) declares, "I do believe, as a good Bayesian, that if we all stayed around long enough and accumulated sufficient evidence then our posterior subjective probabilities would become arbitrarily close to each others'."

The significance of these convergence results may be questioned however. As Miller (1994, p. 139) states, "this supposed convergence of opinion, though demonstrable in some elementary cases, is unobtainable in general."[27] There are certain circumstances in which limited convergence of probability measures by Bayesian conditionalisation on the same pieces of evidence can indeed be demonstrated. Yet such results hold only under very limited conditions. The general applicability of this phenomenon has yet to be demonstrated, and it is certainly highly unlikely to happen if one person assigns a negligible initial probability to a hypothesis to which another assigns a non-negligible one. Even if disputants over a hypothesis assign non-negligible, but still significantly different, initial probabilities, there is certainly no assurance that convergence could be achieved in any biologically feasible time period.

As Miller (ibid, p. 169) writes:

> ...certainly uniform convergence, which is what would be needed to underwrite a claim of objectivity, seems an impossible dream. For it is perfectly obvious that, if the only restriction imposed on degrees of belief is that they should satisfy the probability axioms, it must be possible, however copious the evidence e is, for the agent's degrees of belief to be measured by any probability function assigning probability 1 to *e*. It must be possible, that is, for any hypothesis *h* independent of e to receive any probability in the unit interval; so unless it is deemed possible (on Heaven knows what grounds) for empirical evidence eventually to settle all questions conclusively, there is always the possibility of wide divergence.

Thus, to secure convergence, it seems that there must be some licensing authority on initial probabilities. This option, developed by, for example, Shimony (1970), and dubbed, "tempered personalism", requires that personal probability distributions be rationed only over "seriously proposed" hypotheses: "one of the advantages of the tempered personalist formulation of scientific inference is that it uses a different primary criterion for comparing hypotheses, namely, that of being or not being seriously proposed" (1970, p. 155). Unfortunately, any attempt to demonstrate or justify the claim that there is any logical connection between being "seriously proposed" and being *true* immediately initiates an infinite regress.[28] It should also be noted, moreover, that even if two probability distributions *do* approximate each other this does not entail that they will be in approximate agreement regarding specific events.[29]

Whatever the merits of these convergence theorems, the most fundamental criticism of Bayesianism, as regards theory evaluation, is simply that consensus (or a *promise* of consensus) is not the same as truth. This has been noted by many critics, and by some Bayesians themselves. de Finetti (1937, p. 152), for instance, notes "there are rather profound psychological reasons which make the exact or approximate agreement that is observed between the opinions of different individuals very natural, but... there are no reasons, rational, positive, or metaphysical, that can give this fact any meaning beyond that of a simple agreement of subjective opinions." Indeed, if our aim is truth, consensus is irrelevant, and Bayesianism gives no methodological insights on how we might hope either to attain truth, or to discover error. The pure Bayesian sees no objective significance in either very high or low probabilities—these measure not the logical probability nor the rational acceptability of a theory, but only degrees of subjective confidence. On how we may adjudicate between better and worse probability distributions, Bayesianism is silent. As Jeffrey writes, "there is no Bayesian or probabilistic theory of theoretical preference" (1985, p. 141).

Fetzer also claims, I think accurately, that Bayesianism "disregards the objective of inquiry" (1981, p. 221, italics suppressed) For should a Bayesian suggest that a high personal probability of some hypothesis is a guide to rational acceptance or to truth, they immediately give up subjective Bayesianism proper, and instead revert to traditional probabilistic inductivism. Indeed, it seems to me that much contemporary Bayesianism vacillates on this point—this is especially apparent in their frequent use of the support function $s(h, e) = p(h, e) - p(h)$ of probabilistic inductivism, criticised above, in much the same fashion as earlier inductivists. When it is consistent to its purported principles, "Bayesianism provides a solution to the problem of induction only by wholly abandoning interest in the battle for truth, and opting for a passivist theory of human knowledge that may roughly describe, but certainly does not explain, what happens when science is done" (Miller, 1994, p. 126). Subjective Bayesianism thus either gives up all attempts at theory adjudication, or illicitly employs inductive inferences in the sense rejected earlier. In either case, this approach offers little guidance in demarcation.[30]

To expand upon these general criticisms, it will be worthwhile to examine a specific subjective Bayesian theory of science—that of the aforementioned British philosopher Colin Howson. A treatment of Howson's position is especially appropriate, since his programmatic book with Peter Urbach, *Scientific Reasoning: The Bayesian Approach* is certainly among the most thorough of its kind. Furthermore, his recent book, *Hume's Problem: Induction and the Justification of Belief,* is particularly relevant to the current discussion. Howson, as noted earlier, is no stranger to sceptical arguments—indeed, he is in marked agreement with critical rationalists on many points, and especially on the cogency of Hume's argument. He admits quite readily, for example, that there is no "hole in Hume's argument through which might escape a probabilistic justification of induction" (2000, p. 62). Moreover, he acknowledges that any empirical support or confirmation will involve circularity (ibid, p. 88):

> According to Hume's circularity thesis, every inductive argument has a concealed or explicit circularity. In the case of probabilistic arguments... this would manifest itself on analysis in

> some sort of prior loading in favour of the sorts of 'resemblance' between past and future we thought desirable. Well,... we have seen exactly that: *the prior loading is supplied by the prior probabilities*. What gets supported empirically and what does not will be determined by these.

Further elaborating on this theme of the inevitable circularity of confirmation, Howson writes (ibid, p. 88):

> Despite initially promising appearances the conclusion seems to be that probability theory does not supply a framework for making sound inductive inferences without the assistance of additional assumptions: in particular, about what is to be assigned positive prior probability. In the extensive universe of possibilities implicitly contemplated in discussing the problem of induction, a very large number of these will necessarily be assigned zero prior probability, with the corollary that what is even *allowed* to be inductively supported by observational data is our decision... That prior probability assignments appear to be tantamount to substantive assumptions, as they seem to be, vindicates Hume's circularity thesis, at any rate for probable arguments framed in the mathematical theory of probability.

Where Howson diverges from critical rationalist accounts is in his assessment of the utility of such subjective probability measures. Instead of rejecting such circular probabilistic support as useless for theoretical adjudication, Howson argues that dogmatic probability values should be assigned to hypotheses (ibid, p. 77):

> For it is just not true that we can only consider denumerably many hypotheses. We have seen that in the language of ordinary analysis hypothesis spaces of uncountably many elements are dealt with as a matter of course. The fact is that these are all possibilities and they cannot be ignored at the behest of an arbitrary restriction on language. They cannot all be assigned non-zero probabilities, and consequently any assignment of a positive probability is not something that can be justified by appeal to considerations of non-dogmatism. We have to be dogmatic, or so it appears, whether we like it or not.

We may well ask whether this is an adequate solution to the problem of theory adjudication—what, after all, does mere dogmatic (albeit consistent) personal degrees of belief have to do with the search for truth? Traditional inductivism, according to Howson, holds that (ibid, p. 119), "for the principles of scientific reasoning to be correct means that they should lead in some guaranteed way to truth, or to some surrogate, like 'approximate truth' or probable truth." Here the link with the demarcation problem is clear. In Howson's reconstrual however, (ibid, p. 170), "sound induction has nothing to do with coming to a correct understanding of the way the world is structured, but is merely the result of applying a constraint on beliefs which maintains their internal consistency." Howson explicitly addresses the issue of the link to truth on p. 170 of his (2000). There he writes:

> It might be objected (and indeed it has been) that science is not about people's beliefs. It is about truth; so a theory of consistent belief cannot in principle provide an account of scientific inference. Powerful and highly influential advocacy for this view was supplied by Fisher himself: 'advocates of inverse probability [Bayesian probability] seem forced to regard mathematical probability... as measuring merely psychological tendencies, theorems respecting which are useless for scientific purposes' (Fisher 1947: 6–7).

Howson's reply runs as follows (ibid):

> Fisher's inference is a non sequitur: Bayesian probabilities might be subjective objects, but the rules they must obey to be consistent are anything but subjective; and so far from being 'useless for scientific purposes' these supply a wholly objective theory of inductive inference; so objective, indeed, that they are infringed on pain of making genuine and possibly costly mistakes. The sanction is not just the (usually remote) theoretical possibility of being Dutch Booked were you to bet indiscriminately at your fair betting quotients (there is no presumption in the earlier discussion that you will and certainly not that you ought to do this), but those arising in general from accepting fallacious arguments with probabilities: we have only to look at the Harvard Medical School test to see what these might be.

This response is, I think, inadequate. Firstly, as Howson himself notes (pp. 135-6) the updating rules that are usually taken to be central to Bayesian theory are themselves subject to controversy, even amongst Bayesians. Secondly, although Howson is quite correct that mistakes in probabilistic reasoning can indeed be to an agent's detriment, this by itself has no bearing on whether we should allocate subjective probability assignments to scientific hypotheses rather than simply to hold them as conjectures and submit them to objective tests. Take Howson's example—the famous Harvard Medical School test. In this case, the marked tendency of people to make mistakes in assessing probabilities is primarily of importance because it concerns the *objective* calculation of the frequency of *events*. That is, upon an unwelcome medical diagnosis, it behooves one to correctly determine the rate of false positives. To determine this, one needs to *correctly* calculate using the *objective* frequencies, which are themselves assumed to be *true* in the calculation. Given the *truth* of these objective frequencies, the calculated probability of a false positive will be either *correct* or *incorrect*—its truth value, that is, will be either *true* or *false*. The *only* place where subjective degrees of confidence appear to come in here is in the patients' personal feelings on the diagnosis (which will, presumably, largely be a function of the seriousness of the condition under discussion). To repeat, what is of importance in this example, in terms of rationality, is a) the objective probability of events and b) the objective correctness of the mathematical calculations. In neither case is a recourse to subjective probabilities called for (although it is always possible to interpret such measured relative frequencies as subjective probabilities). The authentic need to be wary of probabilistic fallacies is not, therefore, an argument for Bayesianism. It is an argument, primarily, for the utility of the probability calculus, which is not disputed.

Howson then goes on to remark (p. 170):

> Moreover, probabilistic inconsistency is as self-stultifying as deductive: as we saw in the previous chapter, inconsistency means that you differ *from yourself* in the uncertainty value you attach to propositions, just as deductive inconsistency means that you differ from yourself in the truth-values you attach to them.

Yet this is still not an argument for the introduction of subjective probabilities into scientific reasoning. For, given a (classical) contradiction between two statements, we are forced to choose between them (or else suspend belief on *both*)—we can infer that *at least one of them is false*. Yet we can infer no such thing from an inconsistency in subjective probability assignments. The most an inconsistency can oblige us to do on this schema is to compel us to change our probability assignments so as to restore consistency. No hypothesis need be *rejected*. As a consequence, critical control, instead of being enhanced, is instead loosened.

This last point is similar to an objection by Max Albert, which Howson addresses next. Howson (ibid) quotes Albert:

> Logical [deductive] consistency serves a purpose: theories cannot possibly be true if they are inconsistent...; thus, if one wants truth, logical consistency is necessary (but not, of course,

sufficient). An analogous argument in favor of Bayesianism would have to point out some advantage of Bayesianism unavailable to those relying on non-probabilistic beliefs and deductive logic alone. Such an argument is missing.[31]

Howson's response is as follows (ibid):

> This is fairly thoroughly wrong. The two principal assertions here are both incorrect. First, logical consistency is not necessary for truth. False statements are well known to have true consequences, lots of them, and inconsistent statements the most of all since every statement follows from a contradiction. Current science may well be inconsistent (many distinguished scientists think it is), but it has nevertheless provided a rich bounty of truths. So much for deductive inconsistency.

Yet Howson's response here rests on an equivocation. It is true that false statements may have many true consequences, but Albert's point is completely unaffected—deductive inconsistency does indeed indicate an error. Naive set theory, for example, was fruitful for a time, but the paradoxes render it faulty nonetheless. Similarly, Newton's theory may have true consequences, but it is nevertheless false, precisely because other predictions derived from the theory were inconsistent with observation statements. Thus, Howson's rebuttal fails—logical consistency is not necessary in order to derive true *consequences* from a theory, but it is, as Albert states, a necessary property for the *truth* of the hypothesis or theoretical system under discussion.

Next, Howson proceeds to attempt to provide the requested missing argument—to state a specific advantage of Bayesianism over "non-probabilistic beliefs and deductive logic alone". He writes (p. 171):

> ... ultimately we are talking about *credibility*, the credibility of accounts of what there is in the universe and how it behaves, and how and according to what criteria observational evidence increases or decreases that credibility. It is no good saying that it is not credibility that is the goal of science but truth (as does Miller 1994). That is true but beside the point, for it does not argue that considerations of credibility are redundant. Indeed, on the contrary, they are indispensable. Only if truth-values were revealed unequivocally would criteria of credibility be redundant. But truth-values are seldom if ever revealed unequivocally: we can generally only conjecture them. We therefore need to know how credible our various conjectures are, and for that we need a theory of credibility.[32]

Here, then, is the position that I have labelled, at the beginning of this chapter, *weak justificationism*. Yet Howson's variant is plainly as susceptible to Hume's circularity thesis as any other justificationist strategy. Indeed, Howson acknowledges this fact (ibid):

> Hume showed that such a theory could not without circularity hope to make justified assertions of the form 'this conjecture has such and such credibility given the observational data', where the conjecture is consistent with but transcends the data. Hume has shown us that a successful theory of credibility should not be strong enough to make such categorical assertions without equally strong assumptions.

Given that Howson, earlier in the same book, had brilliantly exposed the inherent circularity in reliabilist, naturalist, evolutionary, and probabilistic responses to Hume, and rejected them precisely because of such circular justification, the admission that the Bayesian theory of credibility is *also* circular is quite startling. However, to differentiate the Bayesian response, Howson then appeals to the circularity of deductive logic as his precedent (pp. 171-2):

> It is a situation we should anyway be familiar with in a theory that purports to be a theory of sound reasoning, for there is already an extant well-known such theory, one of ancient pedigree, whose failure to deliver categorical assertions we are familiar and even happy with: deductive logic. By general agreement arrived at long ago, the valid arguments of deductive logic are all representable in the *conditional* form 'if such and such statements are true then necessarily so is this'. As Ramsey, emphasizing the similarly conditional nature of probabilistic inferences, clearly puts it: 'This is simply bringing probability into line with ordinary formal logic, which does not criticize premisses but merely declares that certain conclusions are the only ones consistent with them' (1926: 91).

Yet this analogy doesn't hold. For deductive logic sanctions no credibility judgements about contingent facts whatsoever. Again, this parallel offers no comfort to a defender of an objective logic of credibility. After this brief digression, Howson returns to the question of the relevance of subjective probabilities in theory adjudication (p. 172): "We come back to Miller's question: Where does, for that matter *can*, truth come into this? And if it doesn't, what is the point of such a logic of science:

doesn't it, indeed, miss the point of science?" Howson responds as follows (ibid):

> ... it doesn't follow that because the logic of inductive reasoning is bound by internal criteria of consistency anyone who applies it is doomed to remain in a Wittgensteinian fly-bottle of his or her own construction. Probabilities may be, and usually will be in these applications, probabilities *of truth*. So there you are: truth has reassuringly got back in (though it was never really out).

Yet this reply is also clearly insufficient to recommend subjective Bayesianism over a falsificationist theory of science. For, as already stated, Howson rejects the view that induction can provide any "guaranteed way to truth, or to some surrogate, like 'approximate truth' or probable truth" (p. 119). On the contrary, he holds that (ibid, p. 170, emphasis added), *"sound induction has nothing to do with coming to a correct understanding of the way the world is structured*, but is merely the result of applying a constraint on beliefs which maintains their internal consistency." Thus, the link between subjective credibility judgements and truth is completely severed by Hume's argument. What advantage they offer over a simple conjecturalist account has yet to be revealed. Moreover, as noted above, all the inferential machinery available to a Bayesian is also available to a deductivist—both positions recognise any and all non-ampliative inferences as valid. The latter position, however, in addition, dispenses with spurious circular justification. This, it seems to me, is clearly an advance.

As his final word on this topic, before moving on to an elaboration of the mechanics of Bayesian confirmation theory, Howson makes a brief appeal to justification via natural selection (p. 172):

> Hume's other point was that inductive inferences require inductive assumptions, and it is quite possible that the ones we habitually employ are reliable assumptions. Darwin's theory, whose possible truth is not in any way impugned by Hume's argument, suggests that in the main they are.

Yet this recourse to evolution cannot *justify* the methodological prescription of subjective probabilities, as Howson had earlier noted in chapter 1 of the same book (p. 18):

> The modern descendant of Kant's transcendental argument is the claim, supposedly based on

> Darwinian theory, that the expectational structures we inherit are likely to be the product of evolutionary pressures. Assuming that they are, it then seems to follow that as adaptive structures they are also likely to generate broadly successful cognitive strategies. There is in principle nothing wrong with such a Darwinian explanation of why we might find it impossible not to think or expect in certain ways, but turning it into a justification would be assuming science to justify science, a palpably circular procedure...

Elaborating on this point in detail, Howson's chapter 6, titled "The Naturalistic Fallacy", contains a brilliant critique of attempts to turn *explanatory* psychological hypotheses based on Darwinian assumptions into a *justification* of inductive procedures. "[S]omething must be wrong," Howson asserts, (p. 113) "with an epistemic ascent which starts by accepting Hume's sceptical argument and ends by denying it. Something is wrong." The problem with such attempts at justification, we are told, is "systematic question-begging" (ibid). Quite so. Such a justification is indeed, as Howson asserts, "a palpably circular procedure." Yet such a circular procedure, which is just as inherent in the use of dogmatic prior probabilities by subjective Bayesians, is completely dispensable. I will argue in the final chapter that we currently already employ a "non-ampliative logic for scientific inference" that is quite sufficient to the task of theory adjudication. That logic, employed both in Popperian falsification and in classical statistics, is simply standard deductive logic. To adequately address the demarcation problem, no dogmatic subjective probability assignments or circular inductive assumptions are necessary. Accordingly, we would do best to relinquish the project of epistemic justification altogether.

5.7 The Inverse Relationship between Logical Probability and Content

The probabilist theory of demarcation faces another decisive objection. As Popper asserts (1959, p. 269 fn.), in science we seek "*simple* hypotheses—hypotheses of a high *content*, a high degree of *testability*." However, "we know that testability is the same as high (absolute) logical *improbability*, or low (absolute) logical *probability*." Hence, Popper concludes, "if you value high probability, you must say very little—or better still, nothing at all: tautologies will always retain the highest probability" (ibid). This simple reductio of the probabilist program was first asserted by Popper in § 83 of his *Logic of Scientific Discovery,* and embodies, Popper maintains, "the crucial point of [his] criticism of the probability theory of induction." That is, the recommendation to seek high probabilities puts a premium on ad hoc hypotheses, which runs counter to the actual aim and procedure of science.

Popper returned to this point in *Objective Knowledge* (1972, p. 18):

> I originally introduced the idea of *corroboration,* or *'degree of corroboration',* with the aim of showing clearly that every probabilistic theory of preference (and therefore every probabilistic theory of induction) is absurd... in many cases, the more *improbable* (improbable in the sense of the calculus of probability) hypothesis is preferable... [thus] *preferability cannot be a probability in the sense of the calculus of probability.*

That is, probabilistic inductivism—or weak justificationism—is in conflict with the actual practice of science, which does not seek to achieve high probabilities. For if it did, science would then put the highest value on (highly probable) trivialities. Instead, it is clear that theoretical science is interested in hypotheses with *a high content,* whose probability inevitably *decreases* in proportion to the increase in content. Hypotheses are not, in practice, judged by how probable they are, either absolutely, or relative to the accumulated evidence. This, however, directly contradicts the position that Popper attributes to Keynes in § 83 of *Logic*—the claim that "[a] theory is regarded as scientifically valuable only because of the close logical proximity... between the theory and [accepted] empirical statements... [which] means nothing else than that the content of the theory must go as little as possible beyond what is empirically established." Yet, if science values highly explanatory theories, which are both simple and highly testable, then it does not value *probable* theories, in the sense of logical probability. This, to repeat, is because the logical probability of a statement or theory is inversely related to its empirical content and its degree of falsifiability.[33] Thus the *preferable* hypothesis will, generally, be the *more improbable* one. As Popper repeated in *Conjectures and Refutations,* "[s]ince we aim in science at a high content, we do not aim at a high probability" (1963, Chapter 11, § 6).

This can be seen if we consider an evidential statement *e*, which states the conjunction of many descriptions of a particular event. The value of $p(h,e)$ will quickly become close to zero if *h* has much descriptive content over and above that stated by *e*. What Rosenkrantz calls the "probability-improbability conundrum" (1977, § 6) is just a *reductio* of the weak justificationist view that probability is to be identified with the "degree of rational belief", or "the amount of trust it is proper to accord to a statement."

A dramatic example of this inverse relationship between probability and content concerns the probability of universal hypotheses. Popper claimed that the logical probability of all universal hypotheses *must* be zero, given any finite amount of evidence, and attempted to prove as much in (1959a, appendix *vii). That is, Popper claimed that whenever *h* is universally quantified then its absolute logical probability *p(h)* is zero. His argument runs as follows (1983, p. 219):

> [E]very universal hypothesis *h* goes so far beyond any empirical evidence e that its probability *p*(*h*,*e*) will always remain zero, because the universal hypothesis makes assertions about an infinite number of cases, while the number of observed cases can only be finite.[34]

This thesis was accepted by Carnap in his (1950), § 110F, and it was a property of all the measures in his λ-continuum (1952). However, the proof contains an error in that Popper erroneously identified probabilistic independence with logical independence. This error was identified by Howson (1973; see also his 1987), who essentially brought to light a consequence of a theorem of Horn and Tarski (1948, theorem 2.5) to the effect that probability measures can be constructed which assign contingent universal statements—even those which have infinitely many distinct instantiations—a positive value. Thus, universal laws can, contra Popper's result, obtain positive probability values in systems of logical probability.

However, Popper's general claim here (and indeed, many of his arguments concerning logical probability), is best considered as a thought experiment illustrating the *relative* degree of probability that could plausibly be conferred to a universal statement by a finite collection of singular statements, given the assumption that something like objective logical probabilities exist at all. Accordingly, the basic intuition underlying this zero-probability claim can be retained with the aid of infinitesimals, the mathematical framework for which was not available when Popper first published his result.[35] It may thus *sometimes* be the case for universal hypotheses that $p(h, e) > p(h)$, yet $p(h, e)$ here remains infinitesimally small. Accordingly we may modify Popper's statement (1974, p. 170), "to my mind such [universal laws] —of which there are, in practice, always infinitely many—*ought* to be given "probability" zero (in the sense of the calculus of probability)" to admit infinitesimally small probability values. Moreover, the only way to consistently avoid assigning universal statements an infinitesimal probability value is to resort to (dogmatic) subjective probabilities, which will necessarily be question-begging.[36]

5.8 Further Critical Remarks on Probabilist Demarcation

The demarcational appeal of probability, or partial support, can best be explained historically.
As Bartley (1984, p. 262) writes:

> The earliest attempted criteria of evaluation were criteria of truth, demarcating good ideas from bad ones coincident with the demarcation between the true and the false... But criteria of truth proved to be either unattainable or practically inapplicable to the issues for which they were needed; and the search for criteria of truth was displaced by a search for weaker but more attainable measures. Probability (in the sense of the probability calculus) is most often used for this purpose.

Probability shares an important feature with truth, which has no doubt encouraged its employment in the weak justificationist demarcation program. That is, probability, like truth, is transmissible from premises to conclusion. The consequences of a true statement are true; truth is transmitted through the deducibility relationship. Likewise, the consequences of a probable statement are a*t least as probable* as that statement, if not more so. Any consequence of a hypothesis is at least as highly probable as the hypothesis from which it was derived. This relationship is central to any justificationist demarcation, in which theories are to be evaluated with regard to a justifying authority. The initially uncertified hypotheses are to receive, through this logical relationship from premises to conclusion, whatever degree of justification is inherit in that authority. If the authority is true, the derived consequence is true, and, if the authority is probable, the derived consequence is at least as probable. This *transmissibility* relationship, as Bartley calls it, has been explicitly promoted in confirmation theory under the names "consequence condition", "entailment condition", and "content condition", by Carnap, Hempel, and Goodman respectively, and should, according to Adolf Grünbaum, be "an ingredient of any theory of corroboration or rational credibility" (See Bartley, ibid, p. 262). Justification is thus entirely parasitic on this transmissibility relationship.

However, despite sharing this common feature with truth, probability measures cannot function in the same way as a criterion of truth could, were one to exist. That is, we are unable to pronounce positively on competing scientific hypotheses solely with the aid of probability measures (although we may use cases of low probability on the evidence, e.g. $P(h, e) = 0$, to *reject* hypotheses). Probability *fails as a positive evaluational property,* and this applies both for theoretical purposes and for practical action.

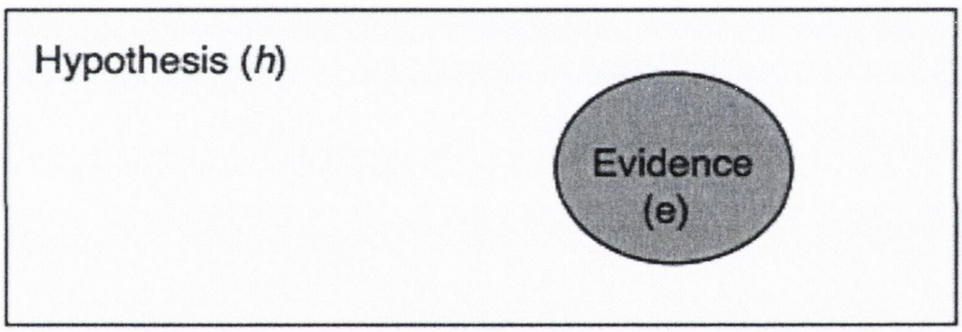
Regarding theoretical preference, Watkins' assertion (1984, p. 42) that "we would surely be out of the sceptical wood if we could, generally, select from a number of alternative hypotheses the one that is most probably true, given our present evidence", concisely summarises the probabilist approach to theory adjudication. This view is quite intuitive, but is, I think, incorrect. For if we consider again Popper and Miller's result that the excess content of any hypothesis *h* over the evidence *e* is not in any way logically supported by *e*, then the relationship between this content and the evidence is completely indeterminate. "The connection between truth and probable truth is no firmer than that between truth and rumoured truth, or between truth and *ex cathedra* truth; or, indeed, between truth and improbable truth," as Miller asserts (1994, p. 128). "That is to say, there is no logical connection at all."

Thus, whilst a criterion of truth, if one were available, could aid in demarcation, measures of probability cannot: "Certain truth deductively implies truth; but this implication dissolves when certainty is diluted to probability. Certain truth may be truth attained with certainty, but—because what is probably true need not be true—probable truth is not truth attained with probability" (ibid). Thus, if truth is our aim, what probabilities our various hypotheses have is a matter of no significance, for a measure of high probability does not sanction an assessment of truth, and this latter classification can only have the status of a conjecture which is unsupported by the evidence. To proceed into the unknown, in other words, we need to make a guess.[37]

What then are the possible relations that can hold between theory and evidence?

Evidence (*e*) $\longleftrightarrow$ Hypothesis (*h*)

We know from Hume that the evidence cannot entail a hypothesis that transcends it. And we know from Popper and Miller that the evidence cannot give any support to that part of the hypothesis which does not overlap with the evidence itself. This may be represented in the diagram below:



Note that, if *h* is a universally quantified statement—describing an empirical regularity for instance—then the circle representing the evidential support would be infinitesimally small. Moreover, the probabilistic support enjoyed by *h* is entirely a result of the fact that e entails itself. Finally, there will always be an infinity of incompatible hypotheses *h1,h2, h3…* that stand in exactly the same relationship to e that *h* does. Even if, *per impossible,* we could overlap the majority of the area of the hypothesis *h* with the evidential circle e, the uncovered area would remain no more probable than before. The evidence tells us nothing about the truth-value of *h*.

Thus, the demand that evaluations be made in terms of probability abandons the original aim of inquiry. Truth and probability, except in the extreme and unattainable limits of certainty, are independent. Probability, just like coherence, is logically unconnected with truth; accordingly its evaluational capabilities are not relevant to inquirers whose aim is truth.

This, I think, although logically compelling, is counter-intuitive. I believe that the explanation might be an unconscious conflation between the physical probability of *events* and the logical probability of *hypotheses*, especially if physical probabilities are understood, as I think they commonly are, as (weighted) measures of possibilities.[38] If we further take these possibilities to be physically real, we arrive at something akin to Popper's propensity theory (1957). Propensities, in this view, are not mere possibilities but are physical realities, analogous to forces, or fields of forces. The converse is also true (1990, pp. 12-13):

> ...forces are propensities. They are propensities for setting bodies in motion. Forces are propensities to accelerate, and fields of forces are propensities distributed over some region of space and perhaps changing continuously over this region (like distances from some given origin). Fields of forces are fields of propensities... The propensity 1 is the special case of a classical force in action: a cause when it produces an effect. If a propensity is less than 1, then this can be envisaged as the existence of competing forces pulling in various opposed directions but not yet producing or controlling a real process. And whenever the possibilities are discrete rather than continuous, these forces pull towards distinct possibilities, where no compromise possibility may exist.

If this conception of probability is applied unconsciously to the probability of hypotheses the natural intuition is that the evidence that renders a hypothesis more probable can then go on to make *future* instances more probable. To take an example, we can interpret a physical probability assertion:

$$p\ (a, b) = r$$

to mean: "The propensity of the state of affairs (or the conditions) *b* to produce *a* equals *r*." (*r here* is some real number between 0 and 1). If the value of *r* increases, then every subsequent occurrence of *a*, when the conditions *b* obtain, is more probable, and on this realist construal, it is a *physically real* propensity or force that is operating to bring about this effect.[39] A *physical* propensity may, so to speak, operate like an attractive force. If a physical propensity increases, the likelihood of future events of that kind increases—there is a physical dependence.

The *logical* probability of a hypothesis on the evidence, on the other hand, is simply a deductive measure of the overlapping content between the two statements. And the content that is overlapping has no connection to the unshared content (besides the *conjectural* connection posited by the hypothesis itself, which has the status of a physical law and is to be tested by experience)—there is no attractive force or disposition that is growing in strength and affecting future instances. Probabilistic dependence does not operate like an attractive or repulsive force; it is simply a measure of overlapping content.

Indeed, if the relationship between a hypothesis and the evidence—i.e. its logical probability—changes, this does not change the relationship between the hypothesis and the *world* in any way. It is only the relationship between the hypothesis and the *evidence* that is changed, as a result of the evidence changing. The hypothesis has the same truth value it had at the outset; either true or false, 1 or 0. No finite evidence can ever determine that its status is true—we cannot verify a universal hypothesis—so the only pertinent information an inquirer can ever infer from the evidence is that the hypothesis is false (or more accurately, that either the hypothesis or the evidence must be rejected, since, in this case, they are inconsistent. The probability of the conjunction of a hypothesis and a falsifying instance is zero). Thus a theory is unchanged by new evidence. Its truth value, which is what we are primarily interested in discerning, is either true or false, and mounting evidence does not change this fundamentally objective correspondence relation. As Miller states (2006a, pp. 54-55):

> Critical rationalism proposes... that theories are unchanged by their interaction with experience. The opposite answer is central to nearly all forms of justificationism... falsificationists are interested only in relations between theories and the world, most importantly correspondence and lack of correspondence, but also subsidiary properties such as explanatory power and problem-solving ability, whilst positivists (and justificationists in general) are as much interested in relations between the theories and ourselves and the evidence that we have in our possession... By something like a sleight of hand, [justificationists] move from the platitude that we can change the relation between theory and evidence by changing the evidence, to the claim that in some way the theory itself becomes changed in the course of this operation.

Another distinction between the probability of hypotheses and the probability of events is that, in the latter case, we are of course interested in any value whatsoever that appears in a statistical or probabilistic hypothesis as long as it is *genuine*—that is, whilst any probabilistic regularity might be interesting, we are (almost) exclusively interested in hypotheses whose truth value is 1(or perhaps closer to 1 than its competitors, assuming a viable theory of verisimilitude is forthcoming). Science may seek laws describing statistical regularities of *events* (whose variables can take any value), but the laws or hypotheses expressing them are either true or false.

Thus, partial support, or probabilistic justification, has no *theoretical* utility—it cannot aid in the classification of theories as true.

But neither does it have any *practical* utility.

This point is made quite clearly by Socrates in Plato's *Meno,* as Miller has drawn attention to in his (1994, p. 64ff). To the question, what is it about knowledge that makes it more valuable than mere true belief? Socrates asserts (98A, C), "for practical purposes right opinion is no less useful than knowledge" and "true opinion when it governs any course of action produces as good a result as knowledge." Accordingly, this problem, concerning what role *justified* belief can serve that *true* belief cannot serve just as well, has, in recent epistemological discussions (e.g., Kvanvig 2003), been dubbed the *Meno* problem.

The exchange between Socrates and Meno is as follows (97 A-D):[40]

> *Socrates:* Let me explain. If someone knows the way to Larissa, or anywhere else you like, then when he goes there and takes others with him he will be a good and capable guide, you would agree?
>
> *Meno:* Of course.
>
> *Socrates:* But if a man judges correctly which is the road, though he has never been there and doesn't know it, will he not also guide others aright?
>
> *Meno:* Yes, he will.
>
> *Socrates :* And as long as he has a correct opinion on the points about which the other has knowledge, he will be just as good a guide, believing the truth but not knowing it.
>
> *Meno:* Just as good.

> *Socrates:* Therefore true opinion is as good a guide as knowledge for the purpose of acting rightly...
>
> *Meno:* It seems so.
>
> *Socrates:* So right opinion is something no less useful than knowledge.
>
> Meno: Except that the man with knowledge will always be successful, and the man with right opinion only sometimes.
>
> *Socrates:* What? Will he not always be successful so long as he has the right opinion?
>
> *Meno:* That must be so, I suppose. In that case, I wonder why knowledge should be so much more prized than right opinion...

Socrates goes on to assert that knowledge is still to be preferred to true belief because it is "tethered", like the mythical statues of Daedalus, which were said to be so life-like that they were tied down to make sure that they did not run away (98A). His point here, according to Pritchard and Turri (2012, § 1), is that "knowledge, unlike mere true belief, gives one a confidence that is not easily lost, and it is this property that accounts for the distinctive value of knowledge over mere true belief." They go on to elaborate:

> ...if one knows the way to Larissa, rather than merely truly believes that such-and-such is the correct way to go, then one is less likely to be perturbed by the fact that the road, initially at least, seems to be going in the wrong direction. Mere true belief at this point may be lost, since one might lose all confidence that this is the right way to go. In contrast, if one knows that this is the right way to go, then one will be more sanguine in the light of this development, and thus will in all likelihood press on regardless (and thereby have one's confidence rewarded by getting where one needs to go).

Socrates, (and Pritchard and Turri), are perfectly correct, I think, that a guarantee of success would be *psychologically* encouraging, but if the arguments in the last three chapters hold, such a guarantee does not exist. Yet such a guarantee is not *necessary* for inquiry, or even for *successful* inquiry. Knowledge in the epistemological sense, if it were to exist, would have no advantage over true belief. And concerning practical action, as Socrates reminds us, "true opinion when it governs any course of action produces as good a result as knowledge" (98A).

Linda Zagzebski (1996, 2003, 2009) makes a very similar point in her critique of reliabilism—on any objectivist view of knowledge (which Zagzebski rejects), the value of a knowledge claim is in no way dependent on the method of its inception or on its certification. What matters is the product itself. Zagzebski gives the example of a good cup of coffee (2009, p. 110): "the espresso made now does not get any better in virtue of the fact that it was produced by a reliable espresso machine. If the espresso tastes good, it makes no difference if it comes from an unreliable machine." Analogously, partial justification does not add any value to true belief. True belief is, metaphorically speaking, a good espresso.

Thus, the appeal to partially justified knowledge cannot solve the demarcation problem, neither in theoretical or practical matters. Pritchard and Turri (ibid) go on to assert that, although "we clearly do value knowledge more than mere true belief... the fact that there is no obvious explanation of why this should be so creates a problem." The puzzlement here is easily solved, I think, if we accept that justification can only be fruitful in theoretical matters when, *per impossible*, the justification is conclusive; when it is weakened to partial justification it has no theoretical value at all. In practical applications, moreover, neither partial n*or* conclusive justification adds any utility to a knowledge claim.

Thus, it seems we must agree with Robert Fogelin (1994, p. 192) that "no justificatory theory seems to show any prospects of solving the Agrippa problem... Pyrrhonism or Neopyrrhonism seems to be the terminating point of the epistemological enterprise." And indeed, as even justificationists have sometimes recognised, justification is completely inessential to scientific inquiry and to everyday life: Salmon (1966, p. 54), for instance, notes:

> It is not difficult to appreciate the response of the man engaged in active scientific research or practical affairs who says, in effect, "Don't bother me with these silly puzzles; I'm too busy doing science, building bridges, or managing affairs of state." No one, including Hume, seriously suggests any suspension of scientific investigation or practical decision pending a solution of the problem of induction.

