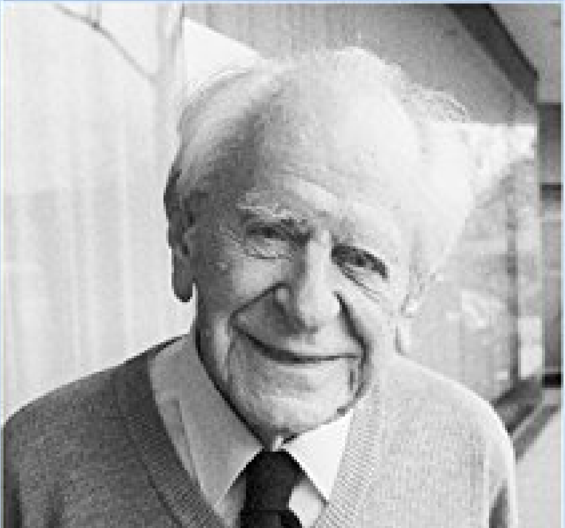
The Cambridge Companion to POPPER



Edited by
JEREMY SHEARMUR
GEOFFREY STOKES

THE CAMBRIDGE COMPANION TO

POPPER

Karl Popper was one of the most influential philosophers of the twentieth century. His criticism of induction and his falsifiability criterion of demarcation between science and non-science were major contributions to the philosophy of science. Popper's broader philosophy of critical rationalism included a distinctive philosophy of social science and political theory. His critique of historicism and advocacy of the open society marked him out as a significant philosopher of freedom and reason. This book sets out the historical and intellectual contexts in which Popper worked, and offers an overview and diverse criticisms of his central ideas. The volume brings together contributors with expertise on Popper's work, including people personally associated with Popper (such as Jarvie, Miller, Musgrave, Petersen and Shearmur), specialists on the topics treated (Bradie, Godfrey-Smith and Jackson) and scholars with special interests in aspects of Popper's work (Andersson, Hacohen, Maxwell and Stokes).

JEREMY SHEARMUR is Emeritus Fellow at the Australian National University. He studied at the London School of Economics with Popper, Watkins and Lakatos, and subsequently worked with Popper as his assistant for eight years. He taught philosophy at the University of Edinburgh, political theory at the University of Manchester and subsequently both subjects at the Australian National University. He is the author of *Hayek and After* and *The Political Thought of Karl Popper* (both 1996), and is a co-editor of Popper's *After the Open Society* (2008).

GEOFFREY STOKES is Deputy Pro Vice-Chancellor (Research) in the College of Business at RMIT University in Melbourne. He is the author of *Popper: Philosophy, Politics and Scientific Method* (1998). He has been a senior editor (2010–16) of the *Australian Journal of Political Science*. From 1994 to 1999 he was the editor (politics) for the *Australian Journal of Politics and History*. In 1985–86 he held a Fulbright Postdoctoral Fellowship in the Center for Philosophy and the History of Science at Boston University.

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The Cambridge Companion to

POPPER

Edited by

Jeremy Shearmur
Australian National University

Geoffrey Stokes
RMIT University

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Contents

Notes on Contributors *page* ix

1 Popper and His Philosophy: An Overview 1
GEOFFREY STOKES AND JEREMY SHEARMUR

2 The Young Popper, 1902–1937: History, Politics and Philosophy in Interwar Vienna 30
MALACHI HAIM HACOHEN

3 On Popper's Contributions to Psychology as Part of Biology 69
ARNE FRIEMUTH PETERSEN

4 Popper's Philosophy of Science: Looking Ahead 104
PETER GODFREY-SMITH

5 The Problem of the Empirical Basis in Critical Rationalism 125
GUNNAR ANDERSSON

6 Karl Popper's Evolutionary Philosophy 143
MICHAEL BRADIE

7 Popper's Paradoxical Pursuit of Natural Philosophy 170
NICHOLAS MAXWELL

8 Metaphysics and Realism 208
ALAN MUSGRAVE

9 Popper's Contributions to the Theory of Probability and Its Interpretation 230
DAVID MILLER

10 Popper's Philosophy of Mind 269
FRANK JACKSON

11 Popper's Philosophy and the Methodology of Social Science 284
IAN JARVIE

12 Popper and Habermas: Convergent Arguments for a Postmetaphysical Universalism 318
GEOFFREY STOKES

13 Popper's Politics and Political Thought 352
JEREMY SHEARMUR

Index of Names 377

Index of Subjects 385

Notes on Contributors

GUNNAR ANDERSSON is Professor Emeritus of Philosophy of Science at the University of Umeå. He is the author of *Criticism and the History of Science: Kuhn's, Lakatos's and Feyerabend's Criticisms of Critical Rationalism* (1994) and of numerous articles on the philosophy of Critical Rationalism.

MICHAEL BRADIE is Professor of Philosophy Emeritus at Bowling Green State University. He is the author of *The Secret Chain: Evolution and Ethics* (1994). He has written extensively on issues in evolutionary epistemology, evolutionary ethics and the role of metaphor in science.

PETER GODFREY-SMITH is Distinguished Professor of Philosophy at the Graduate Center, City University of New York, and Professor of History and Philosophy of Science at the University of Sydney. Among his books are *Theory and Reality* (2003), *Darwinian Populations and Natural Selection* (2009), which won the 2010 Lakatos Award, and *Philosophy of Biology* (2014). He also works on pragmatism, animal behaviour and problems in the philosophy of mind.

MALACHI HAIM HACOHEN is Associate Professor of History, Political Science and Religion, Bass Fellow and Director of the Center for European Studies at Duke University. He is the author of *Karl Popper – The Formative Years, 1902–1945: Politics and Philosophy in Interwar Vienna* (2000) and of essays on the Central European Jewish intelligentsia, Cold War liberalism and cosmopolitanism and Jewish identity in *The Journal of Modern History*, *The Journal of the History of Ideas*, *History and Theory*, *History of Political Economy* and *Jewish Social Studies*. He is completing *Jacob & Esau Between Nation and Empire: A Jewish European History*.

FRANK JACKSON is Emeritus Professor at The Australian National University. His publications include *Perception* (1977), *Conditionals* (1987), *Philosophy of Mind and Cognition* with David Braddon-Mitchell (1996), *From Metaphysics to Ethics* (1998), and *Language, Names and Information* (2010).

IAN JARVIE is Distinguished Research Professor Emeritus at York University, Toronto. He is the author, among others, of *The Revolution in Anthropology* (1964) and *The Republic of Science. The Emergence of Popper's Social View of Science, 1935–1946* (2001).

NICHOLAS MAXWELL has devoted much of his working life to arguing that we need to bring about a revolution in academia so that it seeks and promotes wisdom and does not just acquire knowledge. He has published eight books on this theme, including *From Knowledge to Wisdom* (1984) and *Global Philosophy* (2014). For thirty years he taught philosophy of science at University College London, where he is now Emeritus Reader. For more about his work, see http://www.ucl.ac.uk/from-knowledge-to-wisdom.

DAVID MILLER is Emeritus Reader in Philosophy at the University of Warwick. He is the editor of *Popper Selections* (1985) and, jointly with Ian Jarvie and Karl Milford, of *Karl Popper: A Centenary Assessment* (2006). He is also the author of *Critical Rationalism. A Restatement and Defence* (1994) and of *Out of Error* (2006).

ALAN MUSGRAVE is Emeritus Professor of Philosophy at the University of Otago, New Zealand. He was educated at the London School of Economics, where his PhD was supervised by Sir Karl Popper. His books include *Common Sense, Science and Scepticism* (1993) and *Essays on Realism and Rationalism* (1999), and he co-edited with Imre Lakatos the best seller *Criticism and the Growth of Knowledge* (1970). His views were discussed in the collection *Rationality and Reality: Conversations with Alan Musgrave* (2006).

ARNE FRIEMUTH PETERSEN, formerly professor of general psychology at Copenhagen University, is the author of *Biopsychological Aspects of Individuation* (1993) (in Danish), and of a number of articles on Popper's bio-logical approach to psychology. He is the editor of Popper's *The World of Parmenides – Essays on the Presocratic Enlightenment* (1998).

JEREMY SHEARMUR is Fellow Emeritus in the School of Philosophy at the Australian National University. He is the author of *Hayek and After* and *The Political Thought of Karl Popper* (both 1996), was a co-editor of Popper's *After the Open Society* (2008) and is currently editing Hayek's *Law, Legislation and Liberty* for his Collected Works.

GEOFFREY STOKES is Deputy Pro Vice-Chancellor (Research) in the College of Business at RMIT University in Melbourne. He is the author of *Popper: Philosophy, Politics and Scientific Method* (1998). He also works in the areas of social and political theory, with a particular interest in democratic theory, and on the history of Australian political ideas. He is a senior editor (2010–16) of the *Australian Journal of Political Science*. From 1994 to 1999 he was the editor (politics) for the *Australian Journal of Politics and History*.

1 Popper and His Philosophy: An Overview

0. INTRODUCTION

Karl Popper both provoked and attracted controversy. His work addressed key problems in the fields of epistemology, philosophy of science and social science, logic, political theory and politics, metaphysics and theories of mind. In each field he challenged dominant theories and sought to formulate new ones. Perhaps his most important achievement was to cast doubt upon induction as a criterion of demarcation between science and non-science, and to propose the alternative of falsifiability. Over the course of his life he extended this criterion into a broader philosophy of critical rationalism that would be applicable to many fields. At the heart of this philosophy is the practice of criticism. Popper rejected the idea that we should try to justify our arguments and proposed that we should replace it with the idea that our ideas need to be exposed to, and to survive, criticism. In tandem with attempts to refute opposing views, Popper encouraged scientists to propose bold conjectures and then attempt to refute them.

Popper provoked controversy in part because of his merciless criticism of those philosophies and theories he chose to attack. Logical positivism, Platonism, Marxism and Freudianism, for example, all had powerful proponents and followers who initiated spirited defences. The intellectual reception of his positive proposals was invariably mixed. This was not just due to the novel quality of his ideas. It was the consequence of a number of more mundane factors such as the timing of translations into English, brought about by the disruptions caused by the rise of national socialism, the onset of war, geographical isolation and poor health.

Although primarily known in Europe as a philosopher of science who in his seminal work, *Logik der Forschung* (1934), presumed to have overturned some of the key doctrines of the Vienna Circle, Popper became better known in English-speaking countries as a political philosopher who had written *The Open Society and Its Enemies*

(1945). *Logik der Forschung*, however, was not published in an English translation until 1959, as *The Logic of Scientific Discovery*, which included many new footnotes and appendices. The publication of Popper's *Postscript* to the *The Logic of Scientific Discovery*, which was a major effort to revise and extend his earlier philosophy, was interrupted by poor eyesight and did not appear until the 1980s (Popper 1982a, 1982b, 1983).

Similar difficulties beset his political views and arguments. The *Open Society* was the product of his years (1937–45) of relative isolation at Canterbury University College in Christchurch, New Zealand. Although this book came to prominence during the Cold War, and was generally represented as a liberal critique of communism, much of its policy content reflects a perspective drawn from Austrian social democracy. Whereas many commentators lump Popper and Friedrich Hayek together as classical liberals, their political views on markets and the role of the state in the economy are quite different (see Shearmur 1996; Caldwell 2006). The problem of interpretation is further compounded because during the 1950s and 1960s Popper took on a more liberal persona and propounded liberal doctrines and even aligned himself with some conservative causes. Nonetheless, the extent to which there are fundamental changes over time in Popper's political views is a matter of dispute. Over the course of his life, as would be expected, Popper had abandoned some arguments, revised others and proposed new ones. Neither the order of the publication of his writings nor their reception reflects the order of development of his ideas. For many readers, the Popper they embraced or rejected had moved on.

As Malachi Haim Hacohen (2000, introduction) has argued, in places, Popper's own reconstruction of his intellectual development, *Unended Quest*, hinders attempts to understand accurately the early development of his ideas. Indeed, it is only since the 1990s, when Popper's papers were catalogued and made available in the Hoover Archives, that it has become easier to trace the lines of his intellectual evolution. Based on his archival research, Hacohen (2000, p. 10) has revealed a further difficulty in that parts of Popper's account of his intellectual trajectory cannot be borne out by the documentary evidence. All this is further complicated by Popper's somewhat difficult and combative personal character. He evoked strong feelings among those who knew him. Sentiments of intense loyalty jostle uneasily alongside those of dislike and disapproval. Not surprisingly, such feelings can influence assessments of his contribution to philosophy. This chapter provides an overview of many key strands of Popper's philosophy and offers a brief assessment of its philosophical and political significance.

1. BACKGROUND – HISTORICAL AND INTELLECTUAL CONTEXT

Karl Popper was born in Vienna on 28 July 1902 into a Jewish family that had converted to Protestantism. As Hacohen has shown, the dominant values of the family and much of his social milieu were cosmopolitan and liberal. In the years before the First World War, Vienna was the capital of a sprawling, multi-ethnic Austro-Hungarian Empire. Vienna was also home to numerous 'progressive' movements in art, philosophy, psychology, education, economics and politics. Throughout many of these fields there was an optimism about science, its role in promoting social reform and above all the capacity of scientific rationalism to contribute in creating better societies.

With the collapse of the Hapsburg Empire after the war, and the creation of the Republic of Austria, new political forces were unleashed. Both the ideas and the political struggles of socialists and communists had a profound impact on Popper who, at one stage, considered himself to be a Marxist and communist. He was almost seventeen when he witnessed a bloody confrontation between police and communists, an event that helped to steer him towards democratic socialism, and eventually to what can be called social liberalism. Later experiences of the dogmas and violence of national socialism were also significant in formulating his political views.

Vienna was also home to Sigmund Freud, the founder of psychoanalysis, and Alfred Adler, who formulated an influential theory of individual psychology based on the inferiority complex. Popper was familiar with their work, and became close to Adler, but soon rejected much of their thinking. As Popper recalls it, he was influenced by the implications of Einstein's revolutionary theory of relativity in theoretical physics. He was especially fascinated with what he saw as Einstein's scientific method that encouraged bold theories, dispensed with the goal of certainty and valued rigorous criticism. Popper developed such ideas in the context of arguments with members of the Vienna Circle, who were the most important exponents of scientific philosophy. Their members included Rudolf Carnap, Otto Neurath, Moritz Schlick and Viktor Kraft, and they had extensive network of correspondents and disciples throughout Central Europe and North America.

At the time at which Popper wrote, a widespread view of science was that it was based on induction, which is the inference of universal statements or propositions from a set of singular or particular statements such as the accounts of results of observations or experiments (Popper 1959, chapter I, section 1). In this view, the accumulation of past experiences allows the scientist to devise laws and make predictions about the future. For inductivists – scientists and philosophers of science – the logic of scientific discovery was identical to the 'logical analysis' of inductive methods (Popper 1959, chapter I, section 1). Although Popper previously accepted this principle, by 1930 he had come to see it as problematic. Following David Hume, Popper concluded that, from a strictly logical point of view, we are not justified in inferring universal statements from singular ones. By 1932 he formulated an alternative criterion for demarcating science from non-science, namely the principle of falsifiability. Falsifiability derives its strength from the logical point that, while it is impossible to verify universal statements on the basis of past singular statements, universal statements can be refuted by the acceptance of a basic or singular statement. Popper extended this epistemological principle into a scientific methodology, which became the focus of his book, *Logik der Forschung*.

2. POPPER ON INDUCTION

Although it may seem unusual given its central role in Popper's work, we decided not to commission a paper on Popper's treatment of induction. The reason was that the views of scholars with a specialist interest in this topic are varied but, by now, well entrenched. It seemed to us best if we were to offer a brief guide to Popper's views on the topic, and a consideration of what some of those most closely associated with him have made of his ideas. A useful starting point for any reader who is not already familiar with it would be Popper's (1974b) own treatment of the problem in his 'Replies to My Critics' in *The Philosophy of Karl Popper*, sections 13–15, or the material in section 7 of Popper (1985).

Popper, while accepting Hume's criticism of induction, argued that, in order to solve the problem of induction, one needed to reformulate it. Popper wished, particularly, to get away from formulations such as: 'What is the justification for the belief that the future will resemble the past? [or] What is the justification of so-called inductive inferences' (Popper 1974b, p. 1014). He argued, also, that one should discuss, separately, the logical problem of induction, the psychological problem of induction and the pragmatic problem of induction (see Popper 1972, chapter 1). To his responses to each of these we will turn, shortly.

As Popper (e.g. 1974a; 1976) explains in his intellectual autobiography, *Unended Quest*, he was for many years concerned with the problem of demarcation. That is, he wanted to know what makes scientific ideas distinctive, and how they could be distinguished from metaphysical and pseudo-scientific ideas. Popper has explained that what played the key role here was the idea that scientific theories are open to empirical test. That is, they can in principle be refuted. He also argued that this offered a resolution of the problem of induction. Popper endorsed David Hume's criticism of the claim that induction was a valid form of argument. Russell and others suggested that if Hume was correct, this was an intellectual disaster (e.g. '[Hume] arrives at the disastrous conclusion that from experience and observation nothing is to be learnt' [Russell 1946, p. 645]). Popper responded by arguing not that Hume was wrong in his criticism of induction, but that both science itself and common-sense knowledge are not inductive. Popper was critical of induction as a psychological theory and also at the level of logic.

In his first substantive work on the topic, *Die beiden Grundprobleme der Erkenntnistheorie* (*DbG*) (*The Two Fundamental Problems of the Theory of Knowledge*) (Popper 1930–33; 1979), Popper's approach was systematically anti-psychologistic. Popper developed his arguments in part by way of a detailed critical discussion of some themes in Kant, and of the ideas of the Kantians J. F. Fries and Leonard Nelson. He had had extensive discussions about this material with Julius Kraft (Popper 2008, chapter 1). Although Popper was not a thoroughgoing Kantian, Popper's response to this material served to distance his approach from the empiricism of the Vienna Circle. He also made suggestions about a non-inductivist psychology, which drew in part on studies, including his own, on issues in human and animal psychology (see Petersen's discussion [Chapter 3] in the present volume). He refers in this context to Karl Bühler, Otto Selz, H. S. Jennings and to the biological aspects of Ernst Mach's work. At the same time, however, Popper suggested that his own approach in *DbG* transfers ideas from his epistemology to psychology, and he was later to write of a 'principle of transference': 'what is true in logic is true in psychology' (Popper 1972, p. 6).[1] Exactly when Popper developed his approach is not easy to discern. Troels Eggers Hansen (2006; see also Hacohen [Chapter 2] in the present collection) discusses some of Popper's earlier writings in which he seems to take an inductivist approach to demarcation, and suggests that Popper might have read later ideas into his accounts of his intellectual development.[2] But it is clear that, in Popper's systematic writings on the topic from *DbG* onwards, his approach is non-inductivist.

We will address Popper's ideas about induction at three levels. First, what were they, and how did they change over time? Second, is it the case, as some of Popper's critics have suggested, that Popper's account is, in fact, inductivist in its character? That is, does it depend on covert inductive assumptions for its cogency? Here the discussion will be brief, as a particularly clear and in our view telling account of these issues is given in David Miller's work (Miller 1994, chapter 2; see also Popper 1974b). Miller documents and then critically discusses nine different objections, including ones concerning the presuppositions of science, the repeatability of tests and whether there are problems concerning Popper's account of the severity of tests. Third, there is the question: Is Popper's non-inductive account adequate; does it, in fact, solve the problem of induction? In particular, it has been questioned whether Popper offers an adequate understanding of the way in which, in one way or another, our actions may be guided by scientific theories, or of what is sometimes referred to as the 'pragmatic problem of induction' (Popper 1972, chapter 1). We will discuss these issues, by way of considering not just what Popper has had to say about these topics but also what has been made of them by some of those most closely associated with him. With this discussion, we aim to give an account of the state of play concerning Popper and induction; readers may then be able to pursue their interests in more specialised literature.

3. POPPER'S NON-INDUCTIVE THEORY OF KNOWLEDGE

In his 'Conjectural Knowledge' (Popper 1972, chapter 1), Popper suggested that one should look at induction at three levels.

First, there is psychology. Popper's approach here is to reject the view that we learn inductively (see Petersen's discussion in the present volume). There are various parts to his argument. Popper argues that in response to causal triggers (which we do not experience consciously as such), we offer conjectural interpretations, the adequacy of which may be checked on an ongoing basis. We operate psychologically by conjecture and refutation. Although certain kinds of responses may be biologically pre-formed, this does not mean that they will necessarily be valid. When discussing animals, he indicates that in some cases it would appear as if they are unable to learn that their interpretations are incorrect, and may suffer as a consequence (cf. Popper and Eccles 1977, Part I, section 24). Popper takes a biological – and implicitly an evolutionary – approach to all this. At the level of humans, he stresses the way in which description takes us beyond the content of what is directly experienced. In this vein, he points out the role of theories in influencing our psychological interpretation of the world. There is also a critical side to this approach. In an inductivist account, we start from resemblances between the things that we experience. By contrast with this, Popper argues that resemblance is always from a point of view – thus suggesting that purely inductive accounts of learning by repetition are flawed (Popper 1959, Appendix *x).[3]

Second, there is Popper's account of a non-inductivist epistemology. These are two elements to this: his ideas about 'basic statements' and his ideas about the evaluation of theories.

Popper's account of basic statements is discussed in detail in Andersson's contribution to the present collection (Chapter 5). But three features are here worth bringing out. First, in *Die beiden Grundprobleme*, there are, in effect, two complementary accounts. On the one hand, Popper offers an account of basic statements – influenced by both his non-inductivist psychological ideas and elements from his engagement with Kantianism. In this view, experience is produced as a reaction to a stimulus, but this is a matter of our producing things that are then matched against the world, rather than the content of our experience being given by instruction. There is, then, a more formal account of issues to do with induction, in which context experience is taken as given. Popper here places emphasis on the idea that what counts for the purposes of knowledge is experience that is repeatable. That is to say, the reports against which theories are tested consist not just of reports about what took place when a test was made but also of a formula or instructions for the production and testing of our results. There is, later in the book – and elaborated in *The Logic of Scientific Discovery* – an account in which two points are brought out. First, in line with the first element in *DbG*, Popper stresses the theoretical content in our descriptions, and how they go beyond anything that might be described as 'given'. Second, Popper makes explicit that our 'basic statements' are conjectural in their character, and gives an account in which the 'empirical basis' of science consists of an open-ended consensus as to what is the case. It is open to someone to challenge this consensus, and they might well be prompted to do so as a result of a new theory suggesting that hitherto accepted basic statements are incorrect. But this challenge would itself need to be tested, and if the previously accepted but now questioned basic statements are judged to be problematic, we would like to be able to offer an explanation as to how it was that things had looked as they had done to people in the past (cf. Popper 1972, chapter 5; Agassi 1966). Popper (1959) offers an account of basic statements as being about such publicly observable objects as the readings of pointers in a scientific laboratory. It is against claims understood in terms of such an account of experience that theories are to be tested. In *The Self and Its Brain* (Popper and Eccles 1977, pp. 106–07), Popper makes it clear that, in his view, his account can be extended to refer to people's psychological experiences. (For example, an approach developed by the Würzburg School suggested how claims about the character of people's psychological experiences – e.g. their experience of illusions – might be testable.) But we would use such an approach not to test, say, theories in physics, but rather only when our theories themselves are about aspects of human psychological experience.

This brings us to the second level – that of theories. Popper sees us as typically starting from problems, which may be posed by the disappointment of our expectations, or by the discovery that there is an inconsistency within our ideas. To such problematic situations, Popper depicts us as responding creatively. In *The Logic of Scientific Discovery*, he argued that our ideas here may involve 'creative intuition' (Popper 1959, section 2). But this is obviously compatible with our ideas being influenced by what Popper described as research programmes or by what Kuhn referred to as paradigms, or more generally by prior knowledge. Popper's concern was to stress that ideas were not produced simply by the phenomena that we were trying to explain. On his account, we seek to produce ideas that would serve as an explanation of the phenomena in which we are interested, or that offer a resolution of the problem which we wish to resolve, and which would themselves be independently testable, in their turn. As is well known, on Popper's account, boldness is represented as a virtue, and it is a particular point of merit if our ideas conflict with our previous assumptions. Science, for Popper – and, indeed, our knowledge, generally – is a process in which we come up with such testable hypotheses that are then tested. If and when these hypotheses are found problematic, new ideas are advanced in their place.

Two points are worth emphasising here. The first is that Popper (e.g. 1930–33, 1979; 1934, 1959) was well acquainted with conventionalist theories of science. Popper himself stressed that our knowledge faced the world as a system[4] and he was well aware that it was open to us to make modifications to it. He thought that conventionalism – the view that we should make minimal modifications to our existing knowledge – was a perfectly possible view to take. Nonetheless, he stressed that there was a contrast between conventionalist views on the aims of science and the dynamic character of science that so attracted him (Popper 1959, section 19). Although he did not put it in these terms at the time, Popper was a realist, who thought that we should be bold in our theories and open to the modification of our views in the hope of reaching truth about the world. In the light of this, he suggested various methodological rules that should be adopted with the aim of reaching this goal. For Popper, however, there clearly could be no guarantee of attaining the truth. But the fact that he recognised that someone could coherently be a conventionalist also made it clear that they – if their aim was different – could cogently adopt very different methodological rules to those which he favoured.[5]

The second point to be emphasised is that, as indicated earlier in the chapter, Popper argued that we should prefer bold theories. In *The Logic of Scientific Discovery*, Popper suggested that this approach offered a way in which one might explain the preference for simple over complex theories. He (Popper 1959, chapter VII) argued that, rather than having to make a substantive assumption here – the justification of which (like all justifications) would be problematic – the preference can be explained if simplicity is understood in terms of the degree of falsifiability of our theories. For Popper, such a preference could be argued for in methodological terms. Popper (1963, chapter 10, p. 241) subsequently referred to the idea that a new theory should proceed from 'some simple, new, and powerful unifying idea'. Maxwell's contribution to the present collection includes some critical discussion of these arguments.

For Popper, it is important that our theories be testable and tested. They should be put, especially, to tests that we would not expect them to pass. If they pass such tests, he describes them as having been corroborated. For Popper, our best knowledge will, at any one time, consist of bold, testable and well-corroborated theories. We may conjecture that they are true (or that they may represent progress over our earlier theories[6]). But he stresses that we cannot be sure that they are true. Here, the lesson of Einstein's challenge to Newton played a key role. In this case, Newton's theory, which had been better confirmed than any other, and in spectacular ways, turned out to be only an approximation to what we currently take to be our best theory in this field.

There has been extensive discussion of Popper's views on these topics, both as to their adequacy and as to whether, in some way, they are, in fact, inductive in their character. We can, however, spare the reader an account of this latter issue, because David Miller (1994), in the first part of chapter 2 of his *Critical Rationalism,* has offered a clear account of a range of important objections, to which he offers interesting responses in the second part of the chapter. Popper (1974b) himself also made a number of important rejoinders in his 'Replies to My Critics', sections 13–19 and 32.

We have, so far, briefly discussed two levels at which the problem of induction might be raised: psychology and Popper's non-inductivist epistemology. The third level relates to the question of whether Popper's approach to the problem of induction is able to deal adequately with problems about how scientific knowledge – and, indeed, common-sense knowledge – relates to our actions. What we seem able to accomplish as human beings has been transformed by scientific knowledge. Science has assisted us to explore space, treat diseases and construct computers that in turn aid us in achieving goals we could only have dreamed about in the past. All this takes place in ways that are influenced by our current scientific knowledge. Popper tells us 'that we should prefer the best-tested theory as a basis for action' (Popper 1974b, p. 1025). But can we understand such an idea without assuming some form of induction, and if we can, does such a non-inductive account actually resolve the 'pragmatic problem of induction'?

In discussing these issues, Popper has stressed the fallibility of even our best knowledge. He also pointed out how various examples of well-established regularities turned out to be false (or, as far as we know, are only true if they are reinterpreted).[7] All this can be granted. But there has been persistent concern expressed as to whether Popper's views are adequate to explain what we do seem to know and to do.

This is not the place to offer our own views about this topic. We consider, rather, that it would be useful instead to survey the views of some of those who have been most closely associated with Popper and his work, particularly Imre Lakatos, John Watkins, Alan Musgrave and David Miller. From this survey, the reader will get some sense of the discussion and will be in a good position to explore further literature.

Imre Lakatos (1922–74) was a brilliant Hungarian philosopher of mathematics, who from 1960 was a colleague of Popper's at the London School of Economics. During the late 1960s, his interests turned to the philosophy of science, and, in two major papers, he wrote critically about Popper's work (see Lakatos 1978, chapters 1 and 2). Also, in the course of his contribution to *The Philosophy of Karl Popper* (Lakatos 1974; Lakatos 1978, chapter 3), he argued that Popper's views were, in the end, sceptical, claiming that 'only a positive solution of the problem of induction can save Popperian rationalism from Feyerabend's epistemological anarchism' (Lakatos 1978, p. 166). What was needed, on his account, was a '*synthetic* inductive principle connecting Popperian *analytic* theory-appraisals (like content and corroboration) with verisimilitude' (cf. Lakatos 1978, p. 163).

There are, however, two problems about any such proposal. First, what is its content supposed to be (bear in mind here the known fallibility of both some common-sense knowledge and some of our best scientific theories)? Lakatos suggested (Lakatos 1978, p. 164) that such a principle would need to be related to a 'major research programme' and also to be '*sufficiently richly formulated so that one may … criticize our scientific game from its point of view*' (*ibid*). Lakatos did not himself develop his idea further. One might, however, consider the approach of Nicholas Maxwell (see his contribution to the present volume [Chapter 7] for references) as offering something – albeit not an inductive principle – that might usefully be related to Lakatos's hopes about the criticism of science while at the same time avoiding problems that both Lakatos and Maxwell think face Popper's account (see, for some critical discussion, Miller 2006, pp. 92–94).

Second, is such a principle something that has to be rationally justified? From a Popperian perspective, there is nothing to stop anyone from adopting such a principle, but it would just be a hypothesis. Zahar offers an inductive principle in the tradition of Lakatos's work, which he argues to be synthetic *a priori* (Zahar 2007, p. 45). Zahar, however, also states that it is 'unverifiable and uncriticizable' (*ibid.*). It is not clear why we should adopt such a view, while any attempt to *justify* it seems to lead us straight back to the problem of induction.

The next two approaches, by John Watkins and Alan Musgrave, might be seen as having taken off – albeit in rather different directions – from a remark of Popper's (1974a, p. 119; 1976, p. 149): 'we can never justify a theory. But we can sometimes "justify" (in a different sense) our *preference* for a theory'. Watkins and Musgrave, however, approach this task in rather different ways.

John Watkins (1924–99) was a colleague of Popper's from 1958, and was for many years a dogged but also very creative defender of Popper's approach to philosophy. In his *Science and Scepticism* (Watkins 1984) he set out to offer a systematic 'neo-Popperian' approach to the philosophy of science. With regard to induction, Watkins tried to justify a preference for a theory without involving inductive assumptions. He did this in part by way of description of and argument for an optimum aim for science (briefly, that science is after theories which are possibly true, and also deep), and then arguing that the theory which was best corroborated would best satisfy this aim, and that it was thus rational to choose it in preference to alternatives. He also argued that, if corroboration was indeed non-inductive, the pragmatic problem of induction stood in need of an additional line of argument if it were to be resolved.

This argument Watkins provided by suggesting that a policy of acting in a way that ran counter to our currently best-corroborated theory would involve making additional assumptions not made by the first theory. Watkins suggested that the fact that other theories made these additional assumptions constituted a basis on which the first theory could rationally be preferred to them. Watkins's ideas have been strongly contested (see D'Agostino and Jarvie 1989). As Watkins recounts in his characteristically self-deprecating way in 'How I almost solved the problem of induction' (Watkins 1995a; see also Watkins 1991, section 6), he was in the end convinced that his approach was problematic by an argument offered by Howson (1991). For on the basis of arguments that Watkins had himself used earlier in his book about the relation between probability and content, Howson argued that Watkins, in preferring the theory that he claimed was weaker, was in fact assuming something that implies an inductive principle (Howson 1991, pp. 80–81). Watkins in the end came to the conclusion that his version of a Popperian response to the pragmatic problem of induction was unsuccessful. Nonetheless, the debate initiated by Watkins's book has been wide-ranging, and anyone interested in these issues should consult the book by D'Agostino and Jarvie (1989), which contains a number of chapters critically discussing Watkins's *Science and Scepticism*, and also Watkins (1991), in which Watkins responds to these chapters.

Alan Musgrave was also a one-time colleague of Popper's, and has made many important contributions to the development of critical rationalism; he is a contributor to the present volume. He has been involved in argument with Watkins and with Miller (whom we will discuss below) as to how Popper's approach to the problem of induction is best interpreted. Musgrave (1999, p. 314) notes that it is often said that 'family quarrels are the worst of all', and the argument between Watkins, Musgrave and Miller has certainly been vigorous. Musgrave claims that 'either Popper solves the problem of induction in the way I think, or he has no solution and his numerous critics on the point are right' (*ibid.*). Musgrave's view is that 'if a hypothesis has withstood our best efforts to show that it is false, then this is a good reason to believe it, *but not a good reason for the hypothesis itself*' (Musgrave 1999, p. 322). Musgrave, and also Miller, agree with Bartley (1964, 1984) that a key element in Popper's approach is the abandonment of the idea that we should seek to justify our claims to knowledge. But on Musgrave's account, critical rationalism involves an 'epistemic inductive principle' that 'It is reasonable to adopt as true (to believe) the best-corroborated hypothesis' (Musgrave 1999, p. 327). This principle itself cannot be justified, but Musgrave thinks that we can give a reason for its *adoption* (as distinct from a reason for the principle itself). This is 'because it has withstood criticism better than its rival justificationist epistemic principles' (Musgrave 1999, p. 329).

Miller has argued (1994, chapter 6, part 4), that Musgrave's account runs into problems. He suggests that the reasons offered for its adoption can lead to only something with less content than the premise that Musgrave is seeking to support. While it would be open to Musgrave to adopt his approach simply as a conjecture, Miller argues that claims about the reasonableness of the approach 'are all window dressing' (Miller 1994, p. 124). Miller in his (2006), chapter 5, p. 128, returns to the discussion of Musgrave, and after noting that his own approach is closer to Musgrave's 'than it is to any other species of justificationist', he restates his disagreement, and stresses that their differences are not just verbal.

What, then, is Miller's view? He (1994; 2006) undertakes a spirited defence of a Popperian approach, which eschews any element of justification or induction. Miller (who is also a contributor to the present volume) states that 'I agree with Hume … that there exist no reasonable or rational beliefs, but I disagree with Hume that this plunges us into irrationalism.' (2006, p. 128).

Watkins has usefully summed up Miller's approach, as follows (Watkins 1984, p. 342):

> The aim of science is simply to try to separate empirical hypotheses into those that are true and those that are false. In attempting this it relies solely on expulsion procedures: any testable yet unfalsified hypothesis is admissible into the body of scientific knowledge; but once admitted it is subject to rigorous attempts to expel it by subjecting it to tests which, it is hoped, will reveal it to be false if it is in fact false ... it is rational to act on hypotheses that have, so far, been retained rather than ones that have been expelled, just because we have no reason to suppose that the former are false while we do have reason to suppose that the latter are false.

Such an account – which would need to be interpreted in the light of Miller's disavowal that there are reasonable or rational beliefs (Miller 1994, chapter 3; 2006, p. 111) – would seem to face challenges in two particular areas relating to practical action.

On the one hand, Watkins stressed (1984, p. 342), following Salmon (1968, p. 26; 1981, p. 117), the idea that, while we may have been testing one particular theory, if it has not been refuted, there may well be 'a plethora of unrefuted and mutually conflicting hypotheses relevant to our practical problem'. Nicholas Maxwell (compare his contribution [Chapter 7] in the present collection) has also stressed the significance of such alternative theories. Miller (2006, chapter 5, section 3) has responded to this slightly differently depending on whether or not we are dealing with ordinary theories, or 'grue[8]-style' variants on the theory that we have been testing. The details of his response we will leave to the reader to explore. It is striking that Popper himself, when discussing Goodman's ideas,[9] has stressed that it is not the case that there is a paradox to be overcome, or that a standard theory is rendered more probable by confirming evidence than is some Goodmanesque alternative.[10] The task for the Popperian is to explain why, if this line of argument is used as a stick with which to beat the inductivist, it does not pose a problem when we are concerned with the choice as to whether to make use of a theory, or its Goodmanesque equivalent – which could make different predictions – when we are acting. Responses have been offered to this by Bartley (1968), Popper (1983, pp. 67–71), Watkins (1984, pp. 313–15) and Miller (2006, pp. 123–24 and 130–32). It is a matter of ongoing controversy as to whether such responses are satisfactory.

The second problem relates to whether action is anyway best seen as based on our best and best-tested theories – something that might seem to suggest the need for a link between past success and success in our future endeavours. Can this be a matter just of conjecture, or is something more needed – and, if so, does any such positive response simply lead us back to the problem of induction? Miller, here, has suggested (2006, p. 118) that it is misleading to suggest 'that practical action involves acting "on the basis of theories"'. He suggests, rather, that 'it is not scientific theories that the agent discusses critically, but proposals for action' (2006, p. 119). This is a suggestion that Popper endorsed (see Popper 1974b, p. 1025, which acknowledges the suggestion as Miller's). However, Popper did not elaborate upon it. Miller's (2006, chapter 5) 'Induction: A Problem Solved' sets out in some detail his response to the pragmatic problem of induction, and elaborates upon this suggestion. Musgrave (1999, pp. 332–36) offers some strongly worded criticisms of Miller's approach.

Just because these are issues of ongoing contention between scholars with expertise in these matters, it would not be appropriate for us to say more here. But the books and papers to which we have referred will give the reader a good feel for the points at issue, as well as an introduction to wider literature on Popper and induction.

Popper's discussion also involved a critical analysis of probabilistic theories of induction. His arguments run through much of his work from *Die beiden Grundprobleme* (Popper 1930–33; 1979) to Popper and Miller (1983) and (1987). Other important arguments are offered in Lakatos (1978, chapter 8), Watkins (1984, chapter 2), and in Miller (1994; 2006), as well as in sections 4.1 and 4.2 of Miller's contribution (Chapter 9) to the present volume. The Popperian task of engaging with probabilistic theories of induction rivals that of Sisyphus, in that as soon as a critique of a proposed theory of probabilistic induction is concluded, another proposal turns up to take its place.

To conclude, the problems facing any positive theory of induction would on the face of it look daunting. Clearly, the fact that some of our best-confirmed theories in the past have proved to be incorrect in unexpected ways should give us reason to be cautious. Furthermore, our experience to date may well have been unrepresentative of how things are in general, or God may well simply have been perverse when he designed the fundamental laws that underlie the universe. The only thing that might be said, however, is that all these problems would also seem to face us when evaluating proposals as to how we should act. Nor is it clear that a 'Popperian' solution to the pragmatic problem of induction can be expected to overcome them.

4. FALSIFIABILITY

It is worth setting out here the central principle of falsifiability. Popper argued that a theory or system of theories was only empirical and scientific if it was capable of being tested or refuted by experience (Popper 1959, chapter I, section 6). As Popper presents it in *The Logic of Scientific Discovery*, if a single scientific theory or hypothesis is to be falsifiable, it should have the logical form of 'strictly universal statements', which could be expressed in the form of *prohibitions* or 'negations of strictly existential statements' (Popper 1959, chapter III, section 15). Any hypothesis must be able to generate singular empirical statements that would, if confirmed or corroborated, refute the theory. Once a theory is accepted as falsifiable and therefore scientific, then the theory that is more falsifiable is to be preferred to that which is less falsifiable. The more 'basic statements' or predictions that are forbidden by it, the greater the empirical claims, and the more falsifiable the theory (Popper 1959, chapter VI, section 31).[11] Popper offers both a normative and historical theory of the growth of knowledge that reaches beyond science to offer a more general epistemology and cosmology.

5. POPPER'S INTELLECTUAL HISTORY

The opening of the Popper archive at the Hoover Institution Archive in the 1990s, and the making available of early papers, documents and letters, has stimulated a new period in Popper scholarship (Hacohen 2000, p. 12). One of the outcomes has been a debate between Hacohen (2000; 2006; Chapter 2 in the present volume), John Wettersten (1992; 2005; see also Berkson and Wettersten, 1984), Troels Eggers Hansen (2006) and Michael ter Hark (2004; 2006) about the stages in the evolution of Popper's philosophy at this stage of his life.

For this volume, Malachi Haim Hacohen has reviewed in some detail many of the important features of the early development of Popper's thought, and he explains and comments on some of these controversies, as well as discussing issues concerning the relation between the work of Popper and that of both Leonard Nelson and Heinrich Gomperz. While Hacohen's treatment here clearly cannot replace his remarkable book on Popper's early development (Hacohen 2000), it provides an overview of a number of important themes discussed there, while updating his earlier account.

Arne Petersen examines Popper's early critical engagement with psychology, and demonstrates how it influenced his views on induction, and his later recourse to biology as a resource for understanding epistemology and cosmology. Petersen shows how Popper formulated his criticism of induction and his own deductivist theory from his studies of learning and habit formation in psychology. Although Popper only published papers on the centrality of problem solving in the growth of knowledge from the 1960s, the antecedents can be found in his earlier work. Drawing on Darwin's evolutionary theory, Popper (1940, p. 403;

1963, p. 312) had referred to the vital role that 'the method of trial and error' played in adaptation, and he later generalised this method with reference to conscious problem solving to describe how human beings, philosophers and scientists advance their knowledge. That is, Popper proposed a unified method for understanding both the processes of natural selection *and* learning. It was, however, his criticisms of psychology that first led him to reject the relevance of the psychology for scientific method, and to explore the deductive logic of scientific discovery.

6. PHILOSOPHY OF SCIENCE – EPISTEMOLOGY, METHODOLOGY AND METHOD

Peter Godfrey-Smith discusses four themes in Popper which he believes have particular relevance for current work in the philosophy of science. (These are not the only things that are, in his judgement, of philosophical importance, including Popper's anti-foundationalism; he also refers to Popper's work on probability, which is treated elsewhere in this collection by David Miller.) Godfrey-Smith discusses, first, Popper's stress on theory assessment as operating by way of ruling out alternatives; this he describes as 'eliminative inference'. Second, there is what he calls Popper's 'sceptical realism': Popper's espousal of realism as the aim of science, but without this being coupled with claims about our success, so far, in the pursuit of this aim. Godfrey-Smith argues that this view has been neglected: those who have championed realism have typically done so in a manner that associates the cause of realism with a non-sceptical view of our current theories. Godfrey-Smith, however, himself prefers what he calls a 'particularist' view but he argues that sceptical realism helps make the option visible. Third, there are issues about the importance of risk, and Popper's opposition to the idea that our experience of instances which confirm a generalization should be taken to support it. Godfrey-Smith discusses Watkins' development of a Popperian approach here, and also the way in which there is common ground between Popper and some recent Bayesian writers. Finally, he discusses what he calls Popper's 'diachronic' approach to evidence, which is concerned with changes over time.

The problem of the 'empirical basis' is the subject of Andersson's chapter, which addresses criticisms that have been directed at Popper's early methodological rules on 'basic statements'. Gunnar Andersson contributes a wide-ranging essay on Popper's theory of the empirical basis. After explaining Popper's theory and contrasting it briefly with Neurath's views, he discusses a range of issues that have been raised about Popper's theory. He considers Popper's account of the relation between basic statements and falsification, the relativity of basic statements, and whether and in what sense their acceptance is in part a matter of convention. The latter discussion includes consideration of a topic on which there has been much discussion: the relation of basic statements to people's subjective experience, and whether observations constitute inconclusive reasons for the acceptance of test statements. The latter discussion leads us back to wider questions about Popper's approach to justification.

In a number of places *The Logic of Scientific Discovery* Popper relinquishes his spare mode of expression to make recourse to striking metaphors. The opening quotation from Novalis[12] asserts that hypotheses are 'nets'. In describing the empirical basis of science, Popper (1959, chapter V, section 30) claims that the theories of science arise, as it were, 'above a swamp', into which the 'piles' are driven until they 'are firm enough to carry the structure' (of our knowledge) for the time being. Of particular interest to Popper's larger philosophy is his use of the Darwinian language of *'struggle, competition, fitness and survival'* to describe the general methodology of theory evaluation. Yet, as Michael Bradie points out, Darwin's name does not appear in *The Logic of Scientific Discovery*. From the 1960s, however, Popper addressed Darwinian theory more directly to argue that there is a close connection between, or common logical core to, Darwinian evolutionary theory and epistemology, understood as the growth of animal and human knowledge. The outcome was an 'evolutionary epistemology' that incorporated insights from the natural process of evolution and those from analysis of philosophy and science. Popper saw a similar process of problem solving by trial and error operating in what he called the World 1 of the physical universe, in the World 2 of human consciousness and in World 3 of objective knowledge, as well as in the relations between these worlds. Bradie claims that Popper did not take Darwinian theory as a model for Popper's method of conjectures and refutations, but rather that the latter shed greater light upon evolutionary theory.

For Popper, Darwinism 'provided a unified picture of the evolution of the universe, the evolution of life on earth, and the evolution of human knowledge' (Bradie, Chapter 6 in this volume, p. 153). This is evident in Popper's (1972 [1981], p. 261) reference to the amoeba and Einstein who share the same method of achieving and improving their knowledge: '[W]e try to solve our problems, and to obtain, by a process of elimination, something approaching adequacy in our tentative solutions.' Bradie (p. 165) agrees that Darwinism remains the 'best explanation we have for the emergence of minded creatures capable of knowledge and critical reflection'. But he is doubtful that it is the best model for the growth of human knowledge, and thinks that there may not be anything more than suggestive analogies between the two processes.

7. COSMOLOGY AND METAPHYSICS

As the discussion of evolutionary epistemology may suggest, Popper's epistemology and philosophy of science is part of a larger philosophical, indeed cosmological, project. Nicholas Maxwell presents Popper as a cosmologist who is interested not only in science but in the larger role and place of knowledge in the world. What Maxwell calls 'natural philosophy' raises problems and questions that cannot be confined to conventional disciplinary specialisations. Maxwell argues that Popper's cosmological aspirations are frustrated by a few of his key philosophical arguments. For example, Popper's project to demarcate science from metaphysics would limit 'the scope and viability of natural philosophy, which is based on the *integration* of science and metaphysics' (Maxwell, Chapter 7, p. 175). Maxwell's response is to propose a cosmology that integrates the different components – science, metaphysics, methodology and philosophy of science – within a Popperian critical tradition. For Maxwell, the recovery of both cosmology and the Popperian critical spirit are essential if modern Western societies are to stimulate greater interest and participation in scientific inquiry in schools, and reverse the current 'flight from science'.

The question of metaphysics leads us to consider the substantive content of Popper's interventions in the field, and particularly his realism. Alan Musgrave discusses Popper's arguments about realism, and explains how he can be both a realist and a fallibilist. At base, common-sense realists assume the existence of observable things and objects independent of our knowledge of it. Building on the same assumptions about the objects postulated by scientific investigation, scientific realism rejects instrumentalist, positivist or antirealist philosophies of science. A third form of realism is Platonic in its assertion of the objective existence of numbers, problems and propositions that, while produced by the mind, come to exist independent of it. Popper is a realist on each of these levels. Early in his work, however, Popper tried to avoid metaphysical assumptions by converting them into methodological rules and precepts. He later adopted what Musgrave calls epistemic scientific realism. For Popper, scientists must search for the truth, but can never be sure that they have found it. This drive to find true explanations of the world that must continually be tested enables him to avoid relativism and scepticism, and to retain fallibilism. Popper's 'critical realism' is based upon a thoroughgoing critique of idealism in all its forms. Although scientific instrumentalism and conventionalism may allow successful predictions of phenomena, this was insufficient for Popper who required science to provide explanations. According to Alan Musgrave, Popper's argument and method also enabled him to 'accept scepticism about certainty, yet make room for rationality' (Musgrave, Chapter 8, p. 224). The doctrine about the existence and autonomy of World 3 entities is more problematic and controversial. Musgrave argues that Popper's World 3 does not comprise the eternal and acausal objects of the Platonic kind, or indeed objects of any kind. Although Popper sees their 'causation' lying in their capacity to exert influence on the physical world, Musgrave thinks that this is misconceived. The influence occurs by philosophers and scientists, for example, 'grasping' theories and arguments.

There are other reasons for Popper to propose a World 3, and that is because it forms part of his argument for indeterminism, which in turn supports his arguments for freedom and free will (Stokes 1998, p. 99). In brief, Popper rejects determinism because, as he sees it, it characterises the world as a huge machine in which there is no human freedom, creativity or moral responsibility. Indeterminism, on the other hand, allows for the possibility of freedom and creativity. A theory of freedom requires the 'causal openness' of the physical world to the world of ideas (Stokes 1998, p. 113). In this larger context, one can understand why Popper would want to argue for the objective reality of ideas and their influence in science, society, and politics.

Further support for the idea of causal openness of the physical world comes from Popper's arguments about probability, and especially his propensity theory of probability. Popper originally adopted an objective, frequency theory of probability. Nonetheless, given that no single event or series of events could falsify a probability hypothesis, his early concern was to show how probability statements could be rendered into falsifiable form. To this end, he formulated a methodological rule requiring probability hypotheses to indicate what sequences would be either prohibited or permitted. In practice, scientists would need to stipulate in advance what degree of deviation from the norm is to be allowed in a particular probability estimate. Where reproducible sequences regularly fell outside those permitted, the hypothesis could be said to be falsified.

In the 1950s, however, Popper (1957a, p. 68) formulated a propensity theory of probability that interpreted it as 'a characteristic property of the experimental arrangement rather than as a property of a sequence'. Miller notes that this was the first of three stages over which Popper's propensity theory evolved. The second stage, according to Miller (p. x), 'gives primacy to single-case probabilities as singular dispositions or propensities'. In the third stage, Popper extends his theory beyond dependence upon repeated events produced by a closed experimental arrangement to include the probability of unique events in open systems characteristic of nature and society. Such events could include the origin of life and the emergence of consciousness, or the occurrence of a world war. With this step, Popper brings to prominence the phenomenon of 'emergence' as further evidence in support of indeterminism. Popper's propensity theory signals a new physical and metaphysical hypothesis about the indeterminate nature of the world, and the possibilities for exercising freedom and creativity.

The primary source of human creativity is the individual person and their mind, who has ideas, formulates plans and so on. It is important for Popper's theory of freedom that mind and consciousness are not identical with the physical states or realities of World 1. For this reason, his theory of mind is a form of dualist interactionism. That is, human minds are not just brains, they have an independent existence or reality of their own, and are active interpreters of the physical and social worlds. World 2 is considered real because of its capacity to interact and causally influence the other two worlds. Popper therefore also rejected materialist theories of mind that reduced our mental states to brain states. Popper also rejected materialism because he thought that a mechanical view of the mind was false, and, if adopted, it would tend to undermine a humanist ethic (Popper and Eccles 1977, p. 5). Frank Jackson shows Popper's reasoning about how the three-worlds metaphysics contributes to his argument on the mind, but is doubtful that it defeats sophisticated materialist arguments, or advances the debate in the way Popper wanted.

8. PHILOSOPHY AND METHODOLOGY OF SOCIAL SCIENCE

Questions of metaphysics, theory, methodology and practice come together again with a different focus in Popper's arguments concerning the philosophy and methodology of social science. An abiding motivation for Popper was to demonstrate the power that certain political ideas could have over individuals. Through his critique of historicism and numerous associated arguments, Popper sought to show how methodological ideas and assumptions about social inquiry, such as what he termed 'historicism', could crush individual freedom and legitimate political violence. The major problem with historicism was its unscientific espousal of absolute social and political trends, which constitute unfalsifiable prophecies rather than scientific predictions, and to which individuals must conform.

In response to historicism, he articulated a philosophy and methodology of social science that amounted to a more or less comprehensive or unified theory of methodology that, with some qualifications and adaptations, was applicable to both social and natural science. Popper proposed a 'unity of method' that consisted in focusing on practical problems, formulating conjectures about how to solve them and ruthlessly testing these tentative solutions with critical argument. As Ian Jarvie points out, differences between the sciences were dependent upon the kinds of aims or interests they had, or the problems they wished to solve. Theoretical sciences such as physics or sociology were interested in seeking general explanatory laws; historical sciences such as geology or social history were interested in determining particular sets of 'initial conditions'; applied sciences such as engineering, whether in their technological or social forms, had an interest in practical application and making predictions.

All sciences, however, begin with interpretations or 'untestable general presuppositions' (Jarvie, Chapter 11, p. 287), and at their most general, these pertain to ontology, or the kinds of objects that exist in the world. For Popper, the social world comprises individuals, institutions and traditions. Jarvie stresses that Popper was ambiguous about the reality of collectives and social entities, and that the more important point to be taken from his discussion is his advocacy of methodological individualism. This is the requirement that we must try to understand and explain collective phenomena and sociological models in 'descriptive or nominalist terms, that is to say, *in terms of individuals*, of their attitudes, expectations, relations, etc' (Popper 1957b, part IV, section 29). This is essentially a moral requirement for methodology, rather than an ontological one. Its rationale lies in its allowing individuals to see how they can take greater moral responsibility for their actions, as opposed to relinquishing their responsibilities in the face of apparently overwhelming forces. As Jarvie indicates, such a view does not rule out understanding the role of larger social forces or 'wholes', but suggests that explanations that do not make reference to the 'goal-directed action of individuals are unsatisfactory' (Jarvie, Chapter 11, p. 304).

Another ambiguity arises from Popper's proposals for implementing a morally responsible politics. On the one hand, he warns us about the unanticipated consequences of our actions and the perils of large-scale and ill-conceived government interventions in people's lives. Yet, he did not reject use of the state to engage in piecemeal social engineering for overcoming social ills, provided there was a 'compelling need' (Popper 1963, chapter 17, n. 28). Such qualifications suggest a key role for values in social science.

Like Max Weber, Popper acknowledged that there could not be a value free or neutral social science, if only because science was guided by epistemic values such as truth, relevance and simplicity. Nonetheless, it remained vital to distinguish between these epistemic values, and the non-epistemic ones of religion and politics, while recognising that most significant social science had its origins in problems whose terms were set by non-epistemic values such as social justice, freedom or equality.

For Popper, objectivity is not provided by an absence of bias in the individual social scientist, but by the cooperative social process of intersubjectivity, in which scientists subject theories and arguments to critical scrutiny.

Popper is cognisant of the role of self-fulfilling prophecies as complicating, but not insuperable, factors in social scientific explanation, which are based upon understanding the individuals' knowledge of their situation, and what it may require or lead them to do. This dimension of rationality is distinctive to social science, but it too can be absorbed into a deductive model. Popper argues that there is an element of (instrumental) rationality in most social situations that allow us to create models of them, somewhat along the lines of Weber's ideal model (Popper 1994, chapter 8). Once the logic of the situation (an ideal model) has been constructed, based upon an assumption of complete rationality (goals and knowledge), the social scientist can then predict the patterns of behaviour, and assess any deviation of behaviour from the model. Where the behaviour largely conforms to the model, it is explained. Where human behaviour departs significantly from the model, one of two options is possible. One can either dismiss the model as refuted, or seek to explain how and why the deviation occurred.

As Jarvie sees it, one of Popper's contributions to the rationality debate is to allow rationality to include errors due to imperfect knowledge, incorrect assumptions or even delusion. Thus, Popper accepts that there are degrees of rationality, and this fits with his understanding of a history of science that recognises progress without condemning earlier science to irrationality. The achievements of early scientists were therefore appropriate to the logic of their epistemological situation. Such arguments also avoid relativism, and offer a plausible but largely overlooked counter to the extremes of postmodernism and poststructuralism.[13]

9. POLITICS AND POLITICAL THEORY

It is this qualified epistemic universalism that is the subject of Stokes's chapter, which examines the intersection of social science methodology and politics in the context of Popper's debates with the Marxist philosophers of the Frankfurt School of critical theory. Although Popper's primary opponent was Theodor Adorno, a more junior member of the group, Jürgen Habermas, also took a tough stand against what they considered to be Popper's uncritical support for a liberal capitalist society. Although some lines of agreement seemed to emerge out of the debate over this 'positivist dispute' or 'Positivismusstreit' in German sociology, the personal and intellectual scars remained strong. As a result of these debates in the 1960s, the philosophies of Karl Popper and Jürgen Habermas are often considered to be in irreconcilable conflict. Divided over issues in social science methodology and on political ideology, Popper and Habermas seemed to share little common ground. Nevertheless, Stokes, among others, identify not only common problems and themes but also a number of shared values and assumptions.

The value of freedom of speech and communication, for example, is central to both philosophers. For Popper, a key requirement of the open society is the freedom to criticise political and intellectual authority, while Habermas demonstrates the importance of open, undistorted communication. In Popper's sketch of the 'open society' and Habermas's concept of an 'ideal speech situation' can be seen a normative convergence in their thought. Both philosophers advocate a public sphere characterised by free dialogue and criticism set within a democratic context. Both also give a key role to problem solving in their later philosophies of knowledge and politics, and both are fallibilists. Stokes draws out the similarities between the work of Popper and Habermas, and indicates the nature and significance of this convergence. The chapter also indicates the implications for the theory and practice of democracy. Stokes argues that both Popper and Habermas, in different ways, lead us towards more deliberative forms of democracy, both within nation states and beyond them.

The complex interrelationship between a fallibilist epistemology and political practice are central to Popper's political theory. Given Popper's political evolution through Marxism and social democracy towards liberalism, it is not easy to give a stable characterisation of his position. Nonetheless, whereas much of the policy content of the *Open Society and Its Enemies* appears to have its origins in a reforming social democracy, Jeremy Shearmur points out his affinities with the modern republican tradition. This is arguable because of Popper's distinctive combination of ethical individualism and his requirement that the state protect individuals from economic exploitation. Popper's republicanism comprises two main aspects that bear upon the role of the state and the individual. First, it is set within an epistemology that stresses the limits to our knowledge, which also restrict what governments can and should do. That is, governments can engage in piecemeal social engineering tempered by critical feedback, but should not engage in large-scale holistic, or utopian, social engineering. This is because of the ever present possibility of unintended consequences that may threaten individual liberty. Second, his republicanism also rejects any kind of historical inevitability, and requires individuals to take active moral responsibility for social improvement. In this context, Popper's republicanism has a social democratic dimension in his requirement that governments must intervene to alleviate concrete evils such as economic suffering. Such concerns also lead Popper not to rule out experiments with socialisation of monopolies.

Shearmur offers guidance on what is of lasting value in Popper's political theory and what ideas could fruitfully be pursed further. He finds Popper's arguments for what has been described as negative utilitarianism a promising way to approach decisions about social problems. For Popper, our public values and policy should be directed towards overcoming pain, suffering and misery, whereas the pursuit of pleasure and happiness generally ought to be the province of our private values and individual action. Popper also thinks that it would usually be easier to gain agreement on pressing, concrete social issues from people/citizens, such as liberals and socialists, who may have significantly different and conflicting political outlooks. Accordingly, Popper distinguishes between public values, which are 'negative' in character and oblige governments to attempt to 'minimize pain', and the 'positive', private values, such as 'maximising pleasure', which ought not be the goal of public policy. Nonetheless, the state also ought to intervene in a limited way to protect these private freedoms, and to encourage free competition of thought and freedom of choice (Popper 2008 [1946], p. 126).

Popper's normative theory of democracy, however, is an undeveloped liberal one that considers democracy simply a useful method for being able to change governments without revolution. Although critical dialogue is an essential part of ideal scientific practice and essential for an open society, it does not have the same prominence in his democratic theory. That is, critical intersubjectivity, which resembles the processes of deliberative democracy, is not as developed as in the works of other contemporary political thinkers, such as Habermas. Shearmur explains that Popper was not optimistic about what could be expected from critical debate because he thought that, although we may all learn something new, consensus – often the goal of deliberative theorists – may not be possible.

Shearmur argues that even liberal representative democracy is ill-suited to fulfilling the Popperian values of fallibilism and learning from error. Few political leaders or public servants, for example, are willing to take responsibility for their mistakes. As Shearmur perceives them, politicians will pursue the interests and ideas that will get them re-elected, and this leads to populist tendencies. Historically, the emergence of a deliberative public sphere soon led to the authoritarian machine politics of parties that worked to limit deliberation. These impediments would press those inspired by Popper to give more careful consideration to the institutional arrangements needed to protect and enhance the values of fallibilism, and to counter dogmatism. Shearmur argues further that a Popperian program could well benefit from a dose of classical liberalism and markets, joined with what he calls experimental social holism.

10. CONCLUSION

Although Popper did not set out to create a unified system of thought, the problems he addressed over the years and the solutions he proposed led to an integrated philosophical world view. This world view is predicated upon several epistemic and non-epistemic values. The epistemic commitment to fallibilism is linked with the moral priority he gives to human freedom. His political theory of democracy and the open society, and his metaphysics, exemplified in his realism, indeterminism and the larger three-worlds cosmology, articulate a multifaceted theory of freedom. It is important to understand, however, that Popper's conception of freedom is not that of the classical liberal, but that of an interventionist republicanism. Fallibilism is first a normative epistemological commitment that presses philosophers, scientists and other seekers of knowledge to make imaginative conjectures tempered by ruthless testing and critical discussion. But fallibilism is also an ineradicable feature of our (Darwinian) human condition in which we acquire knowledge by trial and error. Both dimensions of fallibilism, however, have political consequences. By recognising the limits of our knowledge we are encouraged to accept a certain humility that should restrain us from imposing our views on others.

For various historical reasons, the virtues and utility of Popper's epistemology have not been sufficiently appreciated. Indeed, it is paradoxical that Popper's death in 1994 was probably one catalyst for a more rigorous scrutiny of his published and unpublished works. Accordingly, his work continues to set intellectual and political agendas. Many philosophers have yet to absorb and analyse his disparate writings on rationality, or accurately understand his distinctive place in the history of philosophy. A member of neither 'analytic' nor 'continental' traditions, Popper's work traverses most of the same problems but from a perspective that seeks to avoid linguistic puzzles or convoluted prose.

Politically, the rise of religious fundamentalism has made the tasks of achieving and maintaining an open society as pressing as they were when Popper was writing during World War II. Not only are open societies threatened by the dogmatism of fundamentalists, but the growth of the security state and antiterrorist strategies have resulted in systematic invasion of privacy and the erosion of civil and political freedoms. Economically, globalisation has encouraged more open economies, but the social turmoil engendered has often provoked retreats towards ethnic, racial and religious closure. One continuing task therefore will be to assess how much openness is possible in societies, at different stages of political development.

The preceding observations lead to several imperatives. First, there is a need to understand better the sociology and psychology required for an open society and productive critical deliberation. Second, it is vital to revise public policies and re-establish institutions to protect not only freedom of thought but specifically the conduct of science and communication of scientific knowledge. The latter returns us to some of the most valuable contributions that Popper has made to our understanding of science, namely that it is a social process guided by powerful norms, but which can only survive if it is given external political and institutional protection.

NOTES

1 On Popper's psychological work and its relation to that of other writers, see Hacohen (2000) and his contribution to the present collection, as well as Berkson and Wettersten (1984), Wettersten (1992), ter Hark (2004) and Petersen in the present collection.
2 See also Hacohen's contribution to the present collection. Note, however, the point made in Miller (2006, Preface), that it may be illegitimate to judge someone's views on the basis of material from a Ph.D. thesis, which may have been included with an eye to the views of likely examiners.
3 There is a lot of material on these issues in Popper (1930–33, 1979); Popper (1959); Popper (1963, chapter 1), Popper (1972, Appendix 1) and Popper (1976) and his contributions to Popper and Eccles (1977) and in Popper (1983). For further discussion, see Hacohen's (2000) and his and Petersen's contributions to the present collection, as well as Wettersten (1992) and ter Hark (2004).
4 Cf. Popper (1959, section 18), where he discussed this in terms of a theory and initial conditions, and section 20, which considers 'auxiliary hypotheses'.
5 For a fuller discussion, see Shearmur (2006).
6 This raises the problem of verisimilitude. Verisimilitude was developed to try to capture what is involved in the idea – attractive to anyone who is a realist and a fallibilist – that one might look at the progress of scientific knowledge as involving, if we have been successful, moves from one false theory to another false theory, but where, in some sense, the later theories are closer to the truth than are earlier theories. Popper offered a formal theory of verisimilitude – e.g. in his 'Truth, Rationality, and the Growth of Scientific Knowledge' [1960]; see Popper 1963, chapter 10. In the event, his theory ran into major problems (see the first section of Miller 2006, chapter 11 for an account of this). Several alternative accounts have been offered, but it is not clear that there is, at present, any agreement

about these matters. (See Miller 2006 chapter 11, and Oddie 2014 for some discussion.)

7 Popper 1972, chapter 1, pp. 10–11.

8 The problem of 'grue' – i.e. of properties which change (e.g. from green to blue) at a particular point of time – was introduced by Goodman (1954).

9 Agassi came up, independently, with what Goodman was to identify as the same as the idea that he had discussed, as part of an argument against induction. Popper reported on this – referring to 'Agassi-predicates' – in Popper (1963, chapter 11, p. 284 and footnote 72a).

10 See Popper (1983, 'Introduction 1982', section VI, and also Section 4, Part V of the book itself).

11 This draws upon Stokes (1998). There is a vast literature on Popper's falsifiability criterion of demarcation and its associated methodology, much of which takes issue with nearly every aspect of its philosophical arguments, historical claims and methodological merits.

12 Perhaps surprisingly for someone of Popper's anti-Romantic inclinations, Novalis, who had broad knowledge of the philosophy of science, was also a poet whose work was imbued with the spiritual mysticism of German Romanticism.

13 On this point, see Shearmur (1996), Stokes (2006) and Hacohen (2000, pp. 381–82).

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MALACHI HAIM HACOHEN

2 The Young Popper, 1902–1937: History, Politics and Philosophy in Interwar Vienna*

Karl Popper is nowadays celebrated as a great philosopher around the world. In his homeland Austria, he is considered virtually a national philosopher. But for much of the postwar era, he was primarily known as a Western intellectual, an anticommunist prophet of Cold War liberalism. Born and raised in Vienna, he emigrated in 1937 to New Zealand, where he spent World War II. He lived most of the postwar years – the second half of his long life – in or near London, teaching for more than two decades at the London School of Economics. The radical shifts of cultural milieu from interwar Vienna to wartime New Zealand to postwar Britain were a long-standing source of difficulties in understanding his philosophy.

Popper first became famous for *The Open Society and Its Enemies* (1945). He wrote the book during World War II in his New Zealand exile, intending it as a defence of democracy against fascism. The defeat of Austrian socialism and the collapse of Central European democracies were the major experiences informing his analysis. He saw himself as shaping a reform philosophy, an alternative to Marxism that would provide a platform for a united front of socialists and liberals in the postwar years. Instead, published on the eve of the Cold War, *The Open Society* became, with Popper's endorsement, an anticommunist manifesto and a charter of trans-Atlantic liberalism. Popper's philosophy of science, exemplified best in *Logik der Forschung* (1934; *The Logic of Scientific Discovery* 1959), went through a similar transformation. To be sure, it is commonly debated in the context of logical positivism and the Vienna Circle, whose politics was secular and cosmopolitan, ranging from liberal to radical socialist. But the Circle's emigration, the postwar triumph of analytic philosophy in the Anglo-American academy and the rebellion of the New Left against positivism in the

* Dedicated to Troels Eggers Hansen, editor and scholar of Popper's early works, who has put us all in his debt, on his eightieth birthday. This essay uses material published in Malachi Haim Hacohen, *Karl Popper – The Formative Years, 1902–1945* (New York: Cambridge University Press, 2000).

1960s transformed positivism, too, into a primarily Anglo-American philosophy with a conservative reputation. The 'fugue of exile's disruptions' severed the thread connecting interwar Austria and the postwar Atlantic world, progressive Viennese politics and the Cold War (Bretenbach 1991).

The disjunction between interwar Central Europe, the formative milieu for Popper's philosophy and the postwar Atlantic world, where his philosophy first had widespread influence, inhibited historical work on his life and philosophy. Historical scholarship on Popper was late in coming. Few people outside Austria know interwar Vienna well. The major archival sources for his early intellectual development – his manuscripts and correspondence – had remained inaccessible until his archives opened in 1990. His early articles were buried in obscure interwar Viennese journals, and his first book, when finally published in 1979, appeared only in German, awaiting an English translation another three decades. In his Vienna years, Popper worked virtually alone, leaving a patchy public record. His intellectual autobiography – a rational reconstruction leading from 1919 to *Logik der Forschung* – obstructed as much as helped the historian (see discussion later in the chapter).

This chapter focuses on the new view of Popper emerging from recent work on his biography and early philosophy (Wettersten 1992; Hacohen 2000; Hansen 2006a, 2006b; Jarvie, Milford and Miller 2006). He now appears as the foremost philosopher to carry the legacy of progressive Vienna and unknown progressive German philosophers, like Leonard Nelson (1882–1927) and Otto Selz (1881–1943), into the postwar era. In his critiques of the Vienna Circle and Red Vienna, Popper confronted the philosophical and political problems of the Viennese late enlightenment (*Spätaufklärung*), and the interwar crises of scientific reason and Central European democracy. Refashioning progressive Viennese and Central European legacies, he offered revolutionary solutions to the major problems of the philosophy of science and drew novel visions of liberal science and politics, imagining utopian scientific and political communities that were engaged in the pursuit of truth and reform. He made critical debate the acid test of political and scientific rationality. Establishing a free public sphere as the *conditio sine qua non* of the Open Society, he innovated on a familiar liberal motif, shared by thinkers as different as Kant, De Staël, Guizot, Mill and Habermas.

Whereas most of the essays in this Companion address the mature philosopher, this essay focuses on the making of the philosopher, elucidating the intellectual and political contexts that gave rise to his philosophy. The essay draws Popper back from the postwar Atlantic world to fin-de-siècle and interwar Vienna and Central Europe.

1. FIN-DE-SIÈCLE VIENNA: THE PROGRESSIVE LEGACY

Karl Raimund Popper was born on July 28, 1902, in Vienna. His father, Simon Carl Siegmund (1856–1932), came from Bohemia, and his maternal grandparents from Silesia and Hungary. The family reflected the general migration patterns of Habsburg Jews in the aftermath of the abolition of imperial residence restrictions in 1848. Karl Popper's mother, Jenny Schiff (1864–1938), was born in Vienna. All family members adopted German culture and some made a rapid social climb. Having earned a law degree, Simon Popper became the legal partner of Vienna's last liberal mayor, Raimund Grübl (1847–98). Their relations must have been close: Karl Popper received his middle name, Raimund, from the mayor. In 1896, Simon Popper took over the firm, and the family moved into a huge apartment with adjoining offices, across from the St. Stephan Cathedral in the *Innere Stadt.* The apartment had a large library, where Simon Popper spent his limited leisure time writing political satire, studying history and translating Latin poetry.

Simon Popper 'married up'. Popper's maternal grandparents were representative of the Viennese high bourgeoisie. 'Imperial Councilor' Max Schiff (1829–1903), a grandson of Germany's last great Orthodox rabbi, Akiva Eger (1761–1837), came from Breslau. He apparently made a small fortune and married Karoline Schlesinger (1839–1908) from Budapest. Of their six children, the third, Jenny (1864–1938), was Popper's mother. They had an apartment in the ninth district and a villa in Pressbaum, on the outskirts of Vienna, where their grandchildren spent weekends and vacations. Both were supporters of the arts, founders of the *Gesellschaft der Musikfreunde* that built the *Musikvereinsaal.* Their children distinguished themselves in music, the academy and the professions. The Popper household embodied the ideals of *Besitz* (property), *Recht* (law) and *Kultur* (culture) that were held in the highest esteem by Viennese liberals (Verlassenschaftsakt of Max Schiff; Popper 1967–69, 1976, pp. 53, 82; Photos; Schorske 1980)

In 1900, Simon and Jenny Popper renounced their membership in the Jewish community and converted to Lutheranism. (Their two daughters, Dora [Emilie Dorothea, 1893–1932] and Annie [Anna Lydia, 1898–1975], perfunctorily became Protestants, too) (IKG; Verlassenschaftsakt of Simon Popper). Vienna was overwhelmingly Catholic, but Simon Popper shared the vehement anticlericalism of Viennese progressives. He preferred the *Aufklärung*'s religion. Neither acculturation nor religious conversion broke, however, the barriers of ethnicity. To be sure, the assimilated Jewish intelligentsia constructed bridges to progressive secular Austrians opposed to antisemitism. Together they formed the utopian visions of a secular commonwealth that became the hallmark of fin-de-siècle Viennese progressivism. In such a state, free of religious superstition and ethnic prejudice, the assimilated Jewish intelligentsia hoped to finally find their home: no one there would probe their ethnic origin, or challenge their claims to be German. But reality defied utopia. Secular progressive Germans were marginal to their ethnic group. The Poppers spent much of their life in the company of other Jews. They constituted an Austrian German-Jewish community, united by ethnic origins, social class, German education, the Enlightenment's ethos, liberal politics and, of course, the antisemites' malice.

Popper went to the *Freie Schule* for five years, from 1908 to 1913. The *Freie Schule* was a private elementary school providing an alternative educational environment, free from clerical influence (Glaser 1981, pp. 301–06; Boyer 1995, pp. 174–86). The school used advanced pedagogy rather than old-fashioned drilling, encouraging children's natural curiosity and permitting their freedom of movement in the classroom. It had smaller classes, sixteen to twenty students. Most students came from affluent progressive families. The *Freie Schule* was the only teaching environment in which Popper ever felt comfortable as a student. But the school, opened in 1906, became entangled immediately in a series of legal battles with the Church and the school board over religious instruction. Similar battles over confessional issues gave rise, during the decade preceding World War I, to a progressive-socialist anticlerical alliance, establishing the patterns of cultural politics that will carry on to interwar Vienna (Boyer 1995, chapter 4). In the interwar years, Popper would join the socialist school reform effort, designed to extend the *Freie Schule*'s educational methods throughout Viennese schools.

Throughout his youth, Popper was surrounded by progressive intellectuals. They rebelled against the social conservatism of mainstream liberalism and sought an opening to the workers. They opted for a bourgeois-proletarian alliance, under the auspices of an enlightened bureaucracy that would promote social legislation, economic modernization and scientific education. In 1891, they founded the Viennese Fabian Society and, in 1896, the *Sozialpolitische Partei.* The party advocated universal male suffrage and welfare reforms. It ran against the twin obstacles of Catholicism and antisemitism and remained very small, its constituency limited to Viennese districts populated with Jews (Holleis 1978). Several later attempts to reorganize a progressive party found equally limited success. Finding the political path ineffective, the progressives increasingly channelled their efforts into a large network of associations for educational reform, social welfare and economic planning (Belke 1978; Fuchs 1949; Boyer 1978). The Monists were one of these associations. Founded by Ernst Mach's disciples in

1911, the Monists were dedicated to the 'scientific' reform of philosophy, education and law (Stadler 1982). Arthur Arndt, a socialist family friend and the young Popper's personal guide, took him to the Monists' meetings (Popper 1976, pp. 11–12). Militantly secular, politically radical, trusting in social reform, popular education and technological progress – this was the young Popper's social and intellectual milieu (Stadler 1981).

Joseph Popper, known also under his pseudonym Lynkeus, was Vienna's most famous progressive intellectual. Apparently a distant relative of Karl Popper, Lynkeus devoted his life to a utopian plan for resolving the 'Social Question', which epitomized fin-de-siècle progressive utopianism (Popper-Lynkeus 1912). Lynkeus believed that the abject poverty around him resulted from faulty organization of production and distribution. The current state of technology should allow an adequate standard of living to the entire population. He proposed a general draft of young citizens – men and women alike – to a nutrition army for five to eleven years. Upon completing their service, the 'soldiers' would be guaranteed a basic standard of living for the rest of their life. The Association for Nutrition for All promoted Lynkeus's plan.

Lynkeus and other progressives expressed great optimism about social technology. They lived through the second industrial revolution in the chemical and electric industries, witnessing production increasing, with the industrialized areas of the Monarchy growing in wealth and population. Technological advances changed the face of Vienna: electric trams, a steam-powered metropolitan railway network, a municipal water supply system, gasworks and telephone (Horak 2000). With electrification, technology moved from the work sphere to private life. Science's triumphs were human, leading to tangible improvements in the quality of life. Technology was applied science, science was knowledge, their acquisition and spread meant emancipation. The day when society would undertake to reform itself along scientific lines, when the scientific world view would triumph in ethics, economics and politics, was not far off (Popper-Lynkeus 1912).

Progressive Viennese culture was, however, marginal, and the waves of ethnonationalism sweeping over the Empire were its greatest enemy. The progressives failed to understand ethnonationalism, or appreciate its danger, and they responded ambivalently to it. Their ranks included pacifists and federalists, but also German nationalists. They fought antisemitism that offended their humanity and excluded their Jewish members from the nation, but they could see no harm in the expansion of the German cultural sphere in Central Europe. They regarded Slavic nationalism as reactionary. Ethnopolitics was, they thought, a passing frenzy. They denied that Jews were a nationality. Jews should, and will, assimilate in the majority nationality wherever they live (Bauer 1907, pp. 366–81). Striving for recognition as German Austrians, progressive Jews sought to strip religion and ethnicity of significance – their own first and foremost. Their non-Jewish colleagues were happy to oblige. German nationality was a matter of culture, not race. The progressive intelligentsia represented a class that, to overcome the burden of its own ethnicity, needed to dissolve all ethnicity and recover universal humanity.

The Popper family circle represented the cosmopolitan-pacifist pole on the progressive spectrum. Simon Popper was master of the leading freemason's lodge *Humanitas,* and relatives and friends were identified with the Austrian Peace Movement (Popper 1976, pp. 11, 13–14; 1982a, p. 23). Freemasons and pacifists gave the clearest expression of Austrian cosmopolitanism. To Viennese freemasons, humankind was advancing towards cosmopolitanism. Nationality and religion did not matter, only universal humanity. As a pluralist empire, Austria represented a higher developmental stage than national states. Cosmopolitans were the Austrian patriots par excellence. They contributed to political harmony and internal peace (Hubert and Zörrer 1983; Kuéss and Scheichelbauer 1959, pp. 137–73; Laurence 1992). Unlike the socialists whose party structure forced confrontation with the nationality problem, the freemasons drew up no plan for imperial reform. They were content to 'think of themselves as guardians of liberal values and as the intellectual elite of a huge state whose composition gave it the appearance of the international order of humankind in miniature' (Silverman 1984, p. 26).

The progressives' denial of ethnonationalism flew in the face of historical reality. Progressive culture conflicted with the religious beliefs, nationalist values and ethnic identity of most Germans. Class and education limited it to the intelligentsia. Even in the academy, where Jews were heavily represented, progressivism represented a minority. Virulent German nationalism dominated much of the student body, and conservative Catholic and nationalist traditions prevailed in the humanities and social sciences. Through their organizational network, the liberal professions and Vienna's salons and coffee houses, the progressives contributed to the legendary cultural intensity of fin-de-siècle Vienna. But they remained a narrow segment of the German intelligentsia allied with a subgroup of an ethnic minority who posed for a short time as a social and cultural elite: Vienna's 'non-Jewish Jews'.

Karl Popper would spend much of his life refashioning progressive philosophy and politics. Deeply committed to historical progress, he nonetheless discarded progressive utopianism and constructed a new liberal framework for reform and social planning. Carrying progressive opposition to ethnonationalism one step further, he rejected *Deutschtum* (Germanness), dissociated the enlightenment from Germany and advocated radical cosmopolitanism. His relentless hostility towards any nationalism (Zionism was his favourite example), his rejection of any and all religion (Judaism more than Christianity), his belief in an international legal order (rare among a generation witnessing the League of Nations' failure), his passionate defence of the Enlightenment and the Open Society – all were a metamorphosis of progressive Vienna. He remained an assimilated progressive Jew to the end of his life. Through migration and exile, his own as well as others, progressive philosophy, which was a product of marginal Viennese milieus, made cosmopolitan dreams part of mainstream Western culture.

2. WORLD WAR I AND THE AUSTRIAN REVOLUTION

Popper bemoaned the loss of the popular scientific culture that he had experienced in progressive Vienna (Popper 1983, p. 260). He would spend his life trying to restore 'the tradition of rationalism', shattered by war and Central European ethnonationalism. He would envision science and politics in the image of the lost culture: free cosmopolitan communities, engaged in critical debates. In exile, he would recover progressive Vienna as the Open Society and the Republic of Science. This would be, however, a desperate move, signalling the community's historical collapse. As a youth, Popper still attempted to shape a new community in political action. The Austrian Revolution represented the period of his most intense political engagement. Only when politics failed did he withdraw to philosophy and science.

Popper came of age during World War I and the ensuing revolution. At a time when most (middle-class) teens stage their rebellion against the parental and social order, that order collapsed, right before his eyes. Few things are more disenchanting, or disorienting, than a war begun in a patriotic mood, with traditional authority reinforced, and ended in exposing patriotism and authority as corrupt and impotent. Few things encourage more youthful experimentation and dreaming. Nothing liberates more from tradition. When Popper began to develop a distinct intellectual and political identity, he had an unusual freedom to think things anew. The cost in human life and misery, present and future, however, was abysmal. Popper's philosophy reflected both – pain in face of destruction and freedom to invent, with a view to a better world.

The War went badly for Austria-Hungary from the start. The Russian army overran Galicia twice (in 1914 and, again, in 1916), decimating the Austrian officer corps and sending waves of poor Jewish refugees to Vienna (Deák 1985). Popper's relatives of military age – cousins, an uncle, eventually also his sister Dora – served in the army or in the medical corps. They were mercifully spared, but the death of friends had brought the horrors of war home, even before the great shortages of the war's final years began.

Popper grew critical of the regime as the war progressed, but he dared not express his views in public. Students who expressed anti-war sentiments in school were severely reprimanded, even expelled. Popper had enough trouble in gymnasium as it was. His father, persuaded of the obsoleteness of the humanistic curriculum, sent him in 1913 to the *Realgymnasium* in the third district (Bartley 1989, p. 20; Hansen 2006, pp. 547–48). The *Realgymnasium* put less emphasis on classical languages and more on mathematics and natural science. (Popper would need to teach himself classical Greek when he wrote *The Open Society*.) In the next couple of years, Popper changed schools thrice, only to find himself in 1917 back at the *Realgymnasium*. He found classes in all three schools boring and pursued extracurricular interests. He was not an unproblematic student. Bouts of depression, accompanied by illness, real or imaginary, would accompany him for the rest of his life. They showed up already in his teens, his mental and physical health reflecting the gloomy public atmosphere.

When the defeat of Germany and Austria became clear, revolutions broke out in Prague, Zagreb, Cracow and Budapest, and the Czechs and Slovaks, the Southern Slavs, the Poles and the Hungarians declared national independence. In Vienna, the Socialists (together with the Christian Socials and German nationalists) set up a provisional government on October 21 and, on November 12, declared the Republic of German Austria. Six hundred years of Habsburg rule came to an end.

The revolution made open political debate possible, and socialist students argued among themselves whether to continue their education or leave school and staff the barricades. Popper left school. He was going around Vienna dressed in an old military uniform, eager to partake in the revolution. He joined the *Freie Vereinigung sozialistischer Mittelschüler* (Free Association of Socialist High-School Students), founded on December 14, 1918. Representing the *Realgymnasium*, he was section leader in the third district. The *Mittelschüler* included both communists and socialists and cooperated with the socialist university students. Members of the organizations comprised the Who's Who of interwar Austrian and German socialism. The older students turned communist within a few weeks and provided the nucleus for the Austrian Communist Party. Popper found himself in the hotbed of Central European communism (Scheu 1985, pp. 68–77).