However, this is not to say that the *pursuit* of justification is thereby innocuous. As I will detail in the following chapter, justificationism is, in fact, closely linked to relativistic theories of science.

---

[1] Here "fallibilism" should be understood as "fallibilistic justificationism", rather than fallibilism proper, which is compatible with scepticism.

[2] As Salmon notes, (1966, p. 4) "[e]ver since antiquity philosophers had been aware that the senses can deceive us, and this point was emphatically reiterated by Descartes. Those who were engaged in the quest for certainty found in this fact a basis for rejecting the empirical method. Some of the ancient skeptics even had been aware that inductive inference can sometimes lead to false conclusions; again, those engaged in the quest for certainty could reject inductive methods on this ground. Philosophers who recognize that science cannot be expected to yield absolutely certain results can tolerate both of these shortcomings with equanimity."

[3] That Hume's argument was intended to be equally applicable to probabilistic justification has been denied by Stove (1982), Mackie (1980) and van Cleve (1984), amongst others. For instance, Mackie writes that "[r]easonable but probabilistic inferences, then, have not been excluded by Hume's argument, for the simple reason that Hume did not consider this possibility" (1980, p. 15). Yet this is not correct; Hume had read, and explicitly rejected, James Bernoulli's probabilistic theory of induction in his *Ars Conjectandi* (1715). Howson recites what he calls "decisive historical and textual" evidence for this view in his (2000, pp. 13-14):

"Hume certainly knew of the contemporary mathematical theory of probability, and its rudiments, and knew that there was already a keen interest in trying to use it as a logical basis for inductive arguments... Not only does the intellectual context in which Hume wrote make it a more than reasonable presumption that arguments from the formal theory of probability were included in Hume's 'probable arguments'; the presumption is borne out by the Treatise itself... in view of the fact that 'the course of nature may change', and in very different ways, any extrapolation sanctioned by a probabilistic argument will beg the question of why that particular way should be regarded as a probable one. In fact, Hume's circularity thesis applies to arguments from mathematical probability as much as it does to any sort of non-deductive 'probable inference'..."

[4] Howson also alludes to the view, "prevalent between the beginning of the eighteenth century and the end of the nineteenth, that the evaluation of hypotheses in the light of evidence is in terms of probability..." (2000, p. 3)

[5] Hence Goodman writes regarding his (again, so-called) paradox "Thus although we are well aware which of the two incompatible predictions is genuinely confirmed, they are equally well confirmed according to our present definition... We are left once again with the intolerable result that anything

confirms anything" (Goodman 1955, pp. 74–5).

[6] Perhaps the first explicit announcement of this program was in James Bernoulli's *Ars Conjectandi* ("*Art of Conjecturing*"), where he wrote, regarding the judgement of hypotheses in relation to experimental evidence, that "[t]o conjecture about something is to measure its probability" (1713, p. 8).

[7] Prof. Rowbottom, in his role as external examiner, raised the following interesting objection to this claim by Popper: "Popper is plausibly incorrect that objective probabilities must concern physical systems. How about, say, classes of non-physical entities on the relative frequency view? (Consider the probability of choosing an even number when making a random pick from the class of natural numbers.)" This seems a legitimate point; I do not think too much weight should be placed on Popper's dichotomy here.

[8] Gillies (2000), on the other hand, classifies the classical theory as "epistemic", as does Rowbottom (2011).

[9] Popper remarks (1959, p. 252) that "[l]ike inductive logic in general, the theory of the probability of hypotheses seems to have arisen through a confusion of psychological with logical questions." Hence his term, "logico-subjective" for this (inductive) interpretation of logical probability. It is important to note, however, that logical probability need *not* concern degrees of belief, in contrast to e.g. objective Bayesianism.

[10] See for example, Jeffrey 1975, pp. 112, Salmon, 1969.

[11] This is a reference to Carnap's project in his *Aufbau* (1929).

[12] This applies to terms even as intuitively "immediate" as, for example, "red": "all universals are dispositional terms, even such apparently observational terms as 'red'. A body is red if its surface is dispositioned to reflect red light, that is, light of certain frequencies… in this sense, 'the customary distinction between *'observational terms'* (or *non- theoretical terms)* and *theoretical terms* is mistaken, since all terms are theoretical to some degree, though some are more theoretical than others… even 'looking red' is a dispositional term: 'It describes the disposition of a thing to make onlookers agree that it looks red'" (Popper, 1968, p. 292).

[13] Prof. Rowbottom (external examiner report) notes that: "An additional argument against Jeffreys is possible. This is that degrees of belief are not generally determinable by introspection, as Ramsey argues. So then experiments are needed to determine them. And the results of said experiments depend on observation statements... And so on…"

[14] However, it should be noted that this measure, unlike p(*h, e*) is *not* a generalisation of the deducibility relation, undermining the thesis that inductive support is a simple generalisation of deductive support.

[15] This corresponds to the "accepted inductive principle… a principle saying roughly that observed instances conforming to a generalization constitute evidence for it" (Salmon, 1966, p. 7)

[16] See Hempel (1945).

[17] Again, this depends on the assumption that the prior probability of *h* is not 0 or 1.

[18] See, for instance, Popper and Miller (1987), Miller (1994, ch. 3) and Popper (1998, ch. 10).

[19] $h \leftarrow e$ is more usually written $e \rightarrow h$; that is, "if e then *h*", or "*h* in case of *e*".

[20] The more restricted thesis had been suggested by da Costa & French 2003, pp. 146-148.

[21] This, incidentally, adequately dissolves Howson's (2000, pp. 186-187) puzzlement that "[f]ew commentators have pointed out that it is strange that two avowed anti-inductivists should happily accept that the content of H going beyond E could be countersupported by E. Nevertheless, it is as anomalous as if they had shown that it was supported by E, for neither possibility seems to square with the sceptical tenet that E only informs us about E and nothing beyond it."

[22] See his joint paper with Franklin (1985), and Popper and Miller's response (1987).

[23] See Popper's 1959, Preface and appendices *viii and *ix for some critical remarks on such systems. However, it should also be noted that as Carnap's views on probability and confirmation evolved, he increasingly moved towards the subjective interpretation. As Zabell (2008, p. 278) reports "Carnap's 1962 paper "The Aim of Inductive Logic" reflects views very similar to those of Ramsey and de Finetti: decision-making in the face of uncertainty involves utilities and probabilities (in their guise as degrees of belief)."

[24] See, for example, Salmon 1966 pp. 70–79 for an enumeration of such problems, and also Howson 2000 chapter 4, where he writes (p. 69): "...we have as yet no way of further determining the unconditional or prior probabilities P(H) and P(E)... most of the work done trying to model induction probabilistically has been an attempt to use additional heuristic principles to try to determine values for them; sometimes for special cases of H and E (as was the case with Bayes's famous essay), sometimes in a more ambitiously global way (as in Carnap's monumental Logical Foundations of Probability 1950)... [this involved]... a lot of technical work which... lead[s] nowhere..."

[25] It should be noted, however, that subjective Bayesianism is not a univocal movement; indeed, various internal problems have led to a huge proliferation of competing and incompatible personalist positions. Only half-jokingly, Good (1971) states that it is possible to distinguish *46, 656* possible varieties of Bayesianism; a detailed critique of each is beyond the scope of this chapter.

[26] Bayes's theorem itself is uncontroversial of course—it is a trivial deductive consequence of the three basic (Kolmogorov) axioms of the probability calculus:

1. $P(a) \geq 0$
2. $P(a) = 1$ if a is true in all models
3. $P(a \vee b) = P(a) + P(b)$ if a and b are mutually exclusive.

[27] Miller in his (1994, Chapter 8) also raises the interesting possibility of probability distributions that are less well behaved than the familiar parametric distributions (such as the normal, the binomial, and the Poisson distributions), but which instead evolve *chaotically* in a fashion increasingly found unexceptional in the dynamical systems of classical mechanics. He writes "the chaotic distributions [constructed] above, if they have any validity... suggest that agents who start off in almost complete (probabilistic) agreement may in special, perhaps unusual, circumstances come to diverge in their opinions more and more, despite being exposed to the same evidence... those who hold that

convergence of opinion is a hallmark of objectivity need to spell out more plainly the conditions under which such convergence will be found to obtain."

[28] A more recent attempt to reduce the arbitrariness of probability assignments in personalistic Bayesianism is the Objective Bayesianism of Jaynes (2003), which introduces various additional rationality constraints—most notably a "maximum entropy principle." For critical comments, see Rowbottom (2008).

[29] See Weston 1992 § 2.

[30] Salmon (1966, p. 82) also notes that the "personalistic theory… leaves entirely unanswered our questions about inductive inference. It tolerates *any kind* of inference from the observed to the unobserved. This amounts to an abdication of probability from the role of "a guide of life."… a general feature of the personalistic approach… [is that]… additional conditions must be found before we can pretend to have a viable conception of rationality. It is essential that the additional conditions be stated explicitly and justified. This is precisely where Hume's problem of induction lies for the personalistic theory."

[31] The quotation from Albert is from an unpublished manuscript "Bayesian Learning and Expectations Formation: Anything Goes" (1997, p. 29)

[32] It is noteworthy here that Howson explicitly denies that a belief being "*credible*" entails that it is *rational to believe or act on it*. For in the concluding remarks of his book Howson writes (2000, p. 239-240, emphasis added): "Surely [Bayesian inductive reasoning] can't be all there is to scientific rationality? Possibly not: I have scrupulously avoided discussing scientific rationality, partly because it is a highly contested area, but mainly because this is a book about logic, not about rationality. *The rules, if there are any, determining what is rational and what is not to believe or do I am happy to leave to others to fight over.* But what I do believe, and I believe that this extended footnote to Hume shows, is that no theory of rationality that is not entirely question-begging can tell us what it is rational to believe about the future, whether based on what the past has displayed or not." Given this Humean thesis, it is difficult to discern what *use* the judgements of "credibility" derived using Bayesian reasoning could possibly have. Indeed, if this is not an admission that "considerations of credibility are redundant" I fail to see what would be.

[33] The logical content of a theory is the class of (non-tautological) statements entailed by the theory, whilst the empirical or informative content of a theory is a measure of the "size" of the class of the potential falsifiers of that theory. This concepts are explained further in § 7.8 below.

[34] Jan Woleński and Joseph Agassi (2010) report that the Polish logician Jan Łukasiewicz, whilst researching probabilistic induction, had made essentially the same argument as early as 1908.

[35] The pioneering work here was Robinson's (1966).

[36] This is acknowledged by subjective Bayesians: "we cannot be non-dogmatic in our probability assignments across the board, in the sense of universally assigning non-zero prior probabilities to the members of big enough possibility-spaces" (Howson, 2000, p. 80).

[37] A similar point is made in Miller, 2006, p. 69: "there is no known method that allows us to proceed from what is known to what is unknown—if there were such a method known, the unknown would already be known. This means that inductive inferences, if there are any, that genuinely expand our knowledge cannot be conducted according to any known rules (whether formal or informal). It would accordingly be more honest to call such moves conjectures or guesses than to pretend that they are logical inferences... Valid inferences, in contrast, make no attempt to explore the world, but only to explore the conjectures that we already entertain about the world."

[38] This chimes with Alberto Mura's view, in an editorial annotation to de Finetti 2008 (p. 66), that the concept of propensity is "ubiquitous in the history of probability from the very beginning (though rendered with different words like "proclivity", "facility" and so forth)." Compare, however, with de Finetti's own view (1982, p. 5): "The only approach which, as I have always maintained and have confirmed by experience and comparison, leads to the removal of such ambiguities, is that of probability considered—always and exclusively—in its natural meaning of "degree of belief." This is precisely the notion held by every uncontaminated "man in the street."'

[39] C. S. Peirce's "would-be" or "habit" expresses a similar intuitive idea. See Gillies, 2000, Ch. 6 for more details.

[40] Quoted in Miller (1994, p. 64ff).

Chapter Six: Relativistic Responses to the Failure of Justificationism

6.0 Introduction

If the considerations of the last three chapters are correct, nontrivial (synthetic) knowledge claims cannot be justified in a non-authoritarian and non-circular fashion. Such a result has dire consequences for those who hold the justificationist theory of demarcation-cum-rationality. One of the most intellectually disturbing of these is the popularity of extreme social constructivism, postmodernism, and other relativistic and irrationalistic theories of science. Such positions are "the offspring of scepticism, which is correct, and the equation *rationality = justification,* which is incorrect" (Miller, 2006a, p. 153). More precisely, such positions have retained justificationism by limiting the domain of justification procedures to particular traditions or paradigms. In such a way, the possibility of justification is reinstated, but only at the cost of severing it from objective truth. Where the term "truth" is retained it all, it is internal to the framework and is only applicable *within* that framework, by reference to agreed rules. Demarcation and theory adjudication, however, are impossible *across* frameworks—these are *incommensurable*, with no possibility of rational adjudication between them. Adherents of this view have, as Radnitzky asserts, (1987, p. 286) "not just lost, but have abandoned, the problem of rational theory preference."

Such a theory of science is most closely associated with Thomas Kuhn's *The Structure of Scientific Revolutions* (1962). In this chapter I will take Kuhn's theory as an *exemplar*, examining the relativistic consequences of such a theory, and illustrating its roots in justificationism.[1]

6.1 Kuhn's Theory of Science in *Structure*

Thomas Kuhn (1922–1996) is probably the most well-known and influential philosopher of science of the twentieth century. He was instrumental in the "historical turn" of the discipline in the 1960's away from the more formalistic studies of science associated with logical empiricism, and most especially, for our purposes, away from the inductivist-verificationist style inherent in confirmation theory. Early works such as *The Copernican Revolution* (1957) were well-received, but it was his *The Structure of Scientific Revolutions* (1962) which made his reputation and transformed the entire discipline.

Kuhn's theory of science is crucially influenced by his historical studies and his interpretation of what he sees as the *actual* process of theory change, as opposed to the idealised formalistic readings he associated with both the logical empiricists and with falsificationism. According to Kuhn, scientific inquiry occurs within a *paradigm* (or in his later terminology, a *lexicon*)—an amorphous term denoting the network of theories, beliefs, values, methods, and objectives of a scientific community—and is at least partly directed by extrascientific factors. Such paradigms delineate "normal science"—the activity of rule bound problem solving with particular reference to generally accepted *exemplars*. These are "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners" ([1962] 1970 p. viii). Paradigms, and the exemplars within them, provide "accepted examples of actual scientific practice—examples which include law, theory, application, and instrumentation... [they] provide models from which spring particular coherent traditions of scientific research" (ibid. p. 10). However, it happens, according to Kuhn, that anomalies emerge which are inexplicable in terms of the ruling paradigm. These are generally ignored at first, but eventually become so pressing as to precipitate a crisis. Thus an anarchic transition may (potentially) take place—a "scientific revolution"—which inaugurates an entirely new paradigm (and with it a radically new conceptual framework and set of methodological precepts) which is better able to accommodate the anomalies. A new period of normal, rule-bound, science can then commence.

Kuhn's *Structure* is often regarded as marking the "official demise" (Friedman 1991, p. 1) of logical positivism.[2] It was Kuhn's work that, "more than any other factor, according to the standard and informal histories of philosophy of science, caused the decline and eventual overthrow of logical empiricism" (Richardson, 2007, p. 348). Most particularly, it was Kuhn's rejection of the *empiricist justificatory* program which was so influential. Indeed, Kuhn stressed the similarities between his own position and Popper's in that regard (1970, pp. 1-2):

> On almost all the occasions when we turn explicitly to the same problems, Sir Karl's view of science and my own are very nearly identical. We are both concerned with the dynamic process by which scientific knowledge is acquired rather than with the logical structure of the products of scientific research. Given that concern, both of us emphasize, as legitimate data, the facts and also the spirit of actual scientific life, and both of us turn often to history to find them. From this pool of shared data, we draw many of the same conclusions. Both of us reject the view that science progresses by accretion; both emphasize instead the revolutionary process by which an older theory is rejected and replaced by an incompatible new one!; and both deeply underscore the role played in this process by the older theory's occasional failure to meet challenges posed by logic, experiment, or observation. Finally, Sir Karl and I are united in opposition to a number of classical positivism's most characteristic theses. We both emphasize, for example, the intimate and inevitable entanglement of scientific observation with scientific theory; we are correspondingly sceptical of efforts to produce any neutral observation language; and we both insist that scientists may properly aim to invent theories that explain observed phenomena and that do so in terms of real objects, whatever the latter phrase may mean.

Thus, first and foremost in Kuhn's rejection of the "image of science by which we are possessed", (Kuhn [1962] 1970, p. 1) is his rejection of the view of science as *inductive* and *cumulative*. That is, Kuhn rejected the logical empiricist view that theories are "justified via explicit logical arguments that tie them to anticipated experimental results" (Richardson, ibid). Indeed, Kuhn was well aware of the sceptical challenges to traditional justificationism, and in particular the so-called "underdetermination of theory by data" thesis—another way to state Hume's problem of induction. As Kuhn writes ([1962] 1970, p. 76):

> Philosophers of science have repeatedly demonstrated that more than one theoretical construction can always be placed upon a given collection of data. History of science indicates

> that, particularly in the early developmental stages of a new paradigm, it is not even very difficult to invent such alternates.

Kuhn was also no stranger to the sceptical argument that all justification is circular. He writes (ibid. p. 94):

> ...the choice between competing... paradigms proves to be a choice between incompatible modes of community life... When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense.

Thus, Kuhn was both a sceptic regarding induction,[3] and a sceptic regarding the positivist's verificationist theory of demarcation. However, Kuhn did not thereby give up the justificationist theory of rationality, which still underlies his account of theory choice, both in "normal" and in "revolutionary" science. Justificationism, as we shall see, is apparent in virtually every aspect of Kuhn's theory of science (at least in the *Structure)*, and culminates in, at different stages, fideism, relativism, and the denial of objective truth.

6.2 The Justificationist Roots of Fideist "Normal Science"

Kuhn's justificationism is intimately related to his concept of a paradigm, and this is particularly apparent in his account of "normal science." The concept of a paradigm has many intellectual forerunners, chief amongst them being Kant's radical distinction between the phenomenal world, constituted by the concepts and categories of inquirers, and the (largely epistemically inaccessible) *world-in-itself*. In contrast to Kant however, modern writers have tended to assert that there is a plurality of phenomenal worlds, each being dependent on, or constituted by, distinct social groups. As such, this framework idea is central to modern relativistic thought. As Paul O'Grady notes (2002, p.11), it is

> ...the idea that the world is mediated through a structure that yields different accounts of reality to us relative to that structure. Different terms have been used for such a structure - for example,

"conceptual scheme", "paradigm", "linguistic framework" and "language game". That our knowledge of the world comes via such a structure is a distinctive element in a number of eminent twentieth-century philosophers' work and a main plank in relativists' thinking.

Perhaps a bigger influence than Kant on Kuhn's theory of science, however, was Ludwig Wittgenstein; "possibly the most important developer of framework ideas of the twentieth century" (ibid, p.15). Wittgenstein's version of this idea is expressed in his concepts of "language games" and "forms of life". As Maria Baghramian writes (2004, p. 75):

> Linguistic communication is a rule-governed social activity that takes place in the context of a whole host of other purposive social behaviour or what [Wittgenstein] calls a 'language-game'. All human life, including our conceptual life, is played out in a cultural, social and biological context, or a 'form of life'. Language cannot be understood in isolation from the goals and needs of the participants of specific language-games and the background of their form of life. At all times we should remember that 'What has to be accepted, the given, is—so one could say—*forms of life*' (1953: 226e).

These forms of life, according to Wittgenstein, are incapable of justification—they must be accepted as given. Justification can obtain *within* a form of life, but the form of life itself cannot be defended by anything outside it. Each form of life, or language game, is autonomous, and each creates its own standards of evaluation and rules of conduct. There is no cross language game method of adjudication. According to Wittgenstein, "it was most important not to conflate one language game with another, or to think that there are super language games that govern all the others... it is illicit to use criteria from another language game [to challenge a different one]" (O'Grady, 2002, p. 16). The most a philosopher can do, according to Wittgenstein, is *describe* these forms of life or language games: "we may not advance any kind of theory. There must not be anything hypothetical in our considerations. We must do away with all explanation, and description alone must take its place" (1953, § 109).

Indeed, Wittgenstein, like Kuhn, was also sceptical of empiricist attempts to justify knowledge, without thereby being inclined to give up justificationism as a theory of rationality.[4] For instance, he discusses the possibility of inductivist justificationism in his *Philosophical Investigations* (1953). There he writes: "If it is now asked: But how *can* previous experience be a ground for assuming that such-and-such will occur later on?

—the answer is: What general concept have we of grounds for this assumption?" (§ 480) Wittgenstein goes on to explicitly reject both strong justificationism—"here grounds are not propositions which logically imply what is believed... the question here is not one of an approximation to logical inference (§ 481)—and weak justificationism—he regards the attempt to elucidate "grounds" of justification in probabilistic terms as misleading (§ 482). Wittgenstein's *positive* account of justification (§ 483), however, is brief and enigmatic: "A good ground is one that looks *like this.*"

Stephen Toulmin, luckily, elaborates upon Wittgenstein's position (1961, p. 42): "There must always be some point in a scientist's explanations where he comes to a stop: beyond this point, if he is pressed to explain further the fundamental basis of his explanation, he can say only that he has reached rock-bottom." This account accords with Wittgenstein's further comments in *On Certainty* on the subject of "hinge propositions":

> Must I not begin to trust somewhere? . . . somewhere I must begin with not-doubting; and that is not, so to speak, hasty but excusable: it is part of judging. (150) . . . regarding (something) as absolutely solid is part of our *method* of doubt and enquiry.(151) . . . Doubt itself rests only on what is beyond doubt.(519) . . . The *questions* that we raise and our *doubts* depend on the fact that some propositions are exempt from doubt, are as it were like hinges on which those turn. (341) . . . If I want the door to turn, the hinges must stay put.(343) . . . Whenever we test anything, we are already presupposing something that is not tested.(163) . . . At the foundation of well-founded belief lies belief that is not founded.(253) . . . Giving grounds . . . justifying the evidence, comes to an end; - but the end is not certain propositions' striking us immediately as true, i.e., it is not a kind of *seeing* on our part; it is our acting, which lies at the bottom of the language-game. (204) . . . The language- game is . . . not based on grounds. It is not reasonable (or unreasonable). (559)[5]

Justification then, according to Wittgenstein, occurs *within* a language game, but the language game cannot itself be justified. As Norman Malcolm writes in "The Groundlessness of Belief" (1977, pp. 143-157): "[t]he framework propositions of the system are not put to the test." Such a position retains the justificationist thesis that knowledge claims, in order to be rational, must be justified relative to an authority. Where this position differs from traditional justificationism, however, is in the acceptance of the sceptical thesis that any such authority, if it is to be accepted, must be accepted *dogmatically*. This contention, asserts Bartley (1987, p. 205), "that argumentation necessarily involves an end-point or presupposition... that must be accepted without evaluation or criticism,... lies at the heart of all classical dogmatism, fideism [and] relativism." It is a position Mark Notturno (2000, p. 226) has called "Floating Foundationalism", for it "retains the structure and function of bedrock foundationalism, but leaves the foundations themselves floating in midair."

Kuhn applied these ideas, virtually wholesale, to scientific knowledge.[6]

Consider Kuhn's periods of "normal science." These are characterised, according to Kuhn, by universally accepted theories and by commonly agreed upon procedures—both for conducting experiments and for evaluating their results. The norms for theory acceptance and evaluation are thus established by the paradigm, and only exist *within* the paradigm. Crucially, there is no cross paradigm method of adjudication. Rational theory choice can occur within a paradigm, but this is because the paradigm *constitutes* what is to count as rational. As Kuhn asserts, "there is no standard higher than the assent of the relevant community" ([1962] 1970 p. 94).

Moreover, normal science is characterised by the *uncritical* acceptance of a paradigm by its practitioners. According to Kuhn, "it is precisely the abandonment of critical discourse that marks the transition to a science... critical discourse recurs only at moments of crisis when the bases of the field are again in jeopardy... the tradition of critical discussion... does not at all resemble science" (ibid, pp. 6-7). That is, since the fundamental shared beliefs of a scientific paradigm are beyond justification, they must, according to Kuhn, be accepted *dogmatically*. Furthermore, the testing that does occur within a paradigm is not fundamental.[7] As Richardson (2007, p. 349) notes: "when the paradigms of normal science are not actively under dispute, theories are not really tested against experimental evidence, in the sense that mismatches might lead to genuine overthrow of the theories. Mismatches have the status of problems to be solved – and solved according to the means that the paradigm itself posits. It is not the theory but the practitioner's status as a competent scientist that is in test." It is less the paradigm that is tested as the ingenuity of the scientist; the dominant theoretical and methodological framework is beyond criticism and anomalies are largely ignored.

Kuhn's position here was preempted, to some extent, by the chemist-turned-philosopher Michael Polanyi (1891-1976), who had argued in favour of a fideistic faith in science as the only possible response to Hume. Discussing Evans-Pritchard's anthropological studies of the systems of magic of the Azande tribe, Polanyi asserted that "the stability of the naturalistic system which we currently accept instead [of Zande superstitions] rests on the same logical structure... Secured by its circularity and defended further by its epicyclical reserves, science may deny, or at least cast aside as of no scientific interest, whole ranges of experience..." (1958, p. 292). For both Polanyi and Kuhn, the impossibility of impersonal, objective justification necessitates the employment of faith amongst scientific inquirers:

> Science and magic are both comprehensive systems of beliefs, possessing a considerable degree of stability, and a comparison of the two systems has shown that the convincing powers of both are derived from similar logical properties of their conceptual frameworks. Yet the two achievements... are mutually exclusive. If you accept one system you cannot hold the other, and we today overwhelmingly accept science. (Polanyi, 1952, p. 230)

Polanyi's "post-critical" suggestion was simply to appeal to faith. Of course, the problem with this suggestion is that scientific rationality is not the only game in town, despite being Polanyi's preferred ideology. As Wesley Salmon notes (1966, p. 55): "In the face of Hume's arguments and the failure of many attempts to solve the problem, it is easy to conclude that the problem is hopeless... Hume has presented us with a serious challenge to [the] cognitive claim [of science]. If we cannot legitimize the cognitive claim, it is difficult to see what reason remains for doing science. Why not turn to voodoo, which would be simpler, cheaper, less time consuming, and more fun?" As should be clear to any student of history, scientific rationality is not the norm or the default option, as Polanyi seems to assume.

Kuhn too describes a scientist's commitment to a paradigm as his "faith." It might be supposed that Kuhn's account here is meant to be purely descriptive, written in his capacity as a historian. However, in explicit reply to Paul Feyerabend's charge that his theory was ambiguous between being descriptive *or* prescriptive,[8] Kuhn wrote (1970, p. 237):

> The answer, of course, is that [it] should be read in both ways at once. If I have a theory of how and why science works, it must necessarily have implications for the way in which scientists should behave if their enterprise is to flourish.

Popper's response to this theory was not to deny the existence of "normal science in Kuhn's sense, but to criticise it as authoritarian and uncritical (1970, p. 53):

> 'Normal' science, in Kuhn's sense, exists. It is the activity of the non-revolutionary, or more precisely, the not-too-critical professional: of the science student who accepts the ruling dogma of the day, who does not wish to challenge it, and who accepts a revolutionary theory only if almost everybody else is ready to accept it—if it becomes fashionable by a kind of bandwagon effect... The 'normal' scientist, as described by Kuhn, has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination. He has learned a technique which can be applied without asking for the reason why... As a consequence, he has become what may be called an applied scientist, in contradistinction to what I should call a pure scientist. He is, as Kuhn puts it, content to solve puzzles.

Thus Kuhn's justificationism leads to a dogmatic attitude in normal science. It is characterised by commitment—*faith*. As Bartley writes (1987, p. 209), theorists such as Wittgenstein and Kuhn "accept that grounds or reasons or justifications must be given if something is to be rational, but insist that the standards - criteria, authorities, presuppositions, frameworks, or ways of life- to which appeal is made in such justification cannot and need not be themselves justified, and that a commitment must hence be made to them." If justification is construed as arising from consensus and shared beliefs, and if science pursues justified knowledge, criticism must be discouraged and dogmatic fideism endorsed.

6.3 The Justificationist Roots of Relativistic "Revolutionary Science"

Let us now turn to Kuhn's description of "extraordinary" or revolutionary science —those episodes of science that Kuhn associated with theorists such as Copernicus and Einstein. It is Kuhn's account here which illustrates his justificationist approach most strikingly. As I quoted Kuhn earlier (1962, p.94):

> the choice between competing... paradigms proves to be a choice between incompatible modes of community life... When paradigms enter, as they must, into a debate about paradigm choice,

> their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense.

That is, whilst normal science is a rule-governed activity, whose rules are supplied by the paradigm and which constitute "rationality" relative to that paradigm, the choice between paradigms is not rule-governed. It is irrational, because rationality is, necessarily, internal to paradigms. In a conflict, any appeal to paradigm-relative standards is question-begging. Rational inquiry requires a "hinge" (that is, universally agreed upon presuppositions and standards) and none is available, according to Kuhn, in periods of scientific revolution. There is also no criticism or evaluation possible *between* paradigms or *external* to them.

Kuhn accordingly characterises decision-making in scientific revolutions—*paradigm shifts*—in wholly psychological and non-rational terms. He speaks repeatedly of "conversion"—like Polanyi, commitment to a paradigm for Kuhn is analogous to converting to a new religion. It is not rational argument which plays the pivotal role in the adoption of a new paradigm, but instead, "techniques of persuasion" (ibid, p. 152). In periods of crisis, a scientist "may begin to lose faith and then to consider alternatives…[t]hey do not, that is, treat anomalies as counter instances" (ibid, p. 77). "The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving," Kuhn asserts. "He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith" (ibid, p. 158). Although there "must be a basis… for faith in the particular candidate chosen," such a basis "need be neither rational nor ultimately correct" (ibid). Kuhn continues, in one of his most famous passages (p. 22):

> Paradigms are not corrigible by normal science at all. Instead, as we have already seen, normal science ultimately leads only to the recognition of anomalies and to crises. And these are terminated, not by deliberation and interpretation, but by a relatively sudden and unstructured event like the gestalt switch.

Given such a presentation, it is hardly surprising that Kuhn's theory of paradigm change has been dubbed, by Imre Lakatos[9] (1970, p. 178), a matter of "mob psychology." And as Maria Baghramian notes (2004, p. 144), "we are left with a view of science analogous to cultural relativism... Despite Kuhn's protestations and attempts to dispel what he saw as deep misunderstandings of his views, his work has remained a dominant influence on epistemic relativism."

The reasoning behind Kuhn's irrationalist theory of paradigm shifts is fascinating; it is a consequence of a *justificationist theory of argumentation*. This is tied up with Kuhn's view that different paradigms are *incommensurable*—that is, there is no strict translation of the terms and predicates of the old paradigm into those of the new. Kuhn, in his "Postscript, 1969" to the *Structure,* stressed that this did *not* entail that "the proponents of incommensurable theories cannot communicate with each other at all" (1970, pp. 198-199). Critics, on this point, had "seriously misconstrued the intent of these parts of my argument" (ibid). Rather, his point was instead one about *the logical presuppositions of justification*. When scientists debate the choice between competing paradigms,

> ...the parties to such debates inevitably see differently certain of the experimental or observational situations to which both have recourse. Since the vocabularies in which they discuss such situations consist, however, predominantly of the same terms, they must be attaching some of those terms to nature differently, and their communication is inevitably only partial. As a result, the superiority of one theory to another is something that cannot be proved in the debate. Instead, I have insisted, each party must try, by persuasion, to convert the other.

Thus, incommensurable paradigms *are,* in fact, translatable, according to Kuhn. The real problem, however, is that paradigms are incapable of *proof.* Kuhn continues (1970, p. 99):

> Consider... my remarks on proof. The point I have been trying to make is a simple one, long familiar in philosophy of science. Debates over theory-choice cannot be cast in a form that fully resembles logical or mathematical proof. In the latter, premises and rules of inference are stipulated from the start. If there is disagreement about conclusions, the parties to the ensuing debate can retrace their steps one by one, checking each against prior stipulation. At the end of that process one or the other must concede that be has made a mistake, violated a previously accepted rule. After that concession he has no recourse, and his opponent's proof is then compelling. Only if the two discover instead that they differ about the meaning or application of stipulated rules, that their prior agreement provides no sufficient basis for proof, does the debate continue in the form it inevitably takes during scientific revolutions. That debate is about

> premises, and its recourse is to persuasion as a prelude to the possibility of proof.

All this is very revealing. For Kuhn, "proofs" are grounded upon universally accepted premises. Where scientists differ about fundamental assumptions, such as when they use terms in different or incommensurable ways, there can be no "proof" in this sense. But neither, according to Kuhn, can there be any critical use of reason. Because Kuhn regards logic as essentially a tool of justification, he takes it to be powerless in the face of conflicting premises, as might occur, for instance, with his (and Popper's) acceptance that there can be no theory-neutral observation language. The only alternative to *proof*, Kuhn asserts, is "persuasion."

This justificationist view of argumentation also underlies Kuhn's critique of falsificationism. Kuhn likened his use of anomalies in his theory of science to Popper's theory of hypothesis falsification by way of negative outcomes in empirical tests. However, Kuhn writes that (ibid, p. 146):

> Clearly, the role thus attributed to falsification is much like the one this essay assigns to anomalous experiences, i.e., to experiences that, by evoking crisis, prepare the way for a new theory. Nevertheless, anomalous experiences may not be identified with falsifying ones. Indeed, I doubt that the latter exist.

Moreover, in the introduction, Kuhn asserts his hope to "replace the confirmation or falsification procedures made familiar by our usual image of science" (p. 8), and later, in chapter 8, he writes: "No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature." Kuhn reveals the reason for his misgivings about falsification in his "Logic of Discovery or Psychology of Research?" (1970, p. 14):

> Sir Karl is not, of course, a naive falsificationist. He knows all that has just been said and has emphasized it from the beginning of his career. Very early in his *Logic of Scientific Discovery,* for example, he writes: 'In point of fact, no conclusive disproof of a theory can ever be produced; for it is always possible to say that the experimental results are not reliable or that the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding. Statements

> like these display one more parallel between Sir Karl's view of science and my own, but what we make of them could scarcely be more different. For my view they are fundamental, both as evidence and as source. For Sir Karl's, in contrast, they are an essential qualification which threatens the integrity of his basic position. Having barred conclusive disproof, he has provided no substitute for it, and the relation he does employ remains that of logical falsification. Though he is not a naive falsificationist, Sir Karl may, I suggest, legitimately be treated as one.

Thus, the reason Kuhn makes this remarkable claim is that, because observation is theory-laden, no falsification can be *conclusive*. "Falsification" and "refutation" are, according to Kuhn (ibid, p. 13):

> ...antonyms of 'proof'. They are drawn principally from logic and from formal mathematics; the chains of argument to which they apply end with a 'Q.E.D.'; invoking these terms implies the ability to compel assent from any member of the relevant professional community. No member of this audience, however, still needs to be told that, where a whole theory or often even a scientific law is at stake, arguments are seldom so apodictic. All experiments can be challenged, either as to their relevance or their accuracy.

"What is falsification", Kuhn asks, "if it is not conclusive disproof?" (Ibid, p. 15). Thus, for Kuhn the use of logic is confined exclusively to *conclusive* proofs and disproofs, ones which can "compel assent." Since an empirical falsification can never do this, as Popper admits,[10] the "integrity of his basic position" is compromised. Popper is not, in fact, committed to conclusive falsificationism, but, Kuhn suggests, he *should* be. This response perfectly exemplifies, I think, the foundationalist theory of rationality, "according to which it is irrational to accept a belief that has not been justified, and obligatory to accept one that has" (Gattei, 2009, p. 76).

Kuhn's criticism, however, stands and falls with the justificationist theory of rationality-cum-demarcation. For, if logical falsification, instead of compelling us to reject a hypothesis, presents us with a *choice*, the situation is changed completely. Kuhn is quite correct that any justificatory argument will be question-begging. He is incorrect to see this as the primary role of argument in theory adjudication. For, as Notturno notes (2000, p. 232), "the acceptance of the basic statements that corroborate or falsify a theory is not logically or psychologically compelled. Such statements are not justified, but accepted or rejected as a result of free decisions that may be reevaluated and revised at any time... Logic can tell us which statements follow from or contradict which, but it cannot force us to accept or reject a theory." This conception of logic, as the organon, not of proof or disproof, but of rational criticism, will be explored further in the final chapter. What is plain however, is that Kuhn's criticism of Popper, and his theory of irrationalistic and relativistic paradigms shifts, only have plausibility on the assumption of justificationism. The choice Kuhn presents, between demonstrable or justified knowledge and relativistic ultimate commitment, is a false dichotomy.

6.4 The Justificationist Rejection of Truth

Perhaps the most radical consequence of Kuhn's justificationism, however, is his rejection of truth as playing any role whatsoever within scientific inquiry and theory adjudication. Indeed, the term "truth" barely appears in the *Structure* at all, as Kuhn himself notes ([1962] 1970 p.170):

> It is now time to notice that until the last very few pages the term 'truth' had entered this essay only in a quotation from Francis Bacon. And even in those pages it entered only as a source for the scientist's conviction that incompatible rules for doing science cannot coexist except during revolutions when the profession's main task is to eliminate all sets but one.