Vienna's streets were unruly throughout the spring of 1919. Despite the armistice, the Allies did not lift the food blockade, and the city reached near-famine conditions. Returning soldiers, disabled veterans and the unemployed crowded the streets and the councils, demonstrated in front of parliament and voiced radical demands. In March, a Soviet Republic was established in Hungary, and in April in Bavaria. The prospect for a radical Central European communist-socialist bloc seemed imminent. Popper now sought engagement with the communists. In April 1919, or thereabouts, with a few friends, he 'converted' to communism (Scheu 1985, pp. 71–76; Popper 1967–69; 1976, pp. 32–33; 1984a). It is not clear whether he actually became a party member, but he was working as an office boy at communist headquarters in mid-June, when the party staged its last coup attempt. The communists planned to use released soldiers, the most volatile element in interwar Central Europe, to seize power. The authorities, learning of the communist plan, pre-empted the coup by arresting the communist leaders on its eve. The next day, June 15, 1919, thousands of unarmed, unemployed workers marched to begin the takeover. Popper was with them. They soon learned of their leaders' arrest, and hundreds of them attacked the police station in the *Hörlgasse* to release them. The police opened fire, killing twelve workers and injuring eighty. The coup failed. Support for the communists dissolved over the summer. With the ouster of the Hungarian Soviet Republic in July, the revolutionary wave in Central Europe receded (Hautmann 1971, pp. 183–91; Borkenau 1962, pp. 128–29).

The attack on the jail brought about Popper's break with the party. This was, he said, 'one of the most important incidents in my life' (Popper 1976, p. 33). Like most of the people involved in the demonstration, he did not realize that it was part of a coup. Having been spared the Great War's bloodiest scenes, and not yet seventeen, he was horrified by the loss of life and, as a communist, felt guilty for inciting the unarmed workers. Over the spring, he learned to distrust the communists: 'They would reverse their theses on the situation in Austria overnight, when the Russians so demanded', he would recall later, and they 'took it for granted that they were the future leaders of the working class' (Popper 1976, pp. 34–35; 1984a). How could intellectuals so easily claim to represent the proletariat? Most frightening was their callousness in demanding intensification of the class struggle. The logic that justified violence for the sake of an elusive final aim, socialism, was deadly. Once communist premises were accepted, one was drawn into a process whereby moral and intellectual autonomy were sacrificed to fulfil an imagined historical destiny (Popper 1963, chapter 1; 1976, p. 34; 1984a). He began listening again to friends and relatives, who harped on about the Bolshevik terror. He now doubted that the classless society could justify terror. The critique of communism would take long to develop, but, shocked to see people falling dead or wounded right next to him, Popper recoiled when asked to accept communism on faith. He was independent and rebellious, not one to accept party discipline and dogma.

His rejection of Marxism, however, was a prolonged process, stretching over years, not months, as his *Autobiography* might suggest. He was still interested in radical socialist transformation and followed with interest the debates on socialization. *Sozialisierung* was a new concept in the Marxist lexicon. Prior to World War I, Marxists gave little thought to the transition from capitalism to socialism. During the revolutionary years, the Soviets presented one model: the dictatorship of the proletariat. The German and Austrian socialists opted instead for a transition to socialism within the framework of parliamentary democracy. Socializing devastated economies proved problematic, and socialization efforts came to little, but the literature on the subject exploded in 1918–20. Socialist, progressive and liberal advocates of economic planning competed in offering socialization schemes.

The Calculation Debate on the viability of a socialist economy with informative monetary prices was an offshoot of the socialization debate, and in its course, issues relating to social science methodology became central. Methodology and politics became intertwined. Market liberals, such as Ludwig von Mises (1881–1973), challenged socialists and progressives (e.g. Otto Neurath, 1882–1945). The debates continued throughout the interwar period, and provided a first major forum for discussing problems of social planning and welfare economics that would preoccupy industrial societies for the rest of the century. The debates and their terminology – social engineering, technological social science and scientific prediction – would reverberate in *The Open Society* and *The Poverty of Historicism* (Popper 1944–45). Popper would shape his philosophy of science and political philosophy in confrontation with Neurath and the Marxists.

The socialist reform wave in Austria reached its high point in the summer of 1919, and then receded. Parliament passed extensive social welfare legislation: unemployment compensation, social insurance, eight-hour working day, regulation of labour conditions, collective bargaining, workers councils in factories. Once a semblance of order was restored, however, conservative social forces revived, and populist anti-socialist currents spread in rural areas. In June 1920, the socialists left the government. The elections of October 1920 made the Christian Socials the largest party in Austria. If the federal course of reform seemed blocked after 1920, however, a new one opened. In the May 1919 municipal elections, the socialists won an absolute majority in

Vienna (and other industrial centres). The constitution gave Vienna a provincial status, so the socialists were now free to focus their reform effort on the capital, inhabited by well over a third of the population of a reduced Austria. They would build Vienna in the next decade into a model socialist community. Red Vienna would become a Mecca for visitors coming to watch humanity's socialist future in the making. Popper would participate in the socialist project. Red Vienna would be his Vienna.

He continued to spend his time with communist youth. In the summer of 1919 and 1920 he participated in 'vacation colonies' populated by the *Mittelschüler*. In the winter of 1919–20 he left home to live with an eccentric group of political outcasts and wayward students in the Grinzing barracks, a huge military hospital complex built during the war. It now became a residence to impoverished students, intellectuals and German and Hungarian exiles (Popper 1967–69; 1976, p. 39). Each barrack acquired a distinct identity. In barrack 43 lived communist refugees and students, including Popper. Aware of the gap separating intellectuals and workers, Popper also decided to experience worker life firsthand, tried manual jobs, such as roadwork – and had to quit, exhausted – and a variety of social work, often connected with educational reform projects. Few of these efforts were successful but, for the next few years, he remained committed to educational reform.

Social work and educational reform forced Popper to confront Sigmund Freud (1856–1939) and Alfred Adler (1870–1937), psychoanalysis and individual psychology. Socialists deployed psychology to shape their reforms, and arguments between Adler's and Freud's disciples were frequent. As a volunteer in Adler's clinic, Popper formed a fairly close relationship with him and his family. Adler seemed responsive to Popper's queries about the predominance of the inferiority complex, but at some point, Popper may have exceeded boundaries, challenging Adler's method of adducing evidence to support his theories, and the relationship came to an abrupt end (Popper 1963, p. 35; 1967–69; Agassi 1998). About the same time, Popper read, or may have just heard discussed, *The Interpretation of Dreams* and Freud's other major works. He remained sceptical. It was not that psychoanalysis shocked him, it seems, but rather that he thought it incredible. He did not doubt that the unconscious existed, or that dreams had latent content that was revelatory of the unconscious. He thought Freud's interpretation of dreams fundamentally correct. Nor did he question that Oedipal conflicts and Adlerian inferiority feelings existed and were significant. But Freud's and Adler's claims of universality for their interpretations, and their pretension to explain everything, drove him frenetic. Neither seemed amenable to correction by 'experience'. Both Freud and Adler were able to explain a person pushing a child into the water, intending to drown it, and another jumping into the water to save it, by repression and sublimation of the same drives, or the overcoming of inferiority feelings. Under what conditions were Freud and Adler willing to concede that their theories failed?

A few years later Popper discussed the subject with Edgar Zilsel (1891–1944), a socialist educator, student of mathematics, philosophy and sociology, later associated with the Vienna Circle and known for his Marxist account of the social origins of 'genius'. Zilsel was sceptical of psychoanalysis and individual psychology and proposed that they, and philosophy, be excluded from socialist curricula as unscientific. He suggested to Popper that there were multiple psychological types and that Freud's and Adler's classifications were not helpful. Popper's report of their conversation is all too brief, but he thought Zilsel's critique excellent, and it decided the issue for him. Never again would he be interested in Freud or Adler. In his incomplete 1927 thesis he dismissed them as unscientific. Other than in his autobiographical accounts, he returned to Freud only once, in the mid-1950s, in a brief critique, demarcating science from psychoanalysis (Popper 1963, pp. 33–39; 1967–69; 1983, pp. 163–74; Dahms and Stadler 1991, p. 528; Zilsel 1921).

As a youth, in 1919–20, he faced a crisis. He questioned the socialist reformers' prophets, Marx, Adler and Freud, but believed in reform itself. What was he to do? No wonder he was attracted for a while to existentialism, a leap of faith to reform politics, but not for long. Salvation came shortly from natural science, theoretical physics in particular. In the gymnasium, Newtonian cosmology and mechanics reigned supreme, but in May 1919, A.S. Eddington's eclipse observations appeared to corroborate Albert Einstein's theory of gravitation and precipitated a revolution in physics. The excitement among Popper's friends was immense. Early in 1921 Einstein came to Vienna to lecture. Popper went to hear him, but did not understand a thing. He struggled with relativity theory. With the help of Max Elstein, a gentle, dreamy Sephardic Jew from Jerusalem, he finally grasped it. For the rest of his life, no scientific question would deter him. He would inspire his students with confidence that all human endeavours were within their reach if only they put their mind and effort to it. He would endeavour to write his books with the same clarity and simplicity that he found in Einstein and his popularizers (Popper, 1967–69).

Einstein opened *Relativity* with a basic exposition of geometrical physics, questioning the classical Euclidean geometry that Popper had been taught in gymnasium. 'By reason of your past experience', Einstein said, 'you would certainly regard everyone with disdain who should pronounce [a] proposition of this science to be untrue. But perhaps this feeling of proud certainty would leave you immediately if one were to ask you: "What, then, do you mean by the assertion that these propositions are true?"' (Einstein 1920, p. 1). Theoretical physics worked with competing geometries, choice among which was difficult. The problem captured Popper's interest. A decade later it became the focus of his 1929 thesis. His solution, entrusting testing with arbitrating the choice of geometry, was the centrepiece of his epistemological revolution. A vision of adventurous and revolutionary science emerged. As Otto Neurath perceptively observed in 1935, Popper turned Eddington's experiment into a scientific model (Neurath 1935). Science progressed by leaps, an *experimentum crucis* deciding between two competing theories.

Einstein's ridicule of 'proud certainty' appealed to Popper, just as theoretical physics' adventure and risk did. Marxism, psychoanalysis and individual psychology seemed to discover confirmations wherever they looked and explained away contradictory evidence. Such hubris seemed out of place in a world where centuries-old empires had just collapsed and Newtonian cosmology had been overthrown. Certainty should be exceedingly difficult to come by, Popper felt. He looked for a philosophy that would promise progress but remain modest and vulnerable, open to correction and change. Einstein and theoretical physics represented it (Popper 1963, pp. 33–39; 1967–69; 1972b; 1976, pp. 33–44).[1]

In 1919–20 there was still much that Popper found confusing. Contrary to his autobiographical accounts, he had not yet settled on testability, or falsifiability, as demarcating between science and pseudoscience, or science and metaphysics. Indeed, he had not even formulated the question yet. There is no reason to doubt, however, that, in the aftermath of the political and scientific revolutions, he began to regard openness to criticism and refutation as marking the 'scientific attitude'. What was it, he wondered, that made Newtonian mechanics 'science', even though it proved 'wrong', and Marxism, psychoanalysis and individual psychology 'non-scientific'? By 1920, or shortly thereafter, he reached some understanding of his discomfort with Marxism and these forms of psychology. He did not quite put it this way at the time, but he sensed that theories immunized to refutation were pseudoscientific.

In his autobiography, Popper described the immediate post–World War I period as one of the most crucial in his life. He was right. The revolutionary upheaval settled nothing, but it set him on his political and philosophical trajectories. The political and scientific revolutions taking place around him promised new cosmology and polity. In natural science alone, however, not in politics, he found a stance towards life and knowledge that he could adopt. His stance represented a combination of commitment and distance, belief and suspense. World War

I and the Austrian Revolution re-channelled his rebellion from the old order to the political avant-garde, and he developed a life-long suspicion of intellectuals' claims to superior knowledge and leadership. He would remain politically engaged for another five years, but retain an outsider's perspective. In this sense (and, perhaps, only in this), his critique of Marxism in *The Open Society* and *The Poverty of Historicism* began with his political experience in 1919. Science and politics joined in his life from the start. The revolutionary experience made it clear that, when in doubt, he would privilege the former over the latter. But he would always retain the hope that, somehow, he might harness science to politics without putting either at risk.

3. THE EARLY 1920S: SCHOOL REFORM AND SOCIALIST POLITICS

Although the Austrian revolution was over by early 1920, there was no return to prewar days. A 'cold civil war' between the Christian Socials and the socialists began, occasionally flaring up into hot outbursts. Postwar inflation wiped out the Popper family's savings. Simon Popper died in 1932 virtually penniless. These economic dislocations left an indelible impression on Popper. Even when he later abandoned socialism for liberalism, he did not trust the 'free market'.

At the end of the revolution, Popper faced the question of a career. He refused to consider the professions. For two years, between 1920 and 1922, he thought seriously of becoming a musician, and until late in 1921 he was involved with Schönberg's Society for Private Musical Performances. But, 'as with so many other things ... I felt in the end that I was not really good enough' (Popper 1976, p. 54). He had enrolled as a non-matriculated student at the University of Vienna since 1918. He sampled courses in different fields but found most courses disappointing. Mathematics alone attracted him. He spent long hours in the library of the Vienna Mathematical Institute. The Institute included world-renowned mathematicians, and Popper took courses with all of them. But it is 'a huge and difficult subject, and had I ever thought of becoming a professional mathematician I might soon have been discouraged' (Popper 1976, p. 40). He needed some guidance, and could get none at the university. He stopped going to lectures and began studying with a group of socialist friends he had known since the revolution. They composed their own reading list, discussed books, debated politics, climbed the Alps and went to concerts.

Nothing he did during the early 1920s seemed to him successful (Popper 1995, p. 11). In 1921 he failed his first attempt at the *Matura,* the final gymnasium examinations that served as an entry ticket to the university, passing it the year after. At the end of 1922 he was still drifting. His friend at the barracks, Paul Oster, was a carpenter for an orphanage. Recalling his own commitment to manual labour, Popper decided to become a cabinet-maker. For the next two years he worked as an apprentice to a cabinet-maker. He discovered the obvious: he was interested in philosophy more than in cabinet-making. He concluded his apprenticeship in October 1924, receiving a diploma. The only furniture he would ever make would be for his house in New Zealand.

Popper decided to become a teacher. He did social work with the *Kinderfreunde,* the socialist educational association for working-class youth. From day-care centres in the early 1920s, to social work with proletarian youth in 1924–25, to the Pedagogical Institute in 1925–27, his social network consisted of school reformers. Between 1925 and 1931 he contributed three essays on educational philosophy to the journals of the school reform movement. His fierce independence and bad gymnasium experience made him receptive to their ideas on anti-authoritarian education. He, too, dreamt of founding a school where a community of students and teachers would explore the intellectual world.

He was soon disillusioned. Upon completing his apprenticeship, he became, for a year, a *Horterzieher,* an educator (and social worker) in a socialist centre running after-school programs for working-class youth. He found the children extremely difficult. He was a young intellectual confronting street culture and youths who did not trust him. The Viennese civil service seemed unresponsive to front-line social workers. Progressive educational theory disappointed him, too. It had little relevance to the social reality he was facing. To function as a teacher, he had to employ tactics that had nothing to do with 'educational community' or 'self-directed activity', such as challenging a student leader to a boxing match. And the year ended tragically. One of the children under his supervision fell from a climbing frame and fractured his skull. Popper remembered previously asking for the climbing frame to be removed, but the city sued him for negligence. His father defended him, a friend testified on his behalf, and the judge acquitted him, placing responsibility for the accident on the city (Popper 1976, pp. 10, 197, n.2). But all this was too much for him, and he ended his active engagement with school reform. In the coming years he would remain interested in educational theory but focus more on the obstacles to reform than on its prospects.

Popper's early essays marked his growing criticism of socialist educational discourse. His first published article, 'On the Teacher's Position toward School and Student', appeared in 1925 in *Schulreform* (Popper 1925). The controversy over individual versus collective orientation in education, Popper suggested, was founded on a misunderstanding.

Education always dealt with individuals; the question was only whether they should be viewed in their individuality (*Individualität*) or as social types. As an institution, school dealt with students as social types, but Popper sought to free the relationship between teacher and student from institutional pressures and maintain it as an individual relationship. School programs were best constructed, he said, when they imparted 'social experience' indirectly, by facilitating free interaction of student and teacher.

His 1927 (incomplete) thesis for the *Paedagogisches Institut* went further in criticizing the reform project. The reformers promoted self-directed activity and spontaneous learning, but they underestimated the resistance that children's natural conservatism offered to self-directed activity. Psychologists and pedagogues had failed to distinguish between dogmatic and critical thinking. Children were captives of dogmatic thinking. Reform pedagogy treated children's mental processes as if they involved a critical intellect, capable of forming judgements (Bühler 1918; Burger 1923). They were wrong. In the evolution of both the individual and the species, dogmatic thinking preceded critical thinking. Children identified the 'is' with 'ought', searched for order, rejected the unfamiliar and tended to accept authority. Critical thinking and judgement developed at a fairly late stage of human evolution and life (and Popper, at this point, could not explain how). They entailed a challenge to the perceived order of things. Children, and many adults, never broke with dogmatism to challenge order. School reform faced major obstacles (Popper 1927a).

Could reform overcome dogmatic thinking? Popper was not optimistic. Children's passivity was not, as commonly assumed, mere attachment to 'habits' that education may change. Rather, passivity was grounded in children's search for an orderly universe. The search for lawfulness dominated their mental life. They resisted change and refused to recognize difference because they wished to safeguard lawfulness. Reform pedagogy must recognize that this led to distortion and prejudice. Cognitive psychology must refocus on analysing experiences of lawfulness, and reform pedagogy must draw implications for instruction. Otherwise, the hopes put on spontaneous activity would be crushed against the walls of dogmatic thinking (Popper 1927a).

In his 1931 piece, 'Memorization from the Perspective of Self-Activity' – an essay reflecting already Selz's influence and Popper's breakthrough in the philosophy of science – Popper slaughtered another holy cow of the reformers. Most school reformers believed that traditional schools emphasized tedious memorization. 'Self-directed activity' was their alternative to memorization. Popper suggested that reformers ought to rethink their position. Memorization was neither a product of continuous imprinting on a passive mind nor a matter of obsolete school training. This view reflected the mistaken belief in association. It assumed that meaningless sensations and impressions, imprinted on the mind, somehow increased gradually in complexity to form thoughts. But psychological association was nothing but a legend. Memorization was a complex intellectual operation. It required a discriminating mind that simplified a mass of material and selected among an infinite number of facts. The mind reorganized them into meaningful thought-structures, making them manageable for the memory. Memorization was a matter of 'learning how to learn', a worthy goal for the reformed school. The reformed school, which sought the students' active participation in learning, was superior to traditional schools in developing memorization capabilities (Popper 1931).

Popper was less sympathetic to school reform in later years. His post-war correspondence revealed deep ambivalence about the project. 'In the hands of highly gifted teachers', he said, 'school reform was a *great* success' (Popper 1974). (Actually, its achievements were limited: Christian Social opposition, and obstructionism by anti-socialist teachers, stalled reform in most Viennese schools.) But Popper dismissed the reform leaders as 'party-politicians' and reform pedagogy as 'cant'. The 1925 tragedy made him think social reform anew, and his socialism did not emerge from the ordeal unscathed. To Rudolf Carnap he complained in 1947 that the atmosphere among the *Kinderfreunde* was 'totalitarian' (Popper 1947; 2008, pp. 104–05).

Still, he remained a socialist throughout the interwar years. He left Vienna in February 1937 considering himself a member of the then banned Socialist Party. But he was a socialist dissenter, and, in the mid-1920s, he was developing a critique of Marxism and of the Austrian socialists. The socialists controlled Vienna from 1920 to the Civil War of 1934. Their mammoth building projects alleviated housing problems, and a vast network of social and cultural institutions improved conditions for the working class. Popper recognized socialist achievements but thought that their Marxist-inspired policies were leading the workers towards a catastrophe (Popper 1976, pp. 32–41, 107; 1981). Already in 1924 he suggested that Eduard Bernstein's revisionism and Carl Menger's marginalism provided an alternative to Austro-Marxism (Milford [Hilferding] 1999). In his 1927 essay 'Zur Philosophie des Heimatgedankens' (on the philosophy of the homeland idea), Kant, not Marx, showed the way to socialism and internationalism (Popper 1927b). 'From good Germans to good cosmopolitans,' he quoted Eduard Burger, a noted socialist school reformer (p. 22). For socialists, the dictum justified German nationalism. For Popper, good Austrians virtually ceased being German, becoming cosmopolitans.

Popper and his wife were among the crowd in front of the *Justizpalast* on Bloody Friday, July 15, 1927. They watched incredulously as the police opened fire on 'peaceful and unarmed social democratic workers and bystanders. We were lucky to escape' (Popper 1967–69). He thought the police's attack was unprovoked, but, all the same, he blamed the socialist leaders for the 'massacre'. Their 'suicidal' policies gave the government opportunity to use violence, and their constant effort to interpret and predict the course of history, rather than change it, encouraged the workers' passivity and discouraged serious anti-fascist resistance. Popper understood well the event's historical significance. His call in *The Open Society* for a steadfast defence of democracy encapsulated his policy proposals to the Austrian socialists (Popper 1942–43; 1945, 2: chapter 18, nn. 18–22; chapter 19, n. 39; Hacohen 2000, pp. 326–35). He thought that the government's commitment to democracy was shaky and the fascist threat real. The socialists needed to contain their rhetoric so as not to provoke a coup, but failing that, they had to defend democracy by force. The two goals may have been in conflict, but, already as a young man, Popper saw the situation clearly, and his socialism was *sui generis*.

4. COGNITIVE PSYCHOLOGY, 1925–1928

In July 1925 the Vienna City Council merged Vienna's pedagogic and psychological institutes and established a teacher-training program, combining academic and practical training (Fadrus 1926). Popper and his friends were all admitted. In the fall of 1925 he started the two-year program. At the Pedagogic Institute, he expanded his social and intellectual circle and met his future wife, Josefine Anna Henninger (1906–85), known to close friends and associates as 'Hennie'. She and Karl were members of a student group that studied, hiked and mountaineered together. Karl became their intellectual leader. He was intellectually way ahead of most of them. Only 17 per cent of his class had gymnasium education. All the same, he found at the institute an academic milieu conducive to a systematic development of his theoretical interests. He had good intellectual rapport with the institute's leading intellectual light, Karl Bühler (1879–1964), a prominent European psychologist. He attended all his lectures in psychology and logic and found his way into his colloquium already in the first semester. Their styles of thinking differed from the start, but Bühler's spontaneous generosity drew Popper to him. In dialogue with Bühler's work, he systematically explored the psychology of learning and wrote his 1928 PhD dissertation 'On the Methodological Problem of Cognitive Psychology'. 'Bühler', said Popper years later, 'was an original thinker, a man of wide reading and culture, a splendid teacher, and a man who acted bravely under Fascism. Few men are better, and few are more genuine' (Popper n.d.)

Working with Bühler on cognitive psychology, Popper found himself in the only field in progressive Viennese culture where Kant was taken seriously. Bühler was, like Otto Selz, a member of the Würzburg School of psychology, which rejected association psychology and insisted on autonomous thought and a creative mind. Bühler provided rich meditations on the relationship between logic, psychology and biology, which Popper could use to try and answer theoretical queries emerging from his study of two other thinkers, Leonard Nelson (discussed in the next section) and Heinrich Gomperz (1873–1942).

Gomperz was a professor of philosophy at the university, a scion of a patrician Viennese Jewish family. He had an enormous breadth of interests and published extensively on epistemology and classical philosophy. A circle of intellectuals met on Saturdays at his Viennese villa, but Popper did not participate in these meetings (Stadler 1994, pp. 5–6). Rather, beginning in 1926, he met Gomperz privately to discuss psychology. He usually gave Gomperz a manuscript to read, receiving it back with comments. They got along well, and, all in all, may have met eight to ten times (Popper 1967–69; 1976, pp. 20–21, 74–75, 81–85). Popper also took Gomperz's course on Plato in the spring of 1926 and read all his works (Transcripts). They provided Popper with an incomparable guide to epistemology, psychology and the methodology of science.

Gomperz saw equally the faults of all philosophies and never succeeded in forming his own. He made no effort to mould Popper intellectually but clarified for him complex issues, drew his attention to different approaches and provided useful references. Popper's 1928 dissertation on the methodology of cognitive psychology testified to his influence as much as to Bühler's (Popper 1928). Following Gomperz, Popper searched for a secondary science *(sekundäre Wissenschaft),* 'Semasiology', which would delineate the boundaries between disciplines and 'establish a relationship free of contradictions *[Widersprüchslos]* of all those ideas, [emerging] from different scientific disciplines as well as practical life' (Gomperz 1905–08, vol. 1, p. 17). He also shared Gomperz's interest in the biological bases of learning. This would become a life-long concern for him.

Popper graduated from the Pedagogical Institute in July 1927 and worked for a year on a doctoral dissertation, submitted to the University of Vienna in the summer of 1928. He dropped large portions of the manuscript, based on years of research in psychology, and submitted instead a methodological introduction, written at the last minute (Popper 1976, p. 78). The thesis' ninety-odd pages reflected its erratic creation. Popper's argument that Bühler's methodology, as expounded in *Die Krise der Psychologie* (The Crisis in Psychology), was a prerequisite for scientific practice in cognitive psychology did little to advance Popper's larger project: understanding the relationship between logic, psychology and biology (Bühler 1927, p. 29). He hoped that Bühler would provide the answer to Gomperz's quest for a secondary science that would establish methodological ground rules for all sciences, but he faced overwhelming difficulties. Gomperz's *Weltanschaaungslehre* crumbled under the effort to relate the explanatory frameworks of the different sciences. Popper's dissertation became a turning point. Shortly after completing it, Popper confined his interest to the logic of science, then to natural science methodology. There, the problem of the growth of knowledge became manageable.

Popper devoted over half of the dissertation to a critique of Moritz Schlick's physicalism, which, he argued, was not a viable alternative to Gomperz's semasiology. Schlick (1882–1936), professor of philosophy at the University and head of the Vienna Circle, demanded that psychology model its methods on the natural sciences, and, more specifically, on physics: 'The reduction of psychology to brain physiology ... is ... the demand made by our parallelism' (Schlick 1925, p. 288). Popper objected that the positivist ideal of methodological unity of the sciences was logically problematic and the transference of methods from physics to psychology was impractical. An attempt to limit psychology to physiological explanations would spell out the end of the discipline. Psychology must remain autonomous (Popper 1928).

In the rest of the dissertation, Popper showed that, like Bühler's linguistic theory, cognitive psychology operated on three levels: experience, behaviour and intellectual structure. Each level was indispensable and required different methods. He devoted the greatest attention to intellectual structures. How were the various elements of language and thought, chaotic perceptions and words, organized into meaningful statements and conceptual structures? Sympathetic though he was to Selz's account of a task-oriented mind, he declined to assign the mind an active role in organizing perceptions into meaningful structures. He maintained, against Selz, the partial validity of association. The mind's operation, he insisted, did not abide by logical procedures. These refusals left him in a bind. He described 'objective intellectual structures' as logical in character, in a manner not dissimilar to his later views of science and World 3 (the world of intersubjective knowledge). At the same time, he maintained that the mind, and psychological processes, did not conform to logic. Yet, he wanted psychology to explain the formation of intellectual structures. He opened a gap between logic and psychology that seemed impossible to bridge (Popper 1928).

Popper was anxious, above all, about what he called 'logicism' – the imposition of logical structures onto psychological experiences. There was no logic of transference in either direction. He regarded logicism as a greater danger than psychologism, but rejected psychologism all the same. He expressed reservations about Selz's use of psychological experiences to explain scientific breakthroughs. Yet, logical-psychological and biological-psychological parallels fascinated him. He was searching for a method to explore them that would, at one and the same time, translate from one discipline to another and guard the integrity of each. Semasiology was his answer, but it was a counsel of despair (Popper 1928). It reflected his inability to deploy psychology to investigate intellectual structures. Logic and psychology seemed self-contained realms, closed to each other. In his two theses of 1927 and 1928 Popper tried to establish a dialogue between them to explain the production of knowledge. He failed. Now he hoped that semasiology would somehow open them to dialogue. This was hope against hope. His real problem was not fuzzy disciplinary boundaries, but the incompatible explanatory frameworks of psychology and epistemology. One framework had to give in before a solution emerged. In 1928 it was clear that Popper was not willing to give up on scientific autonomy. The question was how long he would persist in attempting to reach a solution in psychology. Semasiology merely bought him time. It did not provide a solution.

Popper must have realized shortly after the dissertation that semasiology was not a cure-all. He chose the only practical way out. He shifted his inquiry to the logic of natural science, and later to epistemology and methodology. Once he radically differentiated between psychology and epistemology, declaring the former irrelevant to the latter, a stalemate in psychology no longer inhibited progress in the logic of science. In *Logik der Forschung,* he created a model of natural science, then extended it to social science in *The Poverty of Historicism,* and finally drew implications for political philosophy in *The Open Society.* Reversing his 1928 position, he pronounced the methodological unity of the sciences, and rejected any *Geisteswissenschaft* (Popper 1944–45). In postwar years he again confronted head on the psychology of knowledge. Conjecture and refutation in science had parallels in expectation, disappointment and correction in psychology (Popper 1963, pp. 42–52; 1972a, pp. 341–48). With growing confidence he formulated, in 1970, the principle of transference, affirming 'logicism': 'what holds in logic must hold in genetics or in psychology' (Popper 1972a, p. 68, n. 30). This permitted development of an evolutionary epistemology. All life, from the amoeba to Einstein, from childhood to death, was problem-solving. Biology, psychology, epistemology and logic came together. Thus Popper completed the ambitious program of semasiology he suggested in 1928.

Critical rationalism was not, however, the semasiology envisioned in 1928. Semasiology was pluralistic. It arbitrated among sciences whose diverse methodologies reflected different perspectives and produced disparate explanations. It was necessary precisely because transference did not work between logic, psychology, physics and biology. Popper knew no more psychology in 1970, when he declared his belief in transference, than he did in 1928, when he rejected logicism. His newly found faith privileged epistemology, imposing its terms on psychology and biology. It created a monolithic science. It also stifled discussion of the psychological and sociological preconditions for scientific and political practice. By narrowing down the field of inquiry and foreclosing intellectual options, Popper made philosophical breakthrough possible. But he left permanently unanswered the interesting, if intractable, questions about the interaction of logic and psychology in scientific practice.

5. THE LOGIC OF SCIENCE AND THE EPISTEMOLOGICAL REVOLUTION

When did Popper become Popper – the philosopher articulating the familiar ideas of critical rationalism? A reader of Popper's 1928 dissertation would not identify the writer as Popper. A reader of *Die beiden Grundprobleme der Erkenntnistheorie* (1930–33; the two fundamental problems of epistemology) could make no mistake (Popper 1979). Some of Popper's major ideas, awkwardly expressed and incompletely developed, are there. Popper's transition from an 'unfamiliar' psychologist of knowledge to a 'familiar' philosopher of science took place between 1929 and 1932. The transition – Popper becoming Popper – has become the central controversy of recent Popper scholarship.

Popper's own accounts of his early intellectual development date back to the 1950s and 1960s. For various reasons, which I have discussed in detail elsewhere, he could not remember well his intellectual development in interwar Vienna, and imposed the teleology of rationally ordered progress on his autobiography (Hacohen 2000; 2007). He read his later views on the logic of science into his psychology of the 1920s. More significantly, he missed the intellectual stalemate of the late 1920s and the great breakthrough of 1929–30. He suggested that, as early as 1919, he formulated, albeit in awkward terms, the problem of demarcating science from pseudoscience. Meditating while apprenticing as a cabinet-maker in 1923–24, he conceived of his critique of induction. He dismissed the idea that, logically or psychologically, we reason (or infer) from particulars to universals and 'what is true of certain individuals of a class is true of the whole class, or that what is true at certain times will be true in similar circumstances at all times' (J.S. Mill

1843, vol. 1, p. 352; Popper 1963, pp. 42–52; 1976, pp. 44–60, 75–78). The only crucial step remaining was recognizing that the problems of demarcation and induction were related: philosophers held tenaciously to induction because they needed a demarcation criterion for science. His solution, the falsifiability of theories as demarcating science, obviated the myth of induction.

It is by now generally recognized among Popper scholars that this is *not* the way things happened. Popper's writings from the 1920s contradict his account. In the 1927 and 1928 theses, Popper unproblematically accepted induction (and verification) as the standard scientific method. He was more partial to inductive (or associationist) psychology than his mentor Bühler, or the Würzburg psychologists. Indeed, as Troels Eggers Hansen (2006a) suggested, he regarded induction as the distinctive mark of scientific theory. He formulated neither the problem of induction nor the solution to it until 1930. He repeatedly visited the question of what distinguishes scientific from non-scientific theory, but he did not clearly formulate the problem of demarcation until late in 1931, possibly even early in 1932 (Hacohen 2006). Falsifiability did not become the cornerstone of his scientific methodology until after the middle of 1932, a time when Popper moved to draw the methodological consequences of his epistemological revolution. This much became clear only once Popper's archives opened and his unpublished early works became accessible. The following account of Popper's intellectual breakthrough reflects, of necessity, my own views, but it was formulated in dialogue with my interlocutors (Dahms 2006; Hansen 2010; ter Hark 2006; Wettersten 1992).

Popper passed with distinction his PhD oral examination in July 1928 with Bühler and Schlick as examiners. In 1929 he wrote an additional thesis on axiomatic systems in geometry, qualifying him to teach mathematics and physics in secondary school (*Hauptschule*) (Popper 1929). The year after, both he and Hennie obtained teaching positions and married. Karl moved into a small house, belonging to Hennie's family, in Hietzing, a working-class suburb of Vienna. The couple lived there until 1935, when Karl went to England. They then moved into a small rental apartment, where they stayed until their emigration early in 1937.

The 1929 geometry thesis reflected Popper's turn from cognitive psychology to the logic and methodology of science. Victor Kraft's work on scientific methods and Edgar Zilsel's work on the 'application problem' may have suggested the topic to him (Kraft 1925; Zilsel 1916; Popper 1976, p. 81). Both works explored mathematics' applicability to reality, tying together logical, epistemological and, in Kraft's case, methodological issues. Kraft emphasized the axiomatic and hypothetico-deductive character of mathematics. He explored the application of mathematical models in scientific theory, most significantly the application of geometrical space in physics. Zilsel discussed the lawfulness of the universe, determinism and induction and raised the problem of scientific rationality. Popper became familiar, at about the same time, with the French mathematician and conventionalist philosopher, Henri Poincaré, possibly by reading Rudolf Carnap (1891–1970) (Popper 1976; 1935; Hansen 1999).[2] Following Kraft, Popper directed his investigation towards the methodology of science.

Divergent concepts of geometrical space competed in theoretical physics, and methodological choice among them proved crucial to Einstein's Relativity Theory. German physicist Hermann Helmholtz (1821–94) and French mathematician Henri Poincaré (1854–1912) disagreed on how the choice of geometry was to be made, and Einstein's negotiations between the two became the legendary stuff of the making of Relativity (Friedman 1995). Helmholtz endeavoured to turn geometry into an experimental science and expected physical experiments to determine geometrical space. Poincaré argued in contrast that preference for non-Euclidian over Euclidean geometry could not be logically or experientially decided (Poincaré 1903). Popper largely agreed with Poincaré, but he thought, all the same, that the application of geometrical space in physics required a clear methodological decision: Relativity Theory was at stake. He added a major caveat that restored authority to experience and experiment. Relativity Theory worked best with non-Euclidean curved space. Attempts to adjust it to Euclidean space, though possible in principle, would require speculation about additional physical forces. It would introduce so many ad hoc hypotheses that the theory would become unusable – a highly complex, even grotesque world view. Scientific theory required economy in the use of hypotheses, the smallest number of assumptions: the simpler the theory, the better.[3]

This was Popper's first statement on natural science methodology. It expressed a clear methodological orientation that would remain consistent through his epistemological revolution. He accepted a strong conventionalist component in methodology, but endeavoured to make tests arbitrators of decisions. His epistemological and methodological probing, while not essential to the thesis, was of great interest to him – and it was new. Applied geometry set the context for Popper's discussion of scientific rationality and brought together his long-held interests in theoretical physics, mathematics and epistemology. It provided the launching pad for his epistemological revolution.[4]

Also new was the influence of the Vienna Circle. Carnap, Kraft and Zilsel were all Circle members. In the preface to the geometry thesis, Popper declared that his orientation was similar to theirs. The Circle was one of the semi-formal groups who lent Viennese intellectual life of the fin-de-siècle and interwar years their legendary intensity. It consisted of philosophers and scientists committed to a radical reform of philosophy. They sought to apply recent advances in logic, mathematics and scientific theory to philosophy. Many members were deeply influenced by Wittgenstein's *Tractatus Logico-Philosophicus* (1922). They declared war on traditional philosophy, denouncing its conceptual imprecision and pseudo-problems. Among the more famous members were Carnap, Neurath and Schlick. Their philosophy became known as 'logical positivism' (Blumberg and Feigl 1931). They were by no means the most influential or well-known group in Vienna, but they established links to like-minded groups in Central Europe's urban centres: Berlin, Prague, Warsaw, Budapest, Lwów, Bratislava. They had disciples throughout Europe and North America. Many of their members emigrated west in the 1930s and had tremendous influence on postwar Anglo-American analytic philosophy (Feigl 1969; Haller and Stadler 1993; Kraft 1953; Ayer 1959).

Popper's relationship to the Circle was problematic. He admired Schlick's early work and took courses with him in 1925–26. He studied carefully Wittgenstein's and Carnap's writings and visited the latter's seminar in 1928 or 1929. He used Carnap's and Hahn's works in his 1929 geometry thesis. Beginning in 1929, he met Victor Kraft (1880–1975) and Herbert Feigl (1902–88) for lengthy discussions, and the two, at the same time impressed and exasperated by his arguments, encouraged him to develop and publish them. Sometime in 1930 Popper set out to write a book criticizing the Circle. This would become *Die beiden Grundprobleme der Erkenntnistheorie.* The pattern of his relationship with the Circle was now set. He was not admitted to the Circle – members regarded him as intellectually gifted but difficult to get along with – but he developed his philosophy in a critical dialogue with theirs, working for long periods on his own, then coming out to confront them with the results. Recognizing his originality, the Circle provided him with opportunities that eventually made him a renowned philosopher, but the disjunction between his philosophy and positivism remained a source of constant tension (Popper 1967–69; 1976, pp. 72–90; Transcripts; Hacohen 2000, chapter 5).

His new ideas owed most to his long engagement with Leonard Nelson. Nelson was an untypical German mandarin (Ringer 1969). Of Jewish origin, he was a militant left-liberal and cosmopolite who preached a universal Kantian ethics and called for the establishment of an international legal system. At the end of the World War I, which he strongly opposed, he founded the *Internationaler Jugendbund* (IJB), a youth league promoting educational and political reform. He had little trust in parliamentary democracy and thought it urgent to cultivate leadership for the Weimar Republic along the Platonic idea of the 'rule of the wise' (Nelson 1928; 1949). He rejected Marxism, but his League joined first the independent socialists (USPD) and then the mainstream socialists (SPD), only to be expelled in 1925. Popper first became familiar with Nelson in 1924 through one of Nelson's students, Julius Kraft (1898–1960). Shortly after Popper apparently declined an invitation to join Nelson's League, voicing political dissent over the 'rule of the wise' (Popper 1991, pp. 20–26; Dahms 2006).

Nelson's influence on Popper was multifarious. His cosmopolitanism informed Popper's political theory, and he was Popper's point of departure for engaging Plato and democracy. Popper's 1927 essay on the idea of *Heimat* carried Nelson's imprint (Popper 1927b). He expressed deep antipathy to romantic visions of the nation as a 'home' and sought to transform it into a Kantian legal association (*Rechtsverband*). Nelson also shaped Popper's view of the history of philosophy as a story of progress and regress. It was, however, his epistemology that proved most essential to Popper's intellectual development. As a student, Nelson discovered the nearly forgotten Kantian philosopher, Jakob Friedrich Fries (1775–1843). Fries considered himself Kant's true successor. He formed a critique of Kant's transcendental proofs in epistemology, ethics and religion. Kant held that certain propositions had an a priori validity because no conception of reality or morality was possible without them. Fries thought that these synthetic a priori propositions left too much of the world closed to the human mind, and, at the same time, ran the risk of subjectivism. He developed a methodological procedure for grounding knowledge in a universal human psychology, thereby eliminating much of Kant's agnosticism and 'subjectivism' (Fries 1831). In his inaugural dissertation, Nelson defended Fries against contemporary Neo-Kantians (Nelson 1904a). In 1904 he re-established the *Annales of the Friesian School*, a journal where he and like-minded colleagues published their work.[5] His voluminous work in epistemology, ethics and jurisprudence carried the imprint of Fries's 'Kantianism with a greater confidence of reason' (Nelson 1904b, p. 33; 1908).

Popper rejected precisely this 'confidence'. He shared Fries's and Nelson's critique of Kant but rejected their solution, and offered his own view of knowledge in its place: uncertain knowledge. His arguments with Julius Kraft over Fries and Nelson set the context for his epistemological revolution (Popper 1962; 1979, sect. 5), beginning with a critique of induction.

The conventional empiricist view was that scientists proceeded from particular statements, which were based on observation or experiment, to general hypotheses and theories. But, as Hume showed in his *Treatise on Human Nature,* there was no logical necessity in proceeding from particular experiences to general laws. Nelson and Popper insisted, therefore, that the inductive principle itself, the assumption that the universe was lawful and repeated observations could be generalized into universal (natural) laws, required justification. None, however, existed. All statements (or theories) could be justified by other statements, but these, in turn, needed justification themselves. We risk either infinite regress or, alternatively, dogmatism – that is, a decision to accept unjustified statements on faith or authority. Inductive knowledge seemed shaky, but Popper thought he had a solution. While working on his 1929 geometry thesis, he encountered deductive models in geometry and explored their scientific uses. Science did not proceed inductively, but deductively – that is, by deducing predictions from theories (natural laws) and testing them. The first stage of his epistemological revolution consisted in substituting an empirical deductive model of science for inductive ones (Popper 1929; 1979, sect. 3, 5–6, 47; Nelson 1911).

Popper's new scientific model found its first application in a critique of the Vienna Circle. Around 1930 Circle members seemed to have held a twofold criterion, enabling them to distinguish between scientific and metaphysical statements (theories). Metaphysical statements were meaningless because they failed to conform to the formal rules of scientific language, but also because they could never be verified against reality. Schlick and his colleagues were familiar with the problems of induction and verification – they recognized that theories went beyond 'the given' and could not be verified (Schlick 1925, pp. 353–67; Feigl 1929, chapter 3) – but, prior to Popper's criticism, they took the problems for granted and tried to work around them (Schlick 1925, pp. 148–56; Carnap 1928, pp. 252–53; Feigl 1929, chapter 3; Kraft 1925, pp. 192–258; Waismann 1930). They soon discovered that scientific theories passed neither the logico-linguistic nor the verification test they set for them. The Circle began the attack on metaphysics seeking to explicate and vindicate science's claims to knowledge and ended up nearly shattering the scientific edifice.

In much of the first volume of *Die beiden Grundprobleme der Erkenntnistheorie,* written between 1930 and early 1932, Popper criticized positivist solutions to the problem of induction. To Hume's problem – there is no logically valid progression from particular experiences to general laws – Popper offered a radical solution: such progression was unnecessary for science and played no role in it. Deductive models made universal theories (or natural laws) logically tenable by testing single reality statements (*Wirklichkeitsaussage*) deduced from the theory against reality. Potential falsification rendered universal theories, which could not be verified because of the induction problem, partially decidable: they could be falsified in a test, hence they were a part of science. Note that, at this stage, Popper was still a foundationist and a 'justificationist' – he believed that single statements could be verified and justify holding a theory. The distinctive mark of scientific statements was their decidability, not their falsifiability. He was mighty proud of having surmounted science's foremost logical problem – induction.

While criticizing Schlick, however, he developed a critique of conventionalism, asserting the possible falsification of a natural law (universal statement) in a crucial experiment. Natural laws were not mere conventions. They could not be verified, but they could be falsified. This was a good enough reason to consider them a genuine part of science. Falsifiability became a marker of scientific statements. To complete his critique of Schlick, Popper engaged with Wittgenstein, assailing the view that natural laws were metaphysical and meaningless. He discovered the problem of demarcating science from metaphysics, and recognized that falsifiability was an alternative demarcation criterion to induction. It dawned on him – it was the early spring of 1932, or thereabouts, and he was at the end of the book – that scientists and philosophers clung so desperately to induction because it provided their demarcation criterion, their way of vindicating science against metaphysics. Induction and demarcation now came together. The concept of two fundamental problems of epistemology emerged.[6]

Popper wrote the book in virtual isolation, but, in the spring of 1932, he had Julius Kraft read at least parts of it (Hacohen 2000). Kraft suggested that Popper still faced the problem of induction – he resolved nothing by deferring verification from universal to singular statements (Kraft 1932). Kraft's input converged with the Circle's challenge. Vacationing with Feigl and Carnap in August 1932 in the Tyrol, Popper discovered that Carnap and Neurath had meanwhile moved beyond the positions he had criticized. Neurath now questioned the finality even of basic scientific reports. In response, Popper radicalized his position. The revised section 11 of volume I of *Grundprobleme,* written during the fall of 1932, as well as fragments of the second volume written about the same time, reflected the change. Not only universal theories, but even basic statements (*Basissätze*), which had been seen traditionally as the foundation of science, the building blocks for complex theories, were provisional. They were low-level theories, verification of which could never reach a closure. Their testing did not always produce incontrovertible results and could continue, in principle, ad infinitum. A decision to stop testing and accept a statement was conventional. Scientific knowledge remained forever conjectural.

Still, the possibility of falsification, of the scientific community agreeing that a theory failed a test and another must be substituted, held the prospect of progressive error elimination. Theories were neither a priori valid nor confirmed beyond doubt by experience, psychological or observational, but they could be falsified in scientific tests. The observation of even one black swan would disprove the theory that all swans were white – if the community of scientists agreed that this is indeed what happened. Moreover, hypotheses that had so far withstood scientific testing had proved their mettle. Scientists were perfectly justified in provisionally accepting them. Objectivity depended not on neutralizing the scientist's subjective dispositions – which was, in any case, impossible – but on subjecting all theories to severe testing. The 'objectivity' of knowledge was guaranteed by its intersubjectivity, that is, by critical scientific discourse. A new conception of science gradually emerged: a hypothetical body of knowledge, ever in flux, but subject to logical control. Science had no firm foundation, but it was eminently rational.

Carnap was greatly impressed by Popper. Considering him a collaborator in developing scientific philosophy, he modified his own positions to meet Popper's criticism, and reported on his work in the movement's journal *Erkenntnis* (Carnap 1932b). Popper felt uneasy about their collaboration, but depended on Carnap for publishing his book. Several publishers rejected his manuscript, and he was desperately trying to interest Schlick through Carnap (Carnap 1932a; Popper, 1932–33). Moreover, engrossed in the critique of positivism, his philosophy's revolutionary implications – the *Weltanschauung* that he spent a lifetime elaborating – were not clear even to him. At points, it seemed as if he was trying to do one better than the positivists in excluding metaphysics by substituting his own demarcation criterion (falsifiability) for theirs. His brilliant demonstration that positivists could not do science without philosophy came at the cost of a diminished philosophical sphere. Scientific philosophy became an instrument for excluding metaphysics.

The only way out was to let metaphysics back into philosophy by relaxing the boundaries between science and metaphysics. Popper gradually did so. In *The Open Society,* he engaged in non-scientific historical interpretation that was excellent political philosophy (Popper 1945, vol. 2, chapter 25). He also extended the criterion of scientific objectivity from intersubjective testing to public criticism (Popper 1945, vol. 1, pp. 109–11, vol. 2, pp. 204–06, 224–26). A decade later, he showed that criticism of metaphysical theories was possible (Popper 1958).[7] Metaphysics re-entered philosophy. But this was all in the future. In the early to mid-1930s he was mostly concerned with consolidating his epistemological revolution by developing a falsificationist methodology to guide scientific work. He sought to establish new ground rules for the formation, testing, corroboration and falsification of theories. These tasks made his differences with the Circle less striking, and his complaints that they were essential seemed unreasonable.

Popper and the Vienna Circle were both committed to scientific reform of philosophy, but they advanced alternative reform programs. The Circle undertook to reconstruct philosophical and scientific language and to overthrow metaphysics. Popper found both the pursuit of a universal scientific language and the crusade against metaphysics futile. Choice of language was pragmatic – each scientific discipline may choose differently. Scientific discovery commonly began with metaphysical speculation that became scientific once formulated as a testable hypothesis. If traditional philosophy became rigorous, philosophy would be alive and well.

This was *not* positivist philosophy. Against various platforms for linguistic reform Popper counter-posed a revolutionary epistemology that rehabilitated philosophy and made the search for uncertain yet 'true' knowledge a compelling task. The Circle overlooked their differences and assimilated his work into their discourse. Most discussions of Popper in the immediate postwar period relied on secondary reports made by other Vienna Circle émigrés. They viewed Popper's contribution from a positivist perspective, and the 'legend' of a positivist Popper emerged. Popper suffered and benefitted from the legend at the same time. Practicing uncommon intellectual openness, the Circle gave their most formidable critic, a contentious young philosopher, a chance to develop and publish his work. He articulated the most compelling philosophy to emerge from interwar Vienna.

6. LOGIK DER FORSCHUNG

Through Schlick's intervention, Springer, the Circle's publisher, awarded Popper a contract in June 1933, subject to revision and shortening. From the summer of 1933 to the spring of 1934, in a year of incredibly intensive work, Popper wrote a virtually new book, *Logik der Forschung.* It was less philosophical and more technical. The longest chapters, on probability and quantum physics, addressed issues that Popper had barely broached before. He devoted the greatest effort to them – so much so that one wonders when he found time to revise the old material. He transformed *Grundprobleme*'s long-winded discussions into concise, fast-moving, sharp arguments. Space limitations dictated the new style, but, more significantly, Popper found his voice. In *Logik der Forschung,* one hears for the first time a confident and mature philosopher.

By Springer's deadline, March 1, 1934, Popper was still working on probability. He was able to settle only the book's new title – *Logik der Forschung.* He submitted the manuscript on May 9. Springer held it for a while, Popper continuing to work on quantum physics. Sometime in June or early July, Springer returned the manuscript, demanding that it be cut by one-third. Popper had the opportunity to revise the physics chapter, indeed, the entire book, but he was utterly exhausted. He reports that his uncle Walter Schiff did much of the editing. A look at the contents clarifies where the major cuts were made. The technical chapters tolerated no significant abbreviation. Indeed, Popper added new material. The first five chapters, outlining *Grundprobleme,* bore the brunt of the cuts (Popper 1935; 1976, p. 85; Carnap 1934). On 8 August, Popper brought the final manuscript to Springer.

Intending, until mid-1933, to complete a second volume of *Grundprobleme,* Popper ended up writing a new book. In the tumult of 1934, *Logik* rapidly changed form under different editors. We do not have the necessary material to reconstruct *Logik*'s emergence from *Grundprobleme,* since the draft of *Logik* we have is relatively late (1934) and incomplete. The confusion gave rise to the postwar search for the lost manuscript of *Grundprobleme* II, which I do not think ever existed as a separate book from the early version of *Logik*. Substantively, we may possess everything Popper produced between 1932 and 1934.[8]

Logik der Forschung opened with two chapters outlining Popper's scientific methodology and its fundamental problems: induction, demarcation, falsification, deductive testing, empirical basis, psychologism and objectivity (intersubjectivity). Popper fine-tuned *Grundprobleme*, saying little that was new but saying it better. He divided *Logik*'s second, and much longer, part – 'some structural components of a theory of experience' – into eight chapters. The first three, 'theories', 'falsifiability', and 'empirical basis', the fifth, 'simplicity', and the last, 'corroboration', betrayed a debt to *Grundprobleme,* the other three, on testability, probability and quantum theory, almost none. All eight chapters investigated the structure and forms of scientific theory, the range of scientific concepts and the rules governing scientific procedures. Popper now argued for philosophy as a critical theory of science (Gomperz 1932; Carnap 1932b, p. 228). His philosophy was superior, he said, *not* because it corresponded to scientific practice (its claim to fame in *Grundprobleme* I), but because it clarified and resolved its problems. He proposed a set of methodological conventions for scientific practice that was logically tight, like few others in the history of philosophy.

Logik der Forschung created a stir in Vienna and beyond, wherever the Circle's network reached. For a specialized book on scientific philosophy, it was widely read and reviewed. Not all reactions were positive. Philipp Frank (1884–1966), Neurath, Hans Reichenbach (1891–1953) and Schlick within the circle, and physicists outside it, especially Werner Heisenberg's entourage, were critical, even outraged. *Logik*'s quantum chapter included an imaginary experiment, which Popper claimed could measure simultaneously an electron's position and momentum, thereby undermining a mainstay of Heisenberg's subjectivist interpretation of quantum theory. Popper was proved wrong, but Einstein, who finally convinced him of it, also praised the book. And Popper had his defenders. Carnap regarded the book as an outstanding achievement. Polish logicians, Kotarbiński and Tarski, thought it extraordinary, and young philosophers, Ayer, Hempel and Nagel, were in awe. *Logik* received three reviews in *Erkenntnis,* and the reviewers' intense exchange demonstrated its vitality.

Popper began corresponding now with philosophers, physicists and mathematicians in Central Europe, England and the United States. He joined Karl Menger's *Mathematisches Kolloquium* where, in company with mathematicians from all over Central Europe, he contributed to probability theory. He lectured in September 1935 at the Paris Congress for Scientific Philosophy, as a member of a distinguished panel, and, in 1935–36, went on a lecture tour in England in search of an academic position. *Logik* did not bring him the Austrian appointment he dreamed about, but it made Central European culture, on the eve of its collapse, a living reality for him, and facilitated his job search abroad. The book may have well saved his life.

7. CONCLUSION

Writing to Carnap on February 17, 1934, just a couple of days after Austrian Chancellor Engelbert Dollfuss shelled the Karl-Marx-Hof in Vienna and crushed the socialist uprising, Popper included a brief handwritten post scriptum: 'On the events in Vienna, better talk only in person' (Carnap 1934). As Popper was writing his book, Central Europe was falling apart: Hitler came to power in Germany, and Dollfuss put an end to democracy in Austria. Just days after *Logik der Forschung* had gone to the printer, an abortive Nazi coup killed Dollfuss. By the time the book came out, the government had completed the first round of academic dismissals and was pressuring schoolteachers, such as Popper, to adapt their pedagogy to the regime. Rumours of another Nazi coup coming were widespread.

None of the congresses for scientific philosophy, beginning in 1935, could take place in Central Europe. Instead, they were held in Paris, Copenhagen, Cambridge in England and Cambridge in the United States. They solidified British and American interest in scientific philosophy and facilitated the Vienna Circle's migration. Within a few years, most members left Central Europe for England and the United States. Popper recognized that he, too, had to leave Austria. With the help of Viennese friends and the Academic Assistance Council, an organization helping academic refugees, he landed a lecturer position in Canterbury University College in Christchurch, New Zealand. On January 5, 1937, he left Vienna for London to make his way to the antipodes (Hacohen 1996).

A brief report on the second Paris Congress from the summer of 1937 mentioned that 'Popper, one of scientific philosophy's greatest hopes, went to New Zealand' (Anon. 1937). This was an appropriate epithet for *Logik der Forschung*. Popper solved some of epistemology's most intractable problems, creating a wonderful vision of science as a rational, adventurous, progressive enterprise, a vision reuniting science and the best of traditional philosophy. Paving his path amidst political and intellectual turmoil, he rejected authority and order without succumbing to relativism and mystification. He carried out a philosophical revolution in *Grundprobleme* and established a post-revolutionary order in *Logik,* providing methodological guidelines for future transformations. His ingenious synthesis of objectivity and incertitude, tradition and change, truth and convention, rationality and criticism constituted a novel view of human knowledge. We do not know, he said, we guess, and our guesses are guided by beliefs that often prove wrong. Science is not an *episteme.* It remains tentative forever. Rationality requires no foundation, only critical dialogue (Hacohen 2013).

The legacy of the progressive Viennese intelligentsia underlay Popper's science. In the late 1930s progressive Central European culture went into exile. Like other émigrés, Popper knew that, if he were ever to return to Central Europe, it would require a new politics. He would devote the next decade to fashioning this new politics and create in *The Open Society* a political vision that will rival for its originality and influence the vision of science he had fashioned in Vienna (Hacohen 1999; 2000).

NOTES

1 In a *Berliner Tageblatt* article, Einstein (1919) stated that a theory's truth could never be proved, as future experience may contradict it. A theory can only be shown incorrect through a logical failure, or contradiction by a fact. Intuition alone can decide between two competing theories agreeing with the facts. Popper (1984b) wrote to Einstein's archivist, John Stachel, that he had never before seen the article. After 1919, it took him at least another decade, I believe, to reach the first two of Einstein's conclusions and find a way to reject the third.

2 Popper mentioned the French conventionalists, Pierre Duhem and Henri Poincaré, numerous times, but, prior to the postwar era, he quoted Poincaré only once (*Grundprobleme*, p. 207) and cited Duhem twice (*ibid.,* p. 23, n. 9; *Logik der Forschung,* p. 225, sect. 1, n. 5). There are no page references to either. Hansen (1999) shows 'that his [bibliographical] references to Poincaré (in Popper 1929) must have been 'cribbed' from Carnap's *Der Raum* (1922]): 75'. (Popper's thesis, in contrast, is novel.) Joseph Agassi (1997) reports that his 1956 review of Duhem's English translation surprised Popper. He had not realized how close they were. He borrowed Agassi's copy and eagerly read it. His critique of conventionalism in 'Three Views Concerning Human Knowledge' (Popper 1963, pp. 97–119) sought to distance his views from Duhem. Joseph Agassi rethinks the relationship in: Agassi 1975, pp. 366–370.

3 Popper gave Mach's 'economy of theory' and Poincaré's 'simplicity' a twist: it was not the simplicity of geometry (or theory) itself that counted, but of all its applications.

4 Michel ter Hark (2006) disagrees. To him, the 1929 thesis shows that Popper had not yet broken through. He did so only by adopting Selz's task-oriented mind, proceeding through trial and error, then translating Selz's psychological search into a methodological one.

5 Ernst Apelt, Fries' student, began the first *Abhandlungen der Fries'schen Schule.* It lasted only two years. The publication of Nelson's new series was interrupted in 1937. Kraft conceived of *Ratio,* begun in 1957, as a renewal.

6 In his Autobiography (Popper 1976, p. 79) and *Conjectures and Refutations* (Popper 1963, pp. 33–59), Popper dated the linkage between induction and demarcation to about 1929, 'after my dissertation'. Having antedated his solutions to both demarcation and induction, he remembered 1929–1934 as one revolution, whereas there were a series of them.

7 'On the Status of Science and Metaphysics' (Popper 1958) drew on the *Postscript to The Logic of Scientific Discovery* (Popper 1982b; 1983), which was intended to be published in 1954 but came out only in the early 1980s.

8 Popper completed only the first volume of *Die beiden Grundprobleme* on induction. The second, on demarcation, was never completed. Hansen collected in Popper (1979) all extant material, dating from late 1932 and early 1933. In July 1933 Popper began work on *Logik der Forschung.* For details, see Hacohen (2000, chaps. 5–6). For a dissent, see Hansen (2010, pp. 599–617).

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ARNE FRIEMUTH PETERSEN

3 On Popper's Contributions to Psychology as Part of Biology*

Popper's relationship with psychology was both critical and creative. Although he did not like it being said that he had developed a psychology of his own, his lifelong enquiry into the philosophy of science, logic and methodology contains a wealth of ideas for dealing with psychological phenomena without falling into inductivist, subjectivist or other traps. He did not arrive at these insights without effort, and the whole adventure demanded both courage and ingenuity.

At the age of sixteen the young Popper had left high school, disappointed with the teaching, to study at university following courses in philosophy, psychology and many other disciplines. Four years later, having become a regular student, he was already bored with his lecturers, and by extensive reading had embarked on the search for better ways to acquire knowledge. He had been captivated by Kant's works which he studied with rolled-up sleeves. To obtain the right understanding and interpretation of Kantian philosophy he would seek advice from specialists in the field, and during that decade he held discussions with and/or read the works of Julius Kraft, Herbert Feigl, Leonard Nelson and Heinrich Gomperz, as well as Karl Bühler, a Kantian psychologist and professor at the Institute of Education from where Popper graduated in 1927. (For a more detailed chronology, see the section entitled 'Cognitive psychology, 1925–1928' of Hacohen's essay, Chapter 2 in this volume.)

These studies supported him in the view that the body of knowledge does not consist of accumulated sense data but of the best tested hypotheses about the world, and that scientific knowledge should only include such hypotheses or theories that may be refuted. For this the necessary tools would be methodology – systematic observation, experimentation and so forth. Realizing that this also applied to psychology,

* The title relates to a remark made by Popper in 1969 in a conversation with the present author: 'If psychology is to survive, it will survive as a biological discipline.'

he chose 'the question of method in cognitive psychology' as a topic for his dissertation (Popper 1928).

When Popper was studying in Vienna, the positivist movement had succeeded in placing psychology at the centre of the sciences. Students were supposed to be acquainted with the basic building bricks of their coming scientific enterprise, '*die Protokollsätze*', those phenomenological mini-reports allegedly employed by all working scientists. The idea was that in order to do science, a certain knowledge of psychology was required, and logical positivists, such as Jørgen Jørgensen in Copenhagen, wrote textbooks on psychology introducing future scientists to the machinery of the psyche – how outside phenomena led to sensory data in the brain that could condense into hypotheses and perhaps later be distilled into scientific theories.

Popper had broken with all that and, when he qualified as a teacher of mathematics and physics in secondary schools on the basis of a tour de force on Euclidean and non-Euclidean geometry, *Axiome, Definitionen und Postulate der Geometrie* (1929), he also turned his back on psychology. In working with geometry he seems to have realized that logic, not psychology, was important for the account of scientific discovery and methodology – a theme he analysed in depth while working on his mammoth manuscript, *Die beiden Grundprobleme der Erkenntnistheorie* (1930–33), which became the precursor of *Logik der Forschung* (1934).

Nonetheless, Popper's preoccupation with psychology was not only an important period of transition. His early orientation within this domain later resulted in a number of contributions to psychology, partly as by-products from his criticism of ideas in philosophy, psychology, biology and other sciences, and partly emerging from his epistemological considerations and logical analyses. In the following the importance of Kant's philosophy for Popper's deductivist psychology should be evident, as it will also be felt in his ideas on problem solving which he developed into a general characteristic of life, as well as in his hypotheses about mind-brain interaction and the functions of mind and self.

1. THE POSSIBILITY OF A DEDUCTIVIST PSYCHOLOGY

In chapter II of *Die beiden Grundprobleme*, Popper outlined a deductivist psychology as a possible alternative to inductivism. In his presentation he started out with an idea from Ernst Mach, who had explained the origin of concepts on a biological basis, more precisely as a result of interactive relationships between perception and reaction. Mach said: 'What is reacted to in the *same way* falls within the *same concept*. There are as many kinds of concepts as there are kinds of reactions' (Popper 1979, p. 24; 2009, p. 26.). This claim was supported by the neurophysiological evidence that the nervous system can be subdivided into an *afferent* and an *efferent* side – corresponding to the division between reception and reaction.

From this idea of Mach's, Popper went on to argue that behavioural reactions may well be triggered by stimuli from the outside, but, with respect to the specific *type of reaction* and its characteristic course (*Reaktionsablauf*), each stimulus is heavily dependent on the reacting apparatus itself. In fact, the releasing, '*objective* stimulus' is only the '*material* condition' for the reaction, whereas it is the reacting apparatus that contains the '*formal* conditions' for releasing the reaction. 'Such reactions may then be considered "*subjectively* pre-formed" … and therefore not "resulting from experience"' (Popper 1977, p. 24; 2009, p. 26).

This reflects Popper's reading of Kant (summarized in his *Scheme 1*) on whose analyses he based his theory of organisms and their ways of life. If one permits the expression 'a priori' to be used in psychobiological contexts as signifying 'not *originating* from experience', then *anticipations* can be considered as 'a priori synthetic judgments'. They are selected predispositions in organisms for navigating in surroundings they have become adapted to during evolution, and they reveal themselves in the way organisms *search for (species-typical) regularities* (*Regelbewusstsein*, Popper 1979, p. 31; 2009, p. 33). When an *anticipation* encounters its 'desired object' (say, a duckling's expectation of a configuration resembling the mother-bird), then, Popper suggested, a *tentative assignment* is formed between the two (in the duckling, its assignment to – imprinting on – the mother-bird is irreversible and may last for life). Whether such anticipations and assignments are retained or not depends – evolutionarily speaking – on their biological value, since the test method in the world of organisms is a *selective* one: 'if the [pre-formed] anticipations are of no value, then they are weeded out' (Popper 1979, p. 26; 2009, p. 28). Here, Popper was inspired by Dedekind who (in a reformulation[1] by Schlick 1925, p. 351) had said: 'Thinking is just one function, that of assignment'. As anticipated in the example just given Popper would later compare assignment to phenomena like imprinting in birds and attachment and pair-bonding in primates – that is to say, phenomena operating according to some kind of theory on the part of the organism.

The *assignment process* can therefore be considered as a *dynamic expression of the capacity of organisms for imposing regularities on the world.* These processes might be ascribed to the aforementioned differentiation between an afferent and an efferent side (between *reception* and *reaction*), especially because the efferent side is normally more autonomous than the afferent one.[2]

Division of statements		A. Logical distinction	
		Analytical judgements	Synthetic judgements
B. Distinction based on grounds of validation	*a priori*	+	?
	a posteriori	–	+

Scheme 1. The question of the validity of statements about reality which divides rationalism and empiricism. (Adapted and translated from Popper 1979, with permission from Karl-Popper-Sammlung, Klagenfurt.)
(A) The criterion for Kant's distinction between analytical and synthetic judgements was a *logical* one. *Analytical judgements* are *tautological*, resting on the principle of non-contradiction: their negations are contradictions, and they can be proved by logical transformation. The truth or falsity of *synthetic judgements* cannot be decided on logical grounds alone: their negations are *not* contradictions, wherefore they are *logically possible*.
(B) Kant's second distinction between a priori and a posteriori statements was *epistemological*, as it refers to the methods by which judgements can be justified. Here, Popper (1979, p. 13; 2009, pp. 14f) points out that 'a priori' and 'a posteriori' are not homologous: 'while the expression "a posteriori" implies specific grounds of validation, namely that of empirical tests, "a priori" only indicates that the statement in question is valid independently of experience'.
The scheme should thus be read: All *analytical judgements* are valid a priori (+). Therefore, all statements valid a posteriori must be *synthetic judgements*, denoted by (–) and (+). This says nothing as to whether synthetic judgements may be valid a priori (?), and Popper (*op.cit.*, p. 16; 2009, p. 17) concludes that while *synthetic judgements may well exist a priori*, they are not, however, *a priori true* but *problematic*, since they are often, when empirically tested, found to be *a posteriori false*.

So, for biological reasons, no *direct* assignment can occur between the sensations themselves (derived from 'repeated perception'), as claimed by traditional learning theory. Sensations first have to be assigned (attached) to the register of pre-existing dispositions to act, before *indirect* assignments can be established between the sensations themselves (those fictive sensorial links that traditional learning theory has wrongly taught as being the basis of all experience – as if experience could only consist of *sensory associations* without any connection to internal dispositions). Learning always occurs 'inside out', not 'outside in'.

Thus the search for regularities can indeed correspond to Kant's 'a priori causal statements'. Popper could then conclude his outline of a deductivist-empiricist pre-formation theory with the striking declaration: '*Synthetic judgements may well exist* a priori. *They are, however, often* a posteriori *false*' (Popper 1979, p. 32; 2009, p. 34.) Later he was to apply this Kantian logic to natural selection and learning by trial and error elimination.

2. HABIT FORMATION AS A SELECTION RESULT OF REPETITION

Being an admirer of Charles Darwin for his exploits and use of scientific methodology, Popper thought from the outset that principles of selection could account, not only for the evolution of species and their bodily 'hardware', but also for their 'software', such as learning ability, individual adaptation, memory, and so on. In other words, he felt the need to explain how pre-formed anticipations may serve as a basis for acquiring experience about the surrounding world. In short, he wanted to demonstrate that organisms learn exclusively through hypothetico-deductive procedures.

To do this, he turned to Herbert Spencer Jennings's (1906) studies of exploratory behaviour in lower organisms and George Bernard Shaw's (1921) notion of trial-and-error in human learning. Later ethological observations by Konrad Lorenz, and others to whom Popper referred in his *Autobiography* (1974, notes 44, 286, 287), illustrated his main points about learning even better.

Particularly informative in this respect is his explanation (Popper 1974, note 44) of Lorenz's description of habit formation in his famous goose, Martina, which occurred when Lorenz had started teaching her to mount the staircase inside the house. This account (Lorenz 1966, pp. 57–58) shows how the progressive shortening, day by day, of a flight route towards a window taken the first time the bird approached the stairs ended up, after one year, as a ritualized behavioural pattern, which the bird had to perform before mounting the steps. In contrast to Lorenz's use of traditional learning theory in *his* interpretation of Martina's development of a behavioural habit, Popper admitted no inductive procedures in his explanation: each time Martina faced the staircase situation, her behaviour changed a little as her detour towards the window became shorter and shorter in space and time. As with other cases of so-called habit formation, Popper (1974, p. 38) argued: '[T]here is no genuine "repetition" [or learning by repetition] but rather … change through error-elimination (following theory-formation) … which make[s] certain actions or reactions automatic, thereby allowing them to sink to a merely physiological level and be performed without attention'. In Martina's initial situation of fear, as with birds in general, the 'theory formation' consisted of a ready, pre-programmed preference for light and open space. Nevertheless, her ritualized detour interfered with her learning task in much the same way as tension and anxiety may complicate learning in both animals and humans.

Popper had started this lifelong inquiry in *'Gewohnheit' und 'Gesetzerlebnis'* (On Habit and Belief in Laws) with his attempt at replacing inductivist views on the origin of habits with a deductivist theory of habit formation (*Gewöhnungstheorie*). However, only a few elements of this theory seem to have survived in this thesis (Popper 1927; 2006). Suffice to say that, during this work, Popper realized that the traditional idea of habit formation by repetition was false and should be replaced by a theory in which repetition gradually transforms conscious learning into automatic, subconscious operations or skills. Organisms, he realized, make *jumps to conclusions* to solve their problems, and the selective effect of their ensuing repeated behaviour may account for the increasing promptness with which the retained, functional trials reappear. Habits can therefore be traced in whatever behavioural pattern has *resisted the selection effect* of repeated executions of the original behavioural trials. Thus 'habit' is not the result of a repeated stimulation which has gradually 'built up the behaviour'. *Repetition can only make things disappear*, and those that disappear are superfluous and non-functional.[3] What brings about novelty for organisms, in the first place, is their 'problem-solving jumps' in the form of 'mutations' resulting in new behavioural dispositions, exploratory behavioural trials and (at least from primate level onwards) sudden insight. Much like the subconscious selection process, which is constantly at work, simplifying innervation patterns on a neural level, all conscious learning happens by trial and error correction.

In retrospect, Popper considered his discovery of lasting importance for his own life, and qualified it by saying (1970; 2006, pp. 501–02): 'This psychological insight led me … to a logical criticism of the theory of induction: it thereby came to be the starting point of my philosophy of science.' His interest in learning had been decisive for his choice, in *'Gewohnheit' und 'Gesetzerlebnis'*, of the theme 'dogmatic and critical reactions in young children when faced with novelty', and it gained momentum with the deductivist psychology outlined in *Die beiden Grundprobleme*. In these works, he was influenced by Karl Bühler, Oswald Külpe and other psychologists of the Würzburg School, as well as recent research on cognition by Otto Selz (1913), on child development by Elsa Köhler (1926) and on instinct and habit by Lloyd Morgan (1909 and 1913).

Nevertheless, the realization that logic, not psychology,[4] was important for his account of the growth of scientific knowledge, published in *Logik der Forschung* (1934), had made him abandon psychology and other interests in the meantime. When he returned to them in the 1960s, he connected to ideas from his earlier works to develop his well-known scheme for problem solving, and the thesis that a core characteristic of life is its ability to solve problems. In so doing, he proceeded mainly from memory, and he rarely took time to locate and read his earlier manuscripts.

3. PROBLEM SOLVING VERSUS CONDITIONING

Popper's '*tetradic scheme for problem solving*' of 1966 could no doubt have been formulated earlier had he looked up his earlier analysis of dogmatic and critical thinking, since any reader can now appreciate (Petersen 2008, tables I and II) that the 'dogmatic stage' in dealing with novelty corresponds to 'tentative solution' (TS) and the 'critical stage' is equivalent to 'error-elimination' (EE) in *Scheme 2* (see later in the chapter). Among his published papers only 'What is dialectic?' contained some words in passing about this connection, conveyed exclusively to a philosophical seminar at Canterbury University College in 1937 before being published in 1940. Here Popper used the idea of 'trial and error' to characterize the development of human thought, especially in philosophy, no doubt in order to place Hegelian thinkers among other erring creatures. Without drawing up any scheme, and with no reference to Alexander Bain (who had introduced the term in 1855 to account for human inventiveness), Popper declared that the usual method by which solutions to problems in life could be found was, indeed, '*the method of trial and error*', and he described it generally as the method used by living organisms in the process of adaptation, the success of which depends very much on the number and variety of trials: 'The more we try, the more likely it is that one of our attempts will be successful' (Popper 1940; 1963, p. 312).

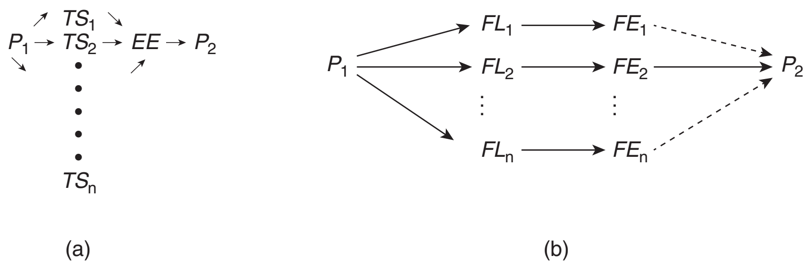
There is also an updated remark about the importance of 'dogmatic thinking' for this process: 'Men seem inclined to react to a problem either by putting forward some theory and clinging to it as long as they can (if it is erroneous they may even perish with it rather than give it up), or by fighting against such a theory, once they have seen its weakness' (Popper 1963 p. 312). An attached footnote makes the point of not discarding a theory too readily: 'The dogmatic attitude of sticking to a theory … is of considerable significance. Without it … we should give the theory up before we had a real opportunity of finding out its strength', thereby missing the events to which it draws attention.

The impact of theories on perception is, however, double-edged, and the impact of *false* theories on perception, which may make their carriers deaf and blind to other ways of viewing the material, illustrates this dynamism very well. Cases to the point are the untenable positivist and other 'mirror-theories' of consciousness and behaviourist theories of learning which make their adherents live not only in a world of *perceptual* illusions but also in a world of *cognitive* illusions.

As we have seen, Popper's theory of problem solving took time to emerge and, due to the war and emigration, many years went by before he had the occasion to introduce it to a larger public. This happened in 1965 at Washington University where he was invited to give 'The Arthur Holly Compton Memorial Lecture'. His now famous theory was presented there as 'an evolutionary sequence of events' (Popper 1966, pp. 23–24; 1972, pp. 242–43): a process that had to do more with the evolution of animal species by natural selection (*long-term problem solving*) than with the individual animal's adaptation to occurring events (*short-term problem solving*). Indeed, the procedure represented in *Scheme 2* is a restatement of evolutionary theory as 'an evolution of new means for problem solving, by new kinds of trials, and by new methods … for *controlling* the trials' (Popper 1966, p. 21; 1972, p. 240). As for the individual organism, this is considered as a hierarchical system of *plastic controls*, in which the regulated subsystems make trial-and-error adjustments that are constantly checked by higher-level systems of control, resulting in some trials being entirely suppressed while other trials are retained and possibly modified by 'local' error elimination.

Popper's theory thus consists of 'a certain *view of evolution* as a growing hierarchical system of plastic controls, and of a certain *view of organisms* as incorporating – or in the case of man, evolving exosomatically – this growing hierarchical system of plastic controls.' The Neo-Darwinist theory of evolution is assumed, but restated by pointing out that 'its "mutations" may be interpreted as more or less accidental trial-and-error gambits, and "natural selection" as one way of controlling them by error-elimination' (Popper 1966, p. 23; 1972, p. 242).

Unlike most evolutionary thinkers who consider natural selection processes and learning processes to be of a different nature, Popper (1975, pp. 73–74) took these processes to be basically alike. In considering such long-term and short-term problem solving, Popper compared three levels of adaptation – (1) genetic adaptation, (2) adaptive



Scheme 2. Popper's tetradic scheme for problem solving by trial and error elimination. (a) The scheme as originally introduced in 1965 (Popper 1966, p. 24). (b) The *full* tetradic scheme in which error elimination (*EE*) is related to the individual tentative solution (*TS*). (2a is reproduced with permission from Karl-Popper-Sammlung, Klagenfurt.)

Commentary. Regarding *conscious problem solving*, on the human level, the scheme implies a number of highly dynamic psychological phenomena: (1) Attempts at solving P_1 require *courage* and *imagination* on the part of the agent(s), also with respect to the nature of the(ir) social surroundings: in many human civilizations, past and present, some sort of permission might be necessary even to consider and work with a given problem. (2) After having launched a tentative solution (*TS*) it requires *self-criticism* to start error elimination (*EE*) in *admitting* that P_1 has *not been solved satisfactorily* and might be in need of *another approach or method*. (3) Having solved P_1 to the satisfaction of the scientific community, it may be a mixed blessing for the agent(s) to have *to confront* P_2, or indeed more problems than have been solved.

behavioural learning, and (3) scientific discovery – and explained the fundamental similarity between these levels by the hypothesis that the mechanism of adaptation was the same: 'Adaptation starts from an inherited *structure* … always transmitted by *instruction* … which is basic for all three levels: by the replication of coded genetic instruction on the genetic and the behavioural levels'. The third level can be regarded as a special case of level two, wherein imitation and social tradition may lead to adaptation by technological means and scientific knowledge. But also on this third level 'the *instruction* comes from *within the structure*. If mutations or variations or errors occur, then these are new instructions, which also arise *from within the structure*, rather than *from without*, from the environment' (Popper 1975, p. 74; 1994, p. 3).

Inspired by immunologist Niels Kaj Jerne's natural-selection theory of antibody formation (Jerne 1967), Popper concluded his argument: '[S]cientific discovery depends on *instruction* and *selection*: on a conservative or traditional ... element, and on a revolutionary use of ... elimination of error by criticism, which includes severe empirical examinations or tests' (Popper 1975, p. 78; 1994, p. 7). In this way we are reminded of Popper's point of departure in the early work on dogmatic and critical thinking and how it was later made to bear on his understanding of animal and human learning.

One of the lessons to be drawn from the Lorenz-Popper debate over habit formation is that Lorenz's firm adherence to the theory of learning by conditioning could not even be shaken or refuted by his own precise descriptions like those of Martina.

Popper, however, continued to develop his deductivist theory of habit formation into a general theory of problem solving which, in his *Autobiography*, he realized was the main characteristic of life (see the next section in this chapter). Some time later, in his contribution to *The Self and Its Brain*, Popper (1977, p. 142) distinguished eight different stages in the process of learning by trial and error-elimination, or by action and selection:

1. Exploration, guided by inborn and acquired 'knowing how', and by background knowledge, 'knowing that'.
2. Production of a trial, conjecture or theory.
3. Testing or criticism of the trial, conjecture or theory.
4. Elimination or rejection of the conjectured solution, and recording that it does not work. ('Not this way.')
5. Repetition of this process (2) to (4) with modifications to the original conjecture or with new conjectures.
6. Discovery that a modified or new conjecture seems to work.
7. Application of this conjecture involving additional tests.
8. Practical and repeated use of the adopted conjecture.

From this logical stronghold Popper bluntly declared that the 'unconditioned' and 'conditioned reflexes' *do not exist*, and he went on to criticize Pavlov's ideas on learning by means of the following points (Popper 1977, pp. 136–37):

1. For Pavlov, an organism is a passive device, waiting for repeated events from outside to stamp regular connections into its memory. Popper, on the other hand, attributed to the organism 'an active interest in its environment', a mainly 'unconscious exploratory instinct' leading to exploratory behaviour. This behaviour is not just a complex of 'reflexes' in Pavlov's sense but the manifestation

of an inborn aim-structure that generates a general curiosity and species-typical activity towards the environment, especially towards other members of the species.

2. The 'unconditioned' and 'conditioned responses' are not just reflexes. From Popper's viewpoint, 'Pavlov's dog … developed the … obvious theory, or expectation, that the food will arrive when the bell rings. This expectation made its saliva flow – exactly as the expectation raised by the visual perception or the smell of food' (Popper 1977, p. 136).
3. Pavlov assumed that all biologically important regularities to which an organism can adapt itself consist of coincidences, like that of the bell and the arrival of food; however, Popper argued, the structure of the environment to which organisms must adapt themselves has 'no similarity with Hume's perpetual conjoined impressions' (Popper 1977, p. 137), or, we may add, with the artificial constructs of contiguity and contingency theories of learning which are unwittingly construed to account for animal behaviour in laboratory settings.
4. Against the inductivist use of repetitive procedures, especially in human learning, Popper had said, two pages before (Popper 1977, p. 134): 'Repetition *does* play a role in behavioural adaptation, but it does not contribute to discoveries.' This is so with skills such as walking, talking, handwriting, music making and driving. 'Repetition, or practising … is a way of turning new adaptations into old ones, into unproblematic background knowledge; into unconscious dispositions.'
5. Regarding the above-mentioned eight stages required for learning by problem solving or by action and selection, Popper (1977, pp. 142–43) concluded that there is no reason to assume that the implied procedures are of the same nature, like certain known reflexes; neurologically, learning could well consist of a 'hierarchical organization of structures of structures', something like a dynamic hologram.

The difference between Pavlov and Popper is far more fundamental than this rendering of opposing interpretations may indicate. It seems to be a case of two fundamentally different ideas about the nature of living beings – or philosophies about the organism. Pavlov regarded organisms as *passive* receivers, *reacting* only to the surroundings that influence and control them by means of 'stimuli'. Popper (1977 pp. 137–38) considered organisms as *active* and *creative*, constantly facing problems to be solved by action and selection. He summarized the differences between Pavlov's and his own view this way: '[O]rganisms actively … impose guessed regularities (and, with them, similarities) upon the world … It is this theory of actively proffered conjectures and their refutation (by a kind of natural selection) which I propose to put in the place of … the conditioned reflex.'

Popper had defended the idea that organisms were active problem solvers from early on, but that a line of demarcation could be drawn according to the very presence of problems in animate matter *versus* their absence in inanimate matter he only put down in writing when he related his discussions with Erwin Schrödinger.

4. PROBLEM SOLVING AS LIFE'S URGE

In Popper's approach, the *problematic* character of '*a priori* synthetic judgments' – that organisms are endowed with *dispositions* and *preferences*, or *anticipations*, relating to the world in which they have evolved – later gave rise to the idea of '*life as problem solving*'. It had already loomed large as part of his opposition to current views on entropy and the allegedly related 'arrow of time', which he had criticized in a number of papers in *Nature* from 1956 to 1967, but it came to full expression in his *Autobiography*.

Erwin Schrödinger had attempted to answer the question of how organisms escape thermodynamic chaos, which is supposed to reach all regions of the universe, by developing the hypothesis that '*life feeds on negative entropy*'. This way out meant that organisms survive and function by absorbing other organic material with low-level entropy. In his *Autobiography* (1974, p. 109), Popper criticized this hypothesis by arguing that many mechanical and chemical machines also 'feed on negentropy': 'In fact every oil-fired boiler and every self-winding watch may be said to be "continually sucking orderliness from its environment".' Thus, in *What Is life?* (1944), Schrödinger's answer to this question cannot be right: feeding on negative entropy is not 'the characteristic feature of life'.