The traditional conception of truth must, according to Kuhn, be replaced by "something like a redundancy theory of truth" (1991, p. 99). For scientific progress cannot be characterised in terms of truth or verisimilitude. According to Kuhn, we "have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth" ([1962] 1970 p. 170). He continues (ibid, pp. 170-171):

> The developmental process described in this essay has been a process of evolution *from* primitive beginnings—a process whose successive stages are characterized by an increasingly detailed and refined understanding of nature. But nothing that has been or will be said makes it a process of evolution *toward* anything. Inevitably that lacuna will have disturbed many readers. We are all deeply accustomed to seeing science as the one enterprise that draws constantly

> nearer to some goal set by nature in advance. But need there be any such goal? Can we not account for both science's existence and its success in terms of evolution from the community's state of knowledge at any given time?

Kuhn continues this attack on truth in the 1969 "Postscript" to the Structure ([1962] 1970 p. 206):

> A scientific theory is usually felt to be better than its predecessors not only in the sense that it is a better instrument for discovering and solving puzzles but also because it is somehow a better representation of what nature is really like. One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. Apparently generalizations like that refer not to the puzzle-solutions and the concrete predictions derived from a theory but rather to its ontology, to the match, that is, between the entities with which the theory populates nature and what is "really there." Perhaps there is some other way of salvaging the notion of "truth" for application to whole theories, but this one will not do.

This rejection of truth can best be understood, I think, as an instance of Miller's schema:

$$\text{scepticism} + \text{justificationism} \rightarrow \text{relativism}$$

Indeed, Kuhn's starting point is scepticism about the possibility of an objective criterion of truth or justification in the logical positivist style; as he asserts, "there is no standard higher than the assent of the relevant community" (1962, p. 94). Yet this is only to accept dogmatism rather than to reject justificationism. His residual justificationism can be discerned in his view that lexicons (his successor concept to paradigms) "are not... the sorts of things that can be true or false" (1993, p. 244). That is, it is not possible to speak of truth *simpliciter*, but only truth *relative* to a particular paradigm a lexicon. Paradigms cannot be true or false, for they determine the truth and falsity of other statements.

This view is a direct continuation, not only of Wittgenstein's "forms of life",[11] but also Carnap's theory of linguistic frameworks and his distinction between decidable "internal questions" and "external questions" as articulated in his 1950 essay, "Empiricism, Semantics and Ontology". Internal questions, in Carnap's theory, presuppose a linguistic framework and are completely decidable relative to that framework. External questions, on the other hand, are undecidable precisely because they transcend the framework. As such, they can be neither true nor false, but can only be appraised pragmatically. As Paul O'Grady notes (2002, pp. 14-15),

> Such questions are improper, because the choice of framework is pragmatically decided on. If it is useful to speak of numbers, then such a framework will be used; if it is useful to speak of properties, then that framework will be used. The only appropriate external questions are ones about the expediency of using a framework or not. Questions about trying to relate the frameworks to some anterior reality are deflated to the practical question of choice of framework.

Such a doctrine is "radically relativistic in its implications... Carnap's linguistic frameworks offer much scope for relativistically inclined philosophers" (ibid). The parallels with Kuhn's theory are striking, as Notturno has noted (2000, p.245)[12]:

> Their idioms are entirely different. But Kuhn's account of paradigms, his distinction between 'normal' and 'revolutionary' science, and his idea that competing paradigms are neither true nor false is a repetition of Carnap's account of linguistic frameworks, his distinction between their 'internal' and 'external' questions, and his view that the answers to external questions are neither true nor false. Far from repudiating verificationism, Kuhn's own procedure, like Carnap's, was to reject, as meaningless, concepts—such as truth—whose meaning could not be reduced to the method of their verification.

Notturno goes on to quip, "Kuhn's institutional picture is, quite literally, logical positivism without the logic"[13] (Ibid).

The point here is that Kuhn has retained the verificationist doctrine that "a concept is vacuous if there is no criterion for its application." This formulation, which appeared in an article in volume 2 of the *Encyclopedia of Philosophy* (1967), expresses, according to Popper, "the very heart of positivistic tendencies" (1972, p. 321). And, if it is accepted as an adequate formulation of positivism, then, Popper asserts, "positivism is refuted by the modern development of logic, and especially by Tarski's theory of truth, which contains the *theorem:* for sufficiently rich languages, there can be no general criterion of truth" (ibid). The justificationist theory that underlies Kuhn's relativisation of truth to paradigms—that is concept is logically legitimate only if a criterion exists which enables us to determine whether or not an object falls under that notion—is itself logically untenable.

Kuhn's relativism is thus a direct result of the justificationist conflation of truth with justified truth, or the concept of truth with a criterion of truth. This conflation is quite plain in the following quotation (1992, p. 115), where Kuhn rejects the notion of scientific progress in terms of truth, as lacking

> ...a fixed, rigid Archimedean platform [that] could supply a base from which to measure the distance between current belief and true belief. In the absence of that platform, it's hard to imagine what such a measurement would be, what the phrase 'closer to the truth' can mean.

Progress towards the truth is meaningless for Kuhn, because "there is no theory-independent way to reconstruct phrases like "really there"; the notion of a match between the ontology of a theory and its "real" counterpart in nature... seems to me illusive in principle" ([1962] 1970, p. 206). And, since talk of truth is meaningless when discussing paradigms, so too is talk of *mistakes*. "A mistake is made, or is committed," asserts Kuhn, when an individual "has failed to obey some established rule of logic, or of language, or of the relations between one of these and experience. The individual can learn from his mistake only because the group whose practice embodies these rules can isolate the individual's failure in applying them" (1970, p. 11). Accordingly, mistakes can only occur *within* a paradigm; comparative appraisals *between* paradigms, such as between Ptolemaic and Copernican astronomy, are impossible (1970, pp. 11-12):

> In our view, then, no mistake was made in arriving at the Ptolemaic system, and it is therefore difficult for me to understand what Sir Karl has in mind when he calls that system, or any other out-of-date theory, a mistake. At most one may wish to say that a theory which was not previously a mistake has become one or that a scientist has made the mistake of clinging to a theory for too long... I am not sure a mistake has been made, at least not a mistake to learn from.

Kuhn's justificationism means he can only recognise mistakes when there is a *decision procedure* to determine when a mistake has been made. Outside normal science, such talk of mistakes is meaningless, because there is no way to *verify* that a mistake has been made. The problem of rational theory choice, on this account, remains unanswered. As Alan Musgrave (2004, p. 16) has observed, weakening the concept of truth is not answering Humean scepticism—it is changing the subject.

6.5 Conclusion

This discussion of Thomas Kuhn's theory of science in *The Structure of Scientific Revolutions* is just a single example of the relativistic-cum-justificationist approach to scientific knowledge, but it has, I think, broad applicability. Wittgenstein's theory that science is grounded in a form of life; Carnap's theory that it is grounded in a linguistic framework; Kuhn's theory that it is grounded in the commitment to a paradigm; Rorty's theory that it is grounded in the solidarity of specific communities—each of these approaches to human knowledge is underwritten by justificationism, and consequently, all, to some extent or other, by design or contrary to the author's intentions, lead to subjectivist relativism and the denial of objective truth.[14] Each leads, it seems to me, to the abandonment of the problem of rational theory adjudication.

---

[1] My focus in this chapter is specifically on the *relativistic consequences of justificationism* in Kuhn's most influential "image of science"—that is, his theory of science in *The Structure of Scientific Revolutions*. However, it must be stressed that Kuhn's views on science were not static, and even in the *Structure,* which I argue has marked relativistic implications, there are many useful and interesting ideas. Specifically, Kuhn's theory of paradigms, at least on one reading, has definite parallels with Popper's theory of "metaphysical research programs", and his later focus on theoretical virtues ("accuracy, consistency, scope, simplicity and fruitfulness" (1977, p. 321)) is comparable, though not identical, with Popper's desiderata for theoretical progress (see sections 7.7 and 7.8 below).

[2] This demise, Friedman asserts (1991, p.1), "took place sometime between the publication of W.V.O. Quine's "Two Dogmas of Empiricism" (1951), and that of Thomas Kuhn's *The Structure of Scientific Revolutions* (1962)."

[3] As Kuhn asserted in July 1965, at the International Colloquium in the Philosophy of Science, held at Bedford College in Regent's Park, London, to an audience that included Popper, "neither Sir Karl nor I is an inductivist. We do not believe that there are rules for inducing correct theories from facts, or even that theories, correct or incorrect, are induced at all. Instead we view them as imaginative posits, invented in one piece for application to nature" (1970, p. 12).

[4] Wittgenstein's scepticism about justification has been noted by Fogelin (1994, p. 202): "[t]hough he clearly rejects Cartesian skepticism, it seems to me that Wittgenstein is clearly committed to the version of skepticism that I have labeled Pyrrhonian skepticism." Wittgenstein, however, has no positive theory

of demarcation, and this, I assert, is tantamount to relativism.

[5] Quoted in Bartley (1987, p. 209)

[6] Indeed, Wittgenstein had already used the term "paradigm" in a similar sense to Kuhn in his (1953), part 1, § 50 and § 54.

[7] "Scientists," Kuhn asserts, (1977, p. 10) "sometimes correct bits of each other's work, but the man who makes a career of piecemeal criticism is ostracized by the profession."

[8] This ambiguity was perhaps first noted by Feyerabend (1995, pp. 355, 360, and 366–368) and has also been noted by Brendan Larvor (2003).

[9] A similar opinion has been voiced by Dudley Shapere (see Earman, 1993 p. 10).

[10] This point is made in several places in *The Logic of Scientific Discovery* (e.g. 1959, p. 28)—"In point of fact, no conclusive disproof of a theory can ever be produced; for it is always possible to say that the experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding... If you insist on strict proof (or strict disproof) in the empirical sciences, you will never benefit from experience, and never learn from it how wrong you are."

[11] Wittgenstein's followers have also displayed a strong tendency to lapse into relativism, as Paul O'Grady notes (2002, p. 95): "Just as Wittgenstein resisted the view that there is a single "transcendental" language game that governs all others, so some systematic philosophers after Wittgenstein have argued for a multiplicity of standards of correctness, and not a single overall dominant one. The most famous application of this idea is the so-called "Wittgensteinian Fideist" school in philosophy of religion, which claims that religious language games have their own internal criteria of correctness and that it is illegitimate to critique them using other external standards, such as those of science.

[12] There are also close parallels with Putnam's "internal realism": see Putnam (1978), part 4; (1981), chaps. 3 & 5–7.

[13] Indeed, it is worth noting in this regard that Kuhn's *Structure* was explicitly endorsed by Carnap. See Carnap's letters to Kuhn, published in Reisch (1991); see also Irzik and Grünberg (1995); Friedman (1991); (1992); (1993); and (2001). Richardson (2007, pp. 354-355) also notes that "none of the most important logical empiricists wrote early reviews of Kuhn's book in which they expressed deep disagreement with Kuhn; this contrasts importantly with the reaction of Popper and his followers, who famously argued with Kuhn quite publicly in 1965."

[14] For a treatment of the relativistic consequences of Wittgenstein's later philosophy, see Peter Munz's *Our Knowledge of the Growth of Knowledge* (1985), along with his more sympathetic consideration of the same philosophy in *Beyond Wittgenstein's Poker* (2004). Munz's essay "Philosophy and the Mirror of Rorty" (1986) is also a useful discussion of Rorty's relativism and its roots to justificationism.

Chapter Seven: Sketch of a Deductivist Alternative

"The aim of science is not to open the door to infinite wisdom, but to set a limit to infinite error."

Bertolt Brecht, *Life of Galileo*

7.0 Introduction

Most contemporary approaches to knowledge, which are not simply irrationalist, set forth as the working mechanism of science something vaguely referred to as "inductive logic". Critical rationalism offers an alternative which is entirely deductive. Unlike the axiomatic-deductive systems of traditional rationalism, deductivism here is not a thesis concerning how theories should be *framed*; rather it describes the method by which theories should be critically examined and exposed to *error elimination*. In its examination and classification of the truth claims of theoretical science no reliance whatsoever is made on justificationist procedures. As such, it offers a genuine replacement for justificationist theories of knowledge and rationality. Most especially, for demarcational purposes, critical rationalism gives what justificationism promised but could not deliver.

However, two conditions must be met if critical rationalism is to constitute a viable successor to justificationist theories of knowledge. Firstly, it must be genuinely non-justificationist. That is, it must adequately respond to the charge (Salmon, 1966, 1968b; Good, 1975; Putnam, 1974; O'Hear, 1980, Howson, 2000; McGinn 2002, p. 48, Velupillai 2008, p. 145) that it illicitly, (somewhere, somehow), relies on induction. Secondly it must actually allow *rational* demarcation; that is, it must, despite suggestions to the contrary (Lakatos 1974; O'Hear 1980; Newton-Smith, 1981; Stove, 1982), avoid relativistic irrationalism. If both charges can be answered we shall be able to reconcile the evident fact of the progress of science with the purported challenges to rationality raised by sceptical criticisms of justification. That is, we will have illustrated the *possibility* of rational demarcation, *without justification*.

7.1 Does Critical Rationalism Presuppose Induction?

The most popular objection to critical rationalism has been that, at some stage or other in its methodological procedure, it illicitly smuggles in inductive assumptions; indeed, it *must* do so, to have any predictive import. And these assumptions, once exposed, will reveal critical rationalism to be just as vulnerable to Humean objections as any inductivist theory of science. Anthony O'Hear's comment (1980, p. 57) is representative of this line of criticism: "inductive reasoning, removed from one part of the picture, crops up in another." A more recent example is Alexander Bird's charge (1998, p.180) that "[it] is a feature of Popper's philosophy… [that] when the going gets tough, induction is quietly called upon to help out." And Colin Howson (2000, p. 100) asserts that Popper "did advance a view inductivist in everything but name." Various candidates for a hidden inductive assumption have been canvassed, but the most popular centre upon Popper's notion of corroboration, or upon the process of theory testing more generally. As Miller notes (2009, p. 3): "[f]or 75 years the principal line of criticism has been to identify in scientific activity places where guesses have to be made, and either to declare these guesses to be the conclusions of 'inductive inferences' or to castigate them for being unjustified." Let us examine the various possibilities.[1]

The most elemental place where an inductive principle might appear is as a presupposition of scientific inquiry itself. That is, the notion is that scientific method "presupposes *the immutability of natural processes*, or the 'principle of the uniformity of nature'" (Popper 1959, p. 250). Such a principle, it is held, is *necessary* if scientific investigation is even to get off the ground. This objection, made by, for example, Trusted (1979, p. 63) and O'Hear, (1980, pp. 57f),[2] and more recently by (Eintalu 2001, note 93; Trigg 2001, p. 36) is, however, unsuccessful. For such an assumption, if construed as a precondition of inquiry, is inherently *dogmatic*—that is, since it is held to be a *presupposition* of inquiry it cannot be *falsified* by inquiry. It rules out, *a priori*, the possibility that there are no immutable regularities in nature. Popper's suggestion, that such an unfalsifiable metaphysical principle should be replaced by the methodological rule to search for spatio-temporally invariant laws (1959, § 79), is thus undoubtedly preferable, not only from the point of view of avoiding dogmatism, but also from considerations of parsimony (such laws combine high empirical content with high testability). Accordingly, such an unfalsifiable presupposition is completely *unnecessary* for inquiry, from either a logical or a psychological perspective. Of course, metaphysical hypotheses play an important role in science, and all empirical theories have various unfalsifiable statements as consequences—whether logical truths, pure existential statements or metaphysical statements, such as the "all-and-some statements" explored by Watkins in his (1958). Yet these gain entry into empirical science *only in virtue* of being derivable from a falsifiable theory. No metaphysical assumption concerning the immutability of natural processes is necessary for a method of conjectures and refutations. As Miller writes (1994, p. 27), "[s]cientific hypotheses propose order for the world; they do not presuppose it." In other words, purely hypothetical scientific theories already *assert* that the future will be like the past in some specific respect—no *additional* inductive assumption of regularity is necessary.

The next place where an inductive assumption has been suspected is as being inherent in scientific hypotheses themselves. Wesley Salmon, for instance, had argued that "[i]f science consists solely of observation statements and deductive inferences... [then science]... is barren" (1966, p. 24).[3] In a similar vein, Salmon also stated that (ibid, p. 20):

> A scientific theory that merely summarized what had already been observed would not deserve to be called a theory. If scientific inference were not ampliative, science would be useless for prediction, postdiction, and explanation. The highly general results that are the pride of theoretical science would be impossible if scientific inference were not ampliative... If science is to amount to more than a mere collection of our observations and various reformulations thereof, it must embody some other methods besides observation and deduction.

In other words, Salmon's view here is that any *advancement* of universal hypotheses (which have, when supplemented with initial conditions, predictive import) is said to be inductive. This criticism, however, seems to confuse critical rationalism with some form of strict positivism, where all knowledge must be obtained, exclusively, from either observation or by deductive inference. Yet the reply to this is plain: scientific hypotheses, which have predictive import, are *conjectures*. No inductive inference is needed to make a conjecture or to propose a hypothesis, and no inductive rules are involved.

Yet another criticism holds that "rules of induction are needed even for the classification of test statements as true or false, that the acceptance of basic statements is unavoidably an inductive step" (Miller, 1994, p. 16). That is, according to this criticism, induction is required in order to *accept* a test statement of the type that figures in an empirical falsification. Proponents of this view include O'Hear (1980, p. 75), and Newton-Smith (1980, p. 152; 1981, p. 64), who states that "there can be no grounded falsification without induction" and that "rejection of theories for Popper… is a matter of mob psychology." Without induction, so this criticism asserts, empirical falsifications are arbitrary. This criticism has been restated more recently by Graham MacDonald (2004, p. 159), who has argued that Popper's rejection of "any epistemological role for experience, allowing it only the role of motivating, or causing, our beliefs… invites the criticism that falsification is only relative, and ultimately unjustified."

Interestingly, essentially the same objection was made by Popper against Otto Neurath's 1933 theory of protocol sentences. In contrast to what Popper regarded as Carnap's infallibilist theory of observation statements,[4] Popper wrote that (1959, pp. 78-9):

> Neurath's view that protocol sentences are not inviolable represents, in my opinion, a notable advance… It is a step in the right direction; but it leads nowhere if it is not followed up by another step: we need a set of rules to limit the arbitrariness of 'deleting' (or else 'accepting') a protocol sentence. Neurath fails to give any such rules and thus unwittingly throws empiricism overboard. For without such rules, empirical statements are no longer distinguished from any other sort of statements. Every system becomes defensible if one is allowed (as everybody is, in Neurath's view) simply to 'delete' a protocol sentence if it is inconvenient. In this way one could not only rescue any system, in the manner of conventionalism; but given a good supply of protocol sentences, one could even confirm it, by the testimony of witnesses who have testified, or protocolled, what they have seen and heard. Neurath avoids one form of dogmatism, yet he paves the way for any arbitrary system to set itself up as 'empirical science'.

Popper presents his solution to this problem in § 29 of *The Logic of Scientific Discovery.* There he denies the possibility of conclusive falsifications: "considered from a logical point of view," Popper writes (1959, p. 86), "the situation is never such that it compels us to stop at this particular basic statement rather than at that, or else give up the test altogether." Thus, no observation statement is completely *determined* or objectively *justified* by the observations or perceptual experiences themselves—such a view leads to the psychologism that Popper had rejected in Carnap's account. There is indeed always a conventional decision involved in the acceptance of an observation report as *correctly* reporting what has been observed—that is, as correctly reporting an empirical *fact.* "Every test of a theory," Popper writes (ibid), "whether resulting in its corroboration or falsification, must stop at some basic statement or other which we *decide to accept.* If we do not come to any decision, and do not accept some basic statement or other, then the test will have led nowhere."[5]

However, the acceptance of basic statements is not therefore *wholly* conventional or arbitrary. This can be seen in that *both* elements are necessary for any empirical inquiry—observation *and* a decision to accept a test statement. The latter, is both *motivated* and *constrained* by the former—by observation, but it is not determined by it. Therein lies the crucial *empirical* component of scientific inquiry; neither component alone is sufficient. Hence Larry Briskman's crisp summary of Popper's account of the empirical basis (quoted in Miller 1994, p. 30): *"Look before you leap!"*

Moreover, the accepted test statements are always *themselves* to be submitted to tests if they are considered problematic or are suspected of being false. Thus Popper writes that (1959, p. 86), "any basic statement can again in its turn be subjected to tests, using as a touchstone any of the basic statements which can be deduced from it with the help of some theory, either the one under test, or another." If investigators cannot reach agreement about the acceptance or rejection of a certain statement "they will simply continue with the tests, or else start them all over again. If this too leads to no result, then we might say that the statements in question were not inter-subjectively testable, or that we were not, after all, dealing with observable events." (Ibid.) In other words, the acceptance of test statements is not arbitrary simply because we demand that test statements be *true*. Crucially, the act of acceptance does not *constitute* the truth of a test statement—it does not justify that the statement is true. However, if no criticism can be produced to indicate that an accepted basic statement is false any allegations of irrationality are themselves arbitrary, being seemingly neither motivated by experience nor by a clash of the observation report with another accepted statement. Thus there is no inductive inference involved in the acceptance of a basic statement, and nor is one necessary. As Popper writes (1959, pp. 87-88):

> The basic statements at which we stop, which we decide to accept as satisfactory, and as sufficiently tested, have admittedly the character of *dogmas*, but only in so far as we may desist from justifying them by further arguments (or by further tests). But this kind of dogmatism is innocuous since, should the need arise, these statements can easily be tested further. I admit that this too makes the chain of deduction in principle infinite. But this kind of *'infinite regress'* is also innocuous since in our theory there is no question of trying to prove any statements by means of it. And finally, as to *psychologism*: I admit, again, that the decision to accept a basic statement, and to be satisfied with it, is causally connected with our experiences—especially with our *perceptual experiences*. But we do not attempt to *justify* basic statements by these experiences. Experiences can *motivate a decision*, and hence an acceptance or a rejection of a statement, but a basic statement cannot be *justified* by them—no more than by thumping the table.

Thus, MacDonald's criticism that falsifying empirical statements are unjustified on Popper's account, and hence must be supplemented by "a reason-giving role of experience" (2004, p. 60), illicitly assumes that the acceptance of a basic statement must be *justified* in order to be *rational*, rather than, as critical rationalism asserts, simply being capable of standing up to criticism.[6] Accordingly, as Salmon correctly notes (1966, p. 23): "Popper's basic statements must in the last analysis be considered hypotheses, falsifiable and subject to test like other scientific hypotheses." Justification plays no role.

Another variant of the "hidden inductive assumption" objection maintains that the process of *testing* hypotheses involves an inductive inference. For, if a hypothesis is rejected as falsified after failing a test, this presupposes that it will fail future instances of this test also, and hence the making of an inductive inference is obligatory. Notable advocates of this line of criticism are Ayer (1956, p. 74), Warnock (1960), Levison (1974, pp. 328- 330), Hesse (1974, p. 95), and O'Hear (1980, p. 45). To quote Hesse's formulation of this objection:

> ...one past falsification of a generalization does not imply that the generalization is false in

future instances. To assume that it will be falsified in similar circumstances is to make an inductive assumption, and without this assumption there is no reason why we should not continue to rely upon all falsified generalizations.

O'Hear's exposition of this objection is also admirably clear:

> From an anti-inductivist standpoint, however, the fact that a theory has survived a certain type of test on occasions can give us no reason to suppose that it is more likely to survive another test of the same type on the next occasion than it was on the first occasion of undergoing a test of that type.

Colin McGinn has made essentially the same protestation more recently (2002, p. 48): "[w]e have to be able to infer that if a falsifying result has been found in a given experiment it will be found in future experiments;... this is clearly an inductive inference."

What all such objections overlook, however, is that a test statement that is inconsistent with a spatio-temporally invariant hypothesis *logically entails* that that hypothesis is false, (provided, of course, that the test statement, along with auxiliary hypotheses, are accepted as true). It is *not necessary* to verify that all future tests will have the same negative result to falsify a hypothesis of the form "All A are B". This is simply an instance of the deductive *modus tollens*—no inductive inference is involved. Moreover, the assumption that the falsifying hypothesis will continue to hold in future tests is itself a *conjecture*, which may itself be wrong. It is precisely for this reason that Popper requires that tests statements be reproducible (1959, p. 66):

> We say that a theory is falsified only if we have accepted basic statements which contradict it... This condition is necessary, but not sufficient; for we have seen that non-reproducible single occurrences are of no significance to science. Thus a few stray basic statements contradicting a theory will hardly induce us to reject it as falsified. We shall take it as falsified only if we discover a *reproducible effect* which refutes the theory. In other words, we only accept the falsification if a low-level empirical hypothesis which describes such an effect is proposed and corroborated.

Yet the hypothesis that an effect is reproducible is itself conjectural; and, needless to say, may even be overturned—the so-called reproducible effect may only be *apparent*. In consequence, no inductive inference is involved here either.

We come now to the aspect of Popper's account which has prompted the most objections: his theory of corroboration.[7] As, Alan Musgrave, a former student of Popper, writes (2004, p. 23):

> Here is the Achilles' heel where induction is smuggled in! Popper must be assuming that predictions from well-corroborated hypotheses will be true, while predictions from refuted hypotheses will be false. Or he must be assuming that predictions from well-corroborated hypotheses are more likely to be true (more probable) than predictions from refuted ones. Either way, induction is smuggled in. His 'degrees of corroboration' were supposed to be backward-looking reports on past successes and failures. Backward-looking reports say nothing about future performance. So Popper must be smuggling in an inductive principle linking past success to future performance, he must be assuming that corroboration is a guide to truth or high probability. Without this assumption, he must endorse Humean irrationalism about all evidence-transcending beliefs.

In other words, some procedure of inductive reasoning or extrapolation is needed, these critics assert,[8] to justify the supposition that an experiment can be successfully repeated—that it will *continue* to be corroborated—and this is precisely what Popper's account of corroboration entails. Salmon, to take one example, writes (1968, pp. 26-28):

> Corroboration is, I think, a nondemonstrative kind of inference. It is a way for providing for the acceptance of hypotheses even though the content of these hypotheses goes far beyond that of basic statements. *Modus tollens* without corroboration is empty; *modus tollens* with corroboration is induction.

This criticism has, I think, more force than the previous objections, since Popper's various presentations of the role of corroboration have not always been satisfactory, and have thus, to some extent, encouraged such misunderstandings. For instance, such formulations as "...the doctrine that *degree of corroboration or acceptability cannot be a probability* [is] one of the more interesting findings of the philosophy of knowledge" (1959, p. 394) certainly seems consistent with Salmon's allegation, as does the statement, "my 'corroboration' and Fisher's likelihood are intended to measure... acceptability" (ibid, p. 388). For, if purportedly summary reports of critical discussion can licence the acceptability of a temporally universal theory as true or truth-like, it seems that some process of inductive inference must be invoked.

Musgrave's own (attempted) solution in this regard is to accept an "epistemic inductive principle", whereby a high degree of corroboration of a hypothesis certifies the reasonableness of a *belief-act* as opposed to the reasonableness (that is, the truth or probability) of a *belief-content* (2004). However, such a return to justificationism, if only on the methodological level, seems to me quite unnecessary. For the above quotations from Popper, which seems to licence the inference from the corroboration of a hypothesis to its acceptability, are not at all representative of the bulk of his expositions of this concept. Corroboration, properly understood, is indeed an appraisal, but *only* of the degree to which a hypothesis has hitherto stood up to tests, along with an evaluation of the severity of those tests (relative to background knowledge). It has *no* predictive import, and is hence not inductive. As Popper stated in his (1959, p. 264):

> The appraisal of the corroboration is not a hypothesis, but can be derived if we are given the theory as well as the accepted basic statements. It asserts the fact that these basic statements do not contradict the theory, and it does this with due regard to the degree of testability of the theory, and to the severity of the tests to which the theory has been subjected, up to a stated period of time.

In other words, corroboration has no *epistemological* significance—it is not needed for the acceptance of a hypothesis, and the fact that a hypothesis has passed a test provides no reason for supposing that it will pass a repetition of that test. Instead, it is *falsifications* which play the key epistemological role. For it is only from an empirical falsification that we can learn from experience; corroborations are completely unnecessary in this regard.[9] We learn from our errors, and by repeatedly positing new hypotheses we may hope to approximate the truth incrementally.[10] The chief import of Popper's concept of corroboration is merely that it embodies the *methodological* exhortation that we should submit our hypotheses to *severe tests*; it is not to be understood in any verificationist sense. Accordingly it should be seen as a appraisal of the critical efforts of the *investigators*, not as a measure of the truth or probability of a hypothesis.[11]

Of course, if science is to be *successful*, some theories must be corroborated, and it is corroborations which illustrate a theory's explanatory content. But again, it is testing that is fundamental to learning from experience and hence to the growth of empirical knowledge. As Miller writes (1994, pp. 120-121), "Corroboration is doubtless needed if science is to exist, for if no theory were ever corroborated there would be no science, but it makes no contribution to the growth, or to the progress, of science... Popper's famous third requirement has to be seen as embodying not "a whiff of verificationism" (1963, p. 248, note 31) but what might be called a whiff of verisimilitudinism." In summary, corroboration reports are entirely analytic; there is no inductive inference involved in assessing the degree to which a hypotheses has hitherto been critically examined. The best corroborated theory, moreover, is not necessarily the *best* theory (the one, that is, which best approximates the truth).[12] However corroboration is understood, it is not a measure of acceptability or rational belief.[13]

A related objection concerns Popper's discussion of the severity of tests, and in particular his assertion that the severity of tests diminishes over time. Thus, Popper wrote in his (1963, p. 240):

> A serious empirical test always consists in the attempt to find a refutation, a counter example. In the search for a counter example we have to use our background knowledge; for we always try to refute first the *most risky* predictions... Now if a theory stands up to many such tests, then, owing to the incorporation of the results of our tests into background knowledge, there may be, after a time no places left where (in the light of our new background knowledge) counter examples can be expected to occur. But this means that the degree of severity of our test declines. This is also the reason why an often repeated test will no longer be considered as significant or severe: there is something like a law of diminishing returns from repeated tests.

According to both O'Hear (1980, p. 45) and Hesse (1975, p. 95), this "law of diminishing returns" can only be explained by employing inductive assumptions. Thus, Hesse writes that "it is impossible even to state [Popper's view] without making some inductive assumptions. For example, it is not clear that the notion of a 'severe test' is free of such assumptions. Does this mean... 'tests which we should expect on the basis of past experience to refute this particular generalization'? [This]... is certainly an appeal to induction." O'Hear agrees, arguing that in "determining test severity background knowledge does appear on any reading to be used inductively." However, this criticism rests on the mistaken view that the severity of tests is inductively inferred from *past experience*, instead of being deductively assessed relative to *background knowledge*. This latter posit contains generalisations and assumptions of numerous kinds; it is not limited to reports of previous tests. Once a test is repeated to check if the observed outcome is reproducible, this effect is (conjecturally) added to our background knowledge, and hence the test, relative to the original hypothesis, loses its severity. And if anyone were to consider the reproducibility of the experimental outcome dubious, they can continue testing. Thus, nowhere is an inductive inference made in assessing the severity of a test, and nowhere is it necessary.

Consequently, it seems that critical rationalism is, in fact, free of *any* justificationist assumptions. But need it make them? Is rational demarcation possible without justification?

7.2 Does Critical Rationalism Lead to Irrationalism?

Various writers have alleged that Popper's theory of science is covertly irrationalist;[14] the most trenchant (and certainly the most entertaining) proponent of this view being, no doubt, the Australian philosopher David Stove (1927-1994). Stove expressed this opinion most stridently in his book *Popper and After: Four Modern Irrationalists* (1982, later republished as *Scientific Irrationalism: Origins of a Postmodern Cult*). However, Stove's major thesis, that Popper inaugurated the irrationalist and relativistic philosophies of science associated with Kuhn and Feyerabend, is wide of the mark. While Stove is quite correct to object to Kuhn's and Feyerabend's abandonment of the problem of rational theory choice, he is quite incorrect to impute such a shortcoming to critical rationalism. As I have explained in chapter 6, the relativistic aspects of modern philosophy of science are almost wholly a consequence of justificationism, and can only be avoided by the total repudiation of this doctrine. Let us, though, examine Stove's allegation in more detail.

The (alleged) irrationalists of Stove's title are Popper, Imre Lakatos, Thomas Kuhn, and Paul Feyerabend, yet the greater part of his criticism is reserved for the latter two philosophers. As I mentioned in chapter 1, Stove's primary critique of Popper himself is merely that he has illicitly misused the word "knowledge"; for Stove (2004, p. 48) "'[k]nowledge', of course, is a success-word: you can know only what is true." Less presumptuously, and more intellectually serious, is Stove's critique of Kuhn's *Structure* *of Scientific Revolutions*. Kuhn propounded in that influential book, Stove tells us (ibid, pp. 45-46):

> ... a philosophy of the most uncompromisingly relativist kind. He will not talk himself, or let you talk if he can help it, of truth in science, or (and this galls the Popperites even more) of falsity: he claims he cannot understand that class of talk... As for 'knowledge', 'discovery', 'progress': why, all that, of course, is no more than the language which the partisans of any paradigm will apply to their own activities. It is no more to be taken at face value than is talk about 'reforms' in politics: after all, whenever your bunch gets its way on some political point, you call the result a 'reform'. Or as Kuhn puts it in his more demure way, all such talk is 'paradigm-relative'. There is nothing rational about paradigm-shift in science according to Kuhn. He constantly compares it to what psychologists call 'gestalt-switch': that non-rational process by which a drawing of an ascending flight of stairs is suddenly seen as a descending one, or what just before seemed only some shrubbery is seen as a human face. That kind of thing is what the history of science comes down to, notwithstanding all the Whiggish rhetoric of the past few centuries about progress and enlightenment.

Roger Kimball, in his preface to Stove's posthumous collection of essays *On Enlightenment* (2002, p. ix), also lauds Stove's critique of the Kuhnian response to the demarcation-cum-rationality problem:

> Kuhn took care to deny that he was an irrationalist. But Stove showed that Kuhn's celebrated notion of "paradigm change" provided not an account but a repudiation of scientific development. Kuhn covertly substituted sociology and history for logic, thus winding up with *a picture of science in which progress is illusory and no scientific theory can be said to be better or worse than another* (emphasis added).

Yet Kuhn's relativism did not represent the nadir for Stove; that position he reserved for Feyerabend (2004, p. 46):

> Kuhn in his turn has been outflanked by a philosopher in whom the Jazz Age has finally come to full fruition: P. K. Feyerabend... Feyerabend calls himself a 'Dadaist' and his philosophy 'epistemological anarchism'... He maintains that science knows, and should know, no rules of method, no logic- inductive, deductive, or whatever. And for his slogan he actually chose, and still defends against all comers, Cole Porter's old title 'Anything Goes'.

Where, then, does Popper come into all of this? After all, Popper was by all appearances, Stove tells us, "the arch-enemy of irrationalism"; he had harshly repudiated such relativistic and irrationalist epistemologies. Despite this fact, the chief source underlying the postmodern cult at the heart of modern philosophy of science is, Stove asserts, the critical rationalist rejection of induction (ibid, p. 47):

> Lakatos, Kuhn, and Feyerabend are all philosophers, like Popper himself, in the broad main stream of empiricism: they all agree that we cannot learn anything about the actual universe except by experience. Who was it who taught them, then, that we cannot learn anything about the actual universe even by experience - that is, that induction is worthless? Popper, of course. You have only to put these two propositions together, to reach the conclusion that we cannot learn anything about the actual universe at all.

Thus, it is, says Stove, the rejection of induction that motivates "the distinctive irrationalism that has infected modern philosophy of science from Popper forward", as well as "deductivism", that is "a conviction that the only arguments that [are] really compelling [are] those that [are] valid in the strict logical sense of the term" (Kimball, 2002, p. xi). Is Stove's charge correct? Does a pure deductivism preclude learning from experience, and hence rational empirical demarcation?

7.3 Learning From Experience- A Deductivist Approach

Stove is quite right that Popper taught that induction, understood in any justificatory sense, is worthless. But he decidedly did *not* teach that "we cannot learn anything about the actual universe, even by experience." For Popper was quite emphatic that we *can* learn from experience—we can learn that we are *wrong*.

The logical basis of this approach, as Popper presented it in *The Logic of Scientific Discovery*, can be modelled by the *modus tollens*, also known as the law of contrapositive ($P \rightarrow Q, \neg Q \vdash \neg P$). As Popper noted (1959, p. 19):

> My proposal is based upon an asymmetry between verifiability and falsifiability; an asymmetry which results from the logical form of universal statements. For these are never derivable from singular statements, but can be contradicted by singular statements. Consequently it is possible by means of purely deductive inferences (with the help of the *modus tollens* of classical logic) to argue from the truth of singular statements to the falsity of universal statements.