Popper went on to suggest that the distinguishing characteristic of life is that *it has problems to solve*. Although machines, computers and robots can be said to solve problems, they do not solve problems *for themselves*, and therefore cannot be regarded as true problem solvers. He then described his position of *irreducibility and emergence* in the following way: 'I conjecture that the origin of *life* and the origin of *problems* coincide … [Although] there is no biological process which … cannot be progressively analysed in physicochemical terms … no physicochemical process can as such solve a *problem*' (Popper 1974, p. 142).

Thus, there is life as long as there are problems, and it may be claimed that there are two main strategies used by organisms to solve problems:

1. Phylogenetically, most basic problems that a given species would have had are solved by *species adaptation* through *variations* within the gene pool, which have then been exposed to *natural selection*, weeding out those variations which did not meet the requirements of the environment. In such *long-term* sequences of 'collective problem solving', the species-typical activities of its members and, in particular, behavioural innovations also contribute to determining which genes may come to expression over generations, a fact that led Popper to consider the behaviour of organisms as '*the spearhead of evolution*' (1974, note 287) – a hitherto missing dynamism in Darwin's theory of natural selection (see the section on the origin of animal consciousness later in the chapter).
2. Ontogenetically, a great variety of other problems linked to sudden or recurring environmental changes must be solved by *short-term* individual adaptation through *a method of trial and error elimination*. Here, organisms adapt themselves individually by *action and selection*, first by having genetically coded preferences and aims, which may have led, or may lead, to conscious expectation, and second by trying them out and discarding or modifying them when they fall short in encounters with the world.

Both of these types of problem solving assume that the adaptive changes arise within *given structures*, which are always transmitted by *instruction*: 'on the genetic level, the structure … the genome, is replicated *qua* template, and thus by instruction'; on the behavioural level, the structures consisting of a 'genetically inherited repertoire of possible forms of behaviour and, in addition, of the rules of behaviour handed on by tradition' are transmitted by direct instruction. But the new adaptive changes in these structures happen on both levels by means of *selection*. As Jerne had pointed out, 'The conservative power is *instruction*; the evolutionary or revolutionary power is *selection*' (Popper 1977, p. 133).

In a similar vein, Karl Duncker, the psychologist of problem solving, considered life to be 'a sum total of solution-processes which refer to innumerable problems, great and small'. Even such complex phenomena as personality and character are in many respects products of problem solving. Duncker (1945, p. 13, n. 13) had a keen eye for such phenomena: 'Character, so far as it is shaped by living, is of the type of a resultant solution' – an aspect of problem solving which Popper was most reluctant to consider, presumably because of his 1925 criticism of the all-embracing theories of personality, which is discussed in the following section.

5. THE HUMAN SELF CONSIDERED AS A PRODUCT OF THEORY-CONSTRUCTION

Popper always emphasized the importance of the individual human being, the uniqueness of individuals, their lives and peculiarities, their freedom and fate. This is perhaps one reason why he did not consider the theory of personality as a possible field of inquiry. For how could we really establish a science to deal with entities that never repeat themselves with any degree of precision?

In his first publication, a science of 'individuality' or 'personality' was not considered possible at all due to an opposition between 'the unique' (the individual) and 'the typical': '*the typical is seen in the individual when we regard it from a given, general point of view*; for this reason the typical changes in accordance with any change of viewpoint.' Therefore science will never be equipped to deal with individuality, since 'a science without a general point of view is impossible' (Popper 1925, p. 204; 2006, p. 4).

This methodological problem of a science of individuality and the self derives from the difficulty of determining *invariants* among phenomena whose variation seems to be more pronounced than anywhere else. Fifty years later, Popper dealt with this problem employing a biological theory of hierarchically organized control systems, of different origins, and with different functions: (1) individuality originated in multicellular organisms with the evolution of 'biochemical individualities' and highly specialized nervous systems; (2) personality in animals and humans evolved as a result of a behavioural ontogeny which became increasingly individual-specific; and (3) the human self originated as a result of the individual person developing theories about himself. As Popper (1974, p. 151) saw it, 'the full consciousness of self is a feedback product of *theory making*'. Thus the self enables the person to direct his own behavioural dispositions and act in accordance with *an aim-structure* which, to a great extent, is *self-chosen.* The self is like a *pilot*, navigating from the top of the hierarchy of controls that constitute the complex system of communication within the individual organism.

Popper considered this potential for ontogenetic individuation, which the human individual possesses for developing a self, to be a consequence of the evolution of the two highest functions of human

	Functions	Values
	(4) Argumentative function	Validity/ invalidity
Man	(3) Descriptive function	Falsity/ truth
Perhaps bees	(2) Signal function	Efficiency/ inefficiency
Animals and plants	(1) Expressive function	Revealing/ not revealing

Table 3.1. Main functions of language according to Bühler and Popper (Popper 1982, p. 49). The direction of phylogeny is indicated by the numbers as the lower communication functions are placed below the higher language functions. The lower functions are always involved in the higher functions, but not the other way around. (Reproduced with permission from Karl-Popper-Sammlung, Klagenfurt.)

Examples: (1) signals of fear, pain, conflict, joy, shame, blushing or flushing, etc. communicate the state of the agent (also named '*symptom-function*'); (2) ritualized signals of flight and alarm, postures or sounds of threat, begging, command, etc. (also named '*appeal-function*'); (3) signals capable of 'species-typical description' (dance-language of bees; bio-sonar communication in toothed whales), pointing and gestural signalling in primates, and (signals turning into) symbols in human description of localities (maps and pictograms), organizations (emblems and logos), people (icons) and more complex shorthand descriptors (also named '*call function*'); (4) symbols capable of referring to a great variety of issues and objects, not necessarily present in the actual situation of communicating (spoken and written representations; numbers and other abstract systems), especially used for determining the truth or falsity of some matter (e.g. true or false descriptions or reports) and furthermore at work in humour, jokes, caricatures and other non-formalized contexts (also named '*critical function*').

language:[5] the *descriptive function* and the *argumentative function* of language (Table 3.1). The use of these functions, Popper conjectured, brings about a more developed conception of time and, with this, the development of a conception of individual continuity and unity (1974, pp. 151–52).

Historically, narration had first appeared in oral transmission of experience between tribal members of the same or succeeding generations, and with the invention of written languages, this transmission became independent of the carrier of experience. Gradually, a world that Popper called *World 3* arose, consisting of descriptive accounts and reasoned theories about the world, an *exosomatic evolution* of scientific and cultural knowledge and tools – entirely a product of human activity and made accessible in a great variety of ways.

On an individual level, early man only became fully conscious when the individual managed in World 3 to anchor spoken or written theories about himself, his body and mind and his plans for action. Such give-and-take processes between World 2 and World 3 enabled him to form (primitive) theories about himself in relation to the rest of the world – and, in many ways, this is still repeated during the ontogeny of each new generation. Popper considered this mediating role of the mind of great importance: 'World 2 – the world of the mind – becomes, on the human level, more and more the link between World 1 and World 3'. Thus the use of physical bodies and processes in World 1 is constantly influenced by the subjective World 2 grasp of knowledge about their functioning stored in World 3. And he concluded: 'This is why it is impossible to understand the human mind and the human self without understanding [World 3] (the "objective mind" or "spirit") ... Both we ourselves and [World 3] grow through mutual struggle and selection' (Popper 1972, pp. 148–49).

Although humans, like individuals among higher animals, are born with individualities differing from one another, and with a certain degree of consciousness, newborn babies cannot be said to possess a self in the sense described here. Popper (1974, p. 152) conjectured that the growing child *learns* to become a self through *the use of language*, and that the following stages will be characteristic of this development: 'only after our knowledge of other persons has developed, and ... we have become conscious of our bodies' extension in space and, especially time', will it be safe to say that complete self-awareness has developed – this also necessitates knowledge about the continuity of our bodies and selves during sleep.

The self reveals itself in children when they are able to talk about their life in the future, and especially when they have formed more or less detailed plans for their lives, described by Bühler as '*Lebensplan*' (1930, pp. 275–78). A child's long-term planning of his future activities will typically integrate his imaginative world, his needs and motivations, mediated by the mentioned feedback from formulated goals and aims. Popper (1977, p. 145) had a similar idea of 'a plan for life' and considered it vital for the growing person's sense of unity and moral character: 'It is the possession of such a ... plan, which makes us transcend ourselves – that is to say, transcend our instinctive desires and inclinations ("*Neigungen*", as Kant called them).' The self, then, is much more than a Cartesian 'substance', a 'pure subject', or 'a stream of consciousness' as William James (and James Joyce) believed.

For Popper (1977, p. 120), the self is a *centre of action*: 'It is acting and suffering, recalling the past and planning ... the future, expecting and disposing. It contains, in quick succession ... wishes, plans, hopes, decisions to act ... it owes this selfhood largely to interaction with other persons, other selves, and with World 3.'

Like Sherrington, Popper referred the identity and integrity of the self to the brain, implying a distinction[6] between personality and the self. He said (1977, p. 115): '[W]e can lose considerable portions of our brain without interference with our personality. On the other hand, damage to our mental integrity seems to be always due to brain damage or some other physical disorder of the brain.' The assumption that the brain plays this integrating role rests on another assumption, that there is a *liaison* between mind and brain, so that *the brain is owned by the self*, not the other way around. Popper (1977, p. 120) summed his view up as follows: 'The activity of selves is, I suggest, the only genuine activity we know. The active, psycho-physical self is the active programmer to the brain ... it is the executant whose instrument is the brain. The mind is, as Plato said, the pilot'.

With his theory of the self, Popper contributed new ideas to our understanding of human consciousness by bringing a theory of language and his three-world view (*Dreiweltenlehre*) to bear on ideas of modern biology – notably those of Peter Medawar (1957) on individuality. Echoing his Kantian formulations in *Die beiden Grundprobleme*, Popper (1977, p. 129) concluded this adventure by saying: '[I]f we look at the long evolution of individuation ... then the fact that consciousness ... and unity are linked to the biological individual organism ... does not seem so surprising. For it is in the individual organism that ... the genome, the programme for life ... has to stand up to tests.'

6. THE ORIGIN OF ANIMAL CONSCIOUSNESS AS A CASE OF ACTIVE DARWINISM

Most of the summary about the self in the preceding section refers to Popper's contribution to *The Self and Its Brain*, a joint work with John Eccles. Some years later he was invited to the 17th Nobel Conference in Minnesota with the theme 'Mind in Nature', for which occasion he prepared a lecture entitled 'The Place of Mind in Nature' (Popper 1982). Here he continued to develop his ideas about the human mind as a product of evolution, but during which the emerging mind played an active part and, employing his version of 'active Darwinism', he argued forcefully that 'we are largely active makers of ourselves; and our minds are largely makers of our place in nature' (Popper 1982, p. 45). Although examples from animal evolution may also illustrate the functioning of active Darwinism, the emergence of consciousness was treated here mainly as a question of the origin of pre-human and human consciousness, without reference to the possibility of consciousness in animals. This was also the case in his Darwin Lecture, 1977, 'Natural Selection and the Emergence of Mind', where the thesis was that 'consciousness originates with the choices that are left open by open behavioural programs' (Popper 1978, p. 352). 'Open behavioural programs' built upon Ernst Mayr's (1976, pp. 707–09) distinction between 'closed and open behavioural programs', and on an exchange with W. H. Thorpe about 'the self-programmed animal' (this 'animal' appeared in print later in Thorpe 1978, pp. 24–25).

Fortunately, an important impetus to Popper's evolutionary thinking arrived with his meeting with the German chemist Günter Wächtershäuser, and no other collaboration in which he engaged later in life could match this teamwork regarding their quest into the origin of life and of consciousness. Wächtershäuser had come under the spell of Popper's introduction into evolutionary theory of the idea of objective problems and their solutions; as for Popper, he had been much impressed by Wächtershäuser's 1987 theory of the nutritional origins of sensory perception. He had at once seen the importance of Wächtershäuser's 1988 biochemical theory of 'surface metabolism', as a first step towards life, for his own evolutionary epistemology and ideas about the origins of consciousness and knowledge. Wächtershäuser (1997, p. 486) later characterized Popper's contribution this way: 'The importance of Popper's approach may be seen in the replacement of the closedness of the end-means relation by the openness of the problem-solution relation ... Popper's "situational logic" of problem-solution relations ... is a regulative idea in the sense of Kant.'

Years before, Popper had been much impressed by Jacques Monod's contributions to biology and his famous pronouncement in 1970 that life might have arisen only once and that the probability of that event, before it happened, would have been close to zero. However, it was only after his acquaintance with Wächtershäuser's theory of the origin of life that he realized that a fundamental shift had taken place in that domain of research in the interim – in Wächtershäuser's words (1997, p. 493): '[T]he origin of life is not a point in time. It is a point in space. From this vantage point, evolution is primarily a spatial affair, time coming in by virtue of the history of the conquering of space.'

So life could still be emerging in privileged places on Earth. The history of life's conquering of space on Earth has been achieved in two main ways: first, by the way of plants, then by the way of animals. As far as we know, the conquest by plants was the most 'generous', since it was from plants that the later animals obtained both oxygen and food. The conquest by animals, on the other hand, has led to even higher forms of life, with more and more complex structures and processes, culminating, so far, in the brain of *Homo sapiens*, with its extraordinary powers of understanding, imagination, creativity and control.

As with most theories on the origin of life, which assume common ancestors for both plants and animals, Wächtershäuser's (1988) theory also operates with a primordial basis for the biochemical unity of life. Plants and animals are, however, radically different in the way they occupy and live in space: (1) Plants are typically rooted in some substratum, whether in water or on land, and, as they are incapable of moving in space, according to Wächtershäuser's theory, they resemble the first surface-bound metabolic cycles growing on pyrite. (2) Single animal cells, on the other hand, migrate in water and over surfaces in an astonishing variety of ways, and only some of the mechanisms are known by means of which they swim, glide, creep, crawl, scuffle, slither and stream along together. So moving around seems to have been such a great advantage to organisms, including the later multicellular ones, that almost any means of locomotion has had survival value. What is more, displacement and active motion almost invariably presupposed some intention or aim on the part of the organism. This, at any rate, was a question that intrigued Herbert Spencer Jennings,[7] the first biologist to describe the behaviour of lower organisms, and who stimulated the young Popper to see protozoa as being endowed with anticipations and preferences.

In paragraphs 1–14 of an unpublished paper written in 1991, entitled 'The Mind-Brain Problem: A Conjectured Solution',[8] Popper reinterpreted Descartes's two 'substances' to mean, respectively, 'the world of extended physical bodies' and 'the world of unextended intensive forces' – forces understood as something that may accelerate or decelerate matter – thereby getting rid of the 'substances' and providing a solution to Descartes' body-mind problem.

The crux of 'the matter of mind' seems to be the following: if the mind is considered as a force field generated by 'organic forces' within the body,[9] upon which it may act, then body *and* mind form 'a highly complex extended process located in the same bag', and since both body and mind may be located in space and time, Descartes's problem, which presupposes non-locality for mind, is solved.

After the Cartesian body-mind problem has been solved, what remains is a new mind-brain problem – the problem of 'mind-brain interaction': '[T]he model of forces [is] something not immediately attached to the body but which can … operate on the brain [just as] a pianist operates on the piano … or an automatic pilot works on the plane…. And clearly our brains are automatic pilots, and they are partly programmed.'[10]

It was in this context, then, that Popper (1991, Sections 18 and 20) advanced his theory of *the origin of consciousness*, which, he conjectured, evolved when the 'organic forces' obtained autonomy and independence from the physical structures with which they interacted. After employing his argument about *genetic dualism*,[11] namely 'in evolution, an organism (or a clan of organisms) will adopt an accidental mutation only (or mainly) if it satisfies some of its needs, or potential needs', Popper (1991, p. 5) went on to say: 'Our ignorance concerning the evolution of consciousness is immense. But we may assume that consciousness satisfied a need. Its functional beginnings are likely to lie not so far from the beginning of life.'

With this interesting thesis Popper (1991, p. 6) had, so to speak, prepared the ground for his conjecture about the origin of consciousness: '[T]he division between modern locally fixed plants and self-moving … animals corresponds, roughly, to … a dwindling degree of consciousness on the one hand, and to the presence of consciousness on the other hand.' Plants in water and on land are here considered to be the descendants of Wächtershaüser's (1988, pp. 452–55) immobile 'surface-metabolists', whereas today's self-moving animals can be traced back to the first 'moving metabolists' that ceased to be surface-bound and, as a consequence of that, came under a new selection pressure which called for new adaptations. Popper (1991, p. 6) then conjectured 'that consciousness is needed to warn and guide an organism that can move about freely'.

Summarizing his hypothesis about the mind-generating 'organic force-field',[12] Popper rephrased Shakespeare in *The Merchant of Venice* (I, I, 1): 'Somehow or other, I have acquired a mind. / "But how I caught it, found it, or came by it, / What stuff 'tis made of, whereof it is born, / I have to learn." ' And he ended by answering this challenge: 'I suggest that forces are the stuff of which minds are made.'

This suggestion may be understood within Popper's conception of evolution as a process in which living organisms take an active part. The idea that behaviour, '*the spearhead of evolution*', could be the 'missing link' in Darwin's theory had made Popper (1986a) propose an improved version of the theory of evolution which, as mentioned above, he named 'active Darwinism' and developed further in his 'Medawar Lecture' given later the same year at The Royal Society, London, but only published posthumously as Popper (2013).

A problem with Darwin's account of 'natural selection' had been that this concept was derived by *analogy* from the artificial selection that cattle-breeders had employed for centuries, and with which evidence he introduced the idea of selection in the first chapters of *On the Origin of Species*. Therefore, his concept of 'natural selection' could only be a *metaphor*, even a mixed one. This is why Popper (1986a, p. 18) wanted to replace this metaphorical concept of selection in Darwin's theory with a true *homological* concept of selection. In so doing, 'selection' in the revised theory became identical to 'the elimination which occurs when organisms do not manage to solve problems themselves and therefore perish, leaving no, or less, offspring'. In 'active Darwinism', then, the selection agent is not the surrounding nature itself. Selection is caused by a failure in the attempts of the participating organism: 'It is, of course, not presupposed that the organism had searched *consciously* for a solution to its problem ... [but] all activity consists of *problem solving*, thereby testing and calibrating hypotheses [about the ecological niche] by a method of trial and error-elimination' (Popper 1986a, p. 18A).

Now, regarding our case in hand, *the emergence of consciousness as a solution to the problem of movement*, this historical conjecture may never be tested as such. However, the logical analysis of the biochemical situations in which this evolution is supposed to have occurred might nevertheless lead to deductions akin to those made by Wächtershäuser on the phylogeny of the precursors of living organisms.

The spectacular evolution of the first mobile cell-like organisms, depicted in Wächtershäuser's (1988) biochemical models, is clearly a case of *active Darwinism*. As Popper (1986a, pp. 9–10) argued. '[T]he ecological niche is ... partly *discovered* or *chosen* by organisms ... every activity and even any expectation will change the niche [and] its selection pressure. This way the activity of the organism plays a decisive role in Darwinian evolution.'

From here it is easy to appreciate the immense revolution brought about by mobile organisms when they evolved new tools for navigation and awareness to meet the problems that originated with their own locomotion – an adaptation 'handed down' to later animal species as a legacy for a world of consciousness.

However, as Popper admitted, in paraphrasing Shakespeare, we still do not really know what 'stuff consciousness is made of', though we know more about how it could have originated and what it does.

7. ON POPPER'S CENTRAL TEST-FUNCTION OF PERCEPTION

On different occasions after the publication of *The Open Society* (1945) Popper confronted the traditional view of knowledge, caricaturing it as a 'bucket-theory', with his own so-called 'searchlight-theory' of knowledge with the aim of highlighting the different statuses that these theoretical positions attributed to observation (Popper 1972, Appendix).

To the question, 'What comes first, the hypothesis (*H*) or the observation (*O*)?' (a) 'the bucket theory' replies that observation (*O*) always precedes any hypothesis (*H*); sense-data are thought to accumulate passively in the mind, as in a bucket, and, when the time is right, hypotheses may then spring from the stored observations by generalization, or association, or classification. (b) 'The searchlight theory' replies, logically, that one or more hypotheses (*H*) always precede observation (*O*); in everyday life and in science we always start out from a 'horizon of expectations' (the sum total of our expectations, whether subconscious or stated in language),[13] since observations are always secondary to hypotheses, even though observation may play an important role in subsequent tests on any hypothesis we may entertain. As shown below, he also suggested that this is at work within the organism since its perceptual system is constantly testing hypotheses and memories about the external world.[14]

Thus the tabula-rasa theory of the mind, and old dicta such as, 'There is nothing in the mind that was not first in the senses',[15] nicely summarize 'the bucket theory', which was held in esteem by Locke, Berkeley and Hume, and, despite its logical untenability, continues to play an important role today in educational and psychological theories, 'information theory' and philosophy. On the other hand, Popper's 'searchlight theory', which is free of induction, has been accepted only by some researchers in theoretical physics and biology, but is practically unknown by researchers in the social and psychological sciences.

Popper later extended his views on observations to recurrent perceptions (Popper 1977, pp. 88–93), and in 1990 he sketched an experiment to show that *perception is our primary 'organ' for testing theories about the world* and, suggesting that it be carried out by the present author, he demonstrated briefly how he thought it should be done.

Having explained that the aim of the experiment was to compare an imagined face of a known person – that is, a visualization of such a person's face, on the one hand, with a photograph or portrait of the same person, on the other – Popper went on to concentrate on visualizing his father's face for a while. Thus prepared, he went into the next room to fetch his father's framed portrait, which he then began to inspect, writing down any feature or detail in the portrait that appeared 'new' to him, or at least 'missing' from the face he had visualized a moment before. In this case it turned out that aspects pertaining to the eyes, beard, hair, certain proportions of the face and details about the clothes appeared to Popper as 'not having been present in the imagined face'. Commenting on the experiment he said (Popper 1990b): 'In this way it should be possible to show that a function of perception is to *test and correct* our hypotheses or theories about faces of people familiar to us, that is, to assist in the identification [of the person], their emotional state, and other changes that can be seen in their face.'

The experiment Popper had designed in 1990 was carried out during the following years and the results presented at a conference in Salzburg (Petersen 1997). Two groups of fifteen students each took part in two series of experiments in which different sets of material for comparison were used. In the first series, four pictures of well-known historical characters (Mona Lisa, Albert Einstein, Marilyn Monroe and Charlie Chaplin) were used for the comparison. In the second series, each subject brought along photographs of four characters he or she knew personally (two family members and two friends) to be compared with the imagined faces each subject would produce, one by one, of the people that only he/she knew. The subjects were given a written instruction, like the one Popper had followed above, and asked to write down their experiences of the comparisons, answering the following two questions each time:

1. Is what you see now in the picture/photo of X *exactly the same* as your visualized face of X? – Is it *almost the same*? Or, is the picture/photo *quite different* from what you had visualized?
2. If the picture/photo of X contains something that was not included in the face you visualized a moment ago, then describe these missing features of which you are now reminded by the picture/photo.

The general results of the first series showed that out of a total of sixty reports, fifty-three (88.3%) indicated that pictured/photographed and visualized faces were *different in respect of more than three features or details*, and seven (11.7%) noted that photographed and visualized faces were *different in respect of fewer than three features.* None of the fifteen subjects (ten females, five males) had found the pictured/photographed faces to be '*the same*' as the imagined ones.

The general results of the second series with another fifteen subjects (eleven females; four males) showed that out of a total of thirty reports, regarding 'family members', fourteen (46.7%) described *more* *than three features* in the photos as missing from the visualized faces; in thirteen reports (43.3%) *fewer than three features* were reported 'not included in the imagined faces', and three reports (10%) declared that the photographed and the imagined faces *had been alike*. The outcome of the experiments with 'faces of friends' was similar: out of a total of thirteen reports, nineteen (63.3%) described *more than three features* in the photos as 'missing from the imagined faces'; nine reports (30%) had *fewer than three features* in the photographs 'missing from the imagined faces'; and two reports (6.7%) claimed that the photographed and the imagined faces *had been alike*.

The accumulated results showed that out of 120 reports, 86 (71.7%) described that *more than three features or details* in the photographs of the familiar people had been absent from the subjects' imagined faces of the same people – which may be taken to reflect the normal workings of a corrective function of perception on visio-cognitive hypotheses and sheer memories about faces of people we know. A more comfortable percentage, based on a weaker criterion, may be obtained if one adds the reports in which *fewer than three (but more than zero) features* were reported to have been absent from the imagined faces, that is, 115 out of 120 reports (92.4%).

The results lend support to Popper's assumption that the perceptual system functions as our first ready means for testing and correcting hypotheses and theories about the world, and the title of the paper (Petersen 1997) ought to have been 'Exploring the Assumption that Perception Implies Current Making and Matching of Visio-Cognitive Hypotheses About the World'.[16] The processes of 'making' and 'matching' were first pointed out by Ernst Gombrich (1968, pp. 99–100), and later commented on by Popper (1977, p. 429): 'The senses have two roles: first, they challenge us to *make* our hypotheses; second, they help us to *match* our hypotheses – by assisting in the process of refutation, or selection.' The second part, the 'matching', often results, as Popper (1990b) put it, in the discovery of 'something *unfamiliar* in the familiar, and I suggest that this phenomenon is an effect of the recurrent reality testing by our sense perception of our hypotheses and theories about the surrounding world, and not just errors of memory'. The fact that perception is under the influence of the whole state of the organism is also an impetus for the 'matching-function' to get going, since any change in the organism's point of view will facilitate new confrontations between its expectations and the perceptions resulting from the changes of perspective.

Popper insistently maintained that animals and humans alike could never enter in *direct* contact with the world and that their interaction with it remained *indirect* due to species-typical and individual-specific expectations, needs and drives, memory, knowledge and so on. The resulting different versions of the world would then invariably be distorted in many ways, consisting of 'phenomena that deceive' them in unpredictable ways, as the Greeks say ('*ta fenomena apatoum*'). Therefore, 'hard' sense data cannot exist as claimed by phenomenalists from Berkeley to Carnap, and our representations of the world remain biased and only amendable by mutual efforts and self-criticism.

A last qualification concerns so-called 'memory images' and 'mental images'. Popper (1986b, p. 3) argued that the 'photographic memory' in eidetic people does *not* prove that the rest of humanity operates with 'visual memory' and 'mental images'. 'Mental images', which association psychology claimed were carried around like 'stills' in our heads and by a process of association turned into 'films' about our world, are the results of processed expectations, whose precision and clarity vary with a great number of individual and situational factors. Eidetic imagery, which only very few children preserve when they grow up, may be due to an incomplete differentiation within the brain – much like sound-colour synopsia, form-colour synesthesia and the like – but in many situations of great service to its owner as composers and painters can testify.

8. THE PRINCIPLE OF TRANSFERENCE: 'WHAT IS TRUE IN LOGIC IS TRUE IN PSYCHOLOGY'

Around 1929, after reading Kant and speculating a great deal about the rise of European polyphonic music, Popper abandoned psychology to devote himself to the logic of discovery. He did so after he had reached the Kantian conclusion that knowledge cannot be a mere copy or impression of reality, and that knowledge is genetically or psychologically a priori to observational evidence, though not necessarily a priori valid, as he later recalled: 'Our theories ... may be merely ill-reasoned guesses ... Out of these, we create ... our own nets in which we try to catch the real world ... what I originally regarded as the psychology of discovery had a basis in logic: there was no other way into the unknown, for logical reasons' (Popper 1974, p. 46).

Some forty years later, in chapter 1 of *Objective Knowledge – An Evolutionary Approach* (1972), he returned to the problem of induction, in particular Hume's psychological part of the problem. To solve it, he advanced 'a principle of transference' – 'what is true in logic is true in psychology' – to make logical principles bear on psychological considerations. In this way, truth content of statements could be carried over to such fields as psychology, sociology, scientific methodology, history of science and others. (For a possible ontogeny of this principle in Popper's thought, see Hacohen's discussion of 'logicism', 'psychologism' and Gomperz's 'semasiology' in this volume.)

Many have since questioned what kind of principle this is. Hints for an answer may be found among the following elements. It is possible that an idea similar to the principle has come down in distorted versions of Kant's teaching via two of Popper's teachers: Carl Stumpf (1892, pp. 481–82), who, unlike Kant, and apparently under the influence of the growing positivism, attached a refereeing role to psychology when he stated: 'Something cannot be epistemologically true and [at the same time] psychologically false'; and Karl Bühler (1922, p. 212), who thought that he was safeguarding himself with the statement: 'I take sides with Stumpf: what is true in logic cannot be false in psychology, and *vice versa*.' Here 'distorted version' means that none of these formulations constitutes a principle of transference, but rather emphasizes the central role of psychology for epistemology, and that it is more positivist than Kantian. (The references to Stumpf and Bühler I owe to Troels Eggers Hansen.)

Judging from the context in which the principle was introduced, and the aforementioned scientific fields to which Popper wanted to bring some logical order, it is necessary to understand the principle of transference as a methodological device – a way of sifting and evaluating scientific results according to the methods employed. If so, the inductive methods would be the first to be discarded along with their results; and also subjectivist and literary approaches held in esteem by psychoanalysts, certain psychologists and sociologists; and some parts of history would also have their results rejected on methodological grounds, and the history of the sciences would have to be rewritten, largely leaving out what could not withstand logical and methodological scrutiny.

'The principle of transference' does not imply, as R. L. Gregory (1981; 1988, p. 428) would have had us believe, that the human mind always operates, or ought to operate, logically, quoting Wittgenstein (1922; 1962), who claimed at one point, 3.0.3: 'We cannot think anything unlogical, for otherwise we should have to think unlogically.' This interpretation is ruled out in many places in Popper's writings, for example in the quotation in the first paragraph of this section, in which our theories are said to be the result of 'ill-reasoned guesses' – our guesses being not always logical, but often erroneous. Human cognition neither operates in an enclosure, logical or otherwise, nor does it produce closed, formalized systems.

As with 'the rationality principle' (Popper 1967), which is only considered to be a good approximation to the way in which people behave and choose to act, the 'principle of transference' may succeed only in transferring a modicum of logical order to the aforementioned disciplines,[17] not at all enough to ensure that present and future scientists will take care and pride in proposing only testable theories about the phenomena under investigation.

9. A SHORT SUMMING-UP OF POPPER'S CONTRIBUTIONS TO PSYCHOLOGY

When Popper argued against inductivism in *Die beiden Grundprobleme*, he did so by showing that a deductivist psychology was possible. Having followed his lifelong interest in psychology, one is tempted to conclude that his contributions to that discipline have the common character of being cases of such a deductivist psychology. For nowhere do inductive procedures enter his explanations and hypotheses about the phenomena. This approach is also Kantian, as can be seen from Popper's account of the actions – not just reactions – of living organisms, their perception, learning, communication and even evolution.

The exposition summarized here has presented Popper's contributions to psychology in an almost chronological order under headings to indicate some of the main points. The following should be added to these: Popper's original hypothesis of *the mind as an electromagnetic force-field* and the bold thesis of *mind-brain interaction*, delineated and alluded to in Section 6; his theory of *ritualization of signals* in animal and human communication, commented on in notes 3, 5 and the examples to Table 3.1, and *the 'critical' language function* included in Table 3.1; his famous *scheme* for *problem solving*, discussed in Section 3 and shown in Scheme 2, and the idea of *aim-structure* mentioned in Sections 3 and 5; his *'searchlight theory'* of perception and mind, employed in Section 7, and the thesis of an *objective mind*, touched upon in Section 5; the idea of *learning by action and selection*, mentioned in Section 4, and *the principle of rationality* in human conduct, referred to in Section 9. This enumeration is far from exhaustive since it must be combined with Popper's contributions to biology of which he considered psychology to be an integral part.

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NOTES

1 Translated here from Popper 1928; 2006, p. 239; in the English edition of Schlick's *General Theory of Knowledge*, p. 383, 'assignment' is unfortunately translated by 'correlating'. However, being a term borrowed from set theory, Dedekind and Schlick used 'assignment' to refer to the attachment of cognitive processes to numbers, words, concepts and other representations – and, for many animal species, we may add the emotional and cognitive attachment to species members (their smells and sounds, colours and shapes) and other living beings and objects – *assignments* that are something else and more than just correlations.

2 Later developments in neurobiological theory, such as Karl Pribram's model of 'test-operate-test-exit' (TOTE), were aimed at explaining precisely this type of central control of the receptive mechanism: '[P]erception is in essence a "motor" phenomenon ... perception *per se* is more a reflection of the response patterns instigated in the brain by an input than it is a resultant of the input patterns' (Pribram 1971, p. 91).

3 Exactly the kind of theory of ritualization that bird-watching ethologists like Julian Huxley (1914 and 1966) and Konrad Lorenz (1941) had been looking for to explain the phylogeny and ontogeny of communicative displays in birds. However, they did not realize that such a theory could be found at close range, and their followers do not seem to have readjusted their binoculars either.

4 In a letter to Professor Adriaan de Groot Popper (1990a) wrote: 'It must have been in 1929 ... that I realized that all thinking processes are problem-oriented, and that this had been discovered by Otto Selz years before. And that all attempts to solve a problem are trial and error-elimination processes, as was also seen by Selz. Thus I felt that my own problem in psychology had been solved, essentially, by Selz. But what he had not seen was that, for *logical* reasons, we are bound to proceed in this way, *especially also if our problem is to discover something unknown....* Logic, it turns out, is deeper than psychology: the logic of discovery can *explain* the psychology of thought, including the discoveries of Selz. No psychologist to whom I told this story has ever clearly admitted that it is of crucial importance for psychology.' (For more details of Popper's inspiration from Otto Selz, see ter Hark 2004 and Hacohen's section on 'Cognitive psychology, 1925–1928' in Chapter 2 of this volume.)

5 According to this interpretation, the functions of symbolic language are supposed to have evolved somehow into prolongation of other communication systems in the animal kingdom, which in Bühler's original version

(1934) amounted only to the third *descriptive* function, to which Popper (1963) added a fourth *argumentative* function. As distinct from animal communication, which functions with a great variety of ritualized *signals* for here-and-now interchanges, human descriptive language may convey messages by means of *symbols*, which have become abstracted from any concrete situation and thus suited for making *representations* of objects (*e.g.* 'icons'), localities (*e.g.* maps) and events (*e.g.* plays) and for *simulating* them in future contexts (*i.e.* planning).

6 For more details on the distinctions between biological individuality, personality and self, see Petersen 1985, pp. 236–48, and on their origins and interactions, see Petersen 1994.

7 In an article in the *American Journal of Physiology*, 1899, Jennings opted for the view that 'the explorative behaviour' of *Paramecium* is not conscious at all: 'I do not see that we are compelled to assume consciousness or intelligence in any form to explain the movements of this creature … Simple irritability, or the property of responding to a stimulus by a fixed set of movements, seems sufficient to account for its activities' (Jennings 1899, pp. 339–40). In reply to this Popper would have said that it is not excluded that reactions of infusoriae are directed by preferences and expectations. Note, however, that Wächtershäuser (1997, p. 493) hypothesized that consciousness arose only when the nervous system entered the scene: 'With the emergence of the central nervous systems comes the next great leap: the emergence of consciousness, which is fundamentally problem awareness.' For other comments on possible origins of consciousness, see Petersen (2000) and Wolpert (2011).

8 The typescript, which consists of 8 pages of short paragraphs, numbered 1–25, and a 180-word summary, is wrongly dated '21 August 1990'; the corresponding holograph (Catalogue no. 508/3) is dated '20.8.91 (8 p.m.) – 21.8.91 (1 a.m.)' (cf. Popper 2012). A discussion on 19 October, the following year, related to the topic of this typescript, with Professor B. I. B. Lindahl, Department of Geriatric Medicine, Karolinska Institute, Huddinge, and Professor P. Århem, The Nobel Institute for Neurophysiology, Karolinska Institute, Stockholm, was published as Popper et al. 1993.

9 Here '*organic forces*' refers to the *non-extended* but *localizable* chemical, electrical, magnetic, mechanical and other supposedly mind-generating forces which have evolved in parallel with, and *inside the bodies of animals* – abbreviating Popper (1991, Section 13): 'those highly complex *extended processes* that constitute localizable individual bags' or bodies (including brains in multicellular organisms) – to interact in this same bag with its bodily processes in regulating and balancing the entire organism; 'mind-generating' is to be understood as *autonomous*, that is, consciousness is supposed to have emerged 'when forces, which are related to biochemical substances, obtain a certain autonomy and independence from these sheer substantial processes' (Popper et al. 1993, p. 169).

10 A special side to this new mind-brain problem is the occurrence of 'conscious' and 'unconscious' controls: 'unconscious' implies that, say, a brain-muscle control executes an action 'without the *attention* of the mind [as] the mind sinks into physiology … a process where mind and brain are no longer distinguishable … Perhaps the unconscious is purely

physiological … the unconscious [being] … unconscious mind' (Popper et al. 1993, p. 172). (The quotation in the text to this note is from p. 170 of the same publication.)

11 A short formulation of Popper's '*genetic dualism* or *pluralism*' may be this: '[O]nce we have obtained a more flexible *central* propensity structure, otherwise lethal mutations of the *executive* organs … may become extremely favourable, even if they were previously unfavourable … The mutations of the central structure will [then] be *leading*' (Popper 1972, pp. 278–79; emphasis added). That is to say, only those mutations of the executive organs that fit into the general tendencies previously established by the changes of the central structure will be preserved.

12 In a second discussion with the Nobel professors, on 18 July 1994, Popper listed the following preconditions for his 'new theory of mind' – 'mind' understood as being generated by fields of electrochemical forces: '(a) the brain is a detector-amplifier providing its own energy (see Eccles 1953), making the law of energy conservation irrelevant for the mind-brain issue; (b) there may exist mental energy convertible into electrochemical forms, and (c) there may exist non-energetic (energy-less pilot waves) influences upon energetic (energy-carrying waves) processes' (Popper, Lindahl and Århem 1994, p. 5). Thus, if mind consists of semi-autonomous fields of electrochemical forces, then it may, *qua* force-fields, operate on propensity fields (Popper et al. 1993, p. 12) or act on bodies, thereby making mind-brain interaction possible. Popper did not live to complete the editing of this second discussion, nor did he see Lindahl and Århem's comments (1994).

13 In this and other places, Popper used the expression 'horizon of expectations' to explain how a frame of references 'confers meaning or significance to our experiences, actions, and observations' (Popper 1963, p. 47), and how small children and animals will be equipped with such frames on lower levels of consciousness than scientists working with their expectations formulated verbally into hypotheses and theories. This points to a biological conception of 'horizon of expectations', since any meaning conferred to observations and experiences from the frame can be traced back to inborn expectations, which are genetically prior to all observational experience. According to *Historisches Wörterbuch der Philosophie*, ed. J. Ritter et al. (1972, Bd. 3, p. 1202), Popper (1949, p. 48) adopted the expression from Karl Mannheim who, in *Mensch und Gesellschaft*, had described how a universally human '*Erwartungshorizont*' may arise from a 'certain constancy in people's experience with social life' (Mannheim 1935, p. 132). Popper's reaction to such an emphasis on consensus is also interesting for the present context of his experiment: 'Observations … can, under certain circumstances, destroy even the frame itself, if they clash with certain of the expectations … [and] may force us to reconstruct … our whole horizon of expectations … [by] correcting our expectations' (Popper 1972, p. 345).

14 As Popper's 'searchlight theory' indicates, the senses are actively engaged in the search for information in the environment and in questioning that information. A clear illustration of this is given by information theorist D. M. MacKay (1972, p. 371–72), who seems to have had similar ideas to Popper about perceptual testing and correction: 'To command muscles to

move a sensory surface (whether tactile or visual) relative to the environment is to give the resulting sensory signals the logical status of answers to questions specified by motor commands.' In other words, when exploring an object, eye movements reveal that the visual system is questioning whether or not the percepts match with memories or concepts about that object. To assist the searching eye or hand in this procedure, a priming of the senses by needs and drives makes them constantly come up with new approaches to the external world, and in questioning 'the internal representation of the environment, [the sensory feedback] must be evaluated as answers to those questions.' And here MacKay could have included involuntary eye movements as well.

15 This empiricist doctrine, famous in its Latin version (e.g. Aquinas), '*nihil est in intellectu quod non sit prius in sensu*', is, however, much older, Popper (1998, p. 89 and 142) argued, since Parmenides exposed it to ridicule (B16), while Protagoras revived it with high-flown declarations such as '*elege te meden einai psyken para tas aistheseis*', that is, 'the soul is nothing if one quells the senses', or simply: 'the soul is nothing apart from the senses' (Diogenes Laertius 1925, vol. II, IX, 51, p. 465.)

16 A practical problem was to have a title like this accepted by the organizers of a conference on facial expression, measurement and meaning. A third experiment, in which similar comparisons were carried out between imagined familiar localities (official buildings, houses, gardens, streets, landscapes) and photographs of the same localities, has since been carried out with a third group of students as participants. The general results resemble those of the results discussed above, but the findings require closer analysis.

17 Had Popper's principle been applied systematically by researchers in these disciplines, we would hardly have needed to place trust, albeit moderate, in institutions like *Clearing House*, to scrutinize research reports to find out which of them have contributed *real knowledge* to the fields in question. But it is precisely the prevailing attitude among certain scientists that '*anything goes*' which has put these disciplines, and even parts of biology and medicine, in a situation nearing methodological anarchy.

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4 Popper's Philosophy of Science: Looking Ahead

Is Popper's philosophy alive or dead? If we make a judgement based on recent discussion in academic philosophy of science, he definitely seems to be fading. Popper is still seen as an important historical figure, a key part of the grand drama of twentieth century thinking about science. He is associated with an outlook, a mindset and a general picture of scientific work. His name has bequeathed us an adjective, 'Popperian', which is well established. But the adjective is used for very general ideas that, according to most current philosophers, Popper did not develop convincingly. His detailed account is often seen as attractive on first impression, but full of holes that become bigger rather than smaller as discussion continues. The picture and the name remain, which is more than most philosophers can hope for. But the name attaches more and more to a set of instincts and intuitions, less and less to views that are seeing ongoing philosophical development.