This form of inference can readily be applied to scientific laws, which are typically universal in form—that is, they are invariant in respect to both space and time. Consider the following universally quantified statement:

$$\forall x \in S,\ P(x)$$

Which states that "all *x* are *P*" (e.g. "all swans are white"). Logically, such a statement can be disproved on the assumption of the truth of its negation:

$$\neg(\forall x \in S,\ P(x))$$

which is in turn equivalent to the existence statement:

$$\exists x \in S,\ \neg P(x)$$

This may be read "There exists an *x* which is not *P*" (e.g. "This is a non-white swan").

Thus, to falsify a universally quantified statement we just need to produce an instance of an $x \in S$ that satisfies $\neg P(x)$. (e.g. a black swan). That is, if a singular statement is shown to validly follow from a universal statement, and we assume that such a singular statement is false, we can deductively conclude that the universal statement is false.

A similar procedure is followed to falsify a conditional statement $P(x) \rightarrow Q(x)$. This statement asserts that for every *x* that makes $P(x)$ true, $Q(x)$ will also be true. The statement can only be false if there is an *x* that makes $P(x)$ true and $Q(x)$ false. In other words, to falsify $P(x) \rightarrow Q(x)$ we need to produce an example of an *x* that makes $P(x)$ true and $Q(x)$ false. In both of the above outlines, the statement is disproved simply by exhibiting an example that shows the statement is not always true—that is, by producing a (conjectural, yet resilient to criticism) counterexample. In the case of hypothesis testing quite generally, we may let *P* denote a theoretical system consisting of theories and initial conditions, and *Q* be some empirical consequence of this system—a test statement, or basic statement in Popper's terminology. Given that *P* entails *Q*, and assuming *Q* is false, we can infer $\neg P$. *P* is thus falsified relative to the observation statement $\neg Q$.

In contrast, there is no empirical method of showing that a universal statement is true—or, to put it differently: there is no inductively valid argument. Therein lies the asymmetry between verifiability and falsifiability. No universal generalisation can be empirically verified, no matter how many evidential statements or auxiliary hypotheses are introduced. (A universal statement may be *derived* if strong enough auxiliary hypotheses are *assumed*, but this would fail to be an empirical verification.)

Of course, such logical truisms are quite trite, and the application to practice requires some additional considerations. For instance, as Popper writes in § 18 of his ([1934] 1959) in reference to what was later to become known as the Quine-Duhem thesis:

> By means of this mode of inference we falsify the whole system (the theory as well as the initial conditions) which was required for the deduction of the statement Q, i.e. of the falsified statement. Thus it cannot be asserted of any one statement of the system that it is, or is not, specifically upset by the falsification. Only if Q is independent of some part of the system can we say that this part is not involved in the falsification [notation modified to match the above examples].

Popper continues in a footnote:

> Thus we cannot at first know which among the various statements of the remaining sub-system *P* ′ (of which *Q* is not independent) we are to blame for the falsity of *Q*; which of these statements we have to alter, and which we should retain... It is often only the scientific instinct of the investigator (influenced, of course, by the results of testing and re-testing) that makes him guess which statements of *P*′ he should regard as innocuous, and which he should regard as being in need of modification. Yet it is worth remembering that it is often the modification of what we are inclined to regard as obviously innocuous (because of its complete agreement with our normal habits of thought) which may produce a decisive advance. A notable example of this is Einstein's modification of the concept of simultaneity.

Popper returned to this point in his (1983, p. 187): "No single hypothesis, it may be said, is falsifiable, because every refutation of a conclusion may hit any single premise of the set of all premises used in deriving the refuted conclusion. The attribution of the falsity to some particular hypothesis that belongs to this set of premises is therefore risky, especially if we consider the great number of assumptions which enter into every experiment."

As these quotations illustrate, Popper recognised that it is possible to "falsify only systems of theories and that any attribution of falsity to any particular statement within such a system is always highly uncertain" (ibid). But in response to this he appeals to the well-established methodological fact that scientists try to devise *crucial* experiments in their investigations. That is, they try to design an experiment which may only falsify one of a pair of competing theories.[15] Although Duhem is quite right to reject crucial experiments if they are thought of as attempted *verifications* of a particular theory, this criticism does not apply, contra Duhem, to *falsifications*. Which premise of the theoretical system we revise in the light of an apparent successful falsification is not logically determined, it is readily admitted: the particular premise in the conjunction of theory, initial conditions, and auxiliary hypotheses, which we single out as responsible for the falsification will itself be *conjectural*.[16] Thus, determining the theoretical significance of an accepted basic statement will generally involve some creative guesswork. But the method of testing, and of experimental design, ensures that it is not *arbitrary* or uncontrolled guesswork—-each candidate hypothesis can generally be tested in a multitude of different ways, so that, to a large extent, auxiliary hypotheses used in the testing can be varied in a controlled manner, hence mitigating the possibility of error elimination being incapacitated by Duhem's logical point.

Moreover, it should be noted that this point does not affect the *logical* status of falsifiability; it simply introduces a methodological complication. While empirical falsifications will always be problematic and uncertain, this is not a criticism of critical rationalism; it is rather one of its key results. As opposed to *actual* falsifications, in practice, the logical criterion of falsifiability—that is, the purely logical relationship between a theory and its class of basic statements—remains unchallenged. That is, whilst falsifications are admittedly *uncertain*, empirical verification is *impossible* in principle. Accordingly, the popular criticism that Duhem's thesis undermines the efficacy of a falsificationist methodology flounders in making an illegitimate demand for *conclusive* falsification; it fails because of its justificationist bias. The decision to attribute the falsification to any particular component of the conjunction of hypotheses from which the prediction was derived is itself *conjectural*; to demand a procedure for *conclusively* determining which hypothesis is at fault is a justificationist imposition that has no place within a critical rationalist methodology. Falsifications are possible, *conclusive* falsifications are not. As such, Stathis Psillos's criticism that "[a]dvocates of falsificationism have not managed to come to terms with the Duhem-Quine thesis" (2007, p. 90) is, to my mind, completely dissolved once justificationist assumptions are abandoned.[17] The take-home message from the Duhem-Quine thesis is simply that the conjecture that a specific hypothesis is responsible for a falsification should itself be testable, and under critical control.[18]

Perhaps it will be best to clear up a related objection to the falsificationist use of the *modus tollens* at this point. Colin Howson raises this objection in his (2000, pp. 99-100), but he has many predecessors. He writes:

> Falsifiability and ease of falsification are red herrings. They led Popper to give undue prominence to universal hypotheses, because these, in the simple world where observation statements are genuinely observation statements, are *modus-tollens*-falsifiable by a single counterinstance. Since the date of the first publication of Popper's principal methodological work, *Logik der Forschung*... a class of statements not falsifiable in this way has assumed a growing importance in science, especially in particle physics and cosmology: these statements are pure existence statements, and some of the currently most important hypotheses are of this type ('Does the Higgs particle exist?'). To claim, as Popper does, that these are not scientific because they are unfalsifiable, is clearly to beg the question. *Modus tollens* is a false god.

We may immediately set aside the allusion that Popper was some kind of naive falsificationist (Howson's comment about "the simple world where observation statements are genuinely observation statements"), for we have already seen that Popper had argued against any such infallibilist interpretation of basic statements—all observation is theory-laden. (Indeed, this fact only serves to *strengthen* the falsificationist position relative to any justificationist rival).[19]

The objection regarding the scientific status of pure existential statements has likewise been answered on numerous occasions. As Popper wrote in his (1983, p. 183; but see also his 1959, §§ 15 & 27, and his 1974b, p. 1038), regarding such purely existential statements as "there exists a perpetual motion machine": "I do not call an isolated purely existential statement 'metaphysical' because it is 'difficult' to verify, but because it is *logically impossible to falsify* it empirically, or to test it." Howson is of course correct that hypotheses about the existence of the Higgs particle belong to empirical science, but this is only because its existence is *a consequence of an* *empirically testable* theory. This may be easily seen by comparing this case to an assertion that "the Biggs particle exists"—a particle that I have just now invented. This existential statement is not part of any testable theory and is completely *unfalsifiable*. Miller elaborates on this point (1997, pp. 69-70), "...falsifiability remains the central consideration even when unrestricted existential statements are in question. 'There exist neutrinos', for example, becomes discussable only when it is rendered into a falsifiable form—as it is by the specification of a [universal] recipe for producing and trapping neutrinos."[20] Stray existential statements are neither verifiable nor falsifiable—they are thus extraneous to empirical science. Existential assertions such as "there exists a Biggs particle" will not earn me a share of a Nobel prize.

More interesting, but also easily answered, is Howson's objection (p. 99) that "Popper [gave] undue prominence to universal hypotheses." The response to this, the reason why scientists in general, and not just Popper's *theory* of science, give prominence to universal hypotheses is simply because of their logical strength, and hence their *explanatory* potential.[21] That is, the primary theoretical task of science is to provide satisfactory *explanations* (in addition to true predictions), and explanations have the form of deductive *derivations*, whose conclusion is the *explicandum* and whose premises consist of the *explicans*. That is, the phenomena or event to be explained is derived from a statement of the explaining laws, auxiliary hypotheses and initial conditions. It is the logical strength of universal statements or law statements that allow them to be *independently testable* (more on this below). Thus, as Popper notes (1972, p. 351), explanations require (at least one) universal law(s), and have the following form:

The Deductivist Model of Explanation	
U (Universal Law) I (Specific Initial Conditions)	Premises (constituting the *Explicans*)
E *(Explicandum)*	Conclusion

This is commonly called the *deductive-nomological,* or the *covering-law* model of explanation, and is usually attributed to C. G. Hempel (1965).[22] It need not, however, be associated with any positivist denial of substantive (metaphysical) theories of causality, thus avoiding critiques such as that of Sylvain Bromberger (1966), that the model is defective in ignoring certain asymmetries in explanation. Contrary to another popular criticism, explanations employing probabilistic hypotheses may also be readily subsumed under this model, on the proviso that certain methodological rules are adopted to allow the falsification of said hypotheses (see, on this point, Popper, 1959, § 65). Thus, as Popper remarked (1983, p. 184):

> As to its logical aspect, there can be no doubt that a (unilaterally falsifiable) universal statement is logically much stronger than the corresponding (unilaterally verifiable) existential statement... and the situation is the same with more complex statements... Owing to their logical power, universal statements may be important as *explanatory hypotheses:* they may explain (especially in conjunction with singular initial conditions) singular events or statements. Purely existential statements, on the other hand, in isolation, or even in conjunction with singular statements, are usually too weak to explain anything.

This, then, is the reason why scientists are primarily interested in *universal* hypotheses, rather than in solitary existential hypotheses. Accordingly, purely existential statements are of no interest to science because of their logical *weakness*, and because "they cannot be falsified unless they form an integral part of a theoretical system" (ibid, p. 185). This, evidently, does not apply to the Higgs Boson. As a consequence, Howson's criticism fails to hit its target.

Returning to our logical schema of learning from experience, it is clear that such an account is impeccably deductive. Stove's charge that deductivism precludes learning from experience is thus without merit. On the contrary, if we wish to learn from experience, "[o]ur whole problem is to make mistakes as fast as possible" (John Archibald Wheeler, 1956, p. 360).

7.4 The Evolving Role of Experience in Empiricist Epistemology

However, Stove no doubt had something stronger in mind when he referred to learning from experience. For modern justificationism is still largely informed by the traditional empiricist account of the role of sense experience in epistemology. According to this account, knowledge is both *derived from* experience, and *justified by* experience—genesis *cum* justification. Popper makes this point in chapter 1 of his *Objective Knowledge* (1972, p. 3):

> The commonsense theory of knowledge (which I have also dubbed 'the bucket theory of the mind') is the theory most famous in the form of the assertion that 'there is nothing in our intellect which has not entered it through the senses'... However, we do have *expectations,* arid we strongly *believe in certain regularities* (laws of nature, theories). This leads to the commonsense problem of induction...:
>
> How can these expectations and beliefs have arisen?
>
> The commonsense answer is: Through *repeated* observations made in the past: we believe that the sun will rise tomorrow because it has done so in the past.
>
> In the commonsense view it is simply taken for granted (without any problems being raised) that our belief in regularities isjustified by those repeated observations which are responsible for its genesis. (Genesis *cum* justification—both due to repetition—is what philosophers since Aristotle and Cicero have called *'epagoge'* or *'induction'*.)

The first component of this doctrine—that we *derive* our knowledge *from* experience—we may label *sensationalism,* and is perhaps most evident in Hume's famous principle that he proposed at the beginning of the *Treatise,* "the principle of the priority of impressions to ideas" (1739- 40, p. 6). It can be traced as far back as Epicurus, and Popper also attributes an early formulation to Parmenides, who remarked satirically that "Most mortals have nothing in their erring intellect unless it got there through their erring senses" (ibid). Hume, however, had shown that this doctrine has absurd consequences. Specifically Hume showed that it had the result that all theoretical terms not reducible to sense experiences should be deemed meaningless —"if you cannot point out *any such impression,* you may be certain you are mistaken, when you imagine you have *any such idea...* Now since nothing is ever present to the mind but perceptions, and since all ideas are deriv'd from something antecedently present to the mind; it follows, that 'tis impossible for us so much as to conceive or form an idea of anything specifically different from ideas and impressions" (ibid, pp. 65-67). Sensationalism thus solves all problems of theoretical knowledge by *abandoning* theoretical knowledge and resorting to idealism.

However, despite some positivist revivals in the late nineteenth and early twentieth centuries, this sensationalist thesis that knowledge, and in particular scientific knowledge, is *derived from* experience has largely been abandoned. For not only does it contradict empirical psychological research,[23] but it is also *logically* untenable. As Kant had pointed out in the *Critique of Pure Reason* (1781, 1787): "Thoughts without content are empty, intuitions without concepts are blind. The understanding can intuit nothing, the senses can think nothing. Only through their unison can knowledge arise." (A51, B75) That is, for there to be any *rational* knowledge whatsoever, sense-experience must be structured; genetically prior categories are logically necessary. Popper (1963, pp. 42-46) had made a related critique of Hume's genetico-psychological theory of habitual knowledge by repetition: there can be no *repetitions* without a prior theoretical categorisation that interprets two events or situations (or impressions) as similar.[24]

Indeed, sensationalism is particularly inadequate as a theory of scientific knowledge. For scientific theories are invariably more than mere extrapolations from empirical reports; a point made with particular emphasis by Pierre Duhem in his *The Aim and Structure of Physical Theory*.[25] Popper notes that (2009, p. 54), "... as Duhem in particular has shown...it is precisely the most significant and typical natural laws that are the furthest removed from simple extrapolations. They always contain a new idea, which is indeed new vis-a-vis the "sequences of observations" - an idea extending far beyond the realm of the sequence of observations." Furthermore, the fact that new theories often *contradict* their predecessors and go on to make novel predictions completely scuppers the idea the theoretical knowledge is simply a generalisation from sense-experience.

The second traditional empiricist thesis—that experience can be used to *justify* knowledge claims—has *not* been abandoned however. This progression is made plain by Herbert Feigl in his 1929 work *Theorie und Erfahrung in der Physik* [*Theory and Experience in Physics*]. That is, Feigl rejects traditional empiricism in the field of the *acquisition* of knowledge, but retains empiricist *justificationism*. He writes (1929, p. 30):

> Theories almost always precede experience, and it is the soundness of these theories that is tested by observation. Even in the case of investigations designed to follow up on an accidental discovery, they are of course always based on a programme, on a guiding idea in one form or another.
>
> All these conceptual operations, which have their place prior to observation, are no doubt of the utmost importance for the emergence and development of scientific knowledge. They are

> extremely interesting from the standpoint of the historian of science and of the psychologist of knowledge. Thus in the writings of Mach and Duhem, who chiefly adopt such a perspective, we also find valuable insights concerning these intellectual activities that are so relevant to the genesis of science.

This point, that theories *precede* experience, that they are *prior* to observation, is crucial. As Popper later wrote in the §1 of *The Logic of Scientific Discovery*, "a hypothesis can only be empirically tested—and only after it has been advanced." Feigl, and with him all justificationist empiricists, demurs on this purely *negativist* role for experience however. He writes (ibid, pp. 115-116):

> What the aforementioned examples prove is relevant solely to the genesis of physical theories. Indeed, the idea of universal gravity is an absolute novelty vis-a-vis Kepler's laws, as is the idea of molecular motion vis-a-vis the laws of gases. Therefore, these theories are not simply inductively acquired from experience. Nevertheless, the *validity* of these theories can be justified only inductively... Even if research does not discover them by induction, the theories still have to be evaluated as inductions with regard to their validity.

Thus, the thesis that theories are freely conjectured, that they are genetically or psychologically *a priori* is today no longer particularly controversial.[26] The question remained, for Feigl's more sophisticated version of empiricism, whether theoretical claims could be inductively *justified* or *confirmed* by experience.[27] I have already argued in chapters 3 to 5 that they cannot be (at least not in any non-circular fashion). Instead—and this is one of the genuinely innovative aspects of the critical rationalist variant of empiricism—theoretical knowledge, including empirical knowledge, is only *negatively* restrained or controlled by experience. Experience, although still of crucial methodological significance, has no *positive epistemological* role. That is, observation sentences cannot justify *universal* statements, and indeed, they are not even *themselves* justified by experience, owing to the fact that universal terms are *dispositional* and hence assert far more than what can be "known" from perception.

What makes observation sentences so consequential methodologically is the fact that observation statements are *intersubjectively testable*—they are public and readily accessible to all.[28] Accordingly, they can play a vital part in *objective* theory adjudication, even though no observation statement is "ultimate"—no statement is immune from testing if considered problematic. Hence every potential falsifier is *itself* potentially falsifiable; no dogmatic appeal to the authority of experience is made. And since justification is given up as unattainable, their is no infinite regress or vicious circularity. Agrippa's trilemma is accordingly dissolved. This, of course, is not to deny that empirical confirmation can sometimes *induce* us to believe a proposition, but to the extent that this effect exists, it is merely a psychological phenomenon, of no objective epistemological significance. Subjective "feelings of conviction" are irrelevant as guides to the merits of a scientific statement. This pursuit of *objectivity* underlies the requirements—formal and material—that Popper (1959, p. 84) held that basic statements must meet. Formally, a basic statement must be a singular existential statement, and materially, a basic statement must be testable, intersubjectively, by observation. Observably testable consequences of a theory are prized not because they *justify* a theory or can make it *probable*, but because they do, very often at least, allow objective, *public* arbitration between competing theoretical accounts.[29]

Popper's contribution to the theory of knowledge is thus more widely applicable than his repute as primarily a philosopher of science would suggest. Miller expresses Popper's reorientation of empiricism quite beautifully in a recent essay, "Overcoming the Justificationist Addiction" (2008a, p. 2):

> Sense experience is doubly demoted in this version of empiricism. Falsificationism regards observation neither as the origin of knowledge nor as its basis. The empirical method rests its decisions on observation reports, not because these reports are firm, which they are not, but because they are easily checked, and easily replaced if they are found to be untenable. Observation remains a primary scientific resource, but it is not a primordial source (Popper 1963, Introduction); it remains fundamental, but it is not foundational (Popper 1934, § 30). The bankrupt enterprise of empirical justification, in which experience and induction were long-standing partners, is unceremoniously dissolved. Experience is reemployed in the new business of empirical falsification and criticism, but induction is permanently retired on an invalidity pension.

In other words, the role of experience, on the falsificationist scheme, is entirely *selective*—experience can neither *impart,* nor *justify* theoretical knowledge. Instead, their primary role is in *error elimination*. In this respect the logical situation of the progress of knowledge is entirely analogous to Darwinian selection. This *homology,* meant quite literally, between biological evolution and the growth of theoretical knowledge is the major inspiration behind Popper's later "evolutionary epistemology."

Although an innovation inaugurated in the later decades of his career, the basic structure is implicit even in *Logik der Forschung*. This may be seen plainly in Popper's characterisation of theory selection in that text (1959, p. 91):

> How and why do we accept one theory in preference to others? The preference is certainly not due to anything like a experiential justification of the statements composing the theory; it is not due to a logical reduction of the theory to experience. We choose the theory which best holds its own in competition with other theories; the one which, by natural selection, proves itself the fittest to survive.

Thus, to be more explicit, Popper's hypothetico-deductive theory of scientific knowledge directly parallels the "blind-variation-and-selective-retention" (BVSR) model of learning as outlined by the evolutionary epistemologist Donald Campbell. The central components of this model are (Campbell 1988, p. 402):

1. A mechanism or mechanisms for introducing variation;
2. Consistent selection processes;
3. A mechanism or mechanisms for preserving and/or propagating the selected variants.

In the familiar case of Darwinian selection, what are selected are genetically non-identical *organisms*, with random *mutation* playing the major role in introducing genetic variation and with selection pressures provided by relatively stable environmental factors. Differential propagation of (so far) adaptive traits is then achieved via Mendelian inheritance. The evolution of theoretical knowledge is, on this view, simply a continuation of biological evolution; Popper's is a "largely Darwinian theory of the growth of knowledge. From the amoeba to Einstein, the growth of knowledge is always the same: we try to solve our problems, and to obtain, by a process of elimination, something approaching adequacy in our tentative solutions" (1972, p. 261). Theories are, according to Popper, "exosomatic tools" analogous to somatic organs (ibid, p. 286); however, concerning epistemology, "[c]ritical error-elimination on the scientific level proceeds by way of a conscious search for contradictions" (ibid, p. 297).[30]

Further elaborating on the selective role of experience in *Conjectures and*

Refutations (1963, p. 46), Popper wrote that "our attempts to force interpretations upon the world [are] logically prior to the observation of similarities... scientific theories [are] not the digest of observations, but... inventions—conjectures boldly put forward for trial, to be eliminated if they clashed with observations." Observation reports are thus used primarily to *eliminate* false trials. Such Darwinian considerations affect not only observation *reports* (which are conjectural due to being conceptualised), but also *observation itself*. Sense organs, that is, are also theory-laden; they incorporate theory-like expectations (1972, pp. 145-6):

> Classical epistemology which takes our sense perceptions as 'given', as the 'data' from which our theories have to be constructed by some process of induction, can only be described as pre-Darwinian. It fails to take account of the fact that the alleged data are in fact adaptive reactions, and therefore interpretations which incorporate theories and prejudices and which, like theories, are impregnated with conjectural expectations; that there can be no pure perception, no pure datum; exactly as there can be no pure observational language, since all languages are impregnated with theories and myths. Just as our eyes are blind to the unforeseen or unexpected, so our languages are unable to describe it (though our languages can grow—as can our sense organs, endosomatically as well as exosomatically).

Popper's theory of scientific knowledge as a trial-and-error process, as (unjustified) conjecture and (deductive) refutation, thus seems to be corroborated and strengthened by this evolutionary account. Giving a evolutionary slant to the model of deductive explanation introduced at the end of the last section, the biological role of theoretical knowledge is essentially its capacity to *anticipate* the environment in which an organism finds itself. Such anticipatory knowledge need not even be conscious; indeed many expectations only become conscious once they have been *falsified* and are hence problematic.[31]

However, it has been maintained by some critics that this exclusively negative role for experience is insufficient to safeguard rationality and adequately answer the demarcation problem. In particular, it has been argued that, since observation is theory-laden, the objectivity of empirical *falsifications* is as compromised as any justificatory account. This problem—whether theory-laden observation can be used to *objectively* arbitrate between differing theories—will be the focus of the next section.

7.5 Circularity, Reductios, and the Theory-Ladenness of Observation

That the epistemological role of perceptual experience is problematic is a commonplace in contemporary philosophy of science. Indeed, Popper had even gone as far as stating that (1959, p. 30), "the main problem of philosophy is the critical analysis of the appeal to the authority of 'experience'—precisely that 'experience' which every latest discoverer of positivism is, as ever, artlessly taking for granted... 'Experience' for [the positivist] is a programme, not a problem (unless it is studied by empirical psychology)." One fact in particular that raises difficulties for the use of empirical experiment as an arbiter in theory choice is that, as Norwood Hanson famously stated, (1958, p. 19), "seeing is a "theory laden" undertaking." The circularity inherent in using theory-laden observation to *confirm* or *justify* theories is immediately apparent, and has been a central shibboleth in modern relativist and irrationalist thought. Consequently, this point concerning the theory-ladenness of observation reports was heavily emphasised by such (de facto) relativistic theorists of science such as Thomas Kuhn and Paul Feyerabend. Both stressed that the reports articulating the observed results of experiment used in scientific inquiry presuppose background theoretical knowledge. This "interconnection between theory and experiment", writes Margaret Morrison, in her *Routledge Encyclopaedia of Philosophy* entry on "Experiment", "severely undermined the idea that experiment could stand as an independent and objective criterion for judging the merits of one theory over another... neither simple experience nor experiments (including crucial experiments) can provide a neutral ground for theory choice" (1998, § 2).

This difficulty for justificationism is further compounded by the fact that the instruments that are used to obtain experimental results often *incorporate* in their construction those very theories which the experimental results are said to confirm.[32] From telescopes to particle colliders, such instruments presuppose theories which are implicit in their operation. As Peter Kosso states (1998, § 0), "the influence of background beliefs is even greater in cases of indirect observation where machines, like microscopes and particle detectors, are used to produce images of the objects of observation... the reliability of the machines, and hence the credibility of the observation, must be based on a theoretical understanding." That is, observation reports obtained using these instruments demand "overt interpretation"[33] (ibid, § 5). Both these points, concerning the theory-ladenness of observation, and the theory dependence of the instruments used in experimentation, were perhaps first clearly examined in Pierre Duhem's *The Aim and Structure of Physical Theory"* (1906).[34]

These facts have led some sociologists of science, such as Bruno Latour, Steve Woolgar, and Andrew Pickering, to argue that the theories that are esteemed as scientific knowledge largely owe their position to social, economic and political factors; hence, scientific theory choice is ultimately non-rational. According to this view, "the way a piece of evidence is interpreted and judged relevant depends on sociological and political factors including the interests of specific groups of scientists who wish to promote particular theoretical views, rather than on its reflecting a determinate way the world is" (Morrison, ibid, § 3). Thus, the combination of the theory-ladenness of observation with the doctrine that theory adjudication is dependent upon justification has marked relativistic consequences; in conjunction, these doctrines constitute "a threat to the objectivity of the process of testing and verification of theories, and hence of science in general" (Kosso, ibid). The problem, as Kosso notes, is fundamentally one of *circularity*: "If theories are allowed to, indeed required to, select their own evidence and then to give meaning and credibility to the observations, the testing process seems to be unavoidably circular and self-serving" (ibid).

However, it may be easily seen that falsification does *not* suffer from the same difficulty; it does not beg *the question under discussion*. This is recognised by Kosso (without reference to Popper). He writes (ibid):

> ...a look at the history of science shows that [theory-laden observation does not guarantee success]... There are plenty of cases of observations that are used to disconfirm theories or at least undermine the *theorist's* confidence... It would be a mistake to think that theory-laden observation is a guarantee that empirical testing will always come out positive and in support of the theories being tested. The history of science is full of examples in which observations went against theory. An interesting case is the solar neutrino problem. According to theory, the sun, like all other stars, is fuelled by nuclear reactions occurring at its core, reactions that produce neutrinos. The neutrinos ought to be detectable on the earth. The observation of neutrinos is enormously complicated and indirect, and it is profoundly influenced by theory. Nonetheless,

the results of observations have been bad news for the theory being tested, the claim of nuclear reactions in the sun. Not nearly as many neutrinos as predicted have been observed. Thus, complicity between theory and observation does not necessarily compromise the testing process in the sense of guaranteeing a supportive outcome.

That is, a proposed theory or hypothesis can be *tested* by being used to make predictions of what will be observed under specified conditions. Yet, if the prediction is successful, the fact that the theory is, or at least may be, *complicit* in the observation undermines the objectivity of any *justificatory* rendering of the experiment. However, the observation of a *falsifying* experimental outcome does *not* beg the question or presuppose the theory under consideration—no theory presupposes a falsifying experimental outcome for itself unless it is internally inconsistent. Of course, the falsifying report will itself be theory-laden, but, crucially, it will not involve any vicious circularity relative to the theory under scrutiny. In other words, falsifications take the form of a *reductio ad absurdum*, as opposed to the blatant circularity inherent in any justificatory argument.

This too is (more or less) recognised by Kosso, who writes that "perhaps all scientific observation does bear the influence of background scientific theories, but not necessarily of the theory the observation is being used to test. This independence between the theories that support an observation and the theory for which the observation serves as evidence can break the circle in the process of testing and perhaps restore objectivity" (1998, § 0). Kosso, however, makes an appeal to a coherentist theory of justification, and hence reinstates the circularity he eschews at the level of theory testing.[35] This is surprising, since Kosso clearly recognises the relativistic dangers of such circularity (2011, p. 22):

> … there is still a possibility of circularity in the method, a logical weakness that is no less compromising than unchecked subjectivity. If theories are allowed to rule on what the evidence means and when it is admissible, and if that evidence is then cited as reason to believe the theories, it's a tidy insularity that seems guaranteed to result in confirmation.

Reductio ad absurdum, on the other hand, does not suffer from this same circularity. Although an indisputably valid form of inference, it cannot be used *constructively* to establish any synthetic statement of fact—it can only be used negatively, to elicit the *rejection* of some proposition.[36] This is especially apparent in the Socratic *elenchus* of Plato's dialogues, wherein Socrates' technique of refuting a hypothesis took the form of provisionally accepting his opponent's premise as true and then proceeding (with the help of background assumptions) to point out its inconsistencies or absurd consequences. Aristotle too commends the reductio as an argumentative tool in Book 8 of the *Topics*, although this form of argument is already evident in Presocratic philosophy, most strikingly in Zeno. Formally, the *elenchus* or *reductio* proceeds as follows: If statement *P* is true, then statement Q is true. But statement Q cannot be true. (Q is absurd, or refuted by experience, or is otherwise unacceptable).[37] Therefore, statement *P* cannot be true; it does not stand up to deductive criticism. In this way, falsifications are immune to any problems of circularity raised by perceptual reports and scientific experiments being influenced by background theory and beliefs. Experiment, despite theory-laden observation, remains an impartial arbiter in theory choice when an exclusively negativist approach is adopted. As Miller writes (2006a, p. 104), "[t]here is no crasser logical error persistently committed by otherwise clear thinkers than the confusion of circular arguments, which assume what they want to establish, and therefore establish nothing, and critical (or reductio ad absurdum) arguments, which assume the negation of what they want to establish."

This distinction, between justificatory arguments and critical arguments—the former commits the fallacy of *petitio principii* whilst the latter does not—is of wider import in epistemology however. For even if observation statements were *not* theory-laden, the problem of circular justification would remain, as made plain in the discussion of the Popper-Miller theorem in section 5.5. Indeed, according to Miller, this difference between justificatory arguments and critical arguments is "the most fundamental feature of the whole of logic and rational thought" (1994, p. 68). This may be corroborated by noting the impressive range of application of the reductio: as Nicholas Rescher has stated (2005, § 1), "at the very minimum such a refutation is a process that can be applied to:

- individual propositions or theses
- groups of propositions or theses (that is, doctrines or positions or teachings)
- modes of reasoning or argumentation

- definitions
- instructions and rules of procedure
- practices, policies and processes."[38]

Of course, critical arguments will rely on *some* premise, which will inevitably be both unjustified and theory-laden. They will hence beg some question or other. Yet, crucially, critical arguments "do not beg the same question at every step. There is inevitably a circle of justification. There is no critics' circle" (Miller, 1994, p. 70). As such, "the dialectic of criticism, unlike the monologue of justification, is not committed to the endless iteration of the same point... we are not launched on to an infinite regress so much as on to an infinite progress" (ibid, p. 69). In other words, justificatory arguments, such as the axiomatic proofs of Euclid, *assume* what they set out to prove or establish, critical arguments do not.

To illustrate this point further, we may examine the following reductio argument:

$$\begin{array}{l} A \\ A \rightarrow B \\ B \rightarrow C \\ C \rightarrow D \\ \text{Not-}D \end{array}$$

Collectively, these statements form an inconsistent set—assuming the conditionals are true, either *A* or not-*D* must be rejected. However, a critical discussion of the truth or falsity of *D* may be much less problematic than a critical discussion of the truth or falsity of *A*, especially if *A* is a particularly abstract theoretical statement. This, of course, is just another form of *modus tollens* argumentation.[39] As Miller writes, "[t]here are many examples of disputes in the empirical sciences that begin at a lofty level and fizzle out in altogether different and humbler territory—for example, in a test of whether passing tramcars cause significant vibrations in nearby laboratories" (ibid, p. 69). In this case, where *D* is an easily testable hypothesis or reproducible effect, and is not, for the moment at least, problematic, *A* will have to be rejected to avoid the absurd and contradictory result of maintaining both *D* and not-*D*. As Miller notes, "[i]n the case of justification the infinite regress or circle is so damaging because without the first prop (which actually does not exist) all the others are worthless... But an infinite progress without a final goal is nothing like as serious. Of course, it has to be brought to a stop somewhere, and normally we shall have no real difficulty in finding somewhere to stop. But we do not need to have any reason for stopping where we do stop" (ibid). That is, we may stop criticism at any premise, as long as that premise is open to criticism, and has survived any criticism that has been made. In such a fashion, critical arguments are possible even when no individual premise is positively justified in any way. Fallible error elimination is both possible, and, by all appearances at least, *actual* too; no justification is necessary.

Reductios, of course, rely on the assumption that contradictions indicate error, and hence are unacceptable. They assume, in other words, both an objectivist theory of truth and the principle of non-contradiction—that is, it cannot be the case both that *p* is true and *p* is false. This assumption has been denied historically by Hegel, and, more systematically and impressively, by contemporary paraconsistent logicians such as Graham Priest and Richard Routley. Formally, such paraconsistent logics seem to me to be of great interest, yet the objections raised against the traditional conception of truth embodied in classical logic are, I think, very weak indeed, and are almost exclusively predicated upon a conflation of truth with justified truth.[40] That is, these difficulties concerning the traditional conception of truth (and hence the formal validity of the *reductio ad absurdum*) disappear with the rejection of justificationism. This point will be elucidated more fully in the following section.

7.6 Truth as Correspondence and the Law of Non-Contradiction

I have been assuming so far that in pronouncements such as "truth is the aim and regulative ideal of science", that the term "truth" is unproblematic. Of course, the traditional, pre-philosophical account of what "truth" means is simply that of "correspondence to reality."[41] However, this intuitive theory of truth, as found, for instance, in Aristotle's *Metaphysics*,[42] has faced both logical and justificationist objections. The former are, I think, legitimate but answerable; the latter are entirely misplaced. In response to both sets of objections, Popper famously made use of the Polish logician and mathematician Alfred Tarski's so-called semantical theory of truth. The fundamental significance of this theory, in Popper's view, was that it rehabilitated the intuitive notion of absolute, objective truth as being correspondence with the facts.[43]

Popper first met Tarski in July 1934, shortly after the publication of his *Logik der Forschung*.[44] In the *Logik* we can discern Popper's initial misgivings about the notion of truth; there Popper attempted to formulate his epistemology and methodology of science without any essential appeal to this notion. Remarking that although "the striving for knowledge and the search for truth are... the strongest motives of scientific discovery" (1959, p. 278), in the face of strong objections it would prove sufficient to employ instead the concepts of deducibility and associated logical relations. Popper wrote, 37 years later, "[i]n those days, before I had learned from Tarski about his theory of truth, my intellectual conscience was far from clear about the assumption that our main concern was the search for truth... The reason... was, of course, that this notion had been for some time attacked by some philosophers, and with good arguments" (1972, p. 319).

What then were these arguments which so troubled the young Popper? First there was the *logical* problem of the semantical paradoxes, in particular the liar, which gave rise to the suspicion that the very notion of truth could not be formulated consistently. Second, and perhaps more influential, was the *justificationist* criticism that the traditional theory was defective because it does not provide a built-in *criterion* of truth. The problem with this account, for justificationists, is that there seems the distinct possibility the we may not ever *know* when this relation of correspondence is satisfied. As Paul O'Grady explains in his book *Relativism* (2002, pp. 40-1):

> The correspondence theory strictly separated truth from knowledge. On the correspondence theory it is possible that all our purported knowledge of the world may turn out to be false, because there is no necessity that our beliefs be true. This allowed the possibility of extreme scepticism: a radical disjunction between thought and reality. Thought could fail to correspond appropriately with reality and so turn out to be false. The world could be wholly other than we think it is. In order to reject this avenue to scepticism, a view of truth was devised that connected it more closely to our thought about the world: the epistemic theory of truth.

Something akin to this objection resides in the view, prevalent especially among the positivists of the Vienna Circle, that if we wish to speak about truth, we should be able to give a criterion of truth. Without such a criterion for its application, the legitimacy of the entire notion was dubious; indeed, the concept was suspected of being vacuous. According to Popper, the primary import of Tarski's work was to show that despite these objections, the common-sense (essentially Aristotelian) theory of truth was not, in fact, teetering on the edge of contradiction. Rather, with the aid of tools that Tarski had developed, this (more or less) intuitive notion of truth became perfectly logically defensible. Such an objectivist account is, moreover, essential to any non-relativistic response to the demarcation problem. As Popper wrote in the 1961 addendum to *The Open Society,* titled "Facts, Standards and Truth: A Further Critique of Relativism", "a dose of Tarski's theory of truth... may go a long way towards curing this malady [that is, epistemological relativism], though I admit that some other remedies might also be required, such as the non-authoritarian theory of knowledge which I have developed elsewhere." (1945, pp. 419-420) Thus, on Popper's interpretation, the primary significance of such an objectivist theory of truth is its necessity to avoid relativism.[45]

Let us examine first the problem of the logical consistency of correspondence. In the introduction to his seminal paper "The Concept of Truth in Formalized Languages" Tarski writes: "The present article is almost wholly devoted to a single problem—the *definition of truth.* Its task is to construct—with reference to a given language—*a materially adequate and formally correct definition of the term 'true sentence'*" (1983, p. 152). As is made clear by his stress on *formal correctness,* a primary motivation for Tarski was the solution (or avoidance) of the semantical paradoxes, including, most conspicuously, the liar. The liar paradox can be expressed, (in its strengthened form), as "This sentence is not true". From the standpoint of classical logic and common sense intuitions, the sentence can, prima facie, be proved to be both true and false. The resulting contradiction raised the spectre that the very notion of truth was inconsistent.

What Tarski went on to prove was that our common sense intuitions were faulty, and in need of critical improvement; in particular he showed truth can be defined in a consistent way only for restricted ("semantically open") languages. This was because the two requirements he sought to fulfil for his definition—(a) the condition of material adequacy, which asserts that the definition of truth must entail all instances of the T-scheme, and (b) the condition of formal correctness—led to contradiction unless one or the other was restricted. To maintain formal correctness Tarski asserts that we are thus compelled to limit the material adequacy of the definition. This was achieved, of course, by way of Tarski's elegant and innovative distinction between an object and a metalanguage. Such Tarskian truth-definitions, moreover, entail the validity of the law of non-contradiction.

Despite Tarski's plainly stated purpose of formulating a *consistent* version of the correspondence theory of truth, much of the dominant exegesis fails to give this crucial point sufficient attention. David Miller, in a recent essay (2006a, p. 173), has drawn attention to various prominent examples—in particular, J. L. Mackie in his 1973 book, *Truth, Probability and Paradox*, and Hartry Field in his 1972 essay "Tarski's Theory of Truth." In Field's essay (1972, p. 91), for instance, the claim is made that Tarski's purpose in seeking to avoid the use of semantical terms in his definition was to enable a reduction of semantics to physicalism; avoidance of the paradoxes is not mentioned.[46] Yet, although of independent interest, this question of physicalism is, I think, a red herring. Instead, the fundamental import of the result was that Tarski had shown that truth was definable in *syntactical* terms which had not hitherto been regarded as logically problematic. He had thus disarmed those critics who had questioned the logical consistency of the correspondence theory. As Popper remarked (1972, p. 328), "this reduction is important because it is a rare event in philosophy that we are able to introduce an entirely new (and suspect) category of terms on the basis of (unsuspect) established categories; it is a rehabilitation, an act of saving the honour of a suspect term... without Tarski's theory, which provides a semantical metalanguage free from any specifically semantical terms, the philosophers' suspicion of semantical terms may not have been overcome." In this way Tarski's work enables us to use the notion of truth, both in epistemology and in metaphysics, without running into specifically logical difficulties.

In response to justificationist critiques of the correspondence theory, a key aspect of Tarski's work that Popper stressed was its *objectivist* character. That is, Tarski's definition of truth demonstrates how we can speak about a statement's correspondence to the facts *even in the absence of a criterion by which we may determine whether or not it actually does correspond to the facts*. Tarski's T-scheme thus specifies the *conditions* under which a statement is true, but his theory does not include a *criterion* by which we can determine whether or not a statement is true. Indeed, for Popper, one of the key results of Tarski's work was his proof that a general criterion of truth was impossible.[47] Thus, as Popper stressed in *The Open Society and Its Enemies* (1945, Vol. II, Addendum II), "[i]t is decisive to realise that knowing what truth means, or under what conditions a statement is called true, is not the same as, and must be clearly distinguished from, possessing a means of deciding—a *criterion* for deciding—whether a given statement is true or false."

Tarski made this point quite explicit with respect to *truth*, yet there are also various other legitimate concepts for which we have no criterion for applicability. For instance, Popper also pointed to the concept of deducibility, a notion whose importance and legitimacy is in no way compromised by the fact that there exists no general criterion of its applicability in specific cases (for undecidable theories, for example). Yet, the notion of validity or theoremhood is, notwithstanding the lack of a general criterion, perfectly coherent. A further example, as Mark Notturno points out (2000, p. 242), is the idea of maximisation of profit. Despite the lack of a criterion for determining whether it has ever been achieved, maximisation of profit remains a vitally important regulative ideal in capitalist economies. In much the same way, Popper asserts that truth functions are an indispensable regulative ideal for science, and it was Tarski's theory which validates this as logically defensible, by sharply separating the *analysis* of truth from the provision of a *criterion* for determining it.

The conflation of these two distinct concepts—truth and justified truth—has, it is worth noting, been a key factor in the proliferation of modern subjectivist theories of truth, such as coherentist, pragmatist, and consensus theories.[48] Indeed, this demand for criteria is, I believe, a characteristic feature of justificationism; the doctrine that "a concept is vacuous if there is no criterion for its application" (Popper, 1972, p. 321). While Tarski's results here are quite unambiguous (a concept may be perfectly valid despite not being linked to any general criterion), the contrary thesis is central to many influential modern philosophies. To cite again the Kuhnian example from the last chapter, while both Popper and Kuhn agree that there is no such thing as an objective or general criterion of truth, Kuhn mistakingly infers from this that truth must therefore play no indispensable role in scientific inquiry. These justificationist doubts about the legitimacy, in the absence of a workable criterion, of truth as correspondence are apparent in Kuhn's disavowal of truth,[49] and his focus instead on *rule-following*, which he prefers for its clear criterion of application. Yet this conflation of the concept with a criterion for its application, (a key element of logical positivism), was precisely what had been shown to be untenable by the logical results of Tarski. The authoritarian and irrationalist consequences of this doctrine, especially in the denial of truth (correspondence) as a regulative ideal, are plain. For if, *pace* Tarski's result, a formulation of truth was vacuous or meaningless when separated from any decision procedure for its determination, these very procedures would thus become the only possible way in which to define truth. The end product to such a line of reasoning, as writers such as David Miller and Mark Notturno have pointed out, is inescapable: "truth becomes a matter of decision, of convention, and ultimately of political might" (Miller, 2006a, p. 176). In more modish jargon, it becomes a product of *consensus*, or *solidarity*.[50]

Thus, what Popper gained from Tarski's theory was the insight that the lack of a criterion of truth need not lead to relativistic disavowal of rational and objective demarcation. For while we do not have an objective criterion of *truth*, we do have a criterion of *falsity*. Contradictory statements cannot both be true; and it is wholly the elimination of contradictions (in addition to, of course, the creative proposal of bold conjectures) that has constituted the phenomenal growth of knowledge from the Ancient Greeks to our own time. Such a possibility of error elimination presupposes a reality about which it is possible for us to be mistaken, and it is what underlies Popper's falsificationist solution to the demarcation problem. In this way Tarski's result was to provide a key logical clarification for Popper's previously developed epistemological methodology.

Before moving on, it is worth noting that a further difficulty with the traditional correspondence theory of truth—that of elucidating satisfactorily the correspondence between linguistic statements and non-linguistic facts—is also clarified somewhat by Tarski's theory. The problem here is how exactly the correspondence between a statement and a fact is to be understood. The prominent theories prior to Tarski had struggled to explain adequately what was taken to be this correspondence between a statement and some linguistically *or* conceptually *untouched* part of the world. Both Moritz Schlick's proposal, for instance—that the correspondence be seen in terms of a one-to-one correspondence—and also Wittgenstein's suggestion of a structural isomorphism (the "picture theory"), had proved untenable. Accordingly, Popper wrote that "[i]t was, I think, the apparent impossibility of discovering or explaining this correspondence which made all pre-Tarskian correspondence theories of truth so suspect; suspect even to people like myself who valued the correspondence theory simply because of its commonsense and realist character" (1972, p. 324). However, what Tarski showed, in making explicit that the correspondence was between a *name of a statement and a linguistically formulated description*, is that this relationship may not be as problematic as supposed. That is, while it has been objected that the T-scheme only provides a correspondence between two types of statements, and not with reality itself, this is of no epistemological import, because, crucially, *it is only what can be said that can be critically discussed.* This—a linguistic description of what would constitute the truth of a statement—is exactly what the instances of the T-scheme elucidate.

Nor should it be assumed here that Popper postulates "facts" as being something like *completely* independent atomic constituents of the world.[51] Indeed, Popper was one of the first to criticise such a view, found, for example, in the *Tractatus*. Instead, he held that "facts are something like a common product of language and reality; they are reality pinned down by descriptive statements" (1963, p. 214). While this may be suspected of opening the door to some kind of social constructivism or idealism, once we realise that experimental reports *can test or select between competing theoretical facts*, this relativistic fear is dispelled. The method by which this is achieved was precisely the focus of the preceding section.

To conclude *this* section, I hope I have outlined some of the factors that led Popper to suggest that Tarski's theory makes the concept of truth as correspondence rationally defensible. Taken together, I suggest that Popper and Tarski open a path between relativism on the one hand, and authoritarian dogmatism on the other. Truth, though hard to come by, is both objective and absolute, and it is the task of both science and philosophy to search for it.[52] Tarski's theory, in divorcing truth from both certainty and justification, and in providing an intuitive solution to the semantic paradoxes, thus constitutes what Popper called "the great bulwark against relativism and all fashions" (1990, p. 4). Its philosophical importance cannot be overrated.

7.7 Critical Preference

To return to Stove's equation of falsificationism with irrationalism, his charge was not *merely* that we cannot learn from experience without induction. It was that the rejection of inductivism leads to *"a picture of science in which progress is illusory and no scientific theory can be said to be better or worse than another."* Does the deductivist have an answer to this charge? Or, in other words, does critical rationalism have any *positive* desiderata for theoretical progress in science—that is, for an existing theory *T1* to be superseded by a better theory *T2?*

Popper addressed this issue in his 1973 Herbert Spencer Lecture "The Rationality of Scientific Revolutions: Selection versus instruction."[53] There he proposed two purely *logical* (and hence objective) criteria for gauging progress in science. According to Popper, the following two logical demands are crucial[54] (1994, p. 12):

> First, in order that a new theory should constitute a discovery or a step forward it should conflict with its predecessor—that is to say, it should lead to at least some conflicting results. But this means, from a logical point of view, that it should contradict its predecessor: it should overthrow it...
>
> My second point is that progress in science, although revolutionary rather than merely cumulative, is in a certain sense always conservative: a new theory, however revolutionary, must always be able to explain fully the success of its predecessor. In all those cases in which its predecessor was successful, it must yield results at least as good as those of its predecessor and, if possible, better results. Thus in these cases the predecessor theory must appear as a good approximation to the new theory, while there should be, preferably, other cases where the new theory yields different and better results than the old theory.

Together, these criteria "allow us to decide of any new theory, even before it has been tested, whether it will be better than the old one, provided it stands up to tests... this means that, in the field of science, we have something like a criterion for judging the quality of a theory as compared with its predecessor, and therefore a criterion of progress. And so it means that progress in science can be assessed rationally" (ibid). That is, "scientific revolutions are rational in the sense that, in principle, it is rationally decidable whether or not a new theory is better than its predecessor" (ibid).

These twin requirements for pairwise theory comparison—success in a crucial test and greater informative content—will be examined further in the following section.

7.8 Crucial Tests and Theoretical Progress

Popper's first thesis asserts that "progress in science - or at least striking progress - is always revolutionary" (ibid). In other words, it requires the overthrow of the predecessor theory by way of crucial *experiments* (*experimenta cruces*). Let us examine deductivist theory choice under such a scenario.[55] Consider the simple case in which we are faced with the choice between two explanatory theories, *T1* and *T2*, which we have submitted to tests. The outcome of those tests is as follows:

(1) *T1* has been falsified (it is false relative to some accepted basic statement).

(2) *T2* has not been falsified.

Given that we prefer truth to falsehood (ignoring for simplicity assessments of verisimilitude) we can derive from the premises (1) and (2)

(3) *T1* should *not* be preferred to *T2*.

This is because relative to the accepted empirical reports, *T1* is false; it is no longer a candidate for truth. *T2*, on the other hand, is still possibly true, given the state of the discussion. Therefore, to prefer the falsified theory over the unfalsified theory, given our preference for truth, is clearly irrational; it is inconsistent with our aims. What these statements (1) and (2) do not entail is that *T2 should* be preferred to *T1*. That is, with respect to truth, the empirical discussion does not mandate or authorise the acceptance of the (so far) unrefuted theory. *T2* may also be false. A preference for *T2* goes *beyond* what can be inferred from the state of the discussion. In other words, *positive preferences (which transcend the evidence) cannot be justified by the evidence.*

To take an example, consider the case of Newtonian and Einsteinian physics. These theories are logically incompatible; amongst other things, Einstein's general theory of relativity predicts that in strong gravitational fields a Keplerian elliptic orbit with significant eccentricity will display a corresponding precession of the perihelion. Such a precession, as had been observed of Mercury, had proved a long-standing problem from the point of view of Newton's equations. Thus, although Einstein's theory contains Newton's theory as an *approximation*, there is the possibility of crucial tests between the two. It is precisely the outcome of such tests which allow rational, but non-justificationist, theory adjudication. The subsequent observation of the bending of light rays in the gravitational field of the sun functioned as another crucial experiment between the two theories; Newton's theory was falsified relative to the observational record. However, this does not entail that Einstein's theory is true or probably true—indeed, both phenomena mentioned might have been explained in ways *other* than by Einstein's theory.

This point should be seen in the context of a more general fact about deductivism: *logic and observation reports, by themselves, do not compel or recommend belief in any hypothesis.* As Miller writes (2006a, Chapter 5, § 4):

> Given our aims and abstract predilections (for truth over falsehood, for truthlikeness and accuracy over inaccuracy, for success over failure) the combination of empirical evidence and deductive logic offers no advice concerning what we should accept or believe or do, or even what theory or course of action we should prefer. Logic contents itself with indicating what we should not accept, the evidence being what it is, and what we should not do, and what we should not prefer... that does not imply that we may not go beyond the recommendations of logic. Logic permits what it does not forbid... although there are beliefs and courses of action that are arrived at with the assistance of rational and critical arguments, there are no rational beliefs or courses of action.[56]

Thus, strict deductivism does not tell us which theory to *positively* prefer. However, a purely deductive methodology *can* tell us, in the light of accepted empirical reports, what we should *not* prefer. To take an example, a preference for Newton's theory over Einstein's in terms of truth will not stand up to deductive criticism (assuming the truth of some readily reproducible, and hence non-problematic, test statements). In this vein, Popper, in *Objective Knowledge* (1972, p. 8), reformulated the demarcation problem as follows: "Can a preference, with respect to truth or falsity, for some competing universal theories over others ever be justified by... 'empirical reasons'?" [57]Popper's response was: "Yes; sometimes it can, if we are lucky... since we are searching for a true theory, we shall prefer those whose falsity has *not* been established" (ibid, emphasis added). Accordingly, it is *falsification* —the rejection of a theory via *modus tollens*—that is of primary importance epistemologically, rather than *corroboration*.

However, since it is the *aim* of science to provide satisfactory explanations, the second requirement for progress states that the successor theory should *explain* the experimental success of the old theory. Indeed, it must explain both the successes *and* failures of the predecessor theory. [58]In other words, it must have greater explanatory content or be of a higher level of universality. In particular, it must exceed its predecessor in *testable or empirical* content. As Popper asserts (1972, p. 197), in the case of any particular theory, "it is the wealth of its content, and thus its degree of testability, which decides its interest, and the results of actual tests which decide its fate." Something must therefore be said here about Popper's idea of informative or empirical content.[59]

Popper had proposed in chapter 6 of the *Logic of Scientific Discovery* that the informative or empirical content of a statement or a theory is the amount of empirical information it conveys about the world, and this is determined by the "size" of the class of potential falsifiers or prohibited events. The rationale here is that a theory gives more information about the world of possible experience in so far as it prohibits more; that is, in so far as the size of its class of potential falsifiers is larger. Any theory *T2* which has a larger class of potential falsifiers than a rival *T1* has more opportunities to be refuted by experience and hence gives more information about the world of possible experience than *T1*. That is, the uncontroversial theoretical virtue of high explanatory power recommends theories which have a higher degree of falsifiability. Since such theories prohibit a larger class of events they thus allow the empirical world only a narrow range of possibilities, and are hence more informative. Thus, if *T2* has greater testable or empirical content that *T1*, $Ct(T2) > Ct(T1)$, it will, *a priori,* be preferable.

However, estimation of the size of the class of falsifiers for any empirical theory is not straightforward, and hence precise comparisons between competing theories are not generally available.[60] As Garcia writes (2006, p. 49):

> ...such a size is not a function of the number of prohibited events, let alone of the number of statements that describe these events (which, on the other hand, are infinite). For logical elementary operations (e.g. the co-obtaining of a prohibited event with any event whatsoever yields another prohibited event) would make it possible to increase artificially the cardinality of

the class of events, hence the size of the class, when measured by this device. To put this another way: the mere counting of prohibited events is not an acceptable way of estimating the degree of falsifiability of a theory.

Nonetheless, comparisons can, at least in some cases, be made using the subclass relation. If two theories refer to the same aspect of reality they may sometimes be capable of being formulated in such a way that their respective classes of potential falsifiers hold a relationship in which one of them contains as a proper subclass the other. In this limited class of cases, the subclass relation between their respective classes of potential falsifiers obtains and a direct comparison can be made. (An important point about degrees of falsifiability is that they are essentially ordinal, rather than cardinal, in nature). Although seemingly rare, such cases are nonetheless illustrative. Popper (1957, pp. 202-203) cites, as an example, the case of Maxwell's electromagnetic wave theory succeeding Fresnel's wave theory of light. Here Fresnel's theory was incorporated into Maxwell's theory without any revision of its empirical content. As Watkins (1984, pp. 198-199) writes "Hertz's dramatic corroborations, in 1887-1888, of Maxwell's theory, involving the transmission and reception of electromagnetic signals, testify to its excess testable content; and there can be no doubt that this excess content was generated with the help of the theory's fundamental assumptions, involving the idea of electromagnetic disturbances spreading through an electromagnetic medium, or ether." As such Maxwell's theory is explanatorily *richer*—it has greater informative content than Fresnel's. Every potential falsifier of Fresnel's theory is also a potential falsifier of Maxwell's, but not vice versa. Hence Maxwell's theory is evidently theoretically preferable—it *explains* the success of Fresnel's theory, in addition to being empirically more informative. Given our theoretical aim of a) true and b) highly explanatory theories, Maxwell's theory is *objectively* more suitable, provided it stands up to tests.[61]

This, as stated before, is not the usual case of scientific progress. Seemingly more common is the case where a theory *T1* is superseded by a *deeper* theory *T2* that *conflicts* with it in various ways. This is especially apparent where a new theoretical ontology is posited, or a new conservation principle regarding some fundamental unit is introduced. [62]Popper gives as an example the progression from Kepler's laws of motion to Newton's in his (1957, p. 201). The conflict arises from the non-derivability of Kepler's laws from Newtonian theory, due to new fundamental assumptions of the latter theory regarding the relationship between mass and gravitational attraction. Despite the fact that the degree of testability, and hence the informative content, of these theories cannot be compared straightforwardly using the subclass relation, Newton's theory may nonetheless be regarded as a clear theoretical advance. This is because it *explains* the success of Kepler's theory: although there is a conflict between the theoretical assumptions of the two theories, the empirical content of Kepler's theory can be *approximated* by Newton's theory, producing very close numerical predictions in a large class of cases. Similar comments may be made concerning the relation between Newtonian mechanics and Einstein's general relativity. Both theories produce predictions whose values are not discernibly different for a wide range of cases, and fine measurements are needed to detect the divergence. Both cases exemplify Popper second requirement: "a new theory, however revolutionary, must always be able to explain fully the success of its predecessor... it must yield results at least as good as those of its predecessor and, if possible, better results" (1994, p. 12).

This second requirement, moreover, is linked to the intuitive idea of the "depth" of a theoretical explanation. "If at all possible," Popper writes (1972, p. 55), "we are after deep theories." This concept—*theoretical depth*—is not easy to analyse logically, but it is plausibly indicated, Popper suggests, when a new theory explains some older theory whilst at the same time *correcting* it. Thus Popper writes in his essay, "The Aim of Science" (1972, p. 202):

> I suggest that whenever in the empirical sciences a new theory of a higher level of universality successfully explains some older theory *by correcting it,* then this is a sure sign that the new theory has penetrated deeper than the older ones. The demand that a new theory should contain the old one approximately, for appropriate values of the parameters of the new theory, may be called (following Bohr) the *'principle of correspondence'.*[63]

Here, "sure sign", is misleading; at best it is merely an indication that we *may* have "penetrated deeper", but this assessment remains entirely conjectural, and open to criticism. "Fulfilment of this demand", Popper continues, "is a sufficient condition of depth" (ibid). Popper goes on to remark (1972, p. 197):

> I believe that this word 'deeper' defies any attempt at exhaustive logical analysis, but that it is

> nevertheless a guide to our intuitions... The 'depth' of a scientific theory seems to be most closely related to its simplicity and so to the wealth of its content... Two ingredients seem to be required: a rich content, and a certain coherence or compactness (or 'organicity') of the state of affairs described. It is this latter ingredient which, although it is intuitively fairly clear, is so difficult to analyse... I do not think that we can do much more than refer here to an intuitive idea, nor that we need do much more. For in the case of any particular theory proposed, *it is the wealth of its content, and thus its degree of testability, which decides its interest, and the results of actual tests which decide its fate.* (Emphasis added).

That is, such logical and objective criteria for theoretical advance (i.e. greater explanatory content, which is *independently* and *publicly* testable) directly rebuts Stove's charge that deductivism entails that there is no scientific progress. Theories such as Newton's and Einstein's, or Maxwell's and Fresnel's, may accordingly be both rationally and objectively compared. [64]What deductivism does not offer, however, is a *guarantee* of scientific progress—it may still subsequently turn out that Einstein's general relativity, has, for example, as many false consequences as Newton's law of universal gravitation. This is an important point. No theoretical advance is secure or demonstrable. This, however, does not mean that progress, understood as an increase in verisimilitude or truth-likeness, is not actual. It just means that assessments of verisimilitude are *conjectural*, and must remain open to criticism.[65] Hence, Watkins' assertion (1984, p. 118) that the "negativist conception of the role of experience in science needs to be accompanied by a methodological theory that entitles us to judge, in cases where two or more rival hypotheses have all so far passed the test of experience, *which of them is best*" is too strong; the only advice we can derive from a crucial test concerning theory choice is wholly negative, whilst our preference for theories of greater content can only guide theory choice—it cannot determine it. Critical rationalism, as a consequence, does seem to offer an adequate response to the problem of rational theory adjudication, to the "fashionable (anti-rationalistic) suggestion that two different theories such as Newton's and Einstein's are incommensurable" (Popper, 1994, p. 28).

7.9 Are Expulsion Procedures Sufficient?

Thus, we may rationally reject, as candidates for truth, those theories which do not survive empirical tests or, as the most satisfactory explanation, those theories which are less informative than independently testable successor theories. This, moreover, requires only *expulsion* procedures. As Miller writes, "Hypotheses that pass all the tests to which they are subjected are retained, and may be classified conjecturally as true. Hypotheses that fail tests are classified as false" (1982, p. 25). However, we have been so far considering only pairwise (and transitive) comparisons of competing theories in the same field. Is the restriction of the comparison to this limited set of theories problematic? Consider Colin Howson's objection to falsificationist theory choice (1993, p. 4): "It is not an acceptable answer, within the falsificationist account, to say that we ever only seriously consider finitely many alternatives, for there is no purely falsificationist reason for restricting the discussion to these." Wesley Salmon (1966, pp. 24-26) makes a similar objection:

> When one particular hypothesis has been falsified, many alternative hypotheses remain unfalsified. Likewise, there is nothing unique about a hypothesis that survives without being falsified. Many other unfalsified hypotheses remain to explain the same facts. Popper readily admits all of this... When we ask, "Why should we reject a hypothesis when we have accepted one of its potential falsifiers?" the answer is easy. The potential falsifier contradicts the hypothesis, so the hypothesis is false if the potential falsifier holds. That is simple deduction. When we ask, "Why should we accept from among all the unfalsified hypotheses one that is highly corroborated?" we have a right to expect an answer.

Salmon's criticism may be sharpened by a consideration of so-called "grue-some" hypotheses—those which can be manufactured at will from existing accepted theories so as to share all the hitherto explanatory success of the extant theories. The allusion, of course, is to Nelson Goodman's (1954, p. 74) hypothetical "all emeralds are grue" hypothesis, where an emerald is grue if it is either (a) examined before time *t* and green or (b) not examined before time *t* and blue, where time *t* is some arbitrarily chosen future point of time. "What *reason,*" asks Worrall in his (1989, Section 2) "can a

Popperian give for barring the gruesome hypothesis in advance of any further evidence?" The implication here, no doubt, is that an inductive principle is necessary to *positively justify* the non-perverse hypothesis "All emeralds are green", and hence disqualify "All emeralds are grue." Yet it seems that such a hypothesis is equally corroborated by the evidence, and hence just as acceptable in terms of theoretical preference. Can't a host of such hypotheses easily be proposed? Isn't this frightful multiplication tantamount to irrationalism? Levison (1974, p. 323) , for instance, is not alone in his suggestion that "it will be possible to show briefly that Goodman's puzzles constitute as much a problem for Popper as for any "inductivist"."

Colin Howson also returns to this point in his recent monograph, *Hume's Problem: Induction and the Justification of Belief* (2000). There Howson devotes chapter 5 to a critical appraisal of deductivism—the view, as Howson concisely describes it, that "assessments of the merits of competing hypotheses in the light of evidence require no dedicated inferential machinery other than deductive logic, together with a clear statement of the aims which the assessments are intended to achieve" (2000, p. 94).[66] The main charge Howson brings against this doctrine is precisely Goodman's new riddle of induction—because deductivism entails the rejection of *all* forms of inductive logic, including the subjective Bayesian variant, it cannot discriminate between a hypothesis and its infinitely many grue-some competitors. Can we really suggest that critical rationalism is an adequate response to the demarcation problem given this fact?

I think we can. Of course, falsificationist expulsion procedures, of the type embodied in the hypothesis testing methods of classical statistics, are in no way a finished product or infallible, and they will undoubtedly continue to develop. However, it cannot be doubted that they offer a powerful arsenal deliberately directed toward error elimination. What they cannot do, and what it is unreasonable to ask of them, is to eliminate *all* possible false hypotheses, particularly ones which have not even been canvassed. If an alternative explanatory theory is proposed, which is independently testable and solves some significant problems, it will of course be considered—it might, after all, be true. But unproposed theories do not present a problem of theory choice. Here Bartley's response (1968, pp. 54f.) seems to me quite sufficient:

> To attempt a decision on the basis of confirmation theory, which would involve an inductive logic, would be impossible because an inductive logic is impossible; to attempt a decision on the basis of a definition of "lawlike" or "projectible" character of predicates would be Cartesian or "essentialistic," not empirical, despite the fact that an empirical study of the actual history of the predicates in the language may be required to discover which is "better entrenched." Rather, what is needed is a crucial experiment between the two theories which would falsify at least one of them. But no crucial experiment of an empirical character is possible before time t. Therefore one must wait for time t.

In other words, the fact is, emeralds *may be grue*. Of course, no one supposes that they are, and this psychological fact about the categories we employ may readily be *explained* in evolutionary terms in the manner of Quine (1969). But from a theoretical perspective, the reason why the hypothesis is not taken seriously is that it solves no problem that "all emeralds are green" does not solve just as well. As Bartley continues:

> Whether we take seriously a whacky, highly counter-intuitive hypothesis depends neither on an analysis of the "predicates" used in the hypothesis, nor on our psychological dispositions, nor on our past experience, nor on the degree to which certain predicates are "entrenched," nor on a combination of these, but rather on the question whether the odd or whacky hypothesis in question is directed to an existing intellectual problem, scientific or otherwise.

Accordingly, as Miller remarks, there is "of course no justification for welcoming 'All emeralds are green' and (in consequence) banishing 'All emeralds are grue.' Still, no justification is needed. Lack of justification is not another name for irrationality." In other words, justification is not necessary for rational demarcation. Feyerabend is thus quite correct that Goodman's paradox "is as clear a refutation as can be desired that a hypothesis can be confirmed, and that the confirmatory force lies in its positive instances" (1968, p. 252). Yet since the grue hypothesis is theoretically *redundant* in the presence of "All emeralds are green", up until time *t*, it need not be seriously considered (if at all) until that time. At time *t* we can perform a crucial experiment. Of course, if an individual wanted to use a grue variant of a hypothesis for *predicting* a *future* event, that is their prerogative. Science is not clairvoyance. We cannot *a priori* rule out inexplicable and unforeseen changes in what we imagine to be spatio-temporally invariant laws. Yet unless the discontinuity is a consequence of a more universal, and hence independently testable theory, it need not *demand* our attention either.

To return to Howson's objection in more detail, the main thrust of his critique of deductivist theory selection relies upon a justificationist construal of what an adequate response to "Hume's problem" would look like. Howson characterises this problem as that of providing a justification of "any sort of 'forward-looking' claim to predictive success or even to truth… for hypotheses that have passed all the tests to which they have been exposed" (2000, p. 94). He claims, moreover, that "Popper's novel suggestion is that Hume's Problem can be solved in a *positive* way without invoking any inference procedures other than those of deductive logic" (ibid, emphasis added). Howson's characterisation of Popper's views on theory choice are based on a passage from an essay "Conjectural Knowledge" and now the opening chapter of *Objective Knowledge* (1972). I do not, however, think that Howson's justificationist interpretation of deductivism can be sustained. In that essay, already partially quoted above, Popper wrote (p. 8):

> This problem situation—that of choosing between several theories—suggests a third reformulation of the problem of induction:
> Can a *preference,* with respect to truth or falsity, for some competing universal theories over others ever be justified by such 'empirical reasons'?
> … Yes; sometimes it can, if we are lucky. For it may happen that our test statements may refute some—but not all—of the competing theories; and since we are searching for a true theory, we shall prefer those whose falsity has not been established.

Popper is here claiming, according to Howson, that deductive procedures can provide "good, non-inductive, grounds for choosing between competing universal theories" (2000, p. 95). "The catch", Howson asserts "is signalled by Popper himself in the phrase 'but not all' in the quotation above. 'Not all' indeed—for what about 'grue' and the underdetermination problem?" (ibid). Howson correctly states that there will always be an uncountably infinite number of alternative theories that stand in exactly the same logical relation to the evidence as the one currently accepted. Deductivism, that is, despite Popper's (alleged) claim here, *cannot* eliminate grue type hypotheses —"there are infinitely many grue… alternatives to H, and there is no single test or even finite number of tests which will eliminate all but a finite number: any finite set of tests will still leave an infinity of alternatives unrefuted" (ibid). Yet it should be stressed that Popper was quite aware of this fact, and stated it emphatically on numerous occasions. As he replied to a similar criticism by Levison in his (1974b, p. 1043): "there *are* no such things as good positive reasons; nor do we need such things." That is, there is no non-question begging way to justify one of these variant hypotheses over the others. To the extent that Popper's formulation in *Objective Knowledge* can be interpreted to imply that there are positive justificatory reasons, it is incorrect. Yet in light of his statements elsewhere, and even in the same essay (Popper goes on to state (p. 22), *"there can be no good reasons* in this sense, and this is precisely Hume's result")—Howson's interpretation is certainly suspect.[67]

What makes this criticism especially perplexing, however, is that Howson explicitly acknowledges that such justificatory reasons do not exist; yet he lambastes Popper for not providing them. For he concludes his criticism of Popper's deductivism in the following fashion (2000, p. 100):

> No purely deductive rule can solve the problem of induction. Jeffreys sums up the position with his usual succinctness:
>
> 'the tendency to claim that scientific method can be reduced in some way to deductive logic... is the most fundamental fallacy of all: it can be done only by rejecting its chief feature, induction.' (1961: 2)

Yet, in the very next paragraph, Howson asserts that "the empirical data cannot discriminate between a hypothesis and any of the myriad alternatives which stand in the same logical relation to it... [this]...fact of fundamental importance [is] recognized explicitly only by the Bayesian model of the logic of scientific reasoning."[68] Then, in a truly remarkable volte-face, after asserting that deductivism is defective because it cannot objectively *justify* the choice of a hypothesis over its grue variants, and endorsing Jeffreys' claim about the *necessity* of induction, Howson remarks (ibid): "the Bayesian model of the logic of scientific reasoning... does not in any way propose to justify any distribution of prior probabilities, and therefore is not really an inductivist model." That is, Howson's critique suffers from a strange inconsistency—he seems to demand that in order for deductivism to solve the demarcation problem in an adequate manner, it must provide a *positive,* or *justificationist* rebuttal to Hume. Yet he does not make this same impossible demand of subjective Bayesianism. Surely, by this logic, subjective Bayesianism must be rejected too?

Not so, according to Howson, for subjective Bayesianism *does* address the grue problem. His explanation of this is quite interesting, and is worth quoting at length. What Hume's result shows, Howson states (2000, pp. 239-240):

> is that no theory of rationality that is not entirely question-begging can tell us what it is rational to believe about the future, whether based on what the past has displayed or not. This is not to say that evidence tells us nothing. The trouble is that what it does tell us cannot be unmixed from what we are inclined to let it tell us. Increasing observational data certainly, *provably,* reinforces some hypotheses at the expense of others, but only if we let it by a suitable assignment of priors. We would like to think that an unbroken sequence of viewings of green emeralds reinforces the hypothesis that all emeralds are green. Unfortunately, it can equally be regarded as reinforcing the hypothesis that all emeralds are grue, which is inconsistent with the favoured hypothesis, unless we prevent it doing so by assigning appropriate prior weights. Without our assistance, the evidence cannot tell us that the course of Nature may not change, or for that matter remain the same in emeralds' continuing grueness. Nothing can. Hume was right.

In other words, in Howson's view an adequate response to the grue problem is to dogmatically assign appreciable prior probabilities to certain favoured hypotheses so that the hypotheses that are so chosen may then be confirmed (or "provably reinforced") by the empirical evidence. But, as Howson asserts (2000, p. 100) the "posterior probability is shown to depend in general on prior probability." Indeed, in his earlier critique of traditional probabilistic induction Howson had declared (p. 76):

> ...*any probability measure must be dogmatic with respect to almost all the hypotheses in* [the set of all possible grue variants of a hypothesis H]. Had we let H be one of these the 'solution' to the grue problem obviously wouldn't have worked. In other words, the grue paradox was 'solved' probabilistically only by a prior decision to give positive prior probability to the 'correct' hypothesis! The decision cannot be justified on the ground that otherwise the probability measure would be dogmatic with respect to that hypothesis, for as we have seen it will necessarily be dogmatic with respect to uncountably many other hypotheses any one of which might, a priori, be the true one. Thus H ended up being the best confirmed *only because a prior decision was taken to allow it to be 'confirmed' at all.*

What exactly, then, is the advantage of subjective Bayesianism over the circular justifications inherent in probabilistic induction? Howson criticises non-Bayesian methodological texts for "leaving the questioning reader puzzled about the epistemic status of results obtained on their basis" (2000, p. 178). The key attraction of *Bayesian* inference, though there is here too "plenty of scope for hand-waving", is primarily that, "since the inferences drawn appear in the form of calculations, crucial assumptions tend to appear explicitly, and, most importantly, in a way that makes it evident exactly how the final probability depends on these: Bayesian inferences are *epistemically transparent*" (ibid). In other words, Bayesian confirmation theory, like all theories of justification, is question-begging; its chief selling point is simply that it is *explicit* about it. This resort to circular inference is especially disappointing, since there is much else to be admired in Howson's position. In particular, his stated program, shorn of both the question-begging introduction of subjective prior probabilities, and the confusing terminological equation of "sound inductive reasoning" with non-ampliative logic, sounds uncannily like a description of falsificationism:

> An assumption of the induction debate, sometimes tacit, sometimes not, and certainly one that Bacon and all his successors accepted, is that the endorsement of scientific procedure must also extend to the endorsement of truth-claims made on behalf of the appropriate scientific theories: that for the principles of scientific reasoning to be correct means that they should lead in some guaranteed way to truth, or to some surrogate, like 'approximate truth' or probable truth. Hume's reasoning is therefore usually taken as showing, if correct, that the procedures of science are without foundation. My claim is that it can equally be seen as a *reductio ad absurdum* of the assumption that sound inductive reasoning will point us the way to truth, or at any rate to justified beliefs about truth. Indeed, I shall try to show that the commonly accepted rules of scientific method are not truth- or probability-oriented in this way, which I shall do by showing that the rules of method are just rules of logic. It will follow that they are demonstrably sound, for they are only logic, and that they do not tell us which theories are true, for, being only logic, they cannot. I shall then proceed to show that, despite *just* being logic, these rules are nevertheless indispensable in the search for truth because, *as rules of logic*, they prevent us making fallacious inferences.