Inside science itself, Popper's standing is quite different. He continues to be just about the only philosopher who can seize the imagination and command the loyalty of successful professional scientists. And he is popular within science not only for purposes of general commentary and public relations. Popper's philosophy is a *resource* drawn on by scientists in their internal debates about scientific matters. This has been especially marked in some parts of biology (Hull 1999).

From the point of view of philosophers, this affection on the part of scientists may have an unflattering explanation. Popper offers a rather heroic view of the scientific character, featuring an appealing combination of creativity and hard-headedness. It is no surprise that scientists like to be described this way. It is no surprise that they prefer this picture to the one often (though inaccurately) associated with Hempel and Carnap – the scientist as a sort of logic-driven pattern recognition machine. The same applies to the picture associated with Kuhn, who presents the normal scientist as a narrow-minded, indoctrinated member of a peculiar collective enterprise, an enterprise devoted to solving tiny puzzles most of the time but prone to occasional fits of crisis and chaos. Popper's picture is much more appealing.

Scientific admirers of Popper might respond with a similarly unflattering explanation of why Popper has faded from professional philosophy of science. Professional philosophy of science is, for many scientists, an irrelevant exercise that is prone to long excursions into fantasy land, obsessed with pointless semantic disputes and very far from the living enterprise of science. From this point of view it would be seen as no surprise that Popper, a philosopher who had the blood of science running through his veins, came to be unwelcome in the drab conference rooms of the Philosophy of Science Association.

So unkind explanations for Popper's mixed reputation can be thrown in both directions. In this chapter, my aim is to isolate and explore some parts of Popper's philosophy of science that seem to me to have continuing relevance. One of them, in fact, I see as a 'sleeper' idea – something that Popper may have been well ahead of his time on, and an idea that will eventually be developed in much more detail. The four ideas I discuss are all entirely philosophical, rather than ideas at the technical or historical fringe of Popper's philosophy. I discuss them in order of increasing complexity.

I do not suggest that the four ideas I discuss are the only ideas of Popper's that have continuing philosophical importance. On the more technical side, Popper's views on probability continue to attract interest. Both the general idea of probabilities as propensities and some of the mathematical details are the subject of ongoing discussion (Hájek 2003a, 2003b). I do not discuss that work here. My focus is on some of the core philosophical – especially epistemological – themes in Popper. I also do not discuss ideas for which Popper deserves some historical credit, but which have now become fully absorbed into the tradition. An example in this category would be Popper's anti-foundationalism, and his vivid metaphor of how we can successfully build scientific knowledge on a swamp, by driving our piles in just as far as they need to go at the present time (Popper 1959: 111). Popper was far from the only philosopher to develop such views in the nineteenth and early twentieth centuries (Peirce, Dewey and Neurath are examples). And the details of Popper's treatment had problems; his was a rather shaky anti-foundationalism. But the contribution was important.

A final preliminary point should be noted. I assume in this essay a particular interpretation of some of Popper's ideas about testing and evidence. Roughly speaking, this is a simple interpretation that emphasizes the sceptical side of Popper's work, especially his scepticism about induction and other mainstream ideas about the support of hypotheses by observations. One problem with Popper is his tendency to try to have things both ways with respect to these issues. Confirmation as a relation of support between observation and theory is rejected with great fanfare, but corroboration is then ushered in, and it can be hard to tell the difference.[1] This uncertainty has been rhetorically useful for Popper. Many of his scientific backers do not realize how sceptical some of Popper's ideas really were. In this chapter I assume a simple, strong interpretation of Popper's critique of mainstream ideas about evidence and testing. All the results of observations can do is refute hypotheses, never support them. Confirmation – a relation of support between evidence and theory that can rationally affect our level of confidence in the theory – is an illusion. The most we can say about a theory that has survived our strenuous attempts to refute it is just that: we can say the theory has survived our attempts to refute it so far. The Popper of this essay is the more sceptical Popper, a Popper who is epistemologically something of a radical, despite his rather proper demeanour. Perhaps this is a simplistic interpretation, but if so, my aim is to work out what we can learn from this figure, the sceptical Popper.

1. ELIMINATIVE INFERENCE

The first idea I discuss is simple and can be handled quickly. This is the very basic Popperian idea that the rational mode of theory assessment proceeds by ruling *out* alternatives. For Popper, this ruling out of options is all we can do. There is no direct epistemic support that a theoretical hypothesis can gain via observation, because confirmation is a myth. And a theory cannot gain indirect support through the ruling out of other options, because the number of options is (in all scientifically significant cases) infinite.

Like most philosophers, I reject many of Popper's claims about the impossibility of justifying theories. But this is compatible with a recognition of the great importance of the Popperian form of 'eliminative inference' in science. And the crucial point here is that the role of this kind of inference was a neglected topic (among those outside Popper's school) in much of the twentieth century, at least until recently (Earman 1992; Kitcher 1993). The great obsession of mainstream philosophical theories of evidence in the twentieth century, exemplified by Carnap and Hempel, was the direct positive support of generalizations by their instances. The formal problems that arose in the attempt to make sense of such support are notorious; some will be discussed later in this chapter. What is important here, however, is the fact that during the development of the largely unsuccessful theories of 'instance confirmation,' little attention was paid to what seems in retrospect to be an obvious and central feature of the epistemology of science. This is the practice of seeking to support one theoretical hypothesis by ruling out others.

Perhaps it was often thought that only the ideal case would be an epistemologically significant one; the ideal case is where we are able to *decisively* rule out *all* options except one. Non-ideal cases depart from this one in two ways. First, there may be a less decisive ruling out of alternatives; maybe we can only hope to show that all alternatives except one are unlikely. Second, there are cases where we might be able to rule out many or most, but not all, of the alternatives to a hypothesis.

Many philosophers have been discouraged by the thought that there will always be an infinite number of alternatives to any theoretical hypothesis. In scientific practice, the problem is made tractable by use of a notion of a *relevant* alternative (Goldman 1986; Kitcher 1993; Forber 2006). Only some options are seen as worth taking the time to exclude. This move alone does not solve the epistemological problem. What guides these judgements about relevance, and what rational basis could they have? It can be argued that scientists constantly tend towards overconfidence on this point (Stanford 2006). Scientists often think that they have ruled out (or rendered very unlikely) all the feasible alternatives to some theory. In hindsight we can see that in many cases they did not do so, as we now *believe* a theory they did not even *consider*. A focus on eliminative inference has the potential to illuminate both the successes and the failures found in scientific reasoning.

So an emphasis on eliminative inference and the introduction of a notion of relevant alternative does not solve the core epistemological problems here. It does, however, pose these epistemological problems in a better form than the one often imposed on them in mainstream twentieth-century discussion. It seems clear that much of the overt, day-to-day practice of theory assessment in science proceeds by the explicit presentation of alternatives and the attempt to rule out as many as possible, by either deductive or probabilistic means. Some scientists have even sought to distinguish between fields that apply this procedure as an explicit *strategy*, from those that tend not to (Platt 1964). So whereas the traditional empiricist idea of the confirmation of generalizations by observing their instances is, at best, an ultra-idealized philosophical model of the epistemology of science, eliminative inference is a plain and central feature of scientific practice. The epistemological problems around this form of inference are far from solved, but a focus on this phenomenon will surely be a large part of any good future account of evidence and theory choice in science.

2. SCEPTICAL REALISM

The second theme I discuss relates to the debates about 'scientific realism'. Scientific realism is hard to characterize exactly. Part of the view seems to fall within metaphysics. Another part apparently has to do with the kind of 'contact' with the world that scientific investigation makes possible. I think that in years to come it will appear that much twentieth-century discussion of 'scientific realism' was focused on a rather awkward conglomerate doctrine. In particular, there has often been the forging of a link between scientific realism and a kind of generalized epistemological optimism. The realist side of the debate is often associated with an overall confidence in current scientific theories as descriptions of what the world is like, or perhaps confidence in the trajectory or lineage within which current theories are embedded.

There is nothing wrong with asking about the levels of confidence in scientific theories that might be rational. The problem, as I see it, is the entangling of this family of questions with more basic questions about scientific realism. Roughly speaking, there are two questions (or kinds of questions) that should be considered in sequence. First, we can ask whether it makes sense to say that there is a world existing independently of thought and theory that our scientific theorizing is *directed on*. Second, we can ask how confident we should be that our particular scientific theories are *succeeding* in representing how the world is.

These formulations of the two sets of questions are rough, and the treatment of them as questions to be addressed 'in sequence' is obviously an idealization. But the separation is important. The first question is one about whether it is even *coherent* for us to take our scientific theories as directed on a world that exists independently of thought and theory. That question should be distinguished, as much as we can, from questions about how confident we can be that we are *succeeding* in the goal of representing what this world is like. If the answer to the first question is 'no', then the second question must be dropped or greatly transformed. But if the answer to the first question is 'yes', there are many options regarding the second. Some neglected options here include a sceptical position, and also a sort of 'particularism' that I discuss later in the chapter.

First, though, a connection to Popper: it is part of the overall structure of Popper's philosophy that there is a good separation between these two kinds of questions. For Popper, there is no philosophical impediment to our regarding our theories as directed upon a mind-independent world. It is *possible* that we could devise a theory that is wholly accurate within its domain, where this includes the accurate description of unobservable things. Despite this being possible, the nature of theorizing, evidence and testing precludes us from ever having any confidence that we are *succeeding* in this task. Popper's view is, then, a moderate form of 'sceptical realism' about science, and it is set up in a way that makes the possibility of sceptical realisms very clear.

A surprising amount of discussion of scientific realism in the twentieth century paid little attention to sceptical realism as an option. Instead, 'the question of scientific realism' was very often set up in a way that combined answers to both the questions above, and associated the label 'scientific realism' with an optimistic, non-sceptical attitude towards current scientific theories, towards the scientific tradition as a whole, or both. Examples of this tendency include McMullin (1984), Boyd (1983) and Devitt (1997).

This feature of the debate has been due to influential arguments from both sides. Those in the 'realist' camp have often been attracted to the idea that general arguments can be given from the predictive success of scientific theories to their likely truth. These success-based arguments can be given about particular scientific theories or about the enterprise of science as a whole. On the other side, Larry Laudan (1981) argued that the overall pattern of change in the history of science gives us reason to expect that our present theories are *not* true.[2] So it has been common for philosophers to look for arguments, on one side or another, that would tell us what our overall level of confidence in science ought to be. Optimistic answers to this question have often been attached to the label 'scientific realism'.

As noted above, there are two possibilities that become marginalized by this way of setting things up. One is sceptical realism, which Popper exemplifies. The other possibility is that the answers to the epistemological questions that people associate with realism are complicated and field-specific. That is, they are not summarizable or sloganizable in the manner of the standard debate. According to this 'particularist' option, different scientific fields apply different representational strategies, attended by different kinds of risk and difficulty. Some fields (including parts of physics) are dominated by highly abstract mathematical formalisms, where it is unclear what sorts of entities and facts are being posited at all. In other fields (evolutionary theory, ecology, economics), we know what sorts of entities to believe in, but might wonder about how to treat the highly idealized models that have become the currency of much theoretical discussion. And yet other fields seem able to engage in straightforward mechanistic description of how systems are composed (neuroscience, molecular biology), of a kind that make many traditional philosophical anxieties seem quite misplaced. Different kinds of epistemological optimism will be appropriate in these different areas, and it seems unwise to attempt a blanket summary asserting that 'most posits of well-established theories are real', 'mature and predictively successful theories are approximately true', or anything like that.[3]

So it is not Popperian sceptical realism that I see as likely to inherit the field in the scientific realism debate, but a 'particularist' position that sceptical realism helps to make visible.

3. TESTS, RISKS AND PSEUDO-CONTACT

My third idea is a familiar and central one in Popper, but I discuss it with the aid of a specific set of contrasts and connections. Popper claimed that a good scientific theory should take risks, should 'stick its neck out.' This is central to the whole idea of falsificationism and Popper's attempt to give a 'demarcation' of science from non-science. For Popper, a hypothesis that cannot be falsified by any possible observation might be perfectly meaningful and even quite important, but it is not science. A genuinely scientific hypothesis must take risks, in the form of exposure to potential falsification via observational evidence.

The details of Popper's account of this risk taking have well-known problems. Here I focus on a simple form of this idea, on the underlying intuition rather than the details. The idea is not just intrinsically useful, but it can be used to cast light on key problems with mainstream empiricist epistemology.

The mainstream empiricist tradition claims that experience is the only genuine source of knowledge. Some empiricists add, perhaps metaphorically, that empiricism is the idea that we acquire knowledge of the world by being brought into *contact* with it, via experience. Maybe 'contact' is a questionable term to use, but let us accept it for now. Experience can certainly be described in a low-key way as bringing us into contact with what is going on in the world.

Given this, it seems that the details of an empiricist epistemology will be largely concerned with what this 'contact' is and how it works. From this point of view, an important theme in Popper is the idea that there is something we might call *pseudo-contact* between theory and observation. There can be a genuine and intimate relationship between a theoretical idea and a piece of observational evidence, which has no epistemic value.

In Popper's philosophy this idea appears in various forms. It is central to Popper's 'demarcation' criterion, as noted above. It also appears in a stronger form in the idea that successful prediction of surprising facts does not confirm a theory, if 'confirmation' is seen as something that motivates an increased confidence that the theory is true. But one can accept the importance of the Popperian idea of pseudo-contact within a more moderate view. The key idea is that there are lots of ways for an observation to 'conform' with a theory without that relationship having a genuine evidential *bearing* on the theory.

From some standpoints, this is a very obvious fact. If we develop an epistemology as Popper did, by thinking specifically about science and testing, then it will probably seem obvious. We can appreciate a simple version of the point just by noting that there are lots of ways for data to be cooked up (deliberately or accidentally) in such a way that they *fit* a theory without *telling* us anything significant. More broadly, as Popper said, if you only look for confirmation then you will probably find it everywhere. So from some points of view, the importance of pseudo-contact seems obvious and straightforward. But apparently this message is not clear, obvious and easy to take on board when one approaches the topic within other frameworks.

The fact that this message is not always easy to take on board is seen in an important feature of the literature on confirmation. Here I have in mind the view, especially associated with Hempel and Carnap but taken seriously by many others, that all positive instances of a generalization provide some support for the generalization. Formally, if we are considering a generalization 'All Fs are G', then all instances of Fs that are also G provide some degree of support for the generalization. (Hempel called this 'Nicod's criterion'.) Many people think there are problems with the idea that all positive instances do confirm, but a very large number of philosophers seem to be strongly *attracted* to this view. I have often seen in action the desire to salvage as much of the idea as is humanly possible.

From the Popper-informed standpoint, however, there is no reason whatsoever to believe it. Merely being a positive instance is not nearly enough for there to be confirmation or support. The evidential relation between a hypothesis and an observed instance will depend on much more than that bare logical fact. Roughly speaking, support of this kind can be expected to depend on whether the observation was the product of something like a *genuine test*, a procedure that had the possibility to tell either for or against the hypothesis.

The place where this issue becomes most vivid is the huge literature around the 'ravens problem' (Hempel 1965). Famously, the ravens problem arises from three innocent-looking assumptions. One is the idea that any observation of an F which is also G supports the generalization 'all Fs are G'. The second is the idea that any evidence that confirms a hypothesis H also confirms any hypothesis H^* that is logically equivalent to H. The third is the idea that 'All Fs are G' is logically equivalent to 'All non-G things are not-F', which looks odd initially but is accepted in the most basic and uncontroversial kinds of modern logic.[4] So the hypothesis 'all ravens are black' is logically equivalent to 'all non-black things are not ravens'. But then we note that this last hypothesis is confirmed by the observation of a white shoe, as the shoe is a positive instance. Given the logical equivalence of the two hypotheses, and the fact that anything that confirms one confirms the other, the observation of a white shoe also confirms the hypothesis that all ravens are black. This looks absurd.

The relevance of Popperian ideas to the debate was noted rapidly by John Watkins (1964).[5] The problem only arises if one thinks that all observed positive instances confirm a generalization, regardless of whether those observations were made as part of a genuine test. If we insist that confirmation depends on more than the bare logical relation between instance and generalization, then the problem is easily handled.

Suppose that we know antecedently that an object is black, and we then inspect it to see whether it is a raven. We find that it is. But because there was no possible outcome of this observational process that would tell *against* the hypothesis, we should deny that the hypothesis gains any support from this observation of a black raven. And now suppose that we have an object antecedently known to be non-white in colour, and we inspect to see whether or not it is a raven. We find it to be a shoe. This observation *was* part of a genuine test of the black-ravens hypothesis. It could have come out in a way compatible *or* incompatible with the hypothesis, and in fact it came out as the hypothesis claimed it would. Here, there is support for the hypothesis, because the observation was made as part of a genuine test.

The basic point about the role of 'order of observation' in the ravens problem was also noted by some people outside the Popperian context (Hempel 1965). But the connection to Popperian themes is clear, and it is surely no accident that well-focused versions of the idea were developed by those around Popper's circle.

Since the original discussions, this idea has developed in more detail by various others. Horwich (1982) embeds it within a Bayesian approach. Giere (1970) and I embed it within an approach inspired more by classical statistical ideas (Godfrey-Smith 2003). But what is striking here is that the literature has not, in general, simply accepted and absorbed the point. The more common response to the situation has been to try to salvage something closer to Hempel's original picture, according to which all instances support a generalization as a matter of quasi-logical fact. The tendency has often been to treat the considerations discussed by Watkins and others as extraneous details that concern special cases and do not get to the heart of the issue. From the Popper-informed point of view, though, these considerations are absolutely the heart of the issue. Favourable observations or positive instances may or may not have any real evidential significance in relation to a hypothesis we are considering. Whether they do or not depends on much more than what the hypothesis says and what was observed. It is also essential to consider the context in which the observation was made, for that is essential to determining whether or not the observation was part of a genuine test.

I call this point 'Popper-informed', even though in one way it is anti-Popperian. The aim of this move is to resurrect a distinction between cases where observed instances do, and do not, provide epistemic support for a generalization. For Popper, such concepts of non-deductive support are misconceived (putting to one side, again, some complexities involving corroboration). I take the point to be importantly linked to Popper because of its connection to the ideas of risk taking and possible disconfirmation.

I do not want to overstate the extent to which mainstream discussion rejected the Popper-informed point. Modern Bayesian treatments of the ravens hypothesis respect, more or less, the key constraint. For a Bayesian, evidential support is a contrastive matter. An observation only supports H if there are other hypotheses on the table that treat the observation as more unlikely that H does.[6] Those outside the Bayesian framework seem often to want to hang onto the idea that all instances confirm, however, and even some Bayesians seem to think this is a desirable output for a theory of evidence. To me, there is nothing at all to be said for the idea that all instances, *qua* instances, confirm a generalization. Even the hedged versions of the view (the hypothesis is not already falsified, and there is no special background knowledge of the kind that Good [1967] discussed) should have no appeal. We learn from Popper that there is such a thing as the *illusion of support*, in many non-deductive contexts, and this is a very important illusion.

Why has this idea been so unobvious to those thinking within the mainstream empiricist tradition? I conjecture that this is because of two distinct factors.

One, which is fairly obvious, is the great influence that formal logic had on twentieth-century empiricism. If logic is our tool and our exemplar, then it will be natural to treat confirmation as a logical or quasi-logical relation. That means it will probably be a relation between sentences themselves – between the sentences describing observations and those expressing a theoretical hypothesis. Once we are committed to that framework, it seems attractive to do as much as possible with the relationship between universally quantified generalizations and statements reporting the satisfaction of generalizations by particular cases. If our goal is a logical theory of non-deductive support, this seems as simple and clear as things could get.

The second reason has to do with the other great influence on mainstream empiricism, a tradition of general theorizing about mind and language. The influence of psychologistic assumptions on empiricism was more muted in the twentieth century than the nineteenth, but

I think it still played a role. If we approach epistemology within the kind of psychological picture associated with traditional empiricism, then it might seem very natural to insist that that all positive instances confirm a generalization. The mind is seen as acquiring its epistemologically sanctioned *contents* via the conduit of experience. One *sees* each positive instance – where seeing is a local and particular matter – and the world thereby impresses itself upon the mind. Each episode of this kind will and should increase the confidence the agent has in the generalization. If these episodes do not suffice, nothing else is available to do the job.

So within the mindset of both twentieth-century logic-based epistemology and more psychologistic forms of empiricism, it can be quite hard to move away from the intuition that all positive instances confirm. But as the Popperian tradition and some later work have taught us, if we insist on this, we fail to realize the importance of a simple but crucial kind of illusion of support, involving cases where a positive instance has been observed in the wrong context to have any epistemological significance. From the viewpoint of Popper's epistemology, which is free from the psychologistic assumptions of traditional empiricism and less committed to the primacy of formal logic, it is easy to appreciate the importance of this fact.

4. THE DIACHRONIC PERSPECTIVE ON EVIDENCE

The last theme I discuss is more complicated and controversial. I'll look at the possibility that Popper was seeing something important in some of the most-criticized parts of his work. Here, again, I have in mind his work on testing, and this time the focus is on Popper's rejection of what look like moderate and reasonable concepts of evidential support. Without endorsing Popper's actual claims here, I want to raise the possibility that Popper was successfully seeing past some standard ways of thinking, and glimpsing significant new options. My discussion of these ideas is cautious and qualified.

In this section I use a distinction between *synchronic* and *diachronic* perspectives on evidence. A synchronic theory would describe relations of support within a belief system at a time. A diachronic theory would describe changes over time. It seems reasonable to want to have both kinds of theory. In the case of deductive relationships, formal logic gives us a detailed synchronic account, which can also be the basis of diachronic descriptions of valid reasoning. In the non-deductive case, epistemology in the twentieth century tended to suppose we could have both kinds of theory, but often with primacy given to the synchronic side. The more novel possibility, which I discuss in this section, is the primacy of the diachronic side, once we leave the deductive domain.

This idea of 'primacy' gestures towards a family of ideas and options. A very moderate version of such a view appeared in the previous section. There I looked at the idea that an observation only has the capacity to support a hypothesis in the context of a test or procedure, where a 'test or procedure' is something that extends over time. Musgrave (1974) extended these ideas, taking them to motivate a 'party historical' or 'logico-historical' approach to confirmation. There are also more radical views in the same family. Perhaps there is no substantive non-deductive synchronic theory of evidence possible *at all.* Or perhaps the only synchronic theory that can be given is much weaker and less rich than people have supposed.

A diachronic view of this kind would describe rational or justified change, or movement, in belief systems. The assessment is of motions rather than locations. Such a theory might enable us to recapture some, but not all, of what people wanted from the traditional synchronic account.

In this section I suppose that we do not, at present, have the right framework for developing such a view. But we can trace a tradition of sketches, inklings and glimpses of such a view in a minority tradition within late nineteenth- and twentieth-century epistemology. The main figures I have in mind here are Peirce (1878), Reichenbach (1938) and Popper. This feature of Popper's view is visible especially in a context where he gets into apparent trouble. This is the question of the epistemic status of well-tested scientific theories that have survived many attempts to refute them. Philosophers usually want to say, in these cases, that the theory has not been proven, but it has been shown to have some other desirable epistemic property. The theory has been confirmed; it is well supported; we would be justified in having a reasonably high degree of confidence in its truth.

In situations like this, Popper always seemed to be saying something inadequate. For Popper, we cannot regard the theory as confirmed or justified. It has survived testing to date, but it remains provisional. The right thing to do is test it further.

So when Popper is asked a question about the present snapshot, about where we are now, he answers in terms of how we *got* to our present location and how we should *move on* from there in the future. The only thing Popper will say about the snapshot is that our present theoretical conjectures are not *inconsistent* with some accepted piece of data. That is saying something, but it is very weak. So in Popper we have a weak synchronic constraint, and a richer and more specific theory of movements. What we can say about our current conjecture is that it is embedded in a good process.

In some ways, this development in the shape of epistemological theory is not as alien as it might initially look. Something like this moral, in a moderate form, is implicit in standard versions of Bayesianism, the dominant view in philosophy of science about testing and evidence at present (Howson and Urbach 1993). (The next page or so is more complicated than the rest of the essay and can be skipped by those unfamiliar with Bayesianism. See also n. 6.)

Bayesianism is often seen as completely transforming the issues with which people like Popper, Hempel and Carnap were concerned. Further, it is often seen as coming down on the side of the optimists about traditional philosophical notions of confirmation and induction. But this standard story is not entirely accurate. Bayesianism of the standard kind treats belief systems both synchronically and diachronically. Constraints are placed on belief profiles at a time, and also on change over time as new evidence comes in. But Bayesianism imposes *weak* synchronic constraints, and *stronger* diachronic ones.

The synchronic constraint is often called 'coherence.' (This includes Popper's constraint of deductive consistency.) Degrees of belief must obey the probability calculus. This is a weak constraint; all sorts of very unreasonable-looking belief profiles meet it. You can coherently believe, for example, that the coin in my hand is almost perfectly symmetrical and about to be tossed high with plenty of spin by a normal human, and that it will almost certainly come up tails. Substantive principles that link or coordinate subjective probabilities with objective chances or their physical bases are often discussed, but they are controversial and not intrinsic to the basic Bayesian picture.

The standard Bayesian framework imposes much richer constraints on how a rational agent's belief system changes over time. The updating of subjective probabilities must be done via conditionalization. When a Bayesian agent learns some new piece of evidence e, the agent's new unconditional probability for any hypothesis $P'(h)$ must be set equal to the agent's old conditional probability $P(h|e)$, which is related to various other old subjective probabilities via Bayes' theorem. This diachronic constraint is much richer than the coherence constraint that applies to 'synchronic' assessment of belief systems.

Some (though not all) advocates of Bayesianism urge the importance of a larger-scale diachronic perspective here. Standard Bayesianism allows the initial 'prior' probabilities of hypotheses to be 'freely' set, so long as updating is done properly. The results of updating in the short term then depend on these assignments of prior probability. But Bayesians often argue that in the long term, after many rounds of updating, the initial settings of priors 'wash out'. Two agents with very different distributions of prior probability for a set of hypotheses, who also satisfy some other constraints, will come to have degrees of belief that 'converge', once enough evidence has come in. The details and significance of these 'convergence' arguments are complicated, and not all Bayesians put much stock in them. But for some, an essential part of the Bayesian story concerns these larger-scale dynamic patterns in how bodies of incoming data are handled.

Leaving Bayesianism now, the general possibility on the table is the idea that many traditional epistemological questions might be recast in a way that treats diachronic features of evidence as primary. The discussion below will be more informal than a Bayesian treatment. The discussion will also be idealized in several ways, to bring it close to the general picture of scientific theorizing that Popper, Hempel and others tended to assume in the twentieth century. This is a picture that I would in many ways reject, but it is common in philosophy. According to this picture, what scientists aim to develop are 'theories', often in the form of generalizations. These theories are assessed for empirical adequacy, and when a theory does well under testing, a scientist can hope the theory might be true. Theories are discarded or modified when their predictions fail.

This may look like a fairly neutral account of scientific work, but it is very much a creature of philosophy itself. A more accurate view would include ideas such as these: a lot of theoretical science is concerned with *models* rather than 'theories'. Modelling involves the description and investigation of deliberately idealized, hypothetical structures that can have a range of different resemblance relations to real-world systems (Giere 1988; Weisberg 2007). Scientists often retain multiple models when dealing with a single domain, including deliberately oversimplified ones that are known to be inaccurate in many ways. Traditional philosophical questions about truth and reference are not straightforwardly applicable to scientific work of this kind, and questions about evidence and testing look different as well. But having registered these qualifications, in most of the discussion that follows I operate within the confines of the standard picture of scientific activity that Popper and many other philosophers assume. This is, in a way, a piece of model-building of its own.

Let us now look at some ways in which some problems in epistemology might be transformed by adopting a strongly diachronic view of evidence and testing. I will discuss three issues in turn: conservativism, simplicity and the underdetermination of theory by evidence.

***Conservativism*:** In many discussions in philosophy of science, 'conservativism' is seen as an epistemic virtue. We are told that it is reasonable not to alter or discard our theories unless there is a positive reason to do so. When we are induced to make changes to our theories, we should not change more than we have to. Quine (1990, p. 14) called this principle the 'maxim of minimum mutilation'.

Why should conservatism be a virtue? If our goal is to believe theories that are true, or that are well supported by the evidence, then it is hard to see why conservativism should be a good thing. If we take a snapshot of our current theory and its relation to current evidence, and we then note that some other theory does equally well with this body of evidence, why should the 'incumbent' theory get an advantage?

One reason why philosophers are attracted to principles of conservativism is the fact that they seem to fit to some extent with scientific practice. It is also true that the principle can be justified in part on pragmatic grounds; it will generally be inconvenient to change from one theoretical framework to another. Kuhn (1970, p. 76) emphasized this pragmatic side when he said that 'retooling is an extravagance reserved for the occasion that demands it'. But the kind of pragmatic role being envisaged here seems to be one that is at odds with, rather than being independent of, epistemic considerations.

From the point of view of a diachronic view of evidence, the role of conservativism looks different. It comes to have something closer to an epistemic justification. Or perhaps it would be fairer to say that, from a diachronic point of view, there is a role for conservativism that is in some ways pragmatic but is positively tied to the epistemic rather than being at odds with it.

Suppose we have a view according to which the epistemic credentials of a theory derive from its embedding in an ongoing process. We see science as committed to a particular process of the development of theory in response to observation; our present theory is just where this process has brought us for the time being. To drop that theory and adopt some other theory would be to depart from what we take to be the best process.

Perhaps this move does not really resolve the problem of the status of conservativism. Someone might object: Why is it not *just as good a process* to arbitrarily switch to a different theory once it is shown to be compatible with all available evidence? In general, the best process is to take a theory and modify it as evidence comes in, but in the special case where we note the existence of a rival theory that has all the same relations to evidence, we can switch freely.

There will certainly be good practical reasons not to follow such a procedure. The result will be needless retooling and disruption of the development of theoretical concepts. So perhaps what the shift to a diachronic perspective does in this first case is bring the more 'practical' and more purely 'epistemic' considerations into some kind of concord, rather than having them pull in different directions.

***Simplicity*:** My second example is the problem of simplicity preferences, or 'Occam's razor'. Scientists are usually seen as preferring simple to complex theories whenever possible, and they are usually seen as justified in this preference. This is a more famous, more important and more vexed issue than conservativism.

Occamism has been very hard to justify on epistemological grounds. Why should we think that a simpler theory is more likely to be true? Once again there can be an appeal to pragmatic considerations, but again they seem very unhelpful with the epistemological questions.

This problem has connections with the problem of conservativism discussed above. Sometimes the preference for existing theories is itself described as an Occamist preference, but this can be misleading. We might have a situation where an incumbent theory is more complex than a newcomer, and both deal with the evidence equally well. Then conservativism tells us not to shift, and simplicity pulls us the other way.

The right eventual philosophical response to the problem of Occamism will surely contain a mixture of elements. First, there is an important range of cases in science where substantive assumptions about the objects and processes being studied can justify a limited and field-specific preference for simplicity (Sober 1988). Second, this is one of the areas where philosophical discussion is partially out of step with scientific practice. In some areas in science, the response to the development of a simpler theory that handles the same observations as an existing theory would be to keep *both* theories on the table. This is one of the areas (noted earlier) where a philosophers' obsession with theory *choice,* rather than the development of a range of useful models, can lead us astray. But let us bracket those aspects of the problem and see whether things look different when we switch to a diachronic perspective on evidence and testing.

From a diachronic point of view, simplicity preferences take on a quite different role. Simplicity does not give us reason to believe a theory is true, but a simplicity preference is part of a good *rule of motion*. Our rule is to start simple and expect to get pushed elsewhere. Suppose instead we began with a more complex theory. It is no less likely to be true than the simple one, but the process of being pushed from old to new views by incoming data is less straightforward. Simple theories are good places from which to initiate the dynamic process that is characteristic of theory development in science. Occasionally a very simple theory might actually be true; that is merely a bonus.

This feature of simplicity preferences has been noted, in more specific contexts, before. Popper himself (1959, chapter 7) argued that the importance of simplicity lies in the fact that simple statements are more easily falsified than complex ones, and he used the example of different mathematical relations between variables (linear, quadratic, etc.) to make the point. More recently, Kevin Kelly (2004) has argued, on the basis of formal models, that a simplicity preference will be part of a procedure that reliably approaches the truth via the fewest dramatic changes of opinion en route.

Underdetermination: My third example is more subtle, and even more general than the problem of simplicity. This is the problem of the 'underdetermination of theory by evidence'. In the simplest terms, this is the argument that for any body of evidence, there will always be more than one theory that can, in principle, accommodate it. As observational evidence is all we have to go on, this seems to show that our preference for any specific theoretical view must always be based to some extent on non-evidential factors like aesthetic considerations or convenience.[7] This, in turn, is often taken to be a problem for scientific realism, particularly scientific realism of the 'mixed' kind discussed above.

There are many versions of the underdetermination thesis, some stronger than others. Some versions are both very strong and very general; it is argued that for any theory T_1 we might come to hold, there will be another incompatible theory T_2 that that we cannot hope to empirically distinguish from T_1 via any conceivable evidence. But as Stanford (2006) argues, these very strong versions of the view tend to rely on extreme sceptical hypotheses (perhaps of the Cartesian demon kind), or on small manipulations of T_1 that produce a variant that is not scientifically interesting. There are worked-out illustrations of underdetermination for some physical theories, usually involving space, time and motion, that are neither ultra-sceptical nor trivial, but certainly not for all theories.

The statement of an underdetermination problem that I focus on is more moderate, but still important. My formulation is modified from one used by Psillos (1999, p. 164).

U: For any particular body of evidence we might have, there will always be more than one scientific theory that can, in principle, accommodate it.

In my discussion here I bracket some questions about the role of probability and confirmation. To say that more than one theory can 'accommodate' the data is not saying much, as two theories may both permit various observations but assign very different probabilities to them. But I won't worry about that complication here.

Suppose U is true. How worrying is it? My argument will be that its importance is sometimes overstated because of philosophers' routine assumption of a particular point of view. The usual situation imagined is one in which we assume we have a body of data and a theory T_1 on the table. Principle U then appears in the form of a kind of *barrier* to successful theorizing. But so far at least, U is compatible with another principle that might apply to the situation.

D: For any particular comparison of theories we want to make, there is some possible body of data that will discriminate the two.

That is, many of the usual underdetermination anxieties are compatible with a kind of symmetry in the situation: for any comparison of theories, we can hope to find discriminating data; for any data, there will be rival theories that are not discriminated.

Of course, D might be false. Once we bring in traditional sceptical possibilities, it seems that it may well be false. But most discussion in this area is not supposed to be concerned with those extreme possibilities. Perhaps in the case of some specific hypotheses or scientific domains, D is again a vain hope. But that, again, is not the usual focus or thrust of the discussion. Something like U *alone* is often seen as sufficient to endanger realism.

Suppose U and D are both true, or have similar standing. Then we have a 'glass half full' and 'glass half empty' situation. When we look at U, the glass looks half empty. When we look at D, it seems half full. What must be resisted, or done more cautiously, is the drawing of conclusions solely from the 'glass half empty' side. And here the diachronic point of view is relevant. The glass looks half empty when we think about the problem synchronically in a particular way. If we take a snapshot of the data present at a time, and ask which theoretical possibilities it can distinguish, then we will note the data's limited power. If, however, we think diachronically about what data can do, the situation looks different.

I cannot claim too tight a connection between the diachronic point of view and a glass-half-full impression of the situation. We could, in principle, think diachronically about the introduction of new theoretical possibilities. If at any time we have succeeded in using our data to discriminate between T_1 and T_2, we can expect someone to put T_3, a new theoretical possibility, on the table at the next time-step, which our data cannot rule out. But it seems easier or more natural for people to think diachronically about a flow of data rather than a flow of theoretical options being put on the table. And most of the time people seem to think simply in terms of a snapshot where we hold our data fixed and lament its limited powers of discrimination.

I do not say all this in order to urge that people look only at the bright side, the glass-half-full side. But clearly, we should look at the situation from that side as well. Principles like U and D should be assessed as pairs, when we look for general philosophical morals. The question of what we *should* conclude from the pair of D and U, if both are true, I leave for another occasion.

This concludes my discussion of three epistemological issues that may look different from a diachronic point of view. I emphasize that a *purely* diachronic view in this area seems to lead to bad consequences. For example, consider our present epistemic situation regarding evolution and creationism. When describing this case, it seems misleading and incomplete to say merely that the evolutionary view of life on earth was arrived at by a good process and that we expect to refine it rationally via more good 'motions'. In cases like that, it seems undeniable that we have good reason to believe that one theory, seen as a snapshot, is highly likely to be true, at least with respect to the basic features. We do not always expect to move on.

As has often been noted, the Popperian view of evidence is strongly informed by particular parts of science – the collapse of Newtonianism, the heroic conjectures of Einstein, the permanent sense in twentieth-century physics that more surprises may be just round the corner. Not all science is like this. No one who started their epistemology by thinking about twentieth-century biology would be led to a picture with this overall shape to it. So a future view must somehow strike a balance here.

5. CONCLUSION

I have discussed four Popperian themes: the importance of eliminative inference; sceptical realism and neighbouring possibilities; the link between risk taking and evidence; and (more speculatively) the general viability of a more diachronic perspective on problems in epistemology. The first two of these ideas are straightforward. Philosophical neglect of the first, in particular, looks very strange in retrospect. The second two ideas are more subtle, and their future importance is more uncertain. Especially in these latter cases, my emphasis has not been on the details of Popper's arguments, but on broad possibilities, philosophical priorities and directions of analysis that he seemed to see when others did not.

NOTES

1 See Salmon (1981), Newton-Smith (1981), Godfrey-Smith (2003).
2 Van Fraassen (1980) is a special case. Van Fraassen argued that the properly scientific attitude to have to a successful theory is to 'accept it', where acceptance does not include belief in the claims the theory makes about the unobservable domain. Realism is characterized as a view about the proper *aim* of science, not (explicitly at least) as a view about the chances of success. See Godfrey-Smith (2003) for further discussion.
3 See Psillos (1999) and Devitt (2005) for examples of expressions of optimism of this kind.
4 This will not be the case if law-like generalizations in science are seen as subjunctive conditionals or some other quirky form of conditional.
5 See also Musgrave (1974).
6 The Bayesian model of rational belief change characterizes agents as holding sets of *degrees of belief* in various hypotheses. At any time, a rational or 'coherent' agent's degrees of belief must be related in ways that conform to the axioms of probability theory. As new evidence comes in, the agent updates via 'conditionalization'. Roughly speaking, if the agent observes *e*, the agent's new degree of belief in *H* is set equal to the agent's old degree of belief in *H given e*. See Howson and Urbach (1993) for a good introduction.
7 See Psillos (1999, chapter 8) for a review.

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GUNNAR ANDERSSON

5 The Problem of the Empirical Basis in Critical Rationalism

In order to test general hypotheses and theories scientists use singular statements about observations and experiments. Popper (1959, chapter 5) called these statements *basic statements*, later *test statements* (1974, p. 988). Their character was intensely discussed in the Vienna Circle when Popper wrote *The Logic of Scientific Discovery*. There Popper presented a new solution to the problem of the empirical basis of science. Inspired by Kant's philosophy, he emphasized its *objectivity* and criticized other solutions to the problem as expressions of subjectivism and psychologism. According to Popper (1959, § 30), test statements are objective in the sense that anybody who has learned the relevant technique can test them.

Is Popper's solution of the problem of the empirical basis tenable? For his solution it is important that a test statement can be tested by other test statements. This chapter argues that this is the case, that critical discussions and even falsifications of test statements are possible. This makes Popper's objective and critical solution of the problem of the empirical basis tenable and superior to other solutions involving attempts to verify test statements or justify their content.

According to Popper (1959, § 30), test statements cannot be verified by experience, but are accepted by *conventional decisions*. However, Popper also stressed the importance of a *critical discussion* of test statements and compared the decisions of scientists to accept test statements to the verdicts of a jury. In the last two sections of this essay it is argued that a critical theory of test statements without any traces of conventionalism can be worked out and that test statements can be criticized by experience. The last idea is contentious. However, it is not an expression of psychologism, but a natural consequence of a fallibilist empiricism.

1. FALLIBILITY OF TEST STATEMENTS

Popper (1959, § 25) began his discussion of the problem of the empirical basis by criticizing the idea that test statements can be justified by experience. All attempts to justify lead to a trilemma. If you try to prove that a statement is true, you have to use premises. If you try to show that these premises are true, you are led to an *infinite regress,* which only can be avoided by a *logical circle* or by *dogmatically* introducing statements without proof. All attempts to get certain knowledge by proof lead to this trilemma, to the choice between infinite regress, logical circle and dogmatism.

In order to avoid the trilemma, many philosophers argued that some statements could be known to be certain without proof. The philosophers in the Vienna Circle, the logical empiricists, discussed whether experience is a source of certain knowledge, whether experience can show that some kind of simple basic statements are true and evident. The German philosopher Fries (1828–31) discussed the same problem and argued that in sense experience we have immediate knowledge, which can justify (directly and without any logical proof) the mediate knowledge expressed in statements. Fries saw in the idea that all statements have to be logically justified in order to be accepted an expression of 'the predilection for proof' (quoted by Popper 1959, §25). Fries thought that some statements can be directly justified by experience. Popper criticized this idea as a recourse to psychologism and thought that many modern philosophers also adopted psychologism when they tried to solve the problem of the empirical basis.

Psychologism is not a solution to the problem of the empirical basis. Test statements cannot be justified as true by immediate experience because they go beyond it. Every description uses universals and has theoretical and hypothetical character. Popper used the simple statement 'Here is a glass of water' as an example. The words 'glass' and 'water' denote physical bodies exhibiting a certain *law-like* behaviour and cannot be reduced to classes of experiences (Popper 1959, §25). Thus test statements are fallible and cannot be verified by immediate experience, and psychologism is untenable. How are we to solve the problem of the empirical basis if we want to avoid psychologism, dogmatism and infinite regress? This is the problem Popper (1959, chapter 5) tried to solve.

2. PROTOCOL STATEMENTS IN THE VIENNA CIRCLE: NEURATH

In the Vienna Circle some philosophers maintained that in science, test statements are records of immediate experiences of observers. Popper regarded their views as modern versions of psychologism, of the idea that test statements can be immediately justified by experience.