To return once more to the grue problem, although Howson states that (2000, p. 97) "Popper never explicitly attempted to answer the objection that Goodman's 'Paradox' poses to his theory", this is not quite correct. For in his (1983), which was written in the 1950's, and before he was aware of Goodman's paradox[69], Popper had written (pp. 67-68):

> One difficulty for inductivists, so I gather, is to say why, having observed only black crows up to, say, 1950, one should prefer the law 'All crows are black' to the law 'All crows before 1970 are black, and after 1970 crows will be white'. For both these laws seem to account equally well for the available observational evidence. Nevertheless, it is said, we 'obviously' prefer the first to the second. The problem is to explain why we do so.
> Now it may be thought that the same problem must arise within my theory of problem solving by conjectures... Why, I am asked, do we prefer the first hypothesis to the second and the third, since all three seem to be related to the evidence equally well?

Popper's reply is that "our present scientific theories of the colouring of birds are of a character which makes us suspect that any explanation of the change in question which would not be *ad hoc* would conflict with some well-corroborated theories (of genetics). For these reasons alone, I do not see any difficulty whatever in explaining my preference in a case like this" (ibid). That is, many (but not all) grue-type hypotheses can be rejected as being *deductively* inconsistent with other accepted theories.

However, there is a more fundamental objection to the justificationist demand that alternative hypotheses be excluded in some *a priori* fashion. As Popper went on to state (p. 68):

> All our hypotheses are conjectures, and anybody is free to offer conjectures—even conjectures that may appear quite silly to the majority of us. Only thus can we make way for bold, unconventional, new ideas. We have to pay for this freedom by often being confronted by ideas that seem to be silly. Few of these will be taken seriously; but some may; and some may sometimes, contrary to first impressions, turn out to be moves in the right direction. So, although every scientist who dismisses a theory as silly *a priori* takes a risk, there is no way to avoid such risks; not only is every proposal of a new conjecture risky, but so also is the decision whether to take it seriously or to dismiss it out of hand. As opposed to inductivists, I do not assert that there is *one* (inductively reached) theory which best accounts for or explains any given evidence. On the contrary, the idea of *a plurality of competing conjectures*—which, admittedly, we *try* to reduce by criticism—is essential to my methodology... In practice, many of the more obviously 'silly' conjectures may be eliminated through criticism; as being untestable, or less testable; as being arbitrary, or *ad hoc;* as creating, without excuse, unnecessary new problems; and as conflicting with our most general ideas of what a satisfactory explanation should be like: ideas

> which are somewhat vague, but which, like scientific theories, develop by trial and error: ideas of the 'style' (mechanical, electrical, statistical, etc.) of a good explanation. Of course such considerations may make us sometimes reject a *good* theory. This too is one of the risks we run; it is part of the conjectural character of science.

In other words, although there are so-called "tried and true" theories and proposals—that is, ones that have been exceedingly well-corroborated—this does not rule out, as potentially superior, completely new and *untested* theories and proposals. We must allow for this. Just as the diffusion of ideas or technologies in a marketplace require innovators and early adopters who must take some risks (if only in terms of opportunity costs), so too is the adoption and trial of new and uncorroborated theoretical explanations essentially a gamble. It is, nevertheless, vital for theoretical progress. This remains true even if most of these uncorroborated theoretical explanations lead, in the end, nowhere. Intellectual advances cannot be preempted *a priori*—if we had a *logical* method to proceed into the unknown, the unknown would already be known. Valid inferences can only elicit the consequences of what is *already known*—that is, conjectured to be true.

Such a situation does not offer any guarantee of security or future success, and as such it may be, to some, a decidedly uncomfortable position. This, however, only reinforces the imperative need for searching deductive *criticism*. Any hypothesis may be proposed as a candidate for the truth. Yet due to this non-dogmatic "open door" policy, we ought to be wary that any such hypothesis is not so formulated as to be immune from criticism which might lead to its rejection if it is false. This is the central idea behind Popper's theory of falsifiability, and its generalisation, critical rationalism. Logic, contrary to the justificationist tradition, is not the organon of *proof*, but is instead better seen as *the organon of criticism* (Popper, 1963, p. 64). The rationalist position is thus far more humble (and non-authoritarian) than the justificationist project had assumed. Despite this, rational demarcation is not only logically *possible*, but also, as a matter of fact, *actual*. This position is, of course, not demonstrable; it has, however, so far stood up to criticism.

7.10 Conclusion: Rational Demarcation

The foregoing has been only a sketch of the deductivist solution to the problem of rational theory choice; a schematic outline. Yet I hope to have illustrated its viability. Of course, no guarantee of success or certified reliability can be given to *support* such a proposal; no such guarantees exist. Falsificationism is, nonetheless, an effective method for exploring the world. It is, moreover, subject to none of the logical inconsistencies that beset all theories of epistemic justification. In particular, it is, as I hope I have illustrated, untroubled by Agrippa's trilemma. Accordingly, justificationism may be dispensed with as entirely superfluous to theoretical inquiry. This does not entail, as many think, the abandonment of rationality or reason. For the rationality of science resides not in the content of its theories, but rather in its *method*, and this is the critical method—the method of conjectures and refutations in the search for truth. *Reason* (that is, deductive logic) is not abandoned, only *reasons* in the traditional justificationist sense. Thus, scepticism need not imply cognitive nihilism. On the contrary, as one of the truly great scientists of the twentieth century, Richard Feynman, remarked in his *The Character of Physical Law* ([1965] 1992, p. 151):

> ...experimenters search most diligently, and with the greatest effort, in exactly those places where it seems most likely that we can prove our theories wrong. In other words, we are trying to prove ourselves wrong as quickly as possible, because only in that way can we find progress.

---

[1] The most systematic and detailed overview of these criticisms, to which I am indebted, can be found in Miller 1994, Chapter 2; a revised version of his paper "Conjectural knowledge: Popper's solution of the problem of induction" (Levinson 1982, pp. 17-49).

[2] O'Hear writes that "any coherent conceptualization of experience requires the assumption of a stable order of the world" (1980 pp. 57f), and this assumption is identified with an acceptance of an inductive theory of science.

[3] Other examples of this objection are Salmon, (1968; 1978), Good (1975) and O'Hear (1975; 1980). O'Hear for example writes (1989, pp. 39f.): "Without using some sort of inductive assumptions, how can one move from past experience to calculations of present (or future) probability? ... All we have, on non-inductive grounds, are reports *of past* experience, and generalization from them is forbidden."

[4] That is, Carnap's position before 1932, which prompted Neurath's celebrated objections in the debate over protocol sentences.

[5] I interpret Popper here as advocating *some* degree of doxastic voluntarism, which will vary depending on the case under consideration; however, the central point here is *logical* rather than *psychological*. For further remarks on the role of observation statements in falsificationism, see sections 7.4 and 7.5 below.

[6] MacDonald's constructive thesis is that "[i]n his political and ethical philosophy [Popper] was more charitable to experience; our suffering is a reason for acting so as to minimise that suffering. It is argued here that his evolutionary epistemology also leads to a position that must afford experience an epistemological role" (2004, p. 159). I do not think, however, that MacDonald correctly interprets either Popper's negative utilitarianism or his evolutionary epistemology. Regarding Popper's ethical proposals, they are precisely that—*proposals* for action—which cannot in any way be justified by empirical facts (as Hume noted, you cannot derive an "ought" from an "is"). This does not entail that empirical facts are not pertinent to ethical pronouncements, but the extent to which negative utilitarianism is valid is just the extent to which it has stood up to criticism. The entreaty to minimise avoidable suffering need not be justified in order to be correct; to the extent that that proposal stands up to criticism, no justification is needed.

Regarding Popper's later evolutionary turn, MacDonald cites Popper's claim that (1992, pp. 17-18, italics in original):

"My hypothesis is that the original task of consciousness was to anticipate success and failure in problem-solving and to signal to the organism in the form of pleasure and pain whether it was on the right or wrong path to the solution of the problem... Through the experience of pleasure and pain consciousness assists the organism in its *voyages of discovery*, and in its *learning processes*."

Commenting on this, MacDonald writes (p. 167), "it is a fundamental feature of Popper's account that it is pain itself, not a representation of pain, that does the work in signalling to the organism that trouble lies ahead... it is clearly the nature of the experience itself that is crucial to the role it plays in enabling the organism to solve its problems. The task of consciousness, its function, cannot be replaced by anything less robust, less painful or pleasurable, than the 'raw feels' themselves." And he later goes on to state (p. 168): "one can see that this conception of the role of perceptual experience in selecting out error does not permit the usurpation of that role by 'basic statements'." Yet Popper never denied the essential role of perceptual experience in the acceptance of basic statements; he merely stated that perceptual experiences could not *certify* the truth of a basic statement. There is no inconsistency here, as MacDonald suggests. Moreover, the claim that pain and pleasure can *motivate* action is entirely consistent with Popper's position in *The Logic of Scientific Discovery*.

[7] This objection gained traction with Popper's "Truth, Rationality, and the Growth of Scientific Knowledge"—a paper first published as Chapter 10 of *Conjectures and Refutations*—where Popper claimed that corroboration must be added as a "third requirement" of scientific progress (1963, pp. 240f). The first requirement was that "[t]he new theory should proceed from some simple, new, and powerful, unifying idea about some connection or relation... between hitherto unconnected things... or

facts... or new 'theoretical entities'"; the second was: "the new theory should be independently testable." Popper thus added this third requirement: "the theory should pass some new, and severe, tests...we require of a good theory that it should be successful in some of its new predictions; secondly we require that it is not refuted too soon—that is, before it has been strikingly successful."

[8] Examples include Putnam (1974, p. 224), who detects an "inductive quaver" in Popper's account of corroboration, and Warnock (1960, pp. 100-1), who claims that Popper assumes that a corroborated hypothesis "proves its fitness to survive' in future tests." See also Stokes (1998, p. 30), who finds it difficult "to deny that corroboration involves a trace of induction, in which one accepts a statement as confirmed because of the number of severe tests that it has survived in the past."

[9] However, although my tendency is to downplay the role of corroboration (certainly in comparison to, e.g. Watkins (1984)), some fascinating recent work by Darrell Rowbottom, involving the employment of intersubjective probabilities in corroboration measures, may have the potential to promote the concept to a more central role in scientific methodology. For the intersubjective interpretation of probability see Gillies' (2000, ch. 8); Rowbottom's presentation of intersubjective corroboration can be found in Rowbottom (2011, ch. 3).

[10] Increasing truth approximation is neither guaranteed or even probable in any objective sense, however; appraisals of increasing verisimilitude are conjectural.

[11] Howson (2000, p. 97) also criticises Popper's notion of corroboration because "it is immediately vulnerable to the grue problem...: it cannot discriminate between [competing hypotheses]. C(H, E) has the same value for all hypotheses predicting E, for C(H, E) depends on H only through P(E|H), whose value is 1 for every hypothesis entailing E (note that C(H, E) cannot depend on the priors since by assumption all these are uniformly 0 in Poppers' theory). C(H, E) can't tell grue from green!" We will return to this problem— Goodman's "new riddle of induction— in section 7.9 below.

[12] Popper had written in his (1983, p. 346) some remarks reminiscent of what is often called the No-Miracles argument for scientific realism: "If a theory *h* has been well corroborated, then it is highly probable *that it is truth-like. That is to say, that it agrees well with some of the facts.* This is highly probable in the sense that it is extremely improbable that the predictive success of a powerful and well-tested theory is a mere accident." This, however, should not be taken to be a concession to inductivism, as the next paragraph illustrates: "But it does not make *h* 'probable': for to say that *h* is probable is to say that *it is more probable than not* that *h* is *true.* This would mean that it is more probable than not that *h* agrees with *all the facts in the world:* that there exists no counter example, no fact that contradicts it. But no finite evidence e can ever tell us that." Appraisals of truth-likeness are conjectural, and as Miller notes, (1994, p. 46): "The plain fact is that excellent agreement with a host of precise predictions is by no means a monopoly of truthlike hypotheses; on the contrary, every hypothesis that is strong enough to make many predictions at all makes many very accurate predictions, however wild it may be intuitively... mere predictive accuracy simply cannot be taken as a mark of truthlikeness or nearness to the truth. This does not mean that we may not explain the good performance of a hypothesis in stringent tests by the conjecture that it is approximately true; and, indeed, the joint success of many incompatible

alternatives is open to explanation in the same way." In other words, a well-corroborated theory need not have a high verisimilitude—inferences from empirical success to truth-likeness are invalid.

[13] This opinion, as noted earlier, is certainly in conflict with some of Popper's comments on corroboration, a concept he tended to place greater emphasis on in his work subsequent to *The Logic of Scientific Discovery.* For instance, in a recent paper in the *British Journal for the Philosophy of Science,* Darrell Rowbottom, quoting Popper, has summarised the significance of corroboration as follows (2013, p. 739): "It is important theoretically since it 'is a means of stating *preference with respect to truth*' (Popper [1972], p. 20) and pragmatically since 'we should *prefer* as basis for action the best-tested theory' (Popper [1972], p. 22)." In both areas—theory and practice—I believe Popper overestimated the power of corroboration. Accordingly I prefer the more strictly negativist approach of *The Logic of Scientific Discovery,* and of Miller's interpretation of critical rationalism (see especially his 1994).

[14] Or, at the very least, certain versions of it. Alan Musgrave, for instance, writes that (1999, p. 333) "[c]ritical rationalism as Miller understands it... is wholesale irrationalism in disguise... to defend *reason* while insisting that there are no good *reasons* for anything [is an] attempt to square the circle."

[15] It should certainly not be thought that such considerations as the Duhem-Quine thesis are completely ignored in scientific deliberations. Indeed, a closely analogous problem is that of confounding variables, treated extensively in modern experiment design. Sophisticated methods to avoid such confounding factors have been the norm at least since the great statistician Sir Ronald Fisher's 1935 classic *The Design of Experiments.* Fisher, who, like Popper was a "revolutionary *falsificationist"* (Howson 2000, p. 101), quite literally revolutionised the practice of experimentation in the first half of the twentieth century: "Until Fisher, experiments were idiosyncratic to each scientist... scientists would often engage in "experimentation" that accumulated much data but was useless for increasing knowledge... In the nineteenth century, scientists seldom published the results of their experiments. Instead, they described their conclusions and published data that "demonstrated" the truth of those conclusions..." (Salsburg 2001, pp. 4-5).

Fisher, originally in the context of agricultural research on fertilisers, stressed that for experiments to indicate anything at all, they must be carefully designed so as to distinguish, as far as possible, the differing effects of various factors (year-to-year differences in weather, differences in the composition of artificial manures etc.) As Salsburg writes, "the scientist cannot just go off and "experiment" (ibid); experiments are instead consciously designed to both falsify theories and to attempt to identify the precise cause of the falsification.

[16] Popper again addressed this problem, and presented his non-justificationist response, in his (1963, p. 238): "The fact that, as a rule, we are at any given moment taking a vast amount of traditional knowledge for granted (for almost all our knowledge is traditional) creates no difficulty for the falsificationist or the fallibilist. For he does not *accept* this background knowledge; neither as established nor as fairly certain, nor as yet probable. He knows that even its tentative acceptance is risky, and stresses that every bit of it is open to criticism, even though only in a piecemeal way. We can never be certain that we shall challenge the right bit; but since our quest is not for certainty, this does not matter."

[17] Nola (2005) makes a similar criticism: "A commonly cited obstacle to Popperian falsification is... the Quine-Duhem thesis in one or other of its several forms. This is something which... Popper recognised in *The Logic of Scientific Discovery* (section 18) when he confessed that we falsify a whole system and that no single statement is upset by the falsification. Popper seems to pay little further attention to the problem of how falsification, or even corroboration for that matter, may arise by piercing through any surrounding accompanying statements, to target a hypothesis under test" (quoted in Rowbottom, 2011, p. 96).

[18] Should further methodological precision be thought necessary in this regard, Darrell Rowbottom has recently made the interesting suggestion, in response to the Duhem problem, that we adopt "the methodological proposal that we explore auxiliaries with low (or no) corroboration values first (e.g. by subjecting them to further tests)... By comparing the corroboration values of the auxiliaries... and ranking them, we are identifying those areas where we have made the least (or even no) effort to check for errors... To the extent that there might be an error there *and we have already checked all the other possibilities (more carefully)*, however, we should plausibly make some effort to look there next." (2011, p. 102)

[19] See, for example, Popper (1983, p. 186): "the asymmetry is even stronger than indicated so far. A traditional principle of empiricism which I accept is that theories are to be judged in the light of observational evidence. But this means that we have at least sometimes to make up our minds to accept some basic statement—if only tentatively, and after many tests and deliberations. And once we do accept it, we are, as we have seen, logically bound to *reject* some theory. There is nothing analogous to this as far as the *acceptance of a theory* is concerned, or as far as its *verification* is concerned.
Thus, the logical relation between basic statements and theories, and the uncertainty of basic statements, enforce rather than cancel each other: *both operate against verification; and neither operates unilaterally against falsification."*

[20] Miller's general remark regarding unfalsifiable consequences of scientific theories is also relevant here (1994, p. 10): "all falsifiable hypotheses have amongst their consequences a host of unfalsifiable statements (ranging from tautologies and unrestricted existential statements to meaty metaphysics) that enter science as it were on the coat-tails of their parents. But these unfalsifiable consequences—to the extent that that is all that they are—are not scientific in their own right; their title is one of courtesy. If their parents are rejected from the realm of scientific knowledge, they will have to be rejected too. Thus the metaphysical component of science need not be denied. It simply needs properly and responsibly to be taken care of."

[21] As Popper wrote in *Objective Knowledge,* (1972, p. 349): "The task of science is partly theoretical —*explanation*—and partly practical—*prediction and technical application."*

[22] However, according to Agassi (2008b, p. 56 fn. 35), this model was explicitly advanced much earlier, in Descartes' methodological writings.

[23] For some of these psychological results, see Michel ter Hark's *Popper, Otto Selz and the Rise of Evolutionary Epistemology* (2004).

[24] As Popper writes (1963, p. 45): "We must thus replace, for the purposes of a psychological theory of the origin of our beliefs, the naive idea of events which *are* similar by the idea of events to which we react by *interpreting* them as being similar. But if this is so (and I can see no escape from it) then Hume's psychological theory of induction leads to an infinite regress, precisely analogous to that other infinite regress which was discovered by Hume himself, and used by him to explode the logical theory of induction. For what do we wish to explain? In the example of the puppies we wish to explain behaviour which may be described as *recognizing or interpreting* a situation as a repetition of another. Clearly, we cannot hope to explain this by an appeal to earlier repetitions, once we realize that the earlier repetitions must also have been repetitions-for-them, so that precisely the same problem arises again: that of *recognizing or interpreting* a situation as a repetition of another.
To put it more concisely, similarity-for-us is the product of a response involving interpretations (which may be inadequate) and anticipations or expectations (which may never be fulfilled). It is therefore impossible to explain anticipations, or expectations, as resulting from many repetitions, as suggested by Hume. For even the first repetition-for-us must be based upon similarity-for-us, and therefore upon expectations-precisely the kind of thing we wished to explain... This shows that there is an infinite regress involved in Hume's psychological theory."

[25] Duhem's critique of inductivism is groundbreaking, but it is not complete—he retains some inductivist elements in his theory of science.

[26] As Popper writes, concerning the origin of theories (1994, p. 13): "Great science and great scientists, like great poets, are often inspired by non-rational intuitions. So are great mathematicians. As Poincare and Hadamard have pointed out, a mathematical proof may be discovered by unconscious trials, guided by an inspiration of a decidedly aesthetic character, rather than by rational thought."

[27] Wesley Salmon also clearly exhibits this transition within empiricist epistemology (1966, p. 7 & 21): "Given some conclusion, however arrived at, regarding unobserved facts, and given some alleged evidence to support that conclusion, the question remains whether that conclusion is, indeed, supported by the evidence offered in support of it... The thesis I am defending— that science does embody induction in a logically indispensable fashion...is simply the claim that scientific inference is ampliative." Ampliative inference and empirical justification are the two characteristic doctrines of modern empiricism.

[28] This, I think, marks an important contrast with *intuitions*, which undoubtedly play an important role in the *creation* of scientific theories, but which are in general not intersubjectively accessible.

[29] Observation reports, that is, have no justificatory function; instead their primary role is to help foster intersubjective, public criticism. Popper makes this point quite clearly in the first volume of *The Open Society* (1945, Vol I., Ch. 23): "In the natural sciences [objectivity] is achieved by recognising experience as the impartial arbiter of their controversies. When speaking of 'experience' I have in mind experience of a 'public' character, like observations, and experiments ... and an experience is 'public' if everybody who takes the trouble can repeat it. In order to avoid speaking at cross-purposes, scientists try to express their theories in such a form that they can be tested, i.e. refuted (or else corroborated) by such

experience. This is what constitutes scientific objectivity. Everyone who has learned the technique of understanding and testing scientific theories can repeat the experiment and judge for himself. In spite of this, there will always be some who come to judgements which are partial, or even cranky. This cannot be helped, and it does not seriously disturb the working of the various *social institutions* which have been designed to further scientific objectivity and criticism; for instance the laboratories, the scientific periodicals, the congresses. This aspect of scientific method shows what can be achieved by institutions designed to make public control possible, and by the open expression of public opinion, even if this is limited to a circle of specialists."

[30] For a critical examination of this evolutionary analogy for the falsificationist theory of knowledge, see Darrell Rowbottom's "Evolutionary Epistemology and the Aim of Science" (2011, chapter 7). There the author notes that (p. 124), "an evolutionary analogy is only sufficient to defend the notion that the aim of science is to isolate a particular class of false theories, namely, those that are empirically inadequate." Although I am in broad agreement with his assessment of the capacity of empirical testing, I do not think the case has been made that this thereby invalidates truth-seeking or truth-approximation as an aim or goal of enquiry.

[31] Popper gives a familiar example in his essay "Towards an Evolutionary Theory of Knowledge" (1990, pp. 31-32):

"Our own unconscious knowledge has often the character of unconscious *expectations,* and sometimes we may become conscious of having had an expectation of this kind when it turns out to have been mistaken.

An example of this is an experience that I had several times in my long career: in going down some stairs and reaching the last step, I almost fell, and became aware of the fact that I had unconsciously expected that there was one more step, or one less step, than actually existed.

This led me to the following formulation: when we are surprised by some happening, the surprise is usually due to an unconscious *expectation* that something else would happen."

[32] A detailed analysis of one example of this—the role of optical theories in the use of microscopes—can be found in chapter 11 of Ian Hacking's *Representing and Intervening* (1983).

[33] For instance, interpreting the audible clicks of a Geiger counter as the "observation" of beta particle emission.

[34] Duhem states, with wonderful concision, that *"An Experiment in Physics Is Not Simply the Observation of a Phenomenon; It Is, Besides, the Theoretical Interpretation of This Phenomenon"* (1906, p. 144), and later *"The Theoretical Interpretation of Phenomena Alone Makes Possible the Use of Instruments"* (p. 153).

[35] Kosso writes (2011, p. 22): "The accomplishment of confirmation is achieved to the degree there is coherence between theoretical and empirical information. Each influences the other in a reciprocal way, and the reason to believe the results is not in any single, local agreement. The reason to believe any of it can only be seen in a broad view of widespread coherence among ideas." See section 3.6 above for a criticism of this coherentist view of epistemological justification.

[36] The situation is different in mathematics where interesting results *can* be proved using indirect proofs or proofs by contradiction. A classic example is the Pythagorean proof that $\sqrt{2}$ is an irrational number; it cannot be written as the quotient of two integers. By assuming the negation of this statement a contradiction can be deduced. Turning to empirical science however, the negation of a theory is not itself a theory in any interesting way; it cannot, in any case, be used predictively, and it does not, moreover, rule out any particular empirical occurrence.

[37] That is, the *reductio ad absurdum* may take various forms, as Nicholas Rescher notes (2005, § 1): "In its most general construal, *reductio ad absurdum—reductio* for short—is a process of refutation on grounds that absurd—and patently untenable consequences would ensue from accepting the item at issue. This takes three principal forms according as that untenable consequence is:

1. a self-contradiction (*ad absurdum*)
2. a falsehood (*ad falsum* or even *ad impossible*)
3. an implausibility or anomaly (*ad ridiculum* or *ad incommodum*)

The first of these is *reductio ad absurdum* in its strictest construction and the other two cases involve a rather wider and looser sense of the term."

[38] Rescher goes on to note (2005, § 3): "[t]he use of such *reductio* argumentation was common in Greek mathematics and was also used by philosophers in antiquity and beyond. Aristotle employed it in the *Prior Analytics* to demonstrate the so-called imperfect syllogisms when it had already been used in dialectical contexts by Plato (see *Republic* I, 338C-343A; *Parmenides* 128d). Immanuel Kant's entire discussion of the antinomies in his *Critique of Pure Reason* was based on *reductio* argumentation." Such a historical pedigree accounts for Ryle's pronouncement that the *reductio* is "[a] pattern of argument which is proper and even proprietary to philosophy" (1945, p. 6).

[39] Prof. Rowbottom notes, in his external examiner notes, that "the reductio could be construed as multiple uses of *modus tollens*, or equally one use accompanied with two hypothetical syllogisms."

[40] The other major motivation for the denial of the principle of non-contradiction is the issue of the semantical paradoxes, yet these may, in my view, be easily rendered innocuous in a variety of ways—type and language-level solutions, Zermelo-type solutions, and category solutions, amongst other methods.

[41] Alternatively, that the statement under discussion corresponds to the *facts*, or that what it states to be the case really *is* the case.

[42] This Aristotelian conception of truth can be found in the *Metaphysics,* and states that: "To say of what is that it is not, or of what is not that it is, is false, while to say of what is that it is, or of what is not that it is not, is true" (*Metaphysics,* 1908: 1011b26-9).

[43] Whilst some commentators, such as J.L. Mackie, Herbert Keuth, and Susan Haack have questioned whether Tarski ever presented his theory as a correspondence theory, this view is, I think, invalidated by explicit assertions by Tarski to the contrary. For instance, on the second page of "The Concept of Truth in Formalized Languages", Tarski states, "...throughout this work I shall be concerned exclusively with grasping the intentions which are contained in the so-called *classical* conception of truth ('true-

corresponding with reality') in contrast, for example, with the *utilitarian* conception ('true-in a certain respect useful')" (Tarski, 1983, 153). See Kirkham, 1995, § 5.8 for a useful discussion of this issue.

[44] Speaking of his relationship with Tarski, Popper remarked near the end of his career, "[p]hilosophically, it was the most important friendship of my life." (1990, p. 3). As can be surmised from this comment, the impact of Tarski's theory of truth was, for Popper, truly monumental.

[45] In the same vein, Miller (1994, p. 70) writes that a "drift to relativism is the most common outcome of the clash between the confusion of truth with justified truth and the realization that there can be no justified truth."

[46] Prominent philosophers, such as John McDowell (in his "Physicalism and primitive denotation: Field on Tarski," (1978)) have endorsed this view; Richard Kirkham (1995, Ch. 6) offers a critique of this interpretation.

[47] This had already been known since antiquity however. Alan Musgrave rehearses this sceptical argument, itself a version of Agrippa's trilemma (1993 p. 251-2): "...no general criterion of truth could possibly be known. For suppose someone says: *(C)* A belief's possessing property *C* is an infallible criterion that the belief is true. Sextus Empiricus asked how (C) itself could be known to be true. If you answer that *(C)* itself possesses property C, then you argue in a circle. If you answer that (C) does not possess property C, but some other property *D* which assures you of its truth, then you have admitted that possessing *C* is not an infallible criterion of truth (since *(C)* lacks C yet is true) and an infinite regress opens up when we ask how you know that (C)'s possessing *D* assures you of its truth."

[48] Alan Musgrave, in his *Common Sense, Science and Scepticism* (1993, pp. 249 ff) presents a list of what he calls "subjective-truth theories"; theories, that is, that "define truth as consisting, not of a relation between belief and the outside world, but of some 'internal' property of beliefs... assuming that the believer can know whether his beliefs possess this 'internal' property, the believer can also know the truth... any such theory identifies truth with some subjective property of beliefs" (p. 249). He presents the following list :

1. The self-evidence theory: a belief is true if and only if it is self-evident to me;
2. The indubitability theory: a belief is true if and only if I cannot doubt it;
3. The clear and distinct perception theory: a belief is true if and only if I perceive (conceive) it clearly and distinctly;
4. The coherence theory: a belief is true if and only if it coheres with the rest of my beliefs;
5. The pragmatist theory: a belief is true if and only if I find it useful to have it;
6. The verifiability theory: a belief is true if and only if it is confirmed by my experience;
7. The consensus theory: a belief is true if and only if my intellectual community agrees that it is.

Musgrave notes that the subjective character of such theories is not always acknowledged by their proponents, yet "the antisceptical virtues of these theories (if virtues they be) derive only from such formulations" (p. 250); that is, subjective (and hence relativistic) formulations are *required* to answer the sceptical charge that no criterion of truth exists. Formulations that "go social" or "go ideal" or "go to the long run" "rid subjectivist theories of truth of any anti-sceptical virtues... [they hence] make truth just as inaccessible to human beings as does the common-sense or objective theory of truth" (p. 255).

[49] The key quote here is, once again (Kuhn, 1962 [1970}, p. 171): "Does it really help to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal?" Kuhn's main objection to an objectivist account of truth thus appears to be the lack of a criterion for determining when correspondence has been achieved. Yet the search for truth is not rendered invalid by a lack of a criterion for its achievement, unless justificationist assumptions are already accepted.

[50] Musgrave (1993, p. 252-3) adds that subjective theories of truth "all lead to relativism. They all have the consequence that a statement may be true for me but not for you." In the case of disagreement about practical actions, rational discussion is foreclosed; "force remains the only option… In short, relativism about truth encourages the use of violence to achieve consensus in action (if not in belief)."

[51] Although my focus here is exclusively on the correspondence theory as it pertains to *epistemology* (and specifically the problem of theory adjudication), it should be noted that there is now a burgeoning literature, building upon the pioneering work of Mulligan, Simons and Smith (1984), concerning the *metaphysics* of the correspondence relation. Yet such a metaphysical account is not, I believe, essential to the particular problem I am addressing here. As Musgrave notes, "Tarski shows that we can have an objective or correspondence theory of truth without giving an account of 'the correspondence relation' or of the 'nature of facts'" (1993, p. 263). Further details on this more comprehensive metaphysical project can be found in Simons (1998). MacBride (2013) also provides an impressive overview.

[52] It may be that in stating that "truth is hard to come by" I am being excessively sanguine, especially given the radically sceptical outlook that I have been so far advancing. Darrell Rowbottom (2011, ch. 7), for instance, has argued that, given anti-inductivism, the aim of science (*qua* activity) cannot plausibly be considered the discovery of true (or truth-like) theories. He writes, (p. 124): "…even if our observations and experimental results are reliable, an evolutionary analogy fails to demonstrate why conjecture and refutation should result in: (1) the isolation of true theories; (2) successive generations of theories of increasing truth-likeness; (3) empirically adequate theories; or (4) successive generations of theories of increasing proximity to empirical adequacy (or even structural adequacy)… an evolutionary analogy is only sufficient to defend the notion that the aim of science is to isolate a particular class of false theories, namely, those that are empirically inadequate. The upshot is that (pan) critical rationalists should accept that this is the primary aim of science, given that the evolutionary analogy is apt." This seems to me to be an important clarification of the aim of *empirical criticism*, and also seems to harmonize with my ambivalence about the project of elucidating logically the concept of verisimilitude (see footnote 57 below). Yet I am more uneasy in saying that truth or truth-likeness is not the aim of scientific (or meta-scientific, for that matter) *theorising*. Given my acceptance of all these sceptical theses, this may be merely a difference of nuance in terminology, or perhaps a difference of opinion in how operationally constrained an "aim" must be to be legitimate (truth, in the account given here, is transcendental).

[53] First published in *Problems of Scientific Revolution, Scientific Progress and Obstacles to Progress in the*

Sciences, The Herbert Spencer Lectures 1973, edited by Rom Harre, Clarendon Press, Oxford, 1975, and now in *The Myth of the Framework* (1994).

[54] Similar accounts of desiderata for theoretical progress are presented in chapter 10 of *Conjectures and Refutations* and in chapter 5 of *Objective Knowledge*.

[55] See also Miller's account of theoretical preference in his 1994, Chapter 6, § 2.

[56] I agree with Miller that, properly considered, there are no rational courses of action per se, yet this problem—the so-called *pragmatic problem of induction*—is not the main focus here (rather, my primary focus is on *theory* choice); hence it will not be discussed further.

[57] Here "justified by" should not be understood in any epistemological sense—rather it should be understood only as "deduced or derived from," as the context, I think, makes clear.

[58] Popper (1994, p. 28) states that an even more exacting demand may be made—we "may demand that if the apparent laws of nature should change, then the new theory, invented to explain the new laws, should be able to explain the state of affairs both before and after the change, and also the change itself, from universal laws and (changing) initial conditions." See also Popper's discussion in *The Logic of Scientific Discovery,* section 79.

[59] There is also some affinity here with Kuhn's concept of "scope" (see his 1991).

[60] As Popper notes, "[f]or many purposes, however, comparison by means of the subclass relation does not suffice" (1959, p. 111).

[61] Maxwell's equations, and classical electromagnetism in general, have since been experimentally falsified, and replaced by quantum electrodynamics—a theoretical succession which also instantiates Popper's two requirements for rational progress.

[62] For instance, the Einsteinian reinterpretation of "mass", from being an intrinsic property of an object to being, in relativity theory, a relation, involving relative velocities, between an object and a coordinate system.

[63] For a useful discussion of Bohr's principle of correspondence see Alisa Bokulich's (2010).

[64] For a detailed discussion of the logical comparability of Newton's and Einstein's theories Popper (1994, p. 28) cites in particular a paper by Troels Eggers Hansen: "Confrontation and Objectivity" in the *Danish Yearbook of Philosophy*, 7, 1972, pp. 13-72.

[65] Regarding Popper's ill-fated *definition* of verisimilitude, or approximation to truth, I am inclined to the view that while an objective theory of truth *simpliciter* is essential as a regulative ideal for science, a formal theory of verisimilitude is not. That is, even if the entire project of producing a formal account of verisimilitude was inherently contradictory, and no two false theories could ever be said to differ in their approximation to the truth, this would have *no effect whatsoever* on the falsificationist methodology, which would continue to attempt to eliminate errors, and seek ever more explanatory theories, in the hope of arriving, eventually, at a true account. Popper took a similar stance in the 1982 "Introduction" to his (1983, xxxvi-ii). There he writes: "...a formal definition of verisimilitude is not needed for talking sensibly about it... Why, then, did I try to give a formal definition? ... a new definition is of interest only

if it strengthens a theory. I thought that I could do this with my theory of the aims of science: the theory that science aims at truth *and* the solving of problems of explanation, that is, at theories of greater explanatory power, greater content, and greater testability. The hope further to strengthen this theory of the aims of science by the definition of verisimilitude in terms of truth and of content was, unfortunately, vain. But the widely held view that scrapping this definition weakens my theory is completely baseless... nobody has ever shown that my theory of knowledge, which I developed at least as early as 1933 and which has been growing lustily ever since and which is much used by working scientists, is shaken in the least by this unfortunate mistaken definition, or why the idea of verisimilitude (which is not an essential part of my theory) should not be used further within my theory as an undefined concept."

[66] Included amongst the deductivists in his critique, alongside Popper, are the celebrated statisticians Ronald Fisher (1890- 1962), Jerzy Neyman (1894- 1981), and Egon Pearson (1895-1980).

[67] This fact—that the deductivist methodology is purely negative—is clearly recognised by Quine in his essay "On Popper's Negative Methodology" in Popper's Schlipp volume (1974b, pp. 218-219): "A keynote of Sir Karl's theory of scientific method has been, down the years, his negative doctrine of evidence. Evidence does not serve to support a hypothesis, but only to refute it, when it serves at all... Hempel's paradox thus plagues only the notion of *supporting* evidence... when we talk only of refutation...we tangle with no such paradox, and are not called upon to consider projectibility or natural kinds or natural selection."

[68] This claim is difficult to sustain, as this "fact of fundamental importance" is certainly recognised quite explicitly by critical rationalism.

[69] Popper writes in a footnote (1983, p. 68) "I am uncertain when the above passage was written, but it was almost certainly written before my attention had been drawn by Professor Nelson Goodman to his *Fact, Fiction and Forecast,* 1955... However this may be, my discussion shows that the problem of inductive support raised by Goodman with the help of his new and peculiar predicates ('grue', etc.) can be easily stated without such predicates. Thus the problem of these new predicates, and the problem of their exclusion, appears to me not so much a new riddle, to be solved by a theory of entrenchment, but rather like a red (or perhaps a reen) herring."

Bibliography

Achinstein, Peter

- (1998) "Demarcation Problem" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Agassi, Joseph

- (1959) "Corroboration versus Induction", *British Journal for the Philosophy of Science,* 9, pp. 311-317.
- (1966a) "Sensationalism", *Mind,* 75, pp. 1-24; reprinted in Agassi, *1975,* pp. 92-126.
- (1966b) "The Confusion between Science and Technology in the Standard Philosophies of Science". *Technology and Culture* 7, 3, pp. 348–366. Reprinted in F. Rapp, editor (1974), pp. 40–59. *Contributions to a Philosophy of Technology*. Dordrecht: D. Reidel Publishing Company.
- (1975) *Science in Flux,* Dordrecht: Reidel.
- (1985) *Technology: Philosophical and Social Aspects*. Dordrecht and elsewhere: D. Reidel Publishing Company.
- (2006) "Metaphysics and the Growth of Scientific Knowledge" in Ian Jarvie, Karl Milford, and David Miller, editors, *Karl Popper: A Centenary Assessment. Volume II: Metaphysics and Epistemology*, Ashgate Publishing Company, Aldershot and Burlington VT.
- (2008a) (with Abraham Meidan) *Philosophy from a Skeptical Perspective*, NY and Cambridge: Cambridge University Press, 2008.
- (2008b) *A Philosopher's Apprentice: In Karl Popper's Workshop*. Revised, extended and annotated edition, Amsterdam-New York: Rodopi.

Aikin, S.

- (2005) "Who Is Afraid of Epistemology's Regress Problem," *Philosophical Studies* 126: 191–217.
- (2008) "Meta-epistemology and the Varieties of Epistemic Infinitism," *Synthese* 163: 175–185.
- (2009) "Don't Fear the Regress: Cognitive Values and Epistemic Infinitism," *Think* Autumn 2009: 55–61.
- (2011) *Epistemology and the Regress Problem*, Routledge.

Ainslie, D. C.

- (2003) "Hume's Scepticism and Ancient Scepticisms," in *Hellenistic and Early Modern Philosophy*, J. Miller and B. Inwood (eds.), Cambridge: Cambridge University Press, pp. 251–73.

Albert, Hans

- (1968) *Traktat uber kritische Vernunft.* Tubingen: J. C. B. Mohr. Translated into English as *Treatise on Critical Reason* (Princeton: Princeton University Press, 1985).

Annas, Julia and Barnes, Jonathan

- (1985) *The Modes of Scepticism: Ancient Texts and Modern Interpretations*, Cambridge: Cambridge University Press.

Antonelli, G. Aldo

- (1998) "Definitions" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Armstrong, David M.

- (1973) *Belief, Truth and Knowledge,* Cambridge: University Press.

Audi, Robert

- (1995) editor, *The Cambridge Dictionary of Philosophy*. Cambridge University Press, Cambridge, New York, and Melbourne.
- (1998) "Reasons for Belief" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998)

Austin, J. L.

- (1946) "Other Minds", *Aristotelian Society,* supplementary vol. xx; reprinted in Austin, (1961), pp. 44-84.
- (1961) *Philosophical Papers,* Oxford: Clarendon Press.
- (1974) *Sense and Sensibilia,* reconstructed from manuscript notes by G.J. Warnock, Oxford: Clarendon Press.

Ayer, Alfred J.

- (1936) *Language, Truth, and Logic,* New York: Dover, 2nd edn, 1946.
- (1956) *The Problem of Knowledge,* Harmondsworth: Penguin.
- (1959) *Logical Positivism*. Glencoe, IL: Free Press.
- (1972) *Probability and Evidence*, London: Macmillan.

Bacon, Francis

- (1620) *Novum Organum* ed. Lisa Jardine and Michael Silverthorne, Cambridge University Press 2000 References (e.g. II, v) are to book and numbered aphorism.

Baghramian, Maria

- (2004) *Relativism,* London: Routledge.

Bailey, A.

- (2002) *Sextus Empiricus and Pyrrhonean Scepticism*, Oxford: Oxford University Press.

Bar-Am, Nimrod

- (2008) *Extensionalism: The Revolution in Logic,* Berlin: Springer.

Bar-Hillel, Yehoshua

- (1965) (ed.), *Logic, Methodology and Philosophy of Science*. (Proceedings of the 1964 International Congress for Logic, Methodology, and Philosophy of Science), Amsterdam: North Holland.
- (1974) "Popper's Theory of Corroboration", in Schilpp (ed.), (1974), pp. 332-348.

Barnes, Jonathan

- (1990) *The Toils of Scepticism*, Cambridge: Cambridge University Press.
- (1998) "Arcesilaus (c.316-c.240 BC)" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Bartley, William W., III

- (1962) *The Retreat to Commitment*. London: Chatto and Windus. 2nd revised edition (1984) La Salle and London: Open Court Publishing Company.
- (1964) "Rationality versus the Theory of Rationality." In *The Critical Approach to Science and Philosophy*, edited by Mario Bunge, 3–31. New York: The Free Press, 1964.
- (1968) "Theories of Demarcation between Science and Metaphysics", in Lakatos and Musgrave (eds.), (1968), pp. 40- 64.
- (1984) Expanded edition of (1962), with several new appendices.
- (1987a) *Theories of Rationality* in Gerard Radnitzky and W. W. Bartley, III (eds.), *Evolutionary Epistemology, Rationality, and the Sociology of Knowledge,* Open Court, La Salle, Illinois.
- (1987b) *A Refutation of the Alleged Refutation of Comprehensively Critical Rationalism* in Gerard Radnitzky and W. W. Bartley, III (eds.), *Evolutionary Epistemology, Rationality, and the Sociology of Knowledge,* Open Court, La Salle, Illinois.

Basalla, G.

- (1988) *The Evolution of Technology*. Cambridge and elsewhere: Cambridge University Press.

Bentham, Jeremy

- (1789) *An Introduction to the Principles of Morals and Legislation,* in *The Collected Works of Jeremy Bentham: An Introduction to the Principles of Morals and Legislation.* (1970) Ed. J. H. Burns and H. L. A. Hart. London: Athlone Press.

Bernoulli, James

- (1715) *Ars Conjectandi*, Basle.

Bett, R.

- (2000) *Pyrrho, his Antecedents, and his Legacy*, Oxford: Oxford University Press.
- (2002) "Pyrrho," *The Stanford Encyclopedia of Philosophy* (Summer 2006 edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/sum2006/entries/pyrrho/>.

Bird, Alexander

- (1998) *Philosophy of Science*. London: UCL Press.

Black, Max

- (1950) (ed.), *Philosophical Analysis,* Cornell: University Press.
- (1954) (ed.), *Problems of Analysis,* London: Routledge and Kegan Paul.
- (1954) ""Pragmatic" Justifications of Induction", in Black (ed.), (1954), pp. 157-208.
- (1958) "Self-Supporting Inductive Arguments", reprinted in Black, (1962), pp. 209-218.
- (1962) *Models and Metaphors,* Cornell: University Press.
- (1966) "The Raison d'Etre of Inductive Argument", *British Journal for the Philosophy of Science,* 17, pp. 177-204.

Biondi, Paolo.

- (2004) *Aristotle: Posterior Analytics II.19*. Quebec, Q.C.: Les Presses de l'Universite Laval, 2004.

Bloor, D.

- (1992) *Knowledge and Social Imagery*, Chicago, IL: University of Chicago Press, 2nd edn.

Bokulich, Alisa

- (2010) "Bohr's Correspondence Principle" in *The Stanford Encyclopedia of Philosophy* (Spring 2013 Edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/entries/bohr-correspondence/>.

Bonjour, Lawrence

- (1978) "Can Empirical Knowledge Have a Foundation?" *American Philosophical Quarterly*, 15

(1): 1–13.

- (1985) *The Structure of Empirical Knowledge*, Cambridge, MA: Harvard University Press.

Bovens, L, and Hartmann, S.,

- (2003) *Bayesian Epistemology*, Oxford: Clarendon Press.

Bowell, T. and Kemp, G.

- (2002) *Critical Thinking. A Concise Guide.* London and New York: Routledge.

Broad, C. D.

- (1952) "The Philosophy of Francis Bacon", in *Ethics and the History of Philosophy*, London: Routledge and Kegan Paul.

Bromberger, Sylvain

- (1966) "Why-Questions", in R. Colodny (ed.) *Mind and Cosmos*, Pittsburgh, PA: University of Pittsburgh Press.

Bunnin, Nicholas and Yu, Jiyuan

- (2004) *The Blackwell Dictionary of Western Philosophy,* Oxford: Blackwell Publishing.

Campbell, D. T.

- (1988) "Evolutionary Epistemology", in *Methodology and Epistemology for Social Sciences: Selected Essays.* Chicago: University of Chicago Press, pp. 393–434. (Originally published in P. A. Schilpp (ed.), *The Philosophy of Karl Popper.* La Salle, IL: Open Court, pp. 413–63.)

Carnap, Rudolf

- (1928) *Der logische Aufbau der Welt,* Berlin-Schlachtensee: Weltkreis-Verlag; trans. R. George, The Logical Structure of the World, Berkeley, CA: University of California, 1967.
- (1928b) *"Scheinprobleme in der Philosophie"*; English translation in Carnap, (1967), pp. 301-343.
- (1932a) "Überwindung der Metaphysik durch logische Analyse der Sprache", *Erkenntnis* 2; trans. A. Pap, "The Elimination of Metaphysics Through the Logical Analysis of Language", in A.J. Ayer (ed.) *Logical Positivism*, Glencoe, IL: Free Press, 1959, 60-81.
- (1932b) 'Die physikalische Sprache as Universalsprache der Wissenschaft' (Physical Language as the Universal Language of Science), Erkenntnis 2: 432-65; trans. M. Black as *The Unity of Science*, London: Kegan Paul, Trench, Trubner and Co., 1934.
- (1934) *Logische Syntax der Sprache*, Vienna: Springer; trans. A. Smeaton, *The Logical Syntax of Language*, London: Kegan Paul, Trench, Trubner and Co., 1937.

- (1936) "Testability and Meaning". *Philosophy of Science,* 3 (1936), 419-71; and 4 (1937), 1-40; reprinted (with omissions) on pp. 47-92 of Herbert Feigl and May Brodbeck, eds., *Readings in the Philosophy of Science* (New York: Appleton-Century-Crofts, 1953).
- (1950a) *Logical Foundations of Probability,* London: Routledge and Kegan Paul. 2nd edition 1962.
- (1950b) 'Empiricism, Semantics, and Ontology', *Revue internationale de philosophie* 4: 20-40.
- (1952) *The Continuum of Inductive Methods,* Chicago IL: University of Chicago Press.
- (1963) 'Intellectual Autobiography' and 'Replies and Systematic Expositions', *The Philosophy of Rudolf Carnap*, ed. P.A. Schilpp, La Salle, IL: Open Court, 3-84, 859-1013.
- (1966) "Probability and Content Measure", in P. K. Feyerabend and G. Maxwell, eds, *Mind, Matter, and Method.* Minneapolis MN: University of Minnesota Press, pp. 248-60.
- (1967) *The Logical Structure of the World and Pseudoproblems in Philosophy,* English translation of Carnap, *1928* and 1928 by Rolf A. George, London: Routledge and Kegan Paul.
- (1968a) "The Concept of Constituent-Structure", in Lakatos (ed.), 1968, pp. 218-220.
- (1968b) "Inductive Logic and Inductive Intuition", in Lakatos (ed.), 1968, pp. 258-267.
- (1971, 1980) "A Basic System of Inductive Logic", *Studies in Inductive Logic and Probability,* vol. 1, ed. R. Carnap and R. Jeffrey, vol. 2, ed. R. Jeffrey, Berkeley, CA: University of California Press, 33-165, 7-155.

Carnap, R., Hahn, H. and Neurath, O.

- (1929) *Wissenschaftliche Weltauffassung (The Scientific Conception of the World)*, Vienna: Wolf. in Neurath, 1975, pp. 299-318.

Cherniak, Christopher

- (1986) *Minimal Rationality*. Cambridge, Mass.: MIT Press.
- (1998) "Rational Beliefs" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Chisholm, Roderick

- (1966) *Theory of Knowledge;* 3rd ed., Englewood Cliffs, N.J.: Prentice Hall, 1989.

Cicero

- (45 B.C.) *Academics*, trans. H. Rackham, Loeb Classical Library, Cambridge, MA: Harvard University Press and London: Heinemann, 1933.

Cleve, J. van.

- (1984) "Reliability, Justification and Induction", in P. A. French, T. E. Uehling, and H. K. Wettstein (eds.), *Causation and Causal Theories*, Midwest Studies in Philosophy, 4: 555–67.

- (2011) "Can Coherence Generate Warrant Ex Nihilo? Probability and the Logic of Concurring Witnesses", *Philosophy and Phenomenological Research*, 82 (2): 337–380.

Clifford, W. K.

- (1879) "The Ethics of Belief." *Lectures and Essays*. London: Macmillan.

Cohen, L. Jonathan, and Hesse, Mary B.

- (1980) (eds.), *Applications of Inductive Logic,* Oxford: University Press.

Cohen, Robert S., Feyerabend, Paul K. and Wartofsky, Marx W.

- (1976) (eds.), *Essays in Memory of Imre Lakatos,* Dordrecht: Reidel.

Collins, H. and Pinch, T.

- (1993) *The Golem*, Cambridge: Cambridge University Press.

Copi, Irving and Cohen, Carl

- (1990) *Introduction to Logic* (8th edition), Macmillan.

da Costa, N. C. A. and French, S. R. D.

- (2003) *Science and Partial Truth.* Oxford and elsewhere: Oxford University Press.

Couissin, P.

- (1983) "The Stoicism of the New Academy," in Burnyeat (ed.) 1983, pp. 31–63. Translation of Couissin 1929, "Le stoicisme de la nouvelle Academie," *Revue d'historie de la philosophie*, 3: 241–76.

Dancy, J.

- (1985) *Introduction to Contemporary Epistemology*, Blackwell.

Davidson, D.

- (1990) "A Coherence Theory of Truth and Knowledge," in *Reading Rorty*, ed. A. Malachowski (Cambridge, MA: Blackwell, 1990), 120–38.

Descartes, Rene

- (1620-c.28) *Regulae ad directionem ingenii (Rules for the Direction of the Mind)*, in vol. 1 of *The Philosophical Writings of Descartes*, ed. and trans. J. Cottingham, R. Stoothoff, D. Murdoch and A. Kenny, Cambridge: Cambridge University Press, 1984-91.
- (1637) *Discours de la méthode pour bien conduir sa raison et chercher la vérité dans les*

sciences plus la dioptrique, les meteores, et la geometrie, qui sont des essais de cete methode (Discourse on the Method for Properly Conducting Reason and Searching for Truth in the Sciences, as well as the Dioptrics, the Meteors, and the Geometry, which are essays in this method), in vol. 1 of *The Philosophical Writings of Descartes*, ed. and trans. J. Cottingham, R. Stoothoff, D. Murdoch and A. Kenny, Cambridge: Cambridge University Press, 1984-91.

- (1641) *Meditations on First Philosophy*, in E.S. Haldane and G.R.T. Ross (eds) The Philosophical Works of Descartes, vol. 1, Mineola, NY: Dover Publications, 1955.

Dorling, Jon and David Miller

- (1981) "Bayesian Personalism, Falsificationism, and the Problem of Induction", *Proceedings of the Aristotelian Society,* Supplementary Volumes, Vol. 55 (1981), pp. 109-125+127-141.

Downes, Stephen M.

- (1998) "Constructivism" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Duhem, Pierre

- (1906) *La Théorie Physique: Son Objet, Sa Structure;* 2nd French ed., tr. by Philip P. Wiener, as *The Aim and Structure of Physical Theory,* Princeton: University Press, 1954.

Earman, John

- (1993) 'Carnap, Kuhn, and the Philosophy of Scientific Methodology', in P. Horwich (ed.) *World Changes: Thomas Kuhn and the Nature of Science,* Cambridge, MA:MIT Press: 9–36.

Eintalu, J.

- (2001) *The Problem of Induction: The Presuppositions Revisited.* Tartu: Tartu University Press.

Elgin, C.

- (2005) "Non-foundationalist Epistemology: Holism, Coherence, and Tenability," in *Contemporary Debates in Epistemology*, ed. M. Steup and E. Sosa (Malden, MA: Blackwell, 2005), 156– 67.

Evans-Pritchard, Edward E.

- (1937) *Witchcraft and Magic Among the Azande,* Oxford: Clarendon Press.

Fisher, A.

- (1988) *The Logic of Real Arguments.* Cambridge and elsewhere: Cambridge University Press.
- (2001) *Critical Thinking. An Introduction.* Cambridge and elsewhere: Cambridge University

Press.

Feigl, Herbert

- (1929) *Theorie und Erfahrung in der Physik (Theory and Experience in Physics).*
- (1950) "De Principiis non disputandum... ?" , in Black (ed.), 1950, pp. 113-147.

Feigl, Herbert, and Maxwell, Grover

- (1961) (eds.), *Current Issues in the Philosophy of Science,* New York: Holt, Rinehart and Winston.

Feigl, Herbert, Scriven, Michael, and Maxwell, Grover

- (1958) (eds.), *Minnesota Studies in the Philosophy of Science,* vol. 2, Minneapolis: University of Minnesota Press.

Feldman, Richard and Earl Conee

- (1985) "Evidentialism", *Philosophical Studies* (Minneapolis), 48:1 (1985: July) p.15-34

Fetzer, J. H.

- (1981) *Scientific Knowledge. Causation, Explanation, and Corroboration.* Dordrecht, Boston MA, and London: D. Reidel Publishing Company.

Feyerabend, Paul. K.

- (1963) "How to be a Good Empiricist—A Plea for Tolerance in Matters Epistemological". In B. Baumrin, editor (1963), pp. 3–39. *Philosophy of Science: The Delaware Seminar,* Volume 2. New York NY: Interscience Publications.
- (1975a) *Against Method.* London: New Left Books.
- (1975b) "How to Defend Society against Science". *Radical Philosophy*, 11, Summer 1975, pp. 4–8. Reprinted in I. Hacking, editor (1981), pp. 156–167. *Scientific Revolutions.* Oxford and New York: Oxford University Press.

Feynman, Richard P.

- (1992) *The Character of Physical Law.* Penguin Edition, 1992.

Field, Hartry

- (1972) "Tarski's Theory of Truth". *Journal of Philosophy* LXIX, pp. 347-375. Page references are to the reprint in Platts (1980), pp. 83-110.

Fine, T. L.

- (1973) *Theories of Probability*. New York and London: Academic Press.

de Finetti, Bruno.

- (1937) "La Prévision: ses lois logiques, ses sources subjectives," in *Annales de l'Institut Henri Poincaré* 7 (1937), 1–68; trans. as "Foresight: Its Logical Laws, Its Subjective Sources," in *Studies in Subjective Probability* ed. H. E. Kyburg, Jr and H. Smokler (New York: Kreiger, 1980).
- (1982) "Probability and My Life", in *The Making of Statisticians,* edited by J Gani, New York: Springer.
- (2008) *Philosophical Lectures on Probability,* trans. Hykel Hosni; edited, and annotated by Alberto Mura, with an Introductory Essay by Maria Carla Galavotti. Springer.

Fitelson, B.

- (1999) "The Plurality of Bayesian Measures of Confirmation and the Problem of Measure Sensitivity", *Philosophy of Science* 66(3), 362-78.
- (2007) "Likelihoodism, Bayesianism, and Relational Confirmation", *Synthese* 156(3), 473-89.

Fogelin, Robert

- (1981) "Wittgenstein and Classical Skepticism." *International Philosophical Quarterly* 21, no. 1:3-15.
- (1994) *Pyrrhonian Reflections on Knowledge and Justification*, New York Oxford University Press.

Foley, Richard

- (1987) *The Theory of Epistemic Rationality,* Cambridge, MA: Harvard University Press.
- (1998) "Justification, epistemic" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Fraassen, B. C. van

- (1995) "Fine-grained Opinion, Probability, and the Logic of Full Belief", *Journal of Philosophical Logic* 24(4), 349-77.

Friedman, Michael

- (1987) "Carnap's *Aufbau* Reconsidered", *Nous,* 21, 1987, pp. 521–545; reprinted in Friedman (1999), pp. 89–113.
- (1991) "The Re-evaluation of Logical Positivism", *The Journal of Philosophy,* 88, 1991, pp. 505–519; reprinted as "Introduction" in Friedman (1999), pp. 1–14.
- (1999) *Reconsidering Logical Positivism*, Cambridge: Cambridge University Press, 1999.

Fuller, Steven

- (2004) *Popper vs. Kuhn: The Struggle for the Soul of Science*. New York: Columbia University Press.

Galavotti, Maria Carla

- (2003) "Kinds of Probabilism." In P. Parrini, W. C. Salmon, and M. H. Salmon (eds.), Logical Empiricism: Historical and Contemporary Perspectives. Pittsburgh: University of Pittsburgh Press, 281–303.
- (2005) *A Philosophical Introduction to Probability*. Stanford CA: Center for the Study of Logic and Information.
- (2007) "Confirmation, Probability, and Logical Empiricism" in Alan Richardson and Thomas Uebel (eds.), *The Cambridge Companion to Logical Empiricism*, Cambridge University Press.
- (2008) "Introductory Essay" in de Finetti, (2008).

Garber, Daniel

- (1998) "Descartes, René (1596-1650)" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Gattei, Stefano

- (2006) "Rationality Without Foundations". In Jarvie, Milford, and Miller (2006a), pp. 131–144.
- (2008a) *Karl Popper's Philosophy of Science. Rationality Without Foundations*. New York and London: Routledge.
- (2008b) *Thomas Kuhn's "Linguistic Turn" and the Legacy of Logical Empiricism: Incommensurability, Rationality and the Search for Truth,* Aldershot and Burlington VT: Ashgate.

Gettier, Edmund

- (1963) "Is Justified True Belief Knowledge?" in *Analysis 21:* 121-123, 1963.

Gillies, Donald. A.

- (1995) "Popper's Contribution to the Philosophy of Probability", in A. O"Hear, ed., *Karl Popper: Philosophy and Problems*. Cambridge and elsewhere: Cambridge University Press, pp. 103-20.
- (1998) "Confirmation Theory." In D.M. Gabbay and P. Smets (eds.), *Handbook of Defeasible Reasoning and Uncertainty Management Systems*, vol. 1. Dordrecht: Kluwer, 135–67.
- (2000) *Philosophical Theories of Probability*. London: Routledge.

Goldman, Alvin

- (1967) "A Causal Theory of Knowing", *Journal of Philosophy* 64, no. 12: 355-72.

Good, Irving John

- (1950) *Probability and the Weighing of Evidence*. New York: Hafner Press.
- (1952) "Rational Decisions," *Journal of the Royal Statistical Society B* 14: 107–114.
- (1959) "Kinds of Probability," *Science* 129: 443–447.
- (1965) *The Estimation of Probabilities: An Essay on Modern Bayesian Methods*. Cambridge, MA: MIT Press.
- (1971) "46656 varieties of Bayesians," *American Statistician* 25: 62–63.
- (1975) "Explicativity, Corroboration, and the Relative Odds of Hypotheses". *Synthese* 30, pp. 39-73.
- (1983) *Good Thinking,* Minneapolis: University of Minnesota Press.

Goodman, Nelson

- (1946) "The New Riddle of Induction" in *Fact, Fiction and Forecast* (1954).
- (1947) "The Problem of Counterfactual Conditionals", *Journal of Philosophy,* February 1947; reprinted in Goodman, (1954), pp. 13-34.
- (1954) *Fact, Fiction and Forecast,* London: Athlone Press.

Govier, T.

- (1999) *The Philosophy of Argument.* Newport News VA: Vale Press.
- (2001) *A Practical Study of Argument.* 5th edition. Belmont CA: Wadsworth/ Thomson Learning.

Graves, John C.

- (1974) "Uniformity and Induction", *British Journal for the Philosophy of Science,* 25, pp. 301-318.

Grayling, A.C.

- (2008) *Ideas That Matter: The Concepts That Shape The 21st Century,* NewYork: Basic Books

Greco, John

- (2007) "Epistemology" in *The Edinburgh Companion to Twentieth-Century Philosophies,* Edited by Constantin V. Boundas, Edinburgh University Press.

Groarke, Louis F.

- (2009) *An Aristotelian Account of Induction: Creating Something From Nothing.* Montreal and Kingston: McGill-Queen's University Press, 2009.
- (2011) "Aristotle: Logic" in *The Internet Encyclopedia of Philosophy* URL = <http://www.iep.utm.edu/aris-log/>.

Grünbaum, Adolf

- (1960) "The Duhemian Argument", *Philosophy of Science,* 27, pp. 75-87.
- (1971) "Can We Ascertain the Falsity of a Scientific Hypothesis?" in Mandelbaum (ed.), 1977, pp. 69-129.
- (1976) "Is Falsifiability the Touchstone of Scientific Rationality? Karl Popper versus Inductivism", in Cohen, Feyerabend, and Wartofsky (eds.), 1976, pp. 213-250.
- (1983) "Can Psychoanalytic Theory be Cogently Tested "on the Couch"?", in Laudan (ed.), *1983,* pp. 143-309.
- (1989) "The Degeneration of Popper"s Theory of Demarcation". In F. D'Agostino and I. C. Jarvie, editors (1989), pp. 141–161. *Freedom and Rationality. Essays in Honor of John Watkins.* Dordrecht, Boston MA, and London: Kluwer.

Haack, Susan W.

- (1976) "Is it True What They Say About Tarski?", *Philosophy, 51,* pp. 323-36.
- (1978a) "The Wheel and Beyond". T*he British Journal for the Philosophy of Science* 29, 2, pp. 185–188.
- (1978b) *Philosophy of Logics.* Cambridge: Cambridge University Press.
- (1979) "Epistemology with a Knowing Subject". *The Review of Metaphysics* 33, 2, pp. 309–335.
- (2005) "Trial and Error : The Supreme Court's Philosophy of Science", *American Journal of Public Health* 95 (S1) S66–S73. URL = <http://www.defendingscience.org/upload/HaackSCPHILOSOPHY.pdf>.
- (2009) "Six Signs of Scientism" available online at: http://pervegalit.files.wordpress.com/2011/03/haack-six-signs-of-scientism-october-17-2009.pdf

Hacking, Ian

- (1983) *Representing and Intervening,* Cambridge: Cambridge University Press.

Hamlyn, David

- (1970) *The Theory of Knowledge,* London: Macmillan.

Hankinson, R.J.

- (1994) *The Sceptics*, London: Routledge.
- (1998a) "Aenesidemus (1st century BC)" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).
- (1998b) "Agrippa (1st/2nd century AD)" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).
- (1998c) "Pyrrhonism" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and

New York: Routledge (1998).

- (1998d) "Sextus Empiricus" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Hanson, Norwood Russell.

- (1958) *Patterns of Discovery*, Cambridge, Cambridge University Press.

Harsanyi, John C.

- (1985) "Acceptance of Empirical Statements: A Bayesian Theory Without Cognitive Utilities," *Theory* and *Decision*, 18 (1985), 1-30.

Hatfield, H. S.

- (1948) *The Inventor and His World.* 2nd edition. West Drayton and New York: Pelican Books. First published 1933.

Hawking, Stephen

- (2001) *The Universe in a Nutshell*, London: Bantam Press.

Hempel, Carl Gustav

- (1945) "Studies in the Logic of Confirmation", *Mind,* 54, pp. 1-26 and 97-121; reprinted in Hempel, *1965,* pp. 3-46.
- (1960) "Inductive Inconsistencies", *Synthese* 12(4), 439-69.
- (1965) *Aspects of Scientific Explanation,* New York: Free Press.
- (1966) *Philosophy of Natural Science,* Englewood Cliffs, N.J.: Prentice Hall.

Hesse, Mary

- (1974) *The Structure of Scientific Inference,* London: Macmillan.

Horn, A., and Tarski, A.

- (1948) 'Measures in Boolean Algebras', *Transactions of the American Mathematical Society,* 64: 467–97.

Hosiasson-Lindenbaum, Janina

- (1940) "On Confirmation," *Journal of Symbolic Logic* 5: 133–148.

Howson, Colin

- (1973) "Must the Logical Probability of Laws be Zero?", *The British Journal for the Philosophy of Science* 24(2), 153-163.

- (1987) "Popper, Prior Probabilities and Inductive Inference," *The British Journal for Philosophy of Science* 38: 207–224.
- (2000) *Hume's Problem: Induction and the Justification of Belief.* Oxford: Clarendon Press.
- (2004) "Popper"s Solution to the Problem of Induction". The Philosophical Quarterly 34, 135, pp. 143–147

Howson, Colin and Franklin, A.

- (1985) "Bayesian Analysis of Excess Content and the Localisation of Support", *The British Journal for the Philosophy of Science*, 36: 425–36.

Howson, Colin and Urbach, Peter

- (1989) *Scientific Reasoning. The Bayesian Approach.* Chicago and La Salle: Open Court Publishing Company. 2nd edition, 1993.

Huemer, Micheal

- (1997) "Probability and Coherence Justification," *Southern Journal of Philosophy*, 35: 463–472.
- (2010) "Foundations and Coherence" in *A Companion to Epistemology,* Second Edition edited by J. Dancy, E. Sosa, and M, Steup, Oxford: Blackwell Publishing Ltd.

Hume, David

- (1738-40) *A Treatise of Human Nature*, 2nd edn, ed. L.A. Selby-Bigge, revised P.H. Nidditch, Oxford: Clarendon Press, 1978.
- (1740) *An Abstract of a Book Lately Published, Entitled A Treatise of Human Nature, Etc.* Page references are to the edition of L. Selby-Bigge, 1888, 1978. Oxford: Clarendon Press.
- (1748) *An Enquiry Concerning Human Understanding.* Page references are to the edition of L. Selby-Bigge, 1893, 1975. Oxford: Clarendon Press.

Inwood, Brad

- (1985) *Ethics and Human Action in Early Stoicism* Oxford: Oxford University Press.

Irzik, Gürol and T. Grünberg

- (1995) "Carnap and Kuhn: Arch Enemies or Close Allies?" *British Journal for the Philosophy of Science* 46, 285–307.

Jarvie, I. C., Milford, K. M., and Miller, D. W., editors

- (2006) *Karl Popper: A Centenary Assessment. Volume I: Life and Times, and Values in a World of Facts; Volume II: Metaphysics and Epistemology; Volume III: Science.* Aldershot and Burlington VT: Ashgate.

Jaynes, E. T.

- (2003) *Probability Theory: The Logic of Science*. Cambridge: Cambridge University Press.

Jeffrey, Richard C.

- (1965) *The Logic of Decision,* New York: McGraw Hill.
- (1968) "Probable Knowledge", in Lakatos (ed.), *1968,* pp. 166-190.
- (1975a) "Probability and Falsification. Critique of the Popper Program", *Synthese* 30(1/2), 95-117.
- (1975b). "Replies", *Synthese* 30(1/2), 149-57.

Jeffreys, Harold

- (1939) *Theory of Probability,* Oxford: Clarendon Press. 3rd rev. ed. 1961
- (1957) *Scientific Inference,* Cambridge: University Press.

Jeffreys, Harold, and Wrinch, Dorothy

- (1921) "On Certain Fundamental Principles of Scientific Enquiry", *Philosophical Magazine,* 42, pp. 269-298.

Jones, H. M.

- (1997) *An Introduction to Critical Thinking.* Wentworth Falls NSW: Social Science Press.

Kant, Immanuel

- (1781/1787) *Critique of Pure Reason*, trans. N. Kemp Smith, London: Macmillan, 1963.
- (1783 [1997]) *Prolegomena to Any Future Metaphysics, with selections from the Critique of Pure Reason*, ed. and trans. G. Hatfield, Cambridge: Cambridge University Press.

Kaplan, Mark

- (1999) "Induction, epistemic issues in" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Keuth, H.

- (2005) *The Philosophy of Karl Popper* Cambridge and elsewhere: Cambridge University Press.

Keynes, J. M.

- (1921) *A Treatise on Probability.* London: Macmillan and Co. Ltd.

Kim, Jaegwon

- (1988) “What is Naturalized Epistemology?”. In J. E. Tomberlin, editor (1988), pp. 381-405. *Philosophical Perspectives 2: Epistemology.* Atascadero: Ridgeview Publishing Company.

Kirkham, Richard

- (1995) *Theories of Truth: A Critical Introduction* Cambridge, Mass: MIT Press

Klein, Peter

- (1998) “Certainty” in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998)
- (1999) “Human Knowledge and the Infinite Regress of Reasons” *Philosophical Perspectives* 13, 297–325.
- (2005) “Infinitism is the Solution to the Regress Problem,” in M. Steup and E. Sosa, eds., *Contemporary Debates in Epistemology*, Blackwell.
- (2010a) “Epistemological Self-profile” In *A Companion to Epistemology,* Second Edition edited by Dancy, Sosa, and Steup Oxford: Blackwell Publishing Ltd
- (2010b) "Skepticism", *The Stanford Encyclopedia of Philosophy* (Summer 2013 Edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/sum2013/entries/skepticism/>.
- (2012) “Infinitism and the Epistemic Regress Problem,” in S. Toldsdorf, ed., *Conceptions of Knowledge*, de Gruyter.

Klein, P., and Warfield, T. A.

- (1994) “What Price Coherence?,” *Analysis*, 54: 129–132.
- (1996) “No Help for the Coherentist”, *Analysis*, 56: 118–121.

Kneale, W. C.

- (1949) *Probability and Induction*. Oxford: Clarendon Press.

Kosso, Peter

- (2011) *A Summary of Scientific Method,* Spinger:London.

Kuhn, Thomas. S.

- (1957) *The Copernican Revolution. Planetary Astronomy in the Development of Western Thought*, Cambridge, Massachusetts–London: Harvard University Press, 1957, 1985.
- (1962) *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press. 2nd edition 1970.
- (1970) “Logic of Discovery or Psychology of Research?” In *Criticism and the Growth of Knowledge*, edited by Imre Lakatos and Alan E. Musgrave, 1–23. Cambridge: Cambridge University Press, 1970.

- (1974) "Second Thoughts on Paradigms" in *The Structure of Scientific Theories,* edited by Frederick Suppe, 459–482. Urbana-Chicago-London: University of Illinois Press, 1974, 1977.
- (1977) *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: University of Chicago Press.
- (1989) "Possible Worlds in History of Science." In *Possible Worlds in Humanities, Arts and Sciences,* edited by Sture Allén, 9–32. Berlin: Walter de Gruyter, 1989. Reprinted in Kuhn (2000).
- (1991) "The Road Since Structure." In *PSA 1990. Proceedings of the 1990 Biennial Meeting of the Philosophy of Science Association*, vol. 2, edited by Arthur Fine, Micky Forbes, Linda Wessels, 3–13. East Lansing, Michigan: Philosophy of Science Association, 1991. Reprinted in Kuhn (2000).
- (1992) *The Trouble with the Historical Philosophy of Science, An Occasional Publication of the Department of the History of Science*. Cambridge, Massachusetts: Harvard University, 1992. Reprinted in Kuhn (2000).
- (1993) "Afterwords." In *World Changes: Thomas Kuhn and the Nature of Science*, edited by Paul Horwich, 311–341. Cambridge, Massachusetts- London: The MIT Press, 1993. Reprinted in Kuhn (2000).
- (1997) "'A Physicist who Became a Historian for Philosophical Purposes': A Discussion Between Thomas S. Kuhn and Aristides Baltas, Kostas Gavroglu, Vasso Kindi." *Neusis* 6 (1997): 145–200. Reprinted as "Discussion with Thomas S. Kuhn" in Kuhn (2000).
- (2000) *The Road Since* Structure. *Philosophical Essays, 1970–1993, with an Autobiographical Interview,* edited by James Conant and John Haugeland, Chicago–London: University of Chicago Press, 2000.

Kvanvig, J.

- (2003a) *The Value of Knowledge and the Pursuit of Understanding*, Cambridge: Cambridge University Press.
- (2003b) "Coherentist Theories of Epistemic Justification," in *Stanford Encyclopedia of Philosophy*, ed. E. Zalta, Winter 2003 edition: <http://plato.stanford.edu/archives/win2003/entries/justep-coherence/>.

Kyburg, Henry E., Jr.

- (1965) "Probability, Rationality and a Rule of Detachment", in Bar-Hillel (ed.), 1965, pp. 301-310.
- (1968) "The Rule of Detachment in Inductive Logic", in Lakatos (ed.), *1968,* pp. 98-119.
- (1974) "Propensities and Probabilities", *The British Journal for the Philosophy of Science* 25(4), 358-75.

Kyburg, Henry E., Jr., and Nagel, Ernest

- (1965) (eds.), *Induction: Some Current Issues,* Middletown, Conn.: Wesleyan University Press.

Lakatos, Imre

- (1968a) "Changes in the Problem of Inductive Logic". In I. Lakatos, editor, *The Problem of Inductive Logic*, pp. 315–417. Amsterdam and elsewhere: North-Holland Publishing Company. Reprinted in I. Lakatos (1978b)
- (1968b) (ed.), *The Problem of Inductive Logic,* Amsterdam: North Holland.
- (1970) "Falsification and the methodology of scientific research programmes", in Lakatos and Musgrave (eds), 1970.
- (1973) "Science and Pseudoscience". Open University broadcast, 30.vi.1973. Reprinted in Lakatos (1978a), pp. 1–7.
- (1974) "Popper on Demarcation and Induction". In Schilpp (1974), pp. 241–273. Reprinted in Lakatos (1978), pp. 139–167.
- (1978a) *The Methodology of Scientific Research Programmes. Philosophical Papers, Volume 1*, pp. 139–167. Cambridge and elsewhere: Cambridge University Press.
- (1978b) *Mathematics, Science, and Epistemology. Philosophical Papers, Volume 2*, pp. 128–200. Cambridge and elsewhere: Cambridge University Press.