The members of the Vienna Circle discussed, perhaps influenced by Popper, whether test statements are fallible. According to one of them, Otto Neurath, test statements are fallible. If they contradict a theoretical system, they can be deleted or the theory can be changed. Popper thought that Neurath's view represented a notable advance and a step in the right direction. But it leads nowhere if it is not followed up by a method for the critical discussion of test statements and by giving us rules for their acceptance or rejection. Since Neurath did not present such a method, Popper thought that he unwittingly threw empiricism overboard. If test statements contradicting a theory simply can be deleted, any theory can be immunized against criticism by rejecting inconvenient test statements. Neurath avoided the form of *dogmatism* represented by psychologism, yet he opened the door for *relativism* and made it possible for any arbitrary theory to be defended as scientific. Popper (1959, § 26) found that Neurath's theory of test statements was 'merely a relic - a surviving memorial of the traditional view that empirical science starts from perception'.

Neurath realized that test statements are fallible, but he did not present any method for the critical discussion of them, and therefore unintentionally threw empiricism overboard. Did Popper succeed in presenting such a method preserving the fallibility of test statements and empiricism and avoiding relativism?

3. THE NEW WAY: SCIENTIFIC OBJECTIVITY AND TESTABILITY

In the philosophical discussion of epistemological problems there has been a constant vacillation between dogmatism and relativism. In the discussions in the Vienna Circle there was the same tension: some philosophers sought certainty in immediate experiences, while others, for example Neurath, tried to overcome this kind of dogmatism, but ended up in relativism. Is there a critical solution to the problem of the empirical basis avoiding both dogmatism and relativism? Inspired by Kant's philosophy, Popper (1959, §§ 8 and 27; 2009, § 11) thought that such a solution can be found in the objectivity of science. Kant said that scientific knowledge was objectively justifiable if the justification could be tested and understood by anybody. Popper (1959, § 8) did not think that scientific *statements* are *justifiable*, but thought that they were *testable* and said that 'the *objectivity* of scientific statements lies in the fact that they can be *inter-subjectively tested*'. By using the idea of objective testability Popper thought that he could solve the problem of the empirical basis in a way that avoided relativism and dogmatism. For him, the important question is not how scientific statements can be justified, but how they can be tested and criticized. In order to be scientific, a statement must be presented in such a way 'that anyone who has learned the relevant technique can test it' (Popper 1959, § 27).

Can the problem of the empirical basis be solved with the idea of intersubjective testability? In order to discuss this problem, we must first discuss how scientific statements are tested.

4. TEST STATEMENTS AND FALSIFICATIONS

How are scientific theories tested and falsified by singular test statements? In the *Logic of Scientific Discovery* Popper presented two different types of falsifications: falsifications with the help of the negation of a prognosis (in § 18); and falsifications with the help of a test statement in the form of a potential falsifier (in § 28).

We test theories by deriving conclusions from them (Popper 1959, § 18). In order to derive a singular conclusion from a theory, we need at least one singular initial condition. For example, if we test the hypothesis 'all swans are white', we need the initial condition '*a* is a swan' in order to derive the prognosis '*a* is white'. If the derived conclusion is false, then the tested theory (consisting in this example of a general hypothesis and a singular initial condition) is false. This type of falsification is not specific: it shows that in this simple case the general hypothesis or the singular initial condition is false. In more complicated cases many initial conditions and general hypotheses are necessary in order to derive a testable prognosis. '[W]e falsify *the whole system* (the theory as well as the initial conditions) which was required for the deduction of the statement *p* [the prognosis].' We do not know which statement or statements of the system 'we are to blame for the falsity of *p*; which of these statements we have to alter, and which we should retain' (Popper 1959, § 18 and § 18, n. 2). In this case we have to conjecture which parts of the falsified system are responsible for the falsification.

Why is this type of falsification valid? When Popper wrote *The Logic of Scientific Discovery* in the 1930s, he thought that the reason was the *modus tollens* of classical logic. This is, however, not the case, as Popper (1959, § 18, n. *1) later realized. The reason why falsifications are valid is a fundamental property of valid deductive inferences. Deductive logic is the theory of the *transmission of the truth* from the premises to the conclusion. In all valid logical inferences the conclusion must be true if the premises are true. However, if the conclusion is false, then at least one of the premises must be false. Deductive logic is also the theory of the *retransmission of falsity* from the conclusion to at least one of the premises (Popper 1976, pp. 98–99). When we test scientific theories, we derive a conclusion in the form a prediction. If this prediction is false, then at least one of the premises in the form of initial conditions and hypotheses must be false. Deductive logic is a powerful tool (or organon) of criticism and can be used to falsify theories of different kinds.

Popper called those hypotheses scientific that could be falsified. The philosophers in the Vienna Circle thought that those hypotheses were scientific that could be verified. Popper wanted to replace their way of distinguishing between scientific and unscientific hypotheses and proposed that their criterion of verifiability should be replaced with his new criterion of falsifiability. In order to be able to use his new criterion, Popper had to show that single universal hypotheses are falsifiable. With the first type of falsifications he could, however, only show that theoretical systems consisting of general hypotheses and initial conditions were falsifiable. A second and more specific form of falsification that can hit a single hypothesis had to be found.

For this reason Popper (1959, §28) introduced potential falsifiers that were able to falsify a general hypothesis. For example, the general hypothesis 'All swans are white' has the potential falsifier: 'On the 16th of May, 1934, a swan which was not white stood between 10 and 11 o'clock in the morning in front of the statue of Empress Elizabeth in the Volksgarten in Vienna' (cf. Popper 1983, introduction [1982], part I). If such a swan actually is observed, then the potential falsifier is transformed into an actual falsifier of the general hypothesis.

Why is this second type of falsification valid? Predicate logic shows that an existential statement can contradict a general statement. The existential statement 'There is a swan which is not white' contradicts the general hypothesis 'All swans are white'. The existential statement does not tell us when and where there is a swan that is not white. Without such information the existential statement is not testable. Therefore, Popper (1959, §28) required that all test statements tell us when and where a falsifying event occurs, that all test statements should have the logical form 'in the space-time-region *k* there is a *G*', that is of *singular existential statements* and called this a formal requirement for test statements. This requirement for test statements leads to the problem that a test statement cannot be falsified by another test statement alone. For purely formal reasons, two existential statements cannot contradict each other. This consequence is unintended and unacceptable (cf. Popper 1974, pp. 987–89).

Another problem is that in science we often test theoretical systems consisting of many universal hypotheses. Are such more complex theories also falsifiable and scientific according to Popper's criterion of demarcation? In order to deal with this problem, it is useful to examine how prognoses are derived in a case that is a little more complicated than then the testing of the isolated hypothesis 'all swans are white'. Popper (1957, §28; cf. 1959, §12) gave the following example: We can predict a certain thread breaks 'if we find out that this thread could carry a weight of only one pound, and that a weight of two pounds was put on it'. In this derivation of the prognosis (*P*), we use two universal hypotheses (*H*) and two singular initial conditions (*C*):

For every thread of a given structure *s* (determined by its material, thickness, etc.) there is a characteristic weight *w* such that the thread will break if any weight exceeding *w* is suspended on it. (H_1)
For every thread of the structure s_1, the characteristic weight *w* equals one pound. (H_2)
This is a thread of structure s_1. (C_1)
The weight put on this thread was a weight of two pounds. (C_2)

This thread will break. *(P)*

Assume that we have observed not only that that the thread in question did not break but also that it had structure s_1 and also that the weight put on it was two pounds – that is, that we, on the basis of observations, assume not only that the prognosis is false but also that the two initial conditions are true. Since the prognosis is false, at least one of the premises is false (retransmission of falsity; see Popper 1976, pp. 98–99). But since the two initial conditions are assumed to be true, the falsification hits only the two universal hypotheses. Both cannot be true. The theoretical system consisting of their conjunction is falsified: $\neg(H_1 \& H_2)$. At least one of the universal hypotheses is false. In this case the statement 'this thread did not break, although it had structure s_1 and was loaded with 2 pounds' $(\neg P \& C_1 \& C_2)$ is a potential falsifier. Thus potential falsifiers are connected with prognoses and initial conditions. If the derived prognosis is false and the initial conditions are true, then at least one of the universal hypotheses must be false. In this way complex theoretical systems consisting of many universal hypotheses can be tested and eventually falsified.

The two types of falsification discussed in *The Logic of Scientific Discovery* are special cases. In the first type of them, we only know that derived prognosis is false. Then the theoretical system consisting of the universal hypotheses and the initial singular conditions is falsified. In the second type of falsifications only an isolated single hypothesis was falsified using a potential falsifier. However, using logic as a tool of criticism, many types of theoretical systems are falsifiable, not only isolated single hypotheses but also theoretical systems consisting of many universal hypotheses and including perhaps also some auxiliary hypotheses (Andersson 1994, chapter 2C). Deductive logic is a versatile tool of criticism.

When we use deductive logic as a tool of criticism, it is not necessary to assume that potential falsifiers are existential statements of a special type, as Popper did when he discussed the falsification of a single universal hypothesis (the second type of falsifications). It is sufficient to assume that test statements are singular statements. Since two singular statements can contradict each other, it is clear that a test statement can falsify another test statement. Thus not only theoretical systems but also test statements can be falsifiable and scientific.

5. OBSERVATIONS AND TEST STATEMENTS

5.1. Observable Events

Popper required that test statements should describe *observable* events, events that are intersubjectively testable by observation. This is his material requirement for test statements. Since test statements describe what happens in specific places at specific points of time, they can only be tested by observers suitably placed in space and time (Popper 1959, §28).

Popper did not define 'observable' or 'observable event', but introduced them as primitive terms, which become sufficiently precise in use. Examples of observable objects are planets, white swans and glasses of water. Although statements about such objects go beyond immediate experience and transcend evidence, they can nevertheless be tested by observation and hence are observable. Also objects and events which go far beyond immediate experience will be regarded as observable, for example the event described in the test statement 'The strength of the electrical current in this wire is now 15 amps'. In order to observe such events, we have to use an instrument the construction of which depends on scientific theories. If we wish, we may use the hypotheses used in the construction of the instrument as auxiliary hypotheses and derive simpler test statements about the position of the pointer of the instrument. When such auxiliary hypotheses are unproblematic, they do not have to be stated explicitly, but belong to the unproblematic background knowledge. We look at the instrument and say that we have observed the strength of the electrical current in the wire. Such observations are indirect and fallible. But all our observations are so, including our observations of a glass of water or a black swan.

What is observable does not depend solely on the way the world irritates our sensory nerve endings. It also depends on the conceptual and theoretical resources we possess (cf. Musgrave 1999, p. 343).

A person without any knowledge of the theory of electricity will hardly be able to observe the strength of an electrical current by looking at an instrument, but will ask how this is possible. A scientist without any pedagogical ambitions could say that an answer to this question would require some very long explanations and recommend a course in the theory of electricity (Duhem 1974, p. 145). However, an answer can be given. It is possible to learn how to observe electrical currents with the help of appropriate instruments. It is also possible to test the corresponding test statements about electrical currents in different ways. You can ask what other persons have observed with the same instrument (intersubjective testability). You can use another type of instrument for measuring the strength of electrical currents. And you can criticize the auxiliary hypotheses used in the construction of the instruments.

5.2. The Relativity of Test Statements

Test statements can be tested by other test statements. If one type of test statements should be problematic, it is possible to derive other and hopefully less problematic test statements from it with the help of auxiliary hypotheses. The derivation of test statements is stopped at a kind of test statements that is especially easy to test (Popper 1959, § 29). An example of this relativity of test statements is the following test statement: 'This powder is red mercury oxide' (my translation using modern chemical terminology of Popper 1979, § 11, p. 125). If the test statement 'This powder is red mercury oxide' is problematic, the auxiliary hypothesis, 'If red mercury oxide is heated, then mercury and oxygen are produced', can be used to derive further, hopefully less problematic test statements (cf. Conant 1957, pp. 93–109). The tests can be continued. For example, if it is problematic to determine whether oxygen is produced, further tests are possible. Red-hot iron cuttings ignite if they are put into oxygen. With this auxiliary hypothesis it can be tested whether oxygen was produced by inserting red-hot iron cuttings into the tube with heated red oxide of mercury and observing whether they ignite.

There are simpler examples of the relativity of test statements. Take, for example, the test statement 'In this glass there is now water'. It can be tested in many ways. A very simple way is to taste the water and check how it tastes. There are innumerable ways to test the chemical properties of the liquid. You can also test the physiological effects of drinking the liquid in the glass. If you are intoxicated and have a hangover the next day, you can, with the help of unproblematic background knowledge, conclude that the glass did not contain pure water. This common-sense falsification of a simple test statement has the same logical structure as the falsification of the scientifically more interesting test statements about red oxide of mercury. In both cases auxiliary hypotheses are used in order to derive unproblematic test statements from problematic ones (Andersson 1994, pp. 77–79).

With the help of auxiliary hypotheses a test statement can be tested with other test statements. In principle it is always possible to continue the derivation of test statements. This does not lead to an infinite regress, because the derivation of test statements is stopped when unproblematic test statements have been derived that can easily be tested by observation. In the discussion of test statements with the help of other test statements there is no circular argumentation, but test statements are tested with other types of test statements. It is not maintained that the derived unproblematic test statements are infallible or proved by experience. Therefore, the acceptance of test statements is not dogmatic. Although it is always possible to continue the tests and the derivation of further test statements, this does not lead to any sceptical trilemma. Fundamental for the resolution of the trilemma is that no attempt is made to justify any statements as true (cf. Andersson 2006; Popper 1959, §29).

6. ACCEPTANCE OF TEST STATEMENTS

It is not enough to show logically that theories are falsifiable, that they can be tested empirically. The derived test statements also have to be tested by observation and some test statements have to be accepted and compared with the tested theory.

6.1. Conventional Decisions

Popper thought that experience psychologically can motivate us to accept test statements, but that it cannot justify test statements as true 'no more than by thumping the table' (Popper 1959, §29). Decisions are necessary in order to accept test statements: 'Thus it is *decisions* which settle the fate of theories.... [T]he convention or decision ... enters into our acceptance of the *singular* statements – that is, the basic statements' (Popper 1959, §30). Popper argued that test statements are accepted by decisions that are conventional from a logical point of view. Later (1974, p. 1114) he declared that it would be a complete misunderstanding to assimilate his view to any form of conventionalism. Which character, then, do the decisions on test statements have?

6.2. Classifications of Test Statements as True

In order to avoid psychologism and justificationism, it is not necessary to say that test statements are accepted by *conventional* decisions. According to David Miller (1994, p. 29), we classify test statements as true when we decide to accept them. The acceptance of test statements is not arbitrary or conventional: 'The only complaint that can properly be directed against a test statement is that it is false. Had Popper been less squeamish about mentioning truth [in *The Logic of Scientific Discovery*] he would surely have said this explicitly.'

Which role does experience play when we decide to accept test statements and classify them as true? Watkins thinks that Popper's theory of test statements can be interpreted in different ways. According to one interpretation, perceptual experiences lie outside the domain of epistemology. Test statements are tested only with the help of other test statements. 'All we are getting, under this … interpretation, is a lengthening chain of derivations: no *tests* are being made' (Watkins 1984, pp. 252–53).

Against this interpretation Miller objected that experience plays the role of motivating us to accept basic statements, that the decision to accept basic statements cannot replace experience, because this would be conventionalism. We should observe before we classify test statements as true or false, we should look before we leap from experience to the classification of test statements as true or false. He adds that there 'is nothing arbitrary in the demand that accepted test statements be true; objectively true, that is, not just consistent with other test statements, not just conventionally true' (Miller 1994, p. 30).

Certainly, we are aiming for truth in our critical discussions of test statements. Our decisions to accept test statements should be preceded by experience. But why should we observe before we decide to accept test statements or classify them as true? Why should we look before we leap? Has it any epistemological significance that our decisions to accept test statements are psychologically motivated by experience?

Miller invited those who object to the procedures of first observing and then making a decision (to classify a test statement) to say what they think is wrong with them. There is nothing wrong with these procedures. But they do not explain the epistemological significance of experience. It is not enough only to say that decisions to accept (or classify) test statements should be psychologically motivated by experience or that we should look before we leap.

6.3. *Experience as an Inconclusive Reason for Test Statements*

Watkins discussed a second interpretation of Popper's theory of test statements, according to which perceptual experiences are both causes of and reasons for the acceptance of test statements (Watkins 1984, p. 253). This interpretation is supported by Popper's (1974, p. 1114) later view: '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.'

6.4. *Critical Discussion of Test Reports*

William Warren Bartley did not accept the idea that observations can be inconclusive reasons for test statements. He found Popper's discussion of test statements confusing and argued that the confusing feature was due to Popper's unfortunate tendency to invoke convention, or irrational decision, whenever some point is reached which cannot be justified (Bartley 1984, p. 215). According to Bartley, neither decisions on nor conclusive or inconclusive reasons for test statements are needed. There is only need of an open discussion and criticism of test statements. But how are they to be criticized, according to Bartley? Only by comparing them with other test statements? Then we can, as Watkins expressed it, lengthen our chain of derivation, but no *tests* are made. Bartley wrote that in order to test a particular theory, we determine what sort of event would be incompatible with the theory. Then we try to observe such an event and make or gather reports concerning the test. If the reports go against the theory and are unproblematic, the theory is false relative to the test reports. It is clear that a test statement (or report) can be incompatible with a theory. But what did Bartley mean by saying that some test statements (or reports) are unproblematic? Does experience play any role in such evaluations? Bartley did not discuss this problem.

6.5. *Rational Belief in Test Statements*

Alan Musgrave (1999, pp. 341–43) discussed the relation between perceptions and perceptual beliefs. This discussion is relevant for the problem of unproblematic test statements. Musgrave (2004, pp. 18–19) distinguished between the *act* of believing and the *content* of belief. We must distinguish between good reasons for the *act* or decision to accept a test statement and good reasons for the *content* of the test statement.

A good reason for the content of a statement is, for example, a proof that the statement is true. Some philosophers think that a good reason for the *act* to accept a statement can only be a proof or justification of the statement. Musgrave (2004, p. 19) does not think so:

> [I]t cannot be that the *only* good reason for believing P is that you have inferred it from another belief R, for which you have good reason. That is *logomania*, the view that only reason or reasoning provides a good reason for believing anything. Logomania leads to infinite regress, as sceptics long ago pointed out. All inferential beliefs … must rest on non-inferential beliefs. But logomania entails that non-inferential beliefs are unreasonable – from which follows that all beliefs are unreasonable.

In order to avoid irrationalism, we have to think that there are good reasons for believing P that do not involve inferring P from other propositions we believe. According to Musgrave, experience is a good reason for belief. He (1999, p. 342) formulated the following principle of experience (principle E): 'It is reasonable to perceptually believe that *P* (at time *t*) if and only if *P* has not failed to withstand criticism (at time [*t*]).' Applying this principle to test statements, it is reasonable to accept a test statement *S* (at time *t*) if and only if *S* is in agreement with experience and has not failed to withstand criticism (at time *t*).

The kernel of critical rationalism is a new view of rationality. According to a justificationist view of rationality, it is rational to accept statements that can be justified by sufficient reason. According to critical rationalism, it is rational to accept statements that have survived critical tests. The kernel of critical rationalism is to replace the principle of sufficient reason with the principle of critical testing (Albert 1985, §§2 and 5).

In spite of the fact that test statements are fallible and cannot be proved by experience, experience is a good reason for assuming (tentatively and conjecturally) that test statements are true. Although experience is not a conclusive (or inconclusive) justification of the *contents* of test statements, it is nevertheless a rational *act* to accept test statements that correspond to perceptual experience, and that have not failed to withstand criticism from other test statements.

6.5.1. EVOLUTIONARY THEORY AND EXPERIENCE. Popper (1974, p. 1114) argued that our 'experiences are not only motives for accepting or rejecting an observational statement, but they may even be described as *inconclusive reasons*'. These reasons are inconclusive reasons for the act of *accepting or rejecting* an observational statement; they are not inconclusive reasons for or against the content of an observational statement. In arguing for this view, Popper (1974, p. 1112) presented arguments from evolutionary theory. He wrote that sense organs are part of the decoding mechanism by which certain organisms, especially animals, interpret the state of their environment and anticipate its impeding changes. They work astonishingly well, although they are far from perfect: they are *fallible*. 'They are marvellously powerful and efficient as organs of adaptation; but they are fallible, especially in unfamiliar circumstances.' Evolutionary epistemology supports the view that perceptual experience often gives us information about the environment, which it is rational to accept as reliable in spite of the fact that this information is in principle fallible (cf. Campbell 1974).

These naturalistic arguments in favour of perceptual experience as a good reason for accepting test statements do not mean that Popper's epistemology is merely naturalistic. He (1959, § 10) argued against the naturalistic approach to the theory of method: an epistemology or theory of method relying only on naturalistic arguments is uncritical. In spite of this, naturalistic arguments are very important as critical arguments in the discussion of epistemological and methodological problems (cf. Albert 1987, § 14).

It is not circular to use naturalistic arguments in the discussion of epistemological problems. Our critical discussions are not intended to prove any epistemological position. For an epistemology trying to *justify* positions with the help of sufficient reasons, the charge of circularity would be serious. For an epistemology trying to *test* positions by using arguments from evolutionary theory, this is not the case.

6.5.2. EPISTEMIC ASYMMETRY BETWEEN TEST STATEMENTS AND GENERAL THEORIES. Are there any differences in the critical discussion of general hypotheses and of test statements (cf. Musgrave 1999, p. 342)? Popper (1959, § 30) wrote that there is such a difference: conventions or decisions determine our acceptance of test statements, but not immediately our acceptance of general hypotheses. Musgrave (1999, p. 342) proposed another difference: according to the principle of critical rationalism (CR), it is reasonable to believe non-perceptually in a statement *S* if and only if *S* is that statement which has best withstood serious criticism. This principle applies for general hypotheses. For test statements, the principle of experience (E) discussed above applies, saying that it is reasonable perceptually to believe in a statement if and only if it has not failed to withstand criticism. The principle of experience (E) is a concession to the epistemic primacy of sense-experience and of test statements. According to Musgrave, there is an asymmetry in the epistemic situation of general hypotheses and test statements: rationally accepted general hypotheses should have withstood serious criticism, while rationally accepted test statements should not have failed to withstand such criticism. The reason for the weaker requirement for test statements is that we often accept test statements after perception without having tested them with the help of other test statements. According to Musgrave, such test statements are accepted without having withstood serious criticism. They are perceptual beliefs, that is, the belief in them is caused by perceptions.

6.5.3. EXPERIENCE AS A TEST. Test statements can be tested by comparing them with other test statements. But they can also be tested by comparing them with experience. Unproblematic test statements about observable events are especially easy to test in this way. We should observe before we accept test statements, because observations are tests of the truth-values of the test statements.

If we regard perceptual experience as a special kind of test, we need no special principle of experience (E) in order to explain when it is rational to accept test statements. Test statements can be tested directly by experience and can survive such criticism. Take, for example, the test statement 'There is a planet in position *p* at time *t*'. This statement can be tested seriously by observing position *p* in the sky at time *t*. If a planet is observed, the test statement has withstood the test. Thus the principle of critical rationalism (CR) can be used also for test statements: it is rational to accept a test statement that has withstood serious criticism. The epistemic asymmetry between general hypotheses and test statements does not consist in the use of two different principles, the principle of experience for test statements and the principle of critical rationalism for general hypotheses. The epistemic asymmetry rather consists in two different kinds of testing. General hypotheses are tested by comparing them with test statements. Test statements can be tested in this way. But they can also be tested by comparing them with experience. Test statements have an epistemic primacy just because they can be *directly* tested in this second way.

6.5.4. COMPARISON WITH EXPERIENCE. Many philosophers find the idea that test statements can be compared with and tested by experience strange and even incomprehensible. Philosophers influenced by logical empiricism belong to this group. The intense discussions of 'basic statements' in the Vienna Circle in the 1930s ultimately led many of them to maintain that test statements can be compared neither with reality nor with experience – only with other statements. It is no wonder that many of them were attracted to a coherence theory of knowledge and to the idea that a statement can be tested only by comparison with other test statements. Davidson (1986, p. 324) said that their discussions presented 'very good reason to conclude that there is no clear meaning to the idea of comparing our beliefs with reality or confronting our hypotheses with observations'. It is an irony of fate that logical *empiricists* came to these conclusions. It is true that we have no direct access to reality, that we cannot directly compare statements with reality. But we have indirect contact with reality through our sense organs and can test statements by comparing them with perceptual experience. Most human beings capable of formulating statements perform such tests every day! When we observe the event described by a test statement, we have good reason to think that the test statement is true. Thus experience not only may cause us to accept a test statement but also provides a good reason to think that it is true. Those who think that statements can be compared with experience only in a metaphorical sense cannot satisfactorily explain how theories are tested empirically. Unintentionally they throw empiricism overboard (cf. Popper 1959, § 26; Russell 1940, pp. 140–41).

Ultimately, test statements have to be tested by comparison with experience. But they can be tested in many other ways, for example by comparing them with other test statements or by discussions with other people (cf. Shearmur 2004, p. 105; 2006). Such critical tests and discussions are necessary in a world in which truth is not evident. They are natural consequences of a fallibilist empiricism.

6.5.5. SUBJECTIVISM, PSYCHOLOGISM AND JUSTIFICATIONISM. To compare test statements with experience is to introduce a subjective element. After all, experience is ultimately subjective. It is, however, important that experience is not used as a justification of test statements or as a criterion of truth. Experience is used in order to test, not in order to justify test statements. To admit that experience has such a function is not an expression of psychologism or questionable subjectivism. For any empiricist, a whiff of subjectivism is unavoidable. As long as experience is not used to justify test statements, this is unobjectionable.

Miller found the distinction between justification of the *content* of a statement and justification of the *act* to accept or believe in a statement unimportant. To his mind 'this is only to pour stale justificationist wine into new critical rationalist bottles' (Miller 1994, p. 107). Nonetheless, there are different kinds of justificationism, so perhaps the wine is not so stale after all. Compare the two principles of rationality (Musgrave 1993, pp. 280–81): 'A belief is reasonable if and only if it is certain or justified'; and 'A belief is reasonable if and only if it has withstood serious criticism'.

The difference between these two principles is important. If we accept the first principle, we are confronted with the sceptical trilemma. If we accept the second principle, no such trilemma confronts us and we are able to choose theories and test statements in a rational way (Andersson 2009). We are also able to explain why serious criticism is important. The alternative is to say that there are no good reasons for choosing a test statement, that the classification of a test statement as true is a kind of existential leap. Such a position is irrationalism in disguise.

7. A CRITICAL THEORY OF TEST STATEMENTS AVOIDING DOGMATISM AND SCEPTICISM

The epistemological discussion in the history of philosophy has been a conflict between justificationism and scepticism. The discussion of test statements in the recent philosophy of science mirrors this conflict. The idea of an infallible empirical basis, of verified test statements, can be criticized in many ways. Popper criticized it by showing that every test statement used in science goes far beyond our immediate experience and is impregnated by theories. For him the fallibility of test statements is not a source of relativism. Although test statements cannot be verified by experience, they can be critically tested by it.

Test statements can be tested by other test statements *or* by experience. If a test statement is problematic, we can often derive unproblematic test statements from it with the help of auxiliary hypotheses. These unproblematic test statements can easily be compared with experience. Such tests make it reasonable to decide to accept test statements.

This critical theory of test statements does not contain any concessions to dogmatic justificationism or to relativistic scepticism. It is dogmatic to maintain that experience can verify test statements. In the discussions in the 1930s Popper and many members of the Vienna Circle criticized this type of dogmatism. It is also dogmatic to maintain that test statements are made certain by conventional decisions. According to Popper, decisions do play a role in the acceptance of test statements. However, these decisions are not about conventions, but about fallible singular test statements. The decision to accept a test statement is conjectural and tentative. It is a rational decision if the accepted test statement has withstood critical tests by observation or by comparison with other test statements.

A theory of test statements without such evaluations is sceptical. It is most important to understand that we do not have to choose between dogmatism and scepticism, that a critical theory of test statements is possible. Such a theory overcomes the shortcomings of justificationism, conventionalism and scepticism (cf. Andersson 2006, 2013).

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MICHAEL BRADIE

6 Karl Popper's Evolutionary Philosophy

Karl Popper had a long and ambivalent relationship with evolutionary theory and Darwinism. On the one hand, he told us that he had been an admirer of Darwin since childhood and that he took Darwinism to be the best available explanation of the development of life on Earth. On the other hand, he argued that Darwinism was not a testable theory but was, at best, a valuable metaphysical research programme. He later came to retract that judgement. Nonetheless, his stance towards Darwinism as a scientific theory remained cautious and circumspect. He saw a close parallel between his own methodological analysis of the growth of scientific knowledge and Darwinism, arguing that the former threw light on and supported the latter.

In *The Logic of Scientific Discovery*, which first appeared in German in 1934 and in English in 1959, Darwin's name does not appear in the text, but the language of Darwinism – struggle, competition, fitness and survival – is invoked to characterize the methodological procedure of theory evaluation that came to be codified as the model of conjectures and refutations. Popper (1959 [1961], p. 108) asked: 'How and why do we accept one theory in preference to others?' He answered:

> 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. This will be the one which not only has hitherto stood up to the severest tests, but the one which is also testable in the most rigorous way.

Later, in arguing for the view that theories are corroborated by being subjected to severe tests in an attempt to falsify them, he wrote that 'instead of discussing the "probability" of a hypothesis we should try to assess what tests, what trials, it has withstood; that is, we should try to assess how far it has been able to prove its fitness to survive by standing up to tests' (Popper 1959 [1961], p. 251).

Despite his changing views about the status and nature of Darwinian evolutionary theory, Popper did not waver in his lifelong conviction that there was an intimate connection between the evolution of life on earth and the growth of animal and human knowledge. In his later years, when he developed his view of the evolution of what he came to call World 1 (the physical universe), World 2 (the world of conscious experience) and World 3 (the world of objective knowledge), he saw a Darwinian process underlying it all. In the next section, I sketch Popper's theory of the evolution of the tripartite universe. Section 2 contains an exposition and evaluation of Popper's evolutionary epistemology. In section 3, I try to make sense of Popper's changing views on the status of evolutionary theory. Section 3.1 addresses the question of whether there is a law of evolution. Section 3.2 is a critical assessment of Popper's one time view that Darwinism and evolutionary theory is a tautology. Section 3.3 reviews the various versions of neo-Darwinism that Popper defended. Section 3.4 is a discussion of Popper's views on the limitations of Darwinism. Section 3.5 looks at Popper's contention that Darwinism is a 'metaphysical research programme.' Finally, in Section 4, I draw some conclusions about the permanent significance of Popper's evolutionary philosophy.

1. POPPER ON THE EVOLUTION OF THE UNIVERSE

In *Objective Knowledge,* Popper develops his tripartite theory of the evolution of the real world. If we accept the best views of modern science, we must admit that at some time in the distant past there were no living organisms but only matter and energy (Popper 1972 [1981], p. 225). The physical universe, devoid of life and consciousness, constitutes what Popper calls the first world or World 1. The formation of stars, galaxies and planetary systems followed. One of those planetary systems was our sun and its planets, including Earth. At some point, by processes that are not completely understood, living creatures emerged from non-living matter and life on Earth began. With the emergence of life came the emergence of problems – problems of survival and reproduction. Some organisms are better suited to cope with their environments than others are. According to Darwin, life on Earth evolved and diversified through processes the most important of which was natural selection. Popper construed Darwinism as being committed to the view that natural selection works by singling out those organisms and lineages that are better suited to cope with the problems posed by the environments in which they find themselves. The struggle for existence in environments with scarce or limited resources means that, other things being equal, those organisms most fit to survive do so and reproduce. Those less 'fit' to survive fall by the wayside. Some organisms are more proficient than others in utilizing the resources of their environments to solve their problems. This puts a premium on the development of traits – organs and behavioural repertoires – that increase the 'fitness' of organisms. In the course of the evolution of life on Earth, some organisms became conscious. Being conscious was presumably a huge advantage in the struggle for existence, since conscious organisms are better adapted to deal with the contingencies of their environments. Conscious organisms are capable of interacting with their environments in ways that non-conscious organisms cannot. They can begin to mould their environments in deliberate ways. The emergence of consciousness marks a qualitative change in the structure of reality. A new world – World 2 – the world of consciousness – has emerged. This world is as 'real' as the physical World 1 but is not 'reducible' to it. What makes World 2 as 'real' as World 1 is the fact that each 'world' can interact with and modify the other. This capacity for interaction was Popper's core criterion for what is to counts as 'real.'

The next important development was the emergence of self-consciousness and the capacity for language. This allowed for the production of 'objective knowledge', that is, of criticizable representations of the world. The development of modern science is just the latest stage in the evolution of our understanding of the world around us. This objective knowledge is encoded in books and cultural artefacts but distinct from them. The intellectual and cultural content lives in World 3. The physical instantiations of intellectual and cultural content – books, manuscripts, monuments and so on – all belong to World 1.

In a sense, the production of objective knowledge by human beings is just a further adaptive trait or 'organ' for coping with the environment. Just as a bird's nest is an extrasomatic 'organ' of a bird with a possibility for persistent existence that transcends the limited lifespan of the bird that built it, so theoretical knowledge is an extrasomatic human creation with the same possibilities.

For Popper, although objective knowledge is a product of human ingenuity and imagination (and did not exist before human beings existed), it has a reality that is independent of its origin. This is evidenced for Popper, in part, by the ability of objective knowledge to shape and modify the physical and mental world. For example, physical theories have guided us in the construction of rockets that enable us to travel to other worlds and explore other planets. The information encoded in books has the power to modify human opinions and fire up the imaginations of individuals. A second feature of objective knowledge that qualifies it as independent of the human beings who created it is the fact that our theoretical speculations have unintended consequences. For example, the numbers and numbering systems are human inventions but, once invented, have properties that no one foresaw and appear to contain secrets that we may never be able to decipher.

The real world for Popper, then, is a tripartite reality composed of an interacting triad of worlds that have emerged in the course of the evolution of the universe. Each new world is an emergent property of the world[s] that preceded it. The emergence of consciousness, of self-consciousness and of objective knowledge results in the introduction of qualitatively new features of reality. A general Darwinian world view is the only way, for Popper, to make sense of these developments. At each level of development there is an interaction between organisms or agents and their environments. Natural selection, as Popper saw it, is a two-edged sword. Organisms are shaped by their environments and evolve organs and capacities to deal with the contingencies that they face. On the other hand, organisms shape their environments by the creation or discovery of new niches. Human beings, self-conscious language users as they are, interact with their physical environments and also with the world of theories and values – World 3. Organs, sensory modalities and scientific theories all share a common origin and a common purpose. They are all the selective products of an evolutionary process that manifests itself in different ways at different levels of reality.

The resulting picture may be summarized as follows: Material Universe [World 1] → Living Organisms [Problem Solvers] → Living Organisms with Brains → Conscious Organisms and Consciousness [World 2] → Self-conscious Organisms → Objective Knowledge [World 3]. Each of the three worlds is an irreducibly emergent reality, and all three interact with one another. The emergence of living organisms results in the emergence of problems and problem solvers. The 'evolutionary logic' of the problem-solving organisms is the same 'logic' that manifests itself in the growth of scientific knowledge. There is a strict analogy between the organs of animals and the theories of scientists. As Popper put it, organs are theories and theories are organs (Popper 1972 [1981], p 145; 1984a, p. 30). Although theories are organs, and hence instruments for coping with reality, they are not *mere* instruments. Popper remained committed that realism was a more promising conjecture than idealism or instrumentalism (Popper 1972 [1981], p. 105).

Popper (1972 [1981], p. 261) was at pains to reject the suggestion that this similarity is *merely metaphorical*: '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.' The development of language and with it the evolution of our capacity to reason and criticize means that our conjectures can be published and subject to test. If they fail, we live to make new conjectures. '*By criticizing our theories we can let* *our theories die in our stead.* This is of course immensely important' (Popper 1994, pp. 6f).

2. POPPER'S EVOLUTIONARY EPISTEMOLOGY

In *Objective Knowledge*, Popper credited Donald Campbell with the term 'evolutionary epistemology', acknowledging at the same time that the roots of the view go back to the late nineteenth century (Popper 1972 [1981], p. 67; Campbell 1974; Bradie 1986). Popper saw himself as having been influenced by those early thinkers yet distinguishing the 'genesis or history' of knowledge on the one hand from the 'justification' of knowledge on the other (Popper 1972 [1981], p. 67). As early as *The Logic of Scientific Discovery*, Popper contended, he was arguing for the priority of the *logical* over the *genetic*, despite the fact that many of the problems of the theory of knowledge are suggested by studies of the genesis of knowledge (Popper 1972 [1981], p. 68).

In his contribution to the Schilpp volume on Popper, Campbell (1974) had distinguished between a normative approach to knowledge and a descriptive approach to knowledge. He saw the two as complementary. Evolutionary epistemology was a descriptive account of how organisms with the capacity for knowledge had evolved, how their capacities for knowledge had evolved and how the products of those capacities (our theories and conjectures) had evolved as well. This is the genetic account. The method of conjectures and refutations, which according to Popper shares a common structure with the evolutionary development of knowing organisms and their theories, was intended to be both a description of how human knowledge grows and a logical or normative model for how knowledge *ought* to be evaluated.

Popper often contrasted what he calls the 'commonsense' or 'bucket' theory of knowledge with the 'searchlight' theory of knowledge (Popper 1972 [1981], Appendix 1). The bucket theory of knowledge construes humans as passive receivers of information – knowledge pours into our minds as water pours into a bucket. This view, he argued, is 'pre-Darwinian' (Popper 1972 [1981], p. 65). After Darwin, Popper suggested, we can no longer countenance the view that human beings are passive receivers of knowledge. The point is more than the Kantian one that the mind is an active constructor of knowledge. For Popper, knowledge is acquired by means of an active searching in the light of inherited dispositions (cf. ter Hark, 1993a, 1993b, 2004). The lesson from Darwin is that organisms are born with inherited organs and behavioural potentialities that have been shaped by natural selection to be useful tools for navigating in their environments. We have, for example, a disposition to learn languages that develops in response to pressures from the physical and social environments. The development of language makes possible the formulation of scientific theories. Scientific theories are just the most sophisticated of a long line of tools for exploring the world we live in.

All knowledge, according to Popper, is conjectural. In fact, in a 1986 essay, he went so far as to suggest that not only is all knowledge conjectural but it is 99.9 percent 'biologically innate' (Popper 1999, p. 54). This idea is an extension of his 'searchlight' model. Human beings confront nature with innate expectations honed by evolution but subject to modification in the light of our experience. We make conjectures by means of which we anticipate or have expectations about what is going to happen. We modify these conjectures in the light of our experiences. The world, as it were, modulates our innate expectations. As we learn more about the world of objective human knowledge, as codified in conjectures that have stood up to the test of experience, they become an increasingly better representation of that world.

The commonsense theory of knowledge, on the other hand, fails to recognize the importance of the difference between 'subjective' knowledge in the form of dispositions and expectations and 'objective' knowledge 'which consists of linguistically formulated expectations submitted to critical discussion' (Popper 1972 [1981], p. 66).

> Knowledge in the subjective sense may grow or achieve better adjustments by the Darwinian method of mutation and elimination of the organism. As opposed to this, objective knowledge can change and grow by the elimination (killing) of the linguistically formulated conjecture: the 'carrier' [of the knowledge] can survive – he can, if he is a self-critical person, even eliminate his own conjecture (Popper 1972 [1981], p. 66).

These ideas are part of the core of Popper's evolutionary epistemology. In his reply to Campbell, Popper (1974, p. 1059) said:

> Professor Campbell's remarkable contribution is perhaps the one which shows the greatest agreement with my epistemology, and (what he cannot know) an astonishing anticipation of some things which I had not yet published when he wrote his paper.... For me the most striking thing about Campbell's essay is the almost complete agreement, down even to minute details, between Campbell's views and my own.

Among the special points of agreement that Popper singled out are their joint commitment to 'critical commonsense realism,' the hierarchical, interactive account of the development of animal consciousness and human knowledge, the continuity between organic evolution and the growth of human knowledge, and Campbell's understanding of how the trials in the trial and error method that underlies both biological and epistemic evolution are at once 'blind' but not random.

Popper (1974, pp. 1063–64) took issue with Campbell's account on only two, relatively minor points: the reinterpretation of Kant's categories as psychological, inherited dispositions; and the argumentative nature of human language. Campbell cites a long passage from *Conjectures and Refutations* and credits Popper among others with a psychological understanding of the Kantian categories. True enough, Popper said, but not my main point. He does not say what that point was, but I take it to be the claim that the a priori expectations of finding regularities are not only *psychologically a priori* but are also *logically a priori* (insofar as observations presuppose conjectures) and are *fallible* conjectures about the world rather than necessarily 'objectively valid.' With respect to the argumentative nature of human language Popper (1974, p. 1064) faulted Campbell for not sufficiently distinguishing the merely descriptive, if that, language of the bees from the argumentative nature of human language 'which makes criticism possible, and with it science.'

That said, Popper was generally pleased with Campbell's essay and felt that it spelled out in greater detail views Popper himself had been independently developing. The fact is that Campbell's analysis was designed to be a descriptive account of how knowing organisms acquire their knowledge. As such, it is an account of the *genesis* of knowing organisms and the knowledge they possess. It is clear, though, that Popper thought that the agreement between his epistemological method and evolutionary theory, as he understood it, was a significant fact that, as we have seen, conferred plausibility in Popper's eyes on the biological account. But his main interest, he always insisted, were the normative epistemological issues and not the descriptive biological account.