Lakatos, Imre, and Musgrave, Alan

- (1965) (eds.), *Problems in the Philosophy of Science,* Amsterdam: North Holland.
- (1970) (eds.), *Criticism and the Growth of Knowledge,* Cambridge: University Press.

Latour, B. And Woolgar, S.

- (1986) *Laboratory Life*, Princeton, NJ: Princeton University Press.

Laudan, Larry

- (1983) "The Demise of the Demarcation Problem". In R. S. Cohen and L. Laudan, editors (1983) pp. 111–127. *Physics, Philosophy and Psychoanalysis. Essays in Honor of Adolf Grünbaum.* Dordrecht, Boston MA, and Lancaster PA: Reidel.

Lehrer, K.

- (1974) *Knowledge,* Oxford: Clarendon Press.
- (2000) *Theory of Knowledge*, second edition, Boulder: Westview Press.
- (2003) "Coherence, Circularity and Consistency: Lehrer Replies," in *The Epistemology of Keith Lehrer*, E. J. Olsson (ed.), Dordrecht: Kluwer, pp. 309–356.

Lewis, C. I.

- (1946) *An Analysis of Knowledge and Valuation,* La Salle: Open Court.

Lipton, Peter

- (1995) "Popper and Reliabilism". In O'Hear (1995), pp. 31-13.

Leblanc, H. and van Fraassen, B. C.

- (1979) "On Carnap and Popper Probability Functions", *The Journal of Symbolic Logic* 44(3), 369-73.

MacBride, Fraaser

- (2013) "Truthmakers", *The Stanford Encyclopedia of Philosophy* (Spring 2013 Edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/spr2013/entries/truthmakers/>.

MacDonald, Graham

- (2004) "The Role of Experience in Popper's Philosophy of Science and Political Philosophy", in *Karl Popper: Critical Appraisals,* Edited by Philip Catton and Graham Macdonald, New York Routledge

Mach, Ernst

- (1886) *Beiträge* zur Analyse der Empfindungen, Jena; 5th edn, Die Analyse der Empfindungen, Jena, 1906; trans. C. Williams (1914) *Contributions to the Analysis of Sensations,* La Salle, IL: Open Court, 1986.

Mackie, J. L.

- (1973) *Truth Probability and Paradox.* Oxford: Clarendon Press.
- (1980) *The Cement of the Universe*, Oxford: Clarendon Press.

Magee, Bryan

- (1973) *Karl Popper.* New York: Viking Press.

Malcolm, Norman

- (1942) "Moore and Ordinary Language", in Schilpp (ed.), *1942,* pp. 345-368.
- (1952) "Knowledge and Belief," *Mind,* 61, reprinted in Malcolm, *1963,* pp. 58-72.
- (1963) *Knowledge and Certainty,* Englewood Cliffs, N.J.: Prentice Hall.
- (1977) "The Groundlessness of Belief", in Stuart C. Brown, ed., *Reason and Religion* (Ithaca: Cornell University Press, 1977), pp. 143-57.

Rochefort-Maranda, G.

- (2004) "Probabilité et support inductif. Sur le théorème de Popper-Miller (1983)", *Dialogue* 43(3), 499-526.

Rochefort-Maranda, G. and Miller, D. W.

- (2013) "Bibliography of the Popper-Miller Theorem." URL = <http://www.warwick.ac.uk/go/dwmiller/pm- bibliography.pdf/.>

Mayo, D. G.

- (2006) "Critical Rationalism and Its Failure to Withstand Critical Scrutiny". In C. Cheyne and J. Worrall, editors (2006), pp. 63–96. Rationality and Reality: Conversations with Alan Musgrave. Dordrecht: Springer.

McCaskey, John

- (2007) "Freeing Aristotelian Epagôgê from Prior Analytics II 23," *Apeiron*, 40:4 (December, 2007), pp. 345–74.

McDowell, John

- (1978) "Physicalism and Primitive Denotation: Field on Tarski" *Erkenntnis* 13, pp. 131- 152. Page references are to the reprint in Platts (1980), pp.111-130.
- (2011) *Perception as a Capacity for Knowledge*, Marquette University Press

McGinn, Colin

- (2002) "Looking for a Black Swan". *The New York Review of Books* 49, 18, 21.xi.2002, pp. 46–50.

McKirahan, Richard Jr.

- (1992) *Principles and Proofs: Aristotle's Theory of Demonstrative Species.* Princeton, N.J.: Princeton University Press, 1992.

Mill, J. S.

- (1843) *A System of Logic: Ratiocinative and Inductive*, in *Collected Works of John Stuart Mill,* ed. J.M. Robson, London: Routledge, vols 7 and 8, 1991. Volume and page numbers given below refer to this edition.
- (1873) "Autobiography", in *Collected Works of John Stuart Mill*, London: Routledge, vol. 1, 1-290, 1991.

Miller, David W.

- (1974a) "Popper's Qualitative Theory of Verisimilitude." *The British Journal for the Philosophy of Science* 25 (1974): 166–177.
- (1974b) "On the Comparison of False Theories by their Bases." *The British Journal for the Philosophy of Science* 25 (1974): 178–188.
- (1994) *Critical Rationalism: A Restatement and Defence*. Chicago and La Salle IL: Open Court Publishing Company.
- (1996) "What Use is Empirical Confirmation?" *Economics and Philosophy* 12, 2, pp. 197–206.
- (2004) "How Does Probability Theory Generalize Logic?", URL = <http://www.warwick.ac.uk/go/dwmiller/chuaqui.pdf/.>
- (2006a) *Out of Error: Further Essays on Critical Rationalism*. Aldershot: Ashgate Publishing Company.
- (2006b) "Darwinism is the Application of Situational Logic to the State of Ignorance". In Jarvie, Milford, and Miller (2006), Volume III, pp. 155–162.
- (2006c). "Putting Science to Work". URL = <http://www2.warwick.ac.uk/fac/soc/ philosophy/staff/miller/oxdocs/science-tech.pdf/.>
- (2007) "The Objectives of Science". *Philosophia Scientiæ* 11, 1, pp.21–43.
- (2008a) "Overcoming the Justificationist Addiction", *Iranian Journal of Philosophical Investigations* 4, 11, Spring and Summer 2007, pp. 167–182. Revised version (2011)
- (2008b) "Deductivist Decision Making". *Rationalites contemporaines,* Universitie de Paris IV (Sorbonne), 13.ii.2008
- (2010) "Falsification: Truth, Falsity, and Negation" URL = <http://www2.warwick.ac.uk/fac/soc/philosophy/people/associates/miller/dresden.pdf>
- (2011a) "Overcoming the Justificationist Addiction", Online version of (2008) URL = <http://www2.warwick.ac.uk/fac/soc/philosophy/staff/miller/wroclaw2a.pdf>
- (2011b) "Some Hard Questions for Critical Rationalism", Online version URL = <http://go.warwick.ac.uk/dwmiller/prague.pdf>

Miller, D. W. and Popper, K. R.

- (1986) "Deductive Dependence", *Actes IV Congrés Català de Lògica*. Barcelona: Universitat Politècnica de Catalunya and Universitat de Barcelona, pp. 21-9.

Milton, J.R.

- (1998) "Bacon, Francis (1561-1626)" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Montaigne, Michel

- (1933) "An Apologie of Raymond Sebond", in *The Essays of Montaigne* (New York, Modern

Library, 1933).

Moser, Paul

- (1985) *Empirical Justification*. Dordrecht: Reidel.
- (1997) "Epistemology" in *Routledge History of Philosophy Volume X Philosophy of Meaning, Knowledge and Value in the 20th Century, Edited by* John V. Canfield, Routledge.
- (1998) "A posteriori" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Mulligan, K., Simons, P., and Smith, B.

- (1984) "Truth-Makers," *Philosophy and Phenomenological Research*, 44: 287–321.

Munz, Peter

- (1985) *Our Knowledge of the Growth of Knowledge*, London: Routledge.
- (1986) "Philosophy and the Mirror of Rorty", in Gerard Radnitzky and W. W. Bartley, III (eds.), *Evolutionary Epistemology, Rationality, and the Sociology of Knowledge*, Open Court, La Salle, Illinois.
- (2004) *Beyond Wittgenstein's Poker: New Light on Popper and Wittgenstein*, Ashgate Publishing Company, Aldershot and Burlington VT.

Musgrave, Alan. E.

- (1989a) "Saving Science from Scepticism". In D'Agostino and Jarvie (1989), pp. 297–323.
- (1989b) "Deductivism versus Psychologism". In Notturno (1989), pp. 315-340. Reprinted as Chapter 14 of Musgrave (1999).
- (1989c) "Deductive Heuristics". In K. Gavroglu, Y . Goudaroulis, and P . Nicolacopoulos, editors.
- (1991) "What Is Critical Rationalism?", In A. Bohnen and A. E. Musgrave, editors (1991), pp. 17–30. Wege der Vernunft: Festschrift fu¨r siebzigsten Geburtstag von Hans Albert. Tübingen: J. C. B. Mohr (Paul Siebeck).
- (1993) *Common Sense, Science and Scepticism*. Cambridge and elsewhere: Cambridge University Press.
- (1997) "The T-Scheme plus Epistemic Truth Equals Idealism". *Australasian Journal of Philosophy* 75, pp. 490-496. Reprinted as Chapter 10 of Musgrave (1999).
- (1999) *Essays on Realism and Rationalism*. Amsterdam and Atlanta GA: Rodopi.
- (2004) "How Popper (Might Have) Solved The Problem of Induction" in *Karl Popper: Critical Appraisals,* Edited by Philip Catton and Graham Macdonald, New York Routledge.

Mura, A. M.

- (1990) "When Probabilistic Support Is Inductive", Philosophy of Science 57(2). 278-89.
- (2008) "Can Logical Probability Be Viewed as a Measure of Degrees of Partial Entailment?", *Logic and Philosophy of Science* VI(1), 25-33. URL = <http://www2.units.it/episteme/LandPS Vol6No1/Mura LandPS Vol6No1.pdf. Accessed 21.vi.2013.>

Nagel, Ernest

- (1939) *Principles of the Theory of Probability.* Chicago IL: The University of Chicago Press.
- (1963) "Carnap's Theory of Induction", in Schilpp (ed.), *1963,* pp. 785-825.

Neurath, Otto

- (1983/1932) "Protocol Sentences," in *Philosophical Papers 1913–1946*, R.S. Cohen and M. Neurath (eds.), Dordrecht: Reidel.

Nola, Robert and Sankey, Howard

- (2007) *Theories of Scientific Method,* Stocksfield: Acumen Publishing Limited.

Notturno, Mark, A.

- (1985) *Objectivity, Rationality and the Third Realm: Justification and the Grounds of Psychologism. A Study of Frege and Popper,* Dordrecht–Boston– Lancaster: Martinus Nijhoff Publishers, 1985.
- (2000) *Science and the Open Society. The Future of Karl Popper's Philosophy*, Budapest: Central European University Press, 2000.

Okasha, Samir

- (2002) *Philosophy of Science: A Very Short Introduction.* Oxford: Oxford University Press.

Olsson, Erik

- (2001) "Why Coherence is not Truth-Conducive," *Analysis*, 61: 236-241.
- (2002) "What is the Problem of Coherence and Truth?," *The Journal of Philosophy*, 99: 246–272.
- (2005) *Against Coherence: Truth, Probability, and Justification*, Oxford: Clarendon Press.
- (2012) "Coherentist Theories of Epistemic Justification", in *The Stanford Encyclopedia of Philosophy* (Spring 2013 Edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/spr2013/entries/justep-coherence/>.

O'Grady, Paul

- (2002) *Relativism,* Chesham: Acumen Press.

O'Hear, Anthony

- (1980) *Karl Popper.* London, Boston MA, and Henley: Routledge and Kegan Paul.
- (1985) "Popper and the Philosophy of Science". *New Scientist*, 22/08/1985, pp. 43-45.
  (1989) *An Introduction to the Philosophy of Science*. Oxford: Clarendon Press.
- (1995), editor, *Karl Popper: Philosophy and Problems*. Royal Institute of Philosophy Supplement 39. Cambridge and elsewhere: Cambridge University Press.

Osker, Magaret, J.

- (1998) "Gassendi, Pierre (1592-1655)" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998)

Papineau, David

- (2006) "Three Scenes and a Moral". The Philosophers" Magazine, 38, pp. 63f.

Pappas , George S.

- (1998) "Epistemology; History of" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Peirce, Charles Sanders

- (1893) "The Fixation of Belief" (with revisions), in Peirce, *1931-58,* vol. 5, pp. 223-247.

Peters, F. E.

- (1967) *Greek Philosophical Terms: A Historical Lexicon* New York: NYU Press, 1967.

Pickering, Andrew

- (1984) *Constructing Quarks*, Chicago, IL: University of Chicago Press.

Platts, Mark

- (1979) *Ways of Meaning*, London: Routledge and Kegan Paul.
- (1980) *Reference, Truth and Reality.* Editor, London: Routledge.

Poincare, Henri

- (1902) *La Science et l'hypothese;* tr. by G.B. Halsted, 1913, as *Science and Hypothesis;* reprinted, New York: Dover, 1952.

Polanyi, Michael

- (1946) *Science, Faith and Society*, Chicago: University of Chicago Press, 1946, 1964.
- (1951) *The Logic of Liberty*, London: Routledge and Kegan Paul, and Chicago: University of

Chicago Press, 1951.

- (1952) "The Stability of Beliefs", *British Journal for the Philosophy of Science,* 3, pp. 217-232.
  (1958) *Personal Knowledge;* 2nd rev. ed., 1962, London: Routledge and Kegan Paul.
- (1962) "Tacit Knowing. Its Bearing on Some Problems of Philosophy", *Review of Modern Physics,* 34, 1962, pp. 601–616.
- (1967) The Tacit Dimension. London: Routledge and Kegan Paul.

Popkin, Richard H.

- (1953) "Joseph Glanvill: A Precursor of David Hume", *Journal of the History of Ideas* 14: 293-303.
- (1972) "Fideism:, in Edwards, P. (ed.) *Encyclopedia of Philosophy,* New York: Macmillan.
- (1979) *The History of Scepticism from Erasmus to Spinoza,* Berkeley and Los Angeles, CA, and London: University of California Press.
- (1980) *The High Road to Pyrrhonism.* San Diego: Austin Hill Press.
- (1990) *The Third Force in 17th century Philosophy,* Leiden: Brill.
- (1998a) "Montaigne, Michel Eyquem de (1533-92)" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).
- (1998b) "Sanches, Francisco (1551-1623)" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).
- (1998c) "Scepticism, Renaissance" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Popper, Karl R.

- (1930–1932). *Die beiden Grundprobleme der Erkenntnistheorie.* Tübingen: J. C. B. Mohr (Paul Siebeck). Published in 1979. Translated into English as Popper (2009).
- (1934) *Logik der Forschung.* Vienna: Julius Springer Verlag. 11th edition 2005. Tübingen: Mohr Siebeck. Translated into English as Popper (1959)
- (1940) "What Is Dialectic?". *Mind* 49, pp. 403–426. Reprinted with corrections as Chapter 15 of Popper (1963).
- (1945) *The Open Society and Its Enemies.* London: George Routledge and Sons. 5th edition, 1966. London: Routledge.
- (1948) "On the Theory of Deduction. Part II: The Definitions of Classical and Intuitionist Negation". *Proceedings of the Koninklijke Nederlandsche Akademie van Wetenschappen* 51, 3, pp. 322–331, and *Indagationes Mathematicae* 10, 2, pp. 111–120.
- (1949) "Naturgesetze und theoretische Systeme". In S. Moser, editor (1949), pp. 43–60. *Gesetz und Wirklichkeit.* Innsbruck: Tyrolia Verlag. References are to the English translation, "The Bucket and the Searchlight: Two Theories of Knowledge", in Popper (1972), Appendix 1.
- (1954) "Degree of Confirmation". *The British Journal for the Philosophy of Science* 5, 18, pp.

143–149. Reprinted with corrections in Popper (1959), appendix *ix.

- (1957a) "A Second Note on Degree of Confirmation", *The British Journal for the Philosophy of Science* 7(28), 350 3. Corrections: ibidem 8(32), 294f. Reprinted in Popper 1959a, appendix ∗ix.
- (1957b) *The Poverty of Historicism*. London: Routledge and Kegan Paul.
- (1959a) *The Logic of Scientific Discovery*. London: Hutchinson and Co. Expanded English translation of Popper (1934).
- (1959b) "The Propensity Interpretation of Probability", *The British Journal for the Philosophy of Science* 10(37), 25-42. Corrections: ibidem 10(38), 171.
- (1960) "On the Sources of Knowledge and of Ignorance". *Proceedings of The British Academy* 46, pp. 39–71. Reprinted as the Introduction to Popper (1963).
- (1963) *Conjectures and Refutations: The Growth of Scientific Knowledge*. London: Routledge and Kegan Paul. 5th edition 1989.
- (1968) "Theories, experience, and probabilistic intuitions" In I. Lakatos, editor (1968), pp. 285-303. *The Problem of Inductive Logic*. Amsterdam: North-Holland Publishing Company.
- (1972) *Objective Knowledge: An Evolutionary Approach*. Oxford: Clarendon Press. 2nd edition 1979
- (1974a) "Intellectual Autobiography". In Schilpp (1974), pp. 1–181. Reprinted as *Unended Quest*, (1976) London and Glasgow: Fontana/Collins.
- (1974b) "Replies to My Critics". In Schilpp (1974), pp. 961–1197.
- (1979) *Die beiden Grundprobleme der Erkenntnistheorie*. Tübingen: J. C. B. Mohr (Paul Siebeck). Written in 1930–1932. 2nd edition 1994.
- (1982a) *The Open Universe. Postscript to The Logic of Scientific Discovery,* Volume II. London: Hutchinson and Co. (Publishers) Ltd.
- (1982b) *Quantum Theory and the Schism in Physics. Postscript to The Logic of Scientific Discovery*, Volume III. London: Hutchinson and Co. (Publishers) Ltd.
- (1983) *Realism and the Aim of Science. Postscript to The Logic of Scientific Discovery*, Volume I. London: Hutchinson and Co. (Publishers) Ltd.
- (1990) *A World of Propensities,* Thoemmes, Bristol.
- (1994) *The Myth of the Framework. In Defence of Science and Rationality*, edited by Mark A. Notturno, London–New York: Routledge, 1994.
- (1998) *The World of Parmenides. Essays on the Presocratic Enlightenment*, edited by Arne F. Petersen with the assistance of Jørgen Mejer, London–New York: Routledge, 1998.
- (2009) *The Two Fundamental Problems of the Theory of Knowledge*. London: Routledge. English translation of Popper (1979).

Popper, K. R. and Miller, D. W.

- (1983) "A Proof of the Impossibility of Inductive Probability", *Nature* 302, 5910, 21.iv.1983, pp. 687f.

- (1987) "Why Probabilistic Support Is Not Inductive". *Philosophical Transactions of the Royal Society of London*, series A 321, 1562, 30.iv.1987, pp. 569–591.

Popper, K. R. and Eccles, J. C.

- (1977) *The Self and Its Brain. An Argument for Interactionism.* Berlin and elsewhere: Springer International.

Pritchard, Duncan

- (2009) *What is this Thing Called Knowledge,* London: Routledge, 2nd. Ed.

Pritchard, Duncan and Turri, John

- (2012) "The Value of Knowledge", *The Stanford Encyclopedia of Philosophy* (Winter 2012 Edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/win2012/entries/knowledge-value/>.

Psillos, Stathis

- (2007) *Philosophy of Science A–Z,* Edinburgh University Press.

Putnam, Hillary

- (1974) "The "Corroboration" of Theories", pp. 221-240 of Schilpp (1974).
- (1978) *Meaning and the Moral Sciences.* London-Boston: Routledge and Kegan Paul.
- (1981) *Reason, Truth and History.* Cambridge: Cambridge University Press.

Quine, W.V.O.,

- (1951) "Two Dogmas of Empiricism", *Philosophical Review* 60: 20-43.
- (1960) *Word and Object,* New York: Wiley and Sons.
- (1969) "Epistemology Naturalized", in *Ontological Relativity and Other Essays,* New York: Columbia University Press.

Quine, W.V.O. and Ullian, J.

- (1978) *The Web of Belief,* 2nd edn, New York: Random House.

Radnitzky, Gerard

- (1987) "In Defense of Self-Applicable Critical Rationalism", in Gerard Radnitzky and W. W. Bartley, III (eds.), *Evolutionary Epistemology, Rationality, and the Sociology of Knowledge,* Open Court, La Salle, Illinois

Ramsey, F. P.

- (1931) "Truth and Probability", in R. B. Braithwaite (ed.), *The Foundations of Mathematics*, London: Kegan Paul.

Reisch, George A.

- (1991) "Did Kuhn Kill Logical Empiricism?" Philosophy of Science 58, 264–77.

Reichenbach, Hans

- (1930) *Erkenntnis* 1, 1930.
- (1935) *Wahrscheinlichkeitslehre*. Leyden: Sijthoff. Translated, with augmentation and omissions, by E H. Hutton and M. Reichenbach as *The Theory of Probability*. Berkeley: University of California Press, 1949 Reprinted 1971.
- (1938) *Experience and Prediction,* Chicago: University Press.
- (1951) The Rise of Scientific Philosophy. Berkeley: University of California Press.
- (1971) *The Theory of Probability. An Inquiry into the Logical and Mathematical Foundations of the Calculus of Probability.* Berkeley and Los Angeles CA: University of California Press, and London: Cambridge University Press.

Rescher, Nicholas

- (1973) *The Coherence Theory of Truth*, Oxford: Oxford University Press.
- (1998) "Fallibilism" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).
- (2005) "Reductio ad absurdum" in the *Internet Encyclopaedia of Philosophy*: URL = <http://www.iep.utm.edu/reductio/>.

Richardson, Alan

- (2007) ""That Sort of Everyday Image of Logical Positivism": Thomas Kuhn and the Decline of Logical Empiricist Philosophy of Science" in Alan Richardson and Thomas Uebel (eds.), *The Cambridge Companion to Logical Empiricism*, Cambridge University Press, 2007

Rijk, Lambertus Marie de.

- (2002) *Aristotle: Semantics and Ontology*. Boston, M.A.: Brill, 2002.

Robinson, Abraham

- (1996) *Non-standard analysis, Princeton Landmarks in Mathematics* (2nd ed.), Princeton University Press.

Rorty, Richard

- (1979) *Philosophy and the Mirror of Nature*. Princeton, N.J.: Princeton University Press.

Rosenkrantz, Roger. D.

- (1977) *Inference, Method and Decision*. Dordrecht and Boston MA: D. Reidel Publishing Company.

Rowbottom, Darrell, P.

- (2008) "On the Proximity of the Logical and "Objective Bayesian" Interpretations of Probability", *Erkenntnis* 69, 335–349.
- (2011) *Popper's Critical Rationalism: A Philosophical Investigation.* Oxon: Routledge.
- (2013) "Popper's Measure of Corroboration and *P(h|b)*", *The British Journal for the Philosophy of Science* 64 (4), pp. 739-745.

Russell, Bertrand

- (1911) "Knowledge by Acquaintance and Knowledge by Description", *Proceedings of the Aristotelian Society* 11: 108-28.
- (1912) *The Problems of Philosophy,* Oxford: Oxford University Press, 1974.
- (1914) *Our Knowledge of the External World,* London and Chicago, IL: Open Court; 2nd edn, London: Allen and Unwin, 1926; London: Routledge, 1993.
- (1919) *An Introduction to Mathematical Philosophy,* London: Routledge, 1993.
- (1936) "The Limits of Empiricism", *Proceedings of the Aristotelian Society* 36: 131-50.
- (1945) *A History of Western Philosophy,* London: Routledge, 1993.
- (1948) *Human Knowledge: Its Scope and Limits,* London: Routledge, 1992.
- (1959) *My Philosophical Development,* London: Routledge, 1993.

Ryle, Gilbert

- (1945) *Philosophical Arguments.* Oxford: Clarendon Press. Reprinted as Chapter 14 of Ryle (1972).
- (1949) *The Concept of Mind* London: Hutchinson.
- (1972) *Collected Papers II.* London: Hutchinson & Co. (Publishers) Ltd.

Salmon, Wesley C.

- (1957) "Should we attempt to Justify Induction?", *Philosophical Studies,* 8.
- (1961) "Vindication of Induction", in Feigl and Maxwell (eds.), 1967, pp. 245-256.
- (1963a) "On Vindicating Induction", in Kyburg and Nagel (eds.), 1963, pp. 27-1.
- (1963b) "Inductive Inference", in B. Baumrin (ed.) *Philosophy of Science: The Delaware Seminar,* New York: Interscience Publishers, 353-70.
- (1966) *The Foundations of Scientific Inference,* 2nd ed. 1967, Pittsburgh: University Press.
- (1968a) "Reply". In Lakatos, (ed.), (1968), pp. 74-97. *The Problem of Inductive Logic.*

Amsterdam: North-Holland Publishing Company.

- (1968b) "The Justification of Inductive Rules of Inference", in Lakatos (ed.), (1968), pp. 24-43.
- (1969) "Partial Entailment as a Basis for Inductive Logic", in N. Rescher, ed. *Essays in Honor of Carl G. Hempel.* Dordrecht: D. Reidel Publishing Company, pp. 47-82. Reprinted in W. C. Salmon 2005, Chapter 11.
- (1975) "Confirmation and Relevance", in G. Maxwell and R. M. Anderson Jr, eds 1975, pp. 3-36. *Induction, Probability, and Confirmation.* Minnesota Studies in the Philosophy of Science, Volume VI. Minneapolis: University of Minnesota Press. Reprinted in W. C. Salmon 2005, Chapter 12.
- (1979) "Propensities: A Discussion Review", *Erkenntnis* 14(2), 183-216.
- (1981) "Rational Prediction". *The British Journal for the Philosophy of Science* 32, 1, pp. 115–125.
- (1998) "Reichenbach, Hans (1891-1953)" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998)
- (2005) *Reality and Rationality*. Edited by P. Dowe and M. H. Salmon. Oxford: Oxford University Press.

Salsburg, David

- (2001) *The Lady Tasting Tea: How Statistics Revolutionized Science in the Twentieth Century,* W.H Freeman and Company, New York.

Sanches, Franciscus

- (1581) *Quod nihil scitur,* ed. and trans. E. Limbrick and D.F.S. Thomson, Franciscus Sanches: *That Nothing is Known,* Cambridge: Cambridge University Press, 1989.

Santayana, George

- (1923) *Scepticism and Animal Faith;* reprinted, New York: Dover, 1955.

Schilpp, P. A., editor

- (1963) *The Philosophy of Rudolf Carnap.* La Salle, IL: Open Court.
- (1974) *The Philosophy of Karl Popper*. La Salle IL: Open Court Publishing Company.

Settle, T. W.

- (1974) "Induction and Probability Unfused", in P. A. Schilpp, ed., *The Philosophy of Karl Popper.* La Salle IL: Open Court Publishing Company, pp. 697-749.

Sextus Empiricus

- (c. AD 200) *Outlines of Pyrrhonism,* trans. J. Annas and J. Barnes, *Outlines of Scepticism,* Cambridge: Cambridge University Press, 1994.

Shand, J.

- (2000) *Arguing Well.* London and New York: Routledge.
- (2001/2002) "Induction: The Problem Solved" *Philosophy Now,* 32, pp. 32-34.

Shearmur, J. F. G.

- (2006) "Karl Popper: The Logic of Scientific Discovery", in J. Shand, ed., *Central Works of Philosophy 4. The Twentieth Century: Moore to Popper.* Chesham: Acumen Publishing Ltd, pp. 262-86.

Shimony, Abner

- (1970) "Scientific Inference" in Colodny R.G. (ed.), 1970, pp. 79-172.

Skorupski, John

- (1998) "Mill, John Stuart (1806-73)" in *The Routledge Encyclopedia of Philosophy* (Version 1.0), London and New York: Routledge (1998).

Simons, Peter

- (1998) "How the World can Make Propositions True: A Celebration of Logical Atomism," in M. Omyla (ed.), *Sklonnosci Metafizyczna,* Warsaw: Uniwersytet Warzawski, 113–135.

Godfrey-Smith, P.

- (2003) *Theory and Reality. An Introduction to the Philosophy of Science.* Chicago and London: University of Chicago Press.

Newton-Smith, W.H.

- (1981) *The Rationality of Science.* Routledge and Kegan Paul, Boston, London, and Henley.
- (1995) "Popper, Science and Rationality". In O"Hear (1995), pp. 13-30.
- (1992) "The Rationality of Science: Why Bother?" in *Popper in China* Edited by W. H. Newton-Smith and Jiang Tianji with the assistance of E. James London: Routledge 1992.

Sprat, T.

- (1667) *The History of the Royal Society of London,* London: J. Martin; facsimile repr. ed. J.I. Cope and H. Whitworth Jones, London: Routledge, 1959.

Sokal, Alan

- (1996) "Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum

Gravity". *Social Text*, 46-47, pp. 217-252.

Sokal, A. and Bricmont, J.

- (1998) *Intellectual Impostures*. Profile Books, London.

Sosa, Ernest

- (1980) "The Raft and the Pyramid," *Midwest Studies in Philosophy*, 5: 3–25.

Stegmuller, Wolfgang

- (1977) *Collected Papers on Epistemology, Philosophy of Science and History of Philosophy, 2* vols., Dordrecht: Reidel.

Steup, Matthias

- (2005) "Epistemology", *The Stanford Encyclopedia of Philosophy* (Fall 2013 Edition), Edward N. Zalta (ed.), forthcoming URL = <http://plato.stanford.edu/archives/fall2013/entries/epistemology/ >.

Strawson, Galen

- (2003) "Galen Strawon (interview)". *Believer Magazine*. McSweeney"s. Retrieved July 10, 2013. URL = <www.believermag.com/issues/200303/?read=interview_strawson>.

Strawson, Peter

- (1952) *Introduction to Logical Theory,* London: Methuen.
- (1958) "On Justifying Induction", *Philosophical Studies,* 9, pp. 20-21.
- (1959) *Individuals,* London: Methuen.

Striker, G.

- (1980) "Sceptical Strategies", in M. Schofield, M.F. Burnyeat and J. Barnes (eds) *Doubt and Dogmatism,* Oxford: Clarendon Press.

Stove, David

- (1982) *Popper and After. Four Modern Irrationalists*. Pergamon Press, Oxford and elsewhere.
- (2002) *On Enlightenment,* With a preface by Roger Kimball, Edited by Andrew Irvine, Piscataway, New Jersey: Transaction Books, 2002 (Preface by Roger Kimball).
- (2004) "Cole Porter and Karl Popper: the Jazz Age in the Philosophy of Science" in *Karl Popper: Critical Assessments of Leading Philosophers, Volume II:* Edited by Anthony O'Hear, London: Routledge.

Swinburne, R. G.

- (1973) *An Introduction to Confirmation Theory*. London: Methuen.

Tarski, Alfred

- (1944) "The Semantic Conception of Truth", *Journal of Philosophy and Phenomenological Research* 4: 341-76; repr. in L. Linsky (ed.) *Semantics and the Philosophy of Language: A Collection of Readings*, Champaign, IL: University of Illinois Press, 1969.
- (1956) *Logic, Semantics, Metamathematics*. Oxford: Clarendon Press. Translated by J.H. Woodger. 2nd edition 1983.

ter Hark, Michel

- (2004) *Popper, Otto Selz and the Rise of Evolutionary Epistemology*. Cambridge: Cambridge University Press.

Thomson, A.

- (1996) *Critical Reasoning. A Practical Introduction*. London and New York: Routledge. 2nd edition 2002.

Thorsrud, Harold

- (2004) "Ancient Greek Skepticism" in the *Internet Encyclopaedia of Philosophy*: URL = <http://www.iep.utm.edu/skepanci/>.

Toulmin, Stephen E.

- (1958) *The Uses of Argument*. Cambridge, UK, and elsewhere: Cambridge University Press.

Toulmin, S. E., Rieke, R. D., and Janik, A.

- (1984) *An Introduction to Reasoning*, 2nd edition. New York: Macmillan Publishing Company.

Trigg, R. H.

- (2001) *Philosophy Matters*. Oxford: Blackwell.

Trusted, Jennifer

- (1979) *The Logic of Scientific Inference*, London: Macmillan

Turri, J.

- (2009) "An Infinitist Account of Doxastic Justification," *Dialectica* 63: 209–18.
- (2012) "Infinitism, Finitude and Normativity," *Philosophical Studies*, DOI: 10.1007/s11098-011-9846-7.

Turri, J. And Klein, P.

- (2014) *Ad Infinitum: New Essays on Epistemological Infinitism,* edited by John Turri and Peter D. Klein, Oxford University Press: Oxford.

Urbach, Peter

- (1987) *Francis Bacon's Philosophy of Science,* Chicago, IL: Open Court.

Velupillai, K. V.

- (2008) "Demystifying Induction and Falsification: Trans-Popperian Suggestions". In T. A. Boylan and P. F. O'Gorman, editors (2008), pp. 143–163. *Popper and Economic Methodology. Contemporary Challenges.* London and New York: Routledge.

Vickers, John

- (2006) "The Problem of Induction", Edward N. Zalta, ed., *The Stanford Encyclopedia of Philosophy* (Winter 2012 Edition), http://plato.stanford.edu/entries/induction-problem/ #KarPopVieInd

Vincenti, W. G.

- (1990) *What Engineers Know and How they Know It. Analytical Studies from Aeronautical History.* Baltimore MD and London: The Johns Hopkins University Press.

Vogt, Katja,

- (2010) "Ancient Skepticism" in*The Stanford Encyclopedia of Philosophy* (Winter 2012 Edition), Edward N. Zalta (ed.), URL = http://plato.stanford.edu/entries/skepticism-ancient/

Vollmer, Gerhard

- (1987) "On Supposed Circularities in an Empirically Oriented Epistemology" in Gerard Radnitzky and W. W. Bartley, III (eds.), *Evolutionary Epistemology, Rationality, and the Sociology of Knowledge,* Open Court, La Salle, Illinois.

Walton, D.N.

- (1991) *Begging the Question*, New York: Greenwood Press.

Watkins, John. W. N.

- (1958) "Confirmable and Influential Metaphysics". *Mind 67* (New Series): 344-365.
- (1970) "Imperfect Rationality". In Borger and Cioffi (1970), pp. 167–217.
- (1984) *Science and Scepticism*. London: Hutchinson and Co. (Publishers) Ltd.

- (1995) "Popper and Darwinism". In A. O"Hear, editor (1995), pp. 191–206. *Karl Popper: Philosophy and Problems*. Royal Institute of Philosophy Supplement 39. Cambridge and elsewhere: Cambridge University Press.

Weston, T.

- (1992) "Approximate Truth and Scientific Realism". *Philosophy of Science* 59, pp. 53-74.

Whitehead, A.N. and Russell, B.A.W.

- (1910) *Principia Mathematica, vol. 1*, Cambridge: Cambridge University Press, 2nd edn, 1925

Wittgenstein, Ludwig

- (1922) *Tractatus Logico-philosophicus*.trans. C.K. Ogden and F.P. Ramsey, London: Routledge and Kegan Paul.
- (1953) *Philosophical Investigations*, ed. G.E.M. Anscombe and R. Rhees, trans. G.E.M. Anscombe, Oxford: Blackwell.
- (1969) *On Certainty*, ed. G.E.M. Anscombe and G.H. von Wright, trans. D. Paul and G.E.M. Anscombe, Oxford: Blackwell.

Woleński, Jan and Agassi, Joseph

- (2010) "Łukasiewicz and Popper on Induction", *History and Philosophy of Logic*, 31, 381-8.

Worrall, John

- (1989) "Why Both Popper and Watkins Fail to Solve the Problem of Induction". In F. D'Agostino and I. C. Jarvie, editors (1989), pp. 257-296. *Freedom and Rationality. Essays in Honor of John Watkins*. Dordrecht and elsewhere: Kluwer Academic Publishers.

Zabell, S. L.

- (2007) "Carnap on Probability and Induction", in M. Friedman and R. Creath, eds, *The Cambridge Companion to Carnap*. Cambridge and elsewhere: Cambridge University Press, pp. 273-94. Expanded version: Zabell 2009. "Carnap and the Logic of Inductive Inference", in D. M. Gabbay, S. Hartmann, and J. Woods, eds, *Handbook of the History of Logic. Volume 10: Inductive Logic*. Amsterdam: Elsevier B. V., pp. 265-309.

Zagzebski, Linda

- (1996) *Virtues of the Mind: An Inquiry into the Nature of Virtue and the Ethical Foundations of Knowledge*, Cambridge: Cambridge University Press.
- (1999) "What is Knowledge?", in *Epistemology*, eds. J. Greco and E. Sosa, 92–116, Oxford: Blackwell.

- (2003) "The Search for the Source of the Epistemic Good", *Metaphilosophy*, 34: 12–28; and reprinted in Brady and Pritchard (2003).
- (2009) *On Epistemology* Wadsworth, Cengage Learning Califorrnia.