On the surface, it might seem that Darwinian evolution was being taken as a model for Popper's conjecture and refutation model. But, this is not how Popper saw it. Rather, Popper saw the conjecture and refutation model as throwing light on evolutionary theory. He began by distinguishing the empirical from the logical components of evolutionary theory. Here he suggested that a central problem of evolutionary theory is the following: according to this theory, animals which are not well adapted to their changing environment perish; consequently, those which survive (up to a certain moment) must be well adapted. This formula is little short of tautological, because 'for the moment well adapted' means much the same as 'has those qualities which made it survive so far'. In other words, a considerable part of Darwinism is not of the nature of an empirical theory, but is a *logical truism* (Popper 1972 [1981], p. 69). This is a very crude caricature of 'evolutionary theory' that Popper came to reject. He went on to suggest, however, that the 'empirical' components of Darwinism are basically the initial conditions concerning the state of the environment and the rate of environmental change. The logical content of the theory is expressed by the formula, here crudely expressed, that 'Those that survive are well adapted'.

The main point is that surviving organisms may be well adapted to their environments, but that is no guarantee that they will continue to be so in the future. In a similar vein, theories that have passed rigorous tests may be well adapted to their [data] environments, but that is likewise no guarantee that they will continue to be successful, that is, continue to pass rigorous tests in the future.

For Popper, the evolution of language enabled human beings to *describe* their environments and *critically assess* their expectations and anticipations with respect to their environments. This gives them a tremendous advantage over non-linguistic organisms that can only interact with the environments by means of inherited organ structures and behaviour dispositions. Nevertheless, both employ a method of 'trial and error'. Non-linguistic organisms often pay for their errors with their lives, whereas scientists often suffer no more than a temporary loss of reputation. For Popper (1972 [1981]. p. 70), the basic 'difference between the amoeba and Einstein is that, although both make use of the method of trial-and-error-elimination, the amoeba dislikes erring while Einstein is intrigued by it: he consciously searches for his errors in the hope of learning by their discovery and elimination. The method of science is the critical method.'

There is a common logical core to evolutionary processes, understood from a Darwinian point of view, the theory of knowledge and the scientific method. Each, properly understood, exemplifies a process of trial-and-error-elimination. Biological evolution is guided by natural selection, the growth of knowledge is guided by rational criticism and science proceeds by the subjection of conjectures to severe tests. Similarly, just as successful lineages are not immune from extinction, so successful theories are not immune from falsification.

There are, however, important disanalogies between biological evolution and the growth of knowledge. The most important is that the 'tree of knowledge' converges to a single branch, unlike the 'tree of life', which diverges from a single branch. Popper noted that the evolutionary tree – from one stem, many branches – is diametrically opposed to the evolutionary tree of knowledge, which is from many branches, one unifying stem. The evolution of life exhibits the emergence of diversity as depicted in the 'tangled bank' metaphor that is the concluding paragraph of Darwin's (1859) *Origin of Species*. There Darwin notes that the diverse profusion of different plants, animals and insects that inhabit a riverbank have evolved from only a few forms or perhaps only one. The growth of knowledge, on the other hand, exhibits a trend towards unification, that is, disparate disciplines are discovered to have underlying connections. The operative metaphor here is the search for a unified field theory or, more recently, for TOEs or 'theories of everything'. Popper explained this tendency towards convergence as a result of the fact that the problems of 'pure knowledge' that emerge as a result of the human curiosity are fundamentally 'problems of explanation' (Popper 1972 [1981], p. 263). The explanatory theories we propose are subject to criticism and that, in conjunction with the presumption that there is a real world that acts as the arbiter of our conjectures, accounts for the 'integrative growth of the tree of knowledge' (Popper 1972 [1981], p. 264).

The other element in this view that has drawn much criticism is the contention that scientific conjectures are not 'blind' or 'random' in the same way that the biological mutations that form the source of variation are. Both Popper and Campbell sought to defuse this criticism by pointing to two factors. First, human beings are hierarchical systems that preserve routines that have proved their effectiveness in the past. Second, conjectures are 'blind' not in the sense that they are formulated with no regard to the problem that they are addressing, but rather in the sense that they do not carry any certainty of success (Campbell 1974 [1981]; Popper 1994, p. 5).

Some critics have objected to Popper's emphasis on the search for negative evidence as the proper method of scientific investigation. Individual scientists may, Popper noted, play favourites with their theories, but if they are not inclined to look for negative evidence, their critics will (Popper 1994, p. 7).

3. THE STATUS OF DARWINISM AND EVOLUTIONARY THEORY

3.1. Is There a Law of Evolution?

Sometimes, when talking about evolutionary theory, Popper characterized it in terms of the question of whether there is a law of evolution. For example, in the context of a critique of historicism, Popper (1963 [1968], p. 340) said:

> There exists no law of evolution, only the historical fact that plants and animals change, or more precisely, that they have changed. The idea of a law which determines the direction and the character of evolution is a typical

nineteenth-century mistake, arising out of the general tendency to ascribe to the 'Natural Law' the functions traditionally ascribed to God.

In *The Poverty of Historicism*, he identified the 'evolutionary hypothesis' in the following way (Popper, 1957 [1964], pp. 106 ff.):

> [W]hat we call the evolutionary hypothesis is an explanation of a host of biological and paleontological observations – for instance, of certain similarities between various species and genera – by the assumption of the common ancestry of related forms. This hypothesis is not a universal law, even though certain universal laws of nature, such as laws of heredity, segregation, and mutation, enter with it into the explanation. It has, rather, the character of a particular (singular or specific) historical statement. (It is of the same status as the historical statement: 'Charles Darwin and Francis Galton had a common grandfather'.)

Popper went on to point out that the term 'hypothesis' or (we might add) 'conjecture' has a double meaning. We sometimes use it to signify universal laws and we sometimes use it to signify singular claims, as when a doctor says, 'My conjecture is that the patient is suffering from depression.' What Popper called the 'evolutionary hypothesis' or the hypothesis of common descent he alleged was a singular claim that is conjectured to hold true of life on Earth but need not, we presume, hold for any other planets that may have supported some form of biotic evolution.

The defender of a 'law of evolution' has two options at this point. The first is to reject the contention that the evolution of life on Earth is indeed unique. Popper dismissed this as an attempt to latch onto a discredited ancient theory of cycles or continuous return (Popper 1957 [1964], pp. 110f). The second option is to agree that the evolution of life on Earth is unique but to argue that, nonetheless, some *trends*, either positive or negative, can be discerned in the events. Popper dismissed this on the grounds that trends, no matter how structured, are particular sequents and not general factors that are properly describable in terms of laws. None of this, of course, should be construed as denying that laws play a central role in understanding and explaining the events that make up the history of life on Earth. All that Popper was rejecting is the view that that unique sequence is itself a law (Popper 1957 [1964], p. 109).

Popper characterized Darwinism, variously, as a tautology, as *almost* tautological, as an 'important explanatory theory', as a metaphysical research programme and as a testable hypothesis. Some of this apparent confusion is due to the fact that the scope of what Popper included within the terms 'evolutionary theory' and 'Darwinism' changed over time. Some can be explained as a result of Popper's having changed his mind about the status of Darwinism and evolutionary theory. Some is a result of his having emphasized first one aspect and then another of the 'theory'. Finally, some, I think, can be accounted for by separating out two distinct roles that evolutionary theory play in Popper's view. On the one hand, there is Darwinism as a guiding research programme. On the other, there is Darwinism as a testable, albeit flawed, explanatory scientific theory. In any event, Popper saw Darwinism as a 'limited' theory that, while useful, needed improvement. He suggested several such improvements, but it is not clear whether he meant these 'improvements' to contribute to the testability of the theory or whether he meant his emendations as contributions to the critical assessment of Darwinism as a metaphysical research programme.

Popper's earliest view was that Darwinism and evolutionary theory were essentially tautological. He held this view, as far as I can tell, until the 1970s when he recanted. His earlier assessment is based on a superficial understanding of evolutionary theory and he was, as he acknowledged, criticized for it. In the course of refining his understanding, he formulated several versions of what he took Darwinism to be. Throughout, even when he came to recognize that the theory of natural selection was a falsifiable (and, hence, scientific) hypothesis, Popper held that Darwinism constituted a significant metaphysical research programme, although he did not begin to use this label until the late 1940s (Popper 1974, p. 175, n. 242). Darwinism, he thought, provided a unified picture of the evolution of the universe, the evolution of life on earth and the evolution of human knowledge.

3.2. *Darwinism as a Tautology?*

As late as 1965, Popper (1972 [1981], pp. 241–2) wrote:

> Quite apart from evolutionary *philosophies*, the trouble about evolutionary *theory* is its tautological, or almost tautological, character: the difficulty is that Darwinism and natural selection, though extremely important, explain evolution by 'the survival of the fittest' (a term due to Herbert Spencer). Yet there does not seem to be much difference, if any, between the assertion 'those that survive are the fittest' and the tautology 'those that survive are those that survive'. For we have, I am afraid, no other criterion of fitness than actual survival, so that we conclude from the fact that some organisms have survived that they were the fittest, or those best adapted to the conditions of life.

There are two points of interest here. First, Popper here identified Darwinism, in effect, with the theory of natural selection. Second, he took the theory of natural selection to be the tautological claim that 'Natural selection results in the survival of the fittest' where the 'fittest' are identified as 'those that survive'. Neither of these points is valid. I will not take the time here to survey the enormous literature that has been generated concerning the claim that the theory of natural selection is *inherently* tautological. Suffice it to say that at least on some accounts, including Popper's own later reconsideration, the theory can be given non-tautological formulations and is capable of generating testable claims (Gould 1976; Popper 1978, p. 346).

With respect to Popper's identification of Darwinism with the theory of natural selection, we may say the following. Certainly natural selection plays a central role in Darwin's understanding of evolution, but it is by no means the only component of Darwin's view. Here we come to a problem: What are the components of Darwinism or the modern Synthetic Theory of Evolution that is often referred to as neo-Darwinism? This turns out to be a significant question. The reason is that modern biological theories are not as rigorously formulated as, say, classical mechanics. The key components of Newtonian mechanics, for instance, are easily identified: Newton's three laws of motion and the principle of universal gravitation. There is no such similar consensus about the key components of Darwinism or neo-Darwinism. Ernst Mayr (1991, pp. 36–37) has argued that Darwin's view incorporates *five* distinct theories: (1) Evolution as such: This is the theory that the world is neither constant nor recently created nor perpetually cycling, but rather is steadily changing, and that organisms are transformed in time. (2) Common Descent: This is the theory that every group of organisms descended from a common ancestor and that all groups of organisms, including animals, plants and micro-organisms, ultimately go back to a single origin of life on earth. (3) Multiplication of Species: This theory explains the origin of the (observed) enormous organic diversity. It postulates that species multiply, either by splitting into daughter species or by 'budding,' that is, by the establishment of geographically isolated founder populations that evolve into new species. (4) Gradualism: According to this theory, evolutionary change takes place through the gradual change of populations and not by the sudden (saltational) production of new individuals that represent a new type. (5) Natural Selection: According to this theory, evolutionary change comes about through the abundant production of genetic variation in every generation. The relatively few individuals who survive, owing to a particularly well-adapted combination of inheritable characters, give rise to the next generation. All evolutionists accepted (1). Nonetheless, even some of Darwin's strong supporters had reservations about (3)–(5). Mayr (1991, p. 37) argues that these five 'theories' are actually distinguishable theories, since various nineteenth-century evolutionary theorists (other than Darwin) held some but not others. In addition, from a logical point of view, one can see that it is possible for some to be true while the others are false. So, they are all 'independent'. This list does not include various mechanisms such as the inheritance of acquired characteristics, evolution by 'use and disuse', sexual selection and the theory of pangenesis that Darwin appealed to in the course of refining his views. In addition, the modern 'Synthetic Theory of Evolution' is a loose collection of theories drawn from genetics, developmental biology, ecology, palaeontology and molecular biology, among others, in addition to the classical Darwinian theories that serve to integrate them.

So, Popper's claim here that the theory is tautological reflects a rather unsophisticated view of evolutionary theory. Nonetheless, Popper (1972 [1981], p. 242) concluded 'that Darwinism, with all its great virtues, is by no means a perfect theory. It is in need of a restatement which makes it less vague'. Popper's 'restatement', however, did not produce a more testable version of the theory but, at best, a more fully developed metaphysical programme:

> My [reformulation of the] theory may be described as an attempt to apply to the whole of evolution what we learned when we analysed the evolution from animal language to human language. And it consists of a certain *view of evolution* as a growing hierarchical system of plastic controls ... The Neo-Darwinist theory of evolution is assumed; but it is restated by pointing out that its 'mutations' may be interpreted as more or less accidental trial-and-error gambits, and 'natural selection' as one way of controlling them by error-elimination (Popper 1972 [1981], p. 242).

3.3. Popper's Versions of Neo-Darwinism

Popper (1972 [1981], pp. 242ff.) presented this version of neo-Darwinism in the form of twelve theses:[1]

1. All *organisms* are constantly, day and night, *engaged in problem solving*, and so are all those evolutionary *sequences of organisms* – the *phyla* which begin with the most primitive forms and of which the now living organisms are the latest members.

2. These problems are problems in an objective sense: they can be, hypothetically, reconstructed by hindsight, as it were.... Objective problems in this sense need not have their conscious counterpart; and where they have their conscious counterpart, the conscious problem need not coincide with the objective problem.
3. Problem solving always proceeds by the method of trial and error: new reactions, new forms and new organs, new modes of behaviour, new hypotheses, are tentatively put forward and controlled by error-elimination.
4. Error-elimination may proceed either by the complete elimination of unsuccessful forms (the killing-off of unsuccessful forms by natural selection) or by the (tentative) evolution of controls which modify or suppress unsuccessful organs, or forms of behaviour, or hypotheses.
5. The single organism telescopes into one body, as it were, the controls developed during the evolution of its *phylum* – just as it partly recapitulates, in its ontogenetic development, its phylogenetic evolution.
6. The single organism is a kind of spearhead of the evolutionary sequence of organisms to which it belongs (its *phylum*): it is itself a tentative solution, probing into new environmental niches, choosing an environment and modifying it. It is thus related to its *phylum* almost exactly as the actions (behaviour) of the individual organism are related to this organism: the individual organism and its behaviour are both trials, which may be eliminated by error-elimination.
7. Using 'P' for problem, 'TS' for tentative solutions and 'EE' for error-elimination, we can describe the fundamental evolutionary sequence of events as follows:

   $$P \rightarrow TS \rightarrow EE \rightarrow P.$$

   But this sequence is not a cycle: the second problem is, in general, different from the first: it is the result of the new situation which has arisen, in part, because of the tentative solutions which have been tried out, and the error-elimination which controls them. In order to indicate this, the above schema should be rewritten:

   $$P_1 \rightarrow TS \rightarrow EE \rightarrow P_2.$$

8. But even in this form an important element is still missing: the multiplicity of the tentative solutions, the multiplicity of the trials. Thus our final schema becomes something like this:

TS_1

$\nearrow$

$P_1 \rightarrow TS_2 \rightarrow EE \rightarrow P_2$

$\searrow$

.

.

.

TS_n

9. In this form, our schema can be compared with that of neo-Darwinism. According to neo-Darwinism, there is in the main *one* problem: the problem of survival. There is, as in our system, a multiplicity of tentative solutions – the variations or mutations. But there is only *one* way of error-elimination – the killing of the organism. And (partly for this reason) the fact that P_1 and P_2 will differ essentially is overlooked, or else its fundamental importance is not sufficiently realized.
10. In our system, not all problems are survival problems: there are many very specific problems and sub-problems (even though the earliest problems may have been sheer survival problems). For example, an early problem P_1 may be reproduction. Its solution may lead to a new problem, P_2: the problem of getting rid of, or of spreading, the offspring – the children which threaten to suffocate not only the parent organism but each other.

    It is perhaps of interest to note that *the problem of avoiding suffocation by one's offspring* may be one of those problems which was solved by the evolution of *multicellular organisms*: instead of getting rid of one's offspring, one establishes a *common economy*, with various new methods of living together.
11. The theory proposed here distinguishes between P_1 and P_2, and shows that the problems (or the problem situations) which the organism is trying to deal with are often *new*, and themselves arise as products of the evolution. The theory thereby gives implicitly a rational account of what has usually been called by

the somewhat dubious names of '*creative evolution*' or '*emergent evolution*'.

12. Our schema allows for the development of error-eliminating controls (warning organs like the eye; feedback mechanisms) – that is, controls which can eliminate errors without killing the organism; and it makes it possible, ultimately, for our hypotheses to die in our stead.

What is new and what is an advance over the 'vague' version of neo-Darwinism that Popper was concerned to improve? First, Popper's version does not appear to be any more testable or falsifiable than the original, despite the fact that it is not couched in terms of a tautology. This suggests that Popper's main concern was not to improve the *scientific* status of neo-Darwinism, but rather to create a framework in terms of which the parallel between biological and epistemological evolution would manifest itself. In conceptualizing organisms as 'problem solvers' and biological evolution as a process of trial and elimination, Popper established the sense in which the biological theory and his epistemological methodology are mutually reinforcing. However, the disconnect between the organism's conscious understanding of what it is up to and the 'objective problem' that the theorist conceives as what is going on (in thesis [2]) raises questions about the appropriateness of the theoretical model. In much the same way, externalist accounts of human behaviour that involve discounting the understandings of the agents being investigated are held to be problematic.

Theses (10) and (11) address the supposed narrow focus of neo-Darwinism on problems of survival. But this concern just seems to be mistaken and a reflection of Popper's unjustly narrow formulation of neo-Darwinism in the first place. For one thing, it fails to appreciate the distinction between 'proximate' and 'ultimate' perspectives. Even if we concede that the ultimate biological problems are problems of survival, the immediate problems that organisms face – obtaining food, avoiding predators, finding shelter, finding mates, and so on – are not directly problems of survival at all. The need to solve these proximate difficulties in order to be able to survive leaves plenty of room for new situations to throw up 'new' problems.

Later, in his 'Intellectual Autobiography', Popper (1974, pp. 135–36) characterized neo-Darwinism by two theses:

1. The great variety of the forms of life on earth originate from very few forms, perhaps even from a single organism: there is an evolutionary tree, an evolutionary history (Common Descent).

2. There is an evolutionary theory which explains this. It consists in the main of the following hypotheses. (a) Heredity: the offspring reproduce the parent organisms fairly faithfully. (b) Variation: there are (perhaps among others) 'small' variations. The most important of these are the 'accidental' and hereditary mutations. (c) Natural selection: there are various mechanisms by which not only the variations but the whole hereditary material is controlled by elimination. Among them are mechanisms which allow only 'small' mutations to spread; 'big' mutations ('hopeful monsters') are, as a rule, eliminated. (d) Variability ... is [most probably] controlled by natural selection.

In 1977, Popper (Popper and Eccles 1977, p. 73) summed up what he called the 'Darwinian point of view' in the form of four theses:

1. The theory of natural selection is the only theory known at present which can explain the emergence of purposeful processes in the world and, especially, the evolution of higher forms of life.
2. Natural selection is concerned with *physical survival* (with the frequency distribution of competing genes in a population). It is therefore concerned, essentially, with the explanation of World 1 effects.
3. If natural selection is to account for the emergence of the World 2 of subjective or mental experiences, the theory must explain the manner in which the evolution of World 2 (and of World 3) systematically provides us with instruments for survival.
4. Any explanation in terms of natural selection is partial and incomplete for it must always assume the existence of many (and of partly unknown) competing mutations, and a variety of (partly unknown) selection pressures.

The first thesis is clearly a metaphysical guiding principle and not a part of scientific Darwinism. The second thesis reflects a rather narrow population geneticist take on the principle of natural selection. Even so, the focus on survival is somewhat misplaced. Natural selection is a 'force' for explaining the *changes* in the frequencies of genes in a population (again, if we are focusing on the genetic level).

The fourth thesis is well taken, but of course the same could be said for any theory. The theoretical principles *alone* do not suffice to *explain* any particular facts. One must always supply appropriate initial conditions, boundary conditions and force functions. Natural selection, as an evolutionary 'force', functions in much the same way as Newton's second law. Changes in gene frequencies are a (possible) indicator that natural selection is at work. But this just alerts us to the possible presence of selective pressures. In order to arrive at a more complete understanding of what is going on, we need to identify what those specific factors might be. The same is true in classical mechanics. The presence of accelerated motions is an indicator of the presence of forces. But in order to arrive at a complete understanding of what is going on, we need to identify the specific force functions that may be causing the accelerations.

3.4. The Limitations of Darwinism

In 'The Rationality of Scientific Revolutions', Popper (1994, pp. 8–9) suggested two possible limitations to Darwinism. First, the argument against inheritance of acquired characteristics rests on the assumption of a sharp distinction between those factors that are inherited (the germ line) and those factors that influence the development of the individual organism. But, Popper suggested, that differentiation is itself a relatively late evolutionary development. Second, genetic mutations induced by radiation are inherited, but '[w]hat we cannot say is that this fact contributes in any way to an explanation of genetic adaptation, or of genetic learning – except indirectly, via natural selection' (Popper 1994, p. 9).

These are 'limitations of evolutionary explanation', according to Popper (Popper and Eccles 1977, p. 563), but it is important to realize that these limitations are not of the same kind. Some are empirical deficiencies and some are the result of metaphysical presumptions. In order to appreciate Popper's relation to Darwin and evolutionary theory, we must clearly distinguish what falls under Scientific Darwinism from what falls under Philosophical Darwinism. In the last analysis, when Popper (Popper and Eccles, 1977, p. 563) laments the inadequacies of his picture of how the three worlds that constitute his universe 'interact' with one another, he concludes that what he is capable of producing is 'no explanation, but it is an attempt to penetrate into these mysteries by means of reason'. I take this to be just what one would expect from a metaphysical research programme – a set of criticisable but clearly unscientific claims about the nature of reality. The 3 Worlds of Popper's Reality provide a metaphysical picture that emerges from the acceptance of Philosophical Darwinism. Consistent with Popper's view of the proper function of a metaphysical research programme, we are being directed to places where gaps in our knowledge are to be filled. The gaps caused by empirical conditions demand more scientific firepower in the form of bolder theories and bolder conjectures about the circumstances under which various structures have emerged in the course of evolutionary history. The gaps caused by constraints imposed by adherence to certain methodological norms demand a critical evaluation of the virtues of retaining those norms as opposed to modifying them.

In *The Self and Its Brain*, Popper took issue with Darwin's claim that it is of no material interest whether changed habits lead to new structures or whether new structures lead to changed habits. Popper (Popper and Eccles 1977, p. 13) thought that 'it matters a lot':

> Evolutionary changes that start with new behaviour patterns – with new preferences, new purposes of the animal – not only make many adaptations better understandable, but they re-invest the animal's subjective aims and purposes with an evolutionary significance.... [Thus, for example, w]e could say that in choosing to speak, and to take interest in speech, man has chosen to evolve his brain and his mind; that language, once created, exerted the selection pressure under which emerged the human brain and the consciousness of self.

In 'Evolution and the Tree of Knowledge', Popper made two suggestions for amending Darwinism. One was his conjecture of 'genetic dualism' that stands in contrast to what he took to be the orthodox Darwinian view of genetic monism (Popper 1972 [1981], p. 272). Genetic monism, on Popper's view, is the idea that the same genetic basis underlies both the possession of an organ (such as the eye) and use of that organ (for seeing). Genetic dualism is the conjecture that these two organismic features have distinct genetic bases. The rationale for making this distinction is to allow that behavioural changes may lead to structural changes in underlying organs. The net effect would be a simulation of Lamarckian inheritance. The other was his related suggestion, in an appendix to the original paper, that a behavioral version of Richard Goldschmidt's 'hopeful monster' hypothesis might prove useful in explaining the evolution of complex organs such as the eye (Popper 1972 [1981], p. 283). The idea that behavioral modifications are a significant factor in biological evolution, however, is not new. Stebbins (1977, p. 125) notes that it was first expressed by Darwin. The role of behavioral modifications in both promoting and inhibiting evolutionary change has been discussed by many naturalists both before and after Popper's suggestions (Huey, Hertz and Sinervo 2003).

Throughout his life, and even as his assessment of the scientific status of Darwinian theory was changing, Popper held fast to the view that Darwinism, whatever its scientific shortcomings, was a significant metaphysical hypothesis that was a positive factor in contributing to the growth of our understanding about the evolution of the universe, of life and of knowledge. Darwinism, in this guise, constituted a metaphysical research programme that, for Popper, was not just one research programme among many but was a programme that had no serious competitors. Although metaphysical hypotheses and their associated programmes are not falsifiable or testable, Popper maintained that they are capable of being criticized, and, as such, can be evaluated. What, then, was Popper's view of Darwinism as a metaphysical research programme, and why did he take it to have a significant role in the development of our scientific understanding of the universe?

3.5. Darwinism as a Metaphysical Research Programme

In Section 37 of his intellectual biography, 'Darwinism as a Metaphysical Research Programme,' Popper (1974, p. 133) noted that 'I have always been extremely interested in the theory of evolution, and very ready to accept evolution as a fact. I have also been fascinated by Darwin as well as by Darwinism – though somewhat unimpressed by most of the evolutionary philosophers; with the one great exception, that is, of Samuel Butler.'[2]

In fact, Popper reported that the trial-and-error-elimination model of the growth of knowledge in the *Logic of Scientific Discovery* was intended to suggest the importance of Darwin's theory. Popper (1974, p. 133) saw his epistemological model of the growth of knowledge as possibly illuminating Darwin's biological theory. The first attempt to look at the epistemological consequences of evolutionary theory appeared in some brief remarks in *The Poverty of Historicism* (Popper 1974, p. 133).

Popper associated Darwinism with deductivism, selection and critical error-elimination, as opposed to Lamarckism, which is associated with inductivism, instruction by repetition and justification. This puts Darwinism on the side of the angels in Popper's view. Given Popper's assessment of deductivism, and his view of the selection or critical error-elimination of hypotheses as more or less self-evident rational procedures, he describes Darwinism as 'almost tautological'. The association between deductivism, the critical evaluation of hypotheses and natural selection provides a 'kind of logical explanation of Darwinism' (Popper 1974, p. 134).

Darwinism, as a metaphysical research programme, serves as a 'possible framework for testable scientific theories' guiding and directing research in certain directions and not in others (Popper 1974, p. 134). But Darwinism was not just one metaphysical research programme among others. Popper suggested that 'its close resemblance to situational logic may account for its great success, in spite of the almost tautological character inherent in the Darwinian formulation of it, and for the fact that so far no serious competitor has come forward'. This is an extremely puzzling characterization, since one would suppose, given

Popper's predilection for falsifiable theories that have passed severe tests, that he would not think very highly of an 'almost tautological theory' that presumably is more or less immune to falsification. Popper appeared to have been so impressed with the theory, despite what he took to be its quasi-tautological status, because he saw it exemplifying features that he took to be characteristic of rational procedures in the acquisition of knowledge (Popper 1974, p. 135).

Popper's argument for the non-falsifiability of Darwinism, as he understood it, is not very convincing. He assumed that, on any planet where life forms exhibit variation and heritability, natural selection will act to produce a wide diversity of forms. Suppose, for instance, we discover bacterial life on Mars with only three forms, all closely related to forms on earth. Is Darwinism refuted? Not at all, he said, since we would say that the three forms we found were the only ones sufficiently well adapted to survive. In fact, no matter what the degree of diversity we found, we would say the same. So, Popper (1974, p. 136) concludes: 'Darwinism does not really *predict* the evolution of variety. It therefore cannot really *explain* it.' Perhaps not, but surely this one example does not show that the theory is not testable at all.

Popper further suggested that the theory appears to explain 'adaptations' but does not in fact do so since the word 'adaptation' is used in such a way that 'if the species were not adapted, it would have been eliminated by natural selection'. Contemporary evolutionary theorists, however, draw a distinction between 'adapted' and 'adaptation'. An adaptation is some trait or characteristic that has evolved under natural selection, and so it is a historical question whether or not some characteristic is an adaptation or not. A species or organism is 'adapted' to its environment; however, if there is a 'fit' between the organism and its environment. A given trait may have evolved under natural selection and hence qualify as an adaptation without it being the case that the trait is adaptive for the organism in its current environment. Conversely, an organism might acquire characteristics that enhance its adaptiveness without its being the case that these traits evolved through natural selection. They may have become fixed in the population through random drift or by virtue of their being correlated to traits that did evolve under natural selection. In principle, it is possible to test in the laboratory whether a trait has evolved through drift or selection. Such determinations in the wild are much more problematic but only present technical difficulties and are not, in principle, impossibilities (Popper 1974, p. 137).

Nonetheless, Popper held, the theory 'almost predicts' the great variety of life on earth and by virtue of this it is a useful and 'invaluable' theory despite its untestability. This is somewhat puzzling, since to the extent that Darwinism contributes to the explanation of various facts, one would think that Popper would be committed to its falsifiability. Yet, he does not concede this, but instead emphasizes the extent to which Darwinism guides research in suggestive ways. One way of making sense of these remarks is in terms of the distinction between Darwinism as a scientific hypothesis and Darwinism as a metaphysical programme that is implicit in much of what Popper wrote about Darwinism and evolutionary theory. Popper finally saw the issue in this way in 1977 in his paper 'Natural Selection and the Emergence of Mind', where he finally conceded that the theory of natural selection was indeed testable, although he thought that its role as a metaphysical research programme was still vitally important.

So, what does Darwin's theory do for us, according to Popper? First, it does not predict that a great variety of forms of life on earth will evolve, but it does, Popper claimed, 'suggest it'. Well, perhaps in conjunction with other assumptions it does, but not, as far as I can see, from the characterization supplied by Popper a few pages earlier. Second, Popper thought that Darwin's theory does predict that the evolution of a variety of life forms, if it takes place, will be gradual. In fact, he claimed, this is the central prediction of the theory – in fact (he added, parenthetically), the *only* prediction. Indeed, when Eldredge and Gould (1972) first proposed what came to be known as the theory of 'punctuated equilibrium', a hypothesis that was taken to deny Darwinian gradualism, they were suspected of challenging Darwinism. As the controversy played itself out, it turned out to be more of a tempest in a teapot than a serious challenge to Darwinian theory since what looks gradual from the perspective of one time scale can look quite saltational from the perspective of another. This, of course, occurred after Popper's claims, but it should be clear that without some clear specification of what counts as 'gradual' or what the time scale is, his zeroing in on 'gradual change' as *the* central prediction of Darwinian theory is mistaken. Indeed, in the light of Mayr's reconstruction of Darwinism as a combination of five theories, it is clear that, at least on some plausible understandings of Darwinism, gradualism is *not* a central claim at all and *does not follow* as a predictive consequence from the assumption that natural selection works on variations in populations to produce evolutionary changes (Popper 1974, p. 137).

Having claimed that gradualism is the only prediction of the theory, Popper (1974, p. 138) went on to claim that 'the theory predicts *accidental* mutations'. This led him to suppose that Darwinism is committed to the view that evolution should be more or less of a 'random walk' with changes in one direction followed by changes in other directions, so that the evolutionary history of life on earth ought to resemble the walk of a drunken sailor (Popper 1974, p. 138). But evolutionary history appears to be more unidirectional than that. Popper went on to suggest a modification of the theory to account for the 'orthogenetic trends' that appear in the record of the history of life, but it is not clear that Popper's emendations are either necessary or fruitful (see Watkins 1995).

In *The Self and Its Brain*, Popper argued that adopting Philosophical Darwinism has philosophical implications. First, it rules out epiphenomenalism, since, on Popper's construal, epiphenomenalism cannot account for the influence of World 2 events and processes on World 1 – it cannot account for the causal effect of mind on matter. From the perspective of Philosophical Darwinism, this is not a problem since World 2 emerges from (evolves from) World 1 processes as a result of selection pressures that are generating features that are conducive to the survival and reproductive success of the organisms that possess them. Second, Popper (Popper and Eccles 1977, pp. 86ff) argued that central state materialism or 'identity theories' combined with a Darwinian perspective are also inadequate insofar as it fails to give a proper causal role to World 2 processes and events, since 'mental' processes and events are reduced to physical processes and events (World 1 features). Third, Popper argued that by adopting Philosophical Darwinism and accepting 'the existence of an evolved consciousness, we are led to interactionism' (Popper and Eccles 1977, p. 99).

The bottom line is that Popper saw the Darwinian world view, as he understood it, as a guiding framework for understanding change in the universe at all levels (cf. Cziko 1995).

4. CONCLUSION

What can we conclude about the lasting significance of Popper's evolutionary philosophy? I will leave it to others to address the question of how accurately Popper's views reflect the norms and activities of practicing scientists. Here I want to briefly reflect on two points. First, is there, indeed, the tight connection between Darwinism, evolutionary theory and epistemology that Popper saw and argued for? Second, why did Popper give Darwinism, as a metaphysical research programme, pride of place among other potential competitors?

With respect to the first point, I think we need to distinguish two claims with respect to the relation between evolutionary theory and epistemology. On the one hand, neo-Darwinism is clearly the best explanation we have for the emergence of minded creatures capable of knowledge and critical reflection. The material substrates that make this possible – nervous systems, sensory systems and brains – are characteristic features of organisms that, as far as we can tell, evolved through natural selection and other influences. On the other hand, it is not obvious that the Darwinian model is the best model for the growth of human knowledge either from a phylogenetic or an ontogenetic perspective (Bradie 1986, 1990). There are certainly suggestive analogies between the two processes, but whether there is anything more than that remains to be seen. The problem is that when the details of the evolutionary process that drive biological evolution are spelled out in their gory detail, and when the processes by means of which human knowledge is acquired and developed are spelled out in their gory detail, it is by no means evident that the processes will turn out to be related in interesting ways.

With regards to the second point, Popper saw Darwinian natural selection as providing an alternative explanation for the apparent design in the universe that prompted Paley and others to see the handiwork of God in nature. Nevertheless, Popper (1978, p. 342) says, '[I]t seems that the question [of whether there is evidence of "divine design" in nature] may not be within the reach of science. And yet I do think that science has taught us a lot about the universe that bears in an interesting way on Paley's and Darwin's problem of creative design.' The interesting bearing comes from construing Darwinism as a metaphysical research programme that strongly supports, for Popper, the story of the evolution of the universe and the emergence and distinctive creative nature of World 2 and World 3 (Popper 1978, p. 342).

Writing in 1977, Popper saw science and rationality under attack. The theory of natural selection may have undermined the belief in a Divine Designer, but it opens up the possibility of giving a naturalistic explanation of the emergence of creativity and values. A strictly scientific account may not be forthcoming (as we saw above), but philosophical Darwinism certainly is suggestive about how these capacities might have emerged. Popper (1978, p. 343) concluded that the 'counter-revolution against science is intellectually unjustifiable ... [and] morally ... indefensible'. Science cannot solve all our problems but 'it can sometimes throw some unexpected light even on our deepest and probably insoluble riddles'.

These remarks have a clear bearing on the contemporary challenge to Darwinism posed by defenders of Intelligent Design in the early twenty-first century. This could be construed as a metaphysical research programme as well. What sorts of considerations are relevant in its assessment? The most important reason for not adopting it, from a Popperian standpoint, is its emphasis on searching for completeness or closure. The general strategy of the defenders of Intelligent Design is twofold. First, they argue that there are some phenomena in nature that are not explainable by present Darwinian theories. Second, some of these phenomena are claimed to be 'irreducibly complex' and incapable of being explained by *any* future Darwinian, naturalistic account. Therefore, they conclude, these phenomena must be the product of some intelligent designer. Popper would certainly agree with the first claim. He was also prepared to acknowledge that some phenomena may resist a scientific analysis. For example, when he discussed the emergence of consciousness and the emergence of the world of Objective Knowledge, he was prepared to admit that these transitions may forever lie beyond the scope of scientific explanation (Popper 1978, pp. 343 and 352f.). Nonetheless, he would not endorse the conclusion that the defenders of intelligent design propose. Why not? For one thing, as it stands, the theory of Intelligent Design does not lend itself to the development of a research programme that might lead to testable, falsifiable hypotheses. Now, some defenders of Intelligent Design might be prepared to say that this criticism is just a reflection of a *naturalistic* bias. No doubt Popper would agree. So, why should we be naturalists? This is a large question that I cannot hope to address at any length here. Suffice it to say that to abandon naturalism would be to endorse a methodology that sought completeness and closure over openness and fallibilism. From a Popperian perspective, this would be intellectual suicide.

NOTES

1 I would like to thank the Karl Popper Estate for permission to reproduce this extended quotation from his published work.

2 Popper's admiration for Samuel Butler might seem surprising. But Butler, now best remembered for Erewhon and other satirical novels, also wrote several books on evolutionary theory. *Life and Habit* appeared in 1878, followed by *God the Known and God the Unknown* (1879), *Evolution: Old and New* (1879), *Unconscious Memory* (1880), *Luck, or Cunning* (1886) and *The Deadlock in Darwinism* (1890). Upon reading the *Origin of Species* when it first came out, he became an enthusiastic Darwinian. In time, however, he came to see his views as more in line with those of Lamarck and Darwin's grandfather, Erasmus Darwin. In *Luck, or Cunning*, Butler argued that Darwin's theory could not account for the evolution of goal-directed structures from accidental variations or mutations. Butler's solution was to argue that the emergence of variations is driven by changing acquired needs and desires. Popper's 'genetic dualism' hypothesis is, in part, an attempt to address this same problem. On Popper's view, the evolution of 'behaviour controlling' structures like the central nervous system is driven by genes whose mutations are less likely to be disadvantageous than mutations in the genes driving the evolution of organs. Once the behaviour-controlling structures have evolved, hitherto disadvantageous mutations in the genetic systems for organs become advantageous. Neither Butler's nor Popper's solution has garnered widespread support (but see Akeroyd 2004 for a sympathetic reading of Popper's view).

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NICHOLAS MAXWELL

7 Popper's Paradoxical Pursuit of Natural Philosophy

1. IN PRAISE OF NATURAL PHILOSOPHY

Most philosophers of science assume without question that they pursue a meta-discipline – one that takes science as its subject matter, and seeks to acquire knowledge and understanding about science without in any way affecting, or contributing to, science itself. Karl Popper's approach is very different. His first love is natural philosophy or, as he would put it, cosmology. He expresses the point eloquently in 'Back to the Presocratics' (Popper 1963, p. 136; see also Popper 1959a, p. 15):

> There is at least one philosophical problem in which all thinking men are interested: the problem of understanding the world in which we live; and thus ourselves (who are part of that world) and our knowledge of it. All science is cosmology, I believe, and for me the interest of philosophy, no less than of science, lies solely in its bold attempt to add to our knowledge of the world, and to the theory of our knowledge of the world.

Popper hopes to contribute to cosmology, to our understanding of the world and our knowledge of it; he is not interested in the philosophy of science narrowly conceived as a meta-discipline dissociated from science itself. And yet, as we shall see in more detail below, Popper's pursuit of cosmology is paradoxical: his best-known contribution – his proposed solution to the problem of demarcation – helps to maintain the gulf that separates science from metaphysics, thus fragmenting cosmology into falsifiable science on the one hand and untestable philosophy on the other.

There are several points to note about Popper's conception of cosmology – or natural philosophy as I prefer to call it. The modern sciences of theoretical physics and cosmology are certainly central to natural philosophy. But to say that is insufficient. For, as Popper repeatedly stresses, one should not take disciplines too seriously. What matters are *problems* rather than *disciplines*, the latter existing largely for historical reasons and administrative convenience. The problems of natural philosophy cut across all conventional disciplinary boundaries. How is change and diversity to be explained and understood? What is the origin and the overall structure of the cosmos, and what is the stuff out of which it is made? How are we to understand our existence in the cosmos, and our knowledge and understanding, such as it is, of the universe? These problems are central to the 'disciplines' of theoretical physics, cosmology, biology, history, the social sciences and philosophy – metaphysics, epistemology, scientific method and thought on the brain-mind problem, the problem of how the physical universe and the world of human experience are interrelated.

Popper is at pains to emphasize that modern natural philosophy has its roots in the thought of the Presocratics, around two and a half thousand years ago. The Presocratics were the first to struggle with central problems of natural philosophy in something like their modern form. Their ideas, most notably the idea that there is an underlying unity or invariance in nature, the idea of symmetry, and the idea that nature is made up of atoms in motion in the void, have had a major impact on the development of modern science. But Popper goes further than this. He suggests that modern theoretical physics and cosmology suffer from a neglect of the seminal exploration of fundamental problems undertaken by Presocractic philosophers such as Anaximander, Heraclitus, Xenophanes and above all Parmenides: see especially Popper (1963), chapter 5, and (1998), chapter 7.

Natural philosophy does not just add to the sciences of theoretical physics, cosmology and biology; it gives to these sciences a particular emphasis, aspiration and interpretation. The task is not merely the instrumentalist one of predicting more and more phenomena more and more accurately; it is rather, to explain and understand. This means, in turn, that theoretical physics, pursued as a part of natural philosophy, seeks to enhance our knowledge and understanding of that aspect of the world that lies behind what can be observed, in terms of which observable phenomena can be explained and understood. It commits physics to attempting 'to grasp reality as it is thought independently of its being observed' (Einstein 1949, p. 81). And this, in turn, has implications for specific issues in physics, such as how we should seek to understand quantum theory (QT), irreversibility, relativity theory, the nature of time.

Philosophy and the philosophy of science, pursued as a part of natural philosophy, are, for Popper, very different from the way these disciplines are conceived by most academic philosophers in the twentieth century. Philosophy is not a specialized discipline concerned to solve (or dissolve) technical puzzles about the meaning of words. Its primary task is not to engage in conceptual analysis. Rather, its task is to try to make a contribution to improving our knowledge and understanding of the universe, and ourselves as a part of the universe, including our knowledge. Philosophy has its roots in problems that lie outside philosophy, in the real world, 'in mathematics, for example, or in cosmology, or in politics, or in religion, or in social life' (Popper 1963, p. 72). And the philosophy of science ought to be pursued, not as a meta-discipline with science as its object of study, but rather as an integral part of science itself, an integral part of natural philosophy, seeking to help improve our knowledge and understanding of the cosmos, our place in the cosmos and the miracle of our partial and fallible knowledge of the cosmos.

There is a further point. Popper is adamant that philosophy can learn from science. It is not just that many of the central problems of philosophy have their roots in science. In addition, even though philosophical doctrines, unlike scientific theories, are irrefutable, philosophy can still learn from science how to go about tackling its problems so that progress is made in a way somewhat analogous to progress achieved in science. Philosophical doctrines, even though irrefutable, can be critically assessed from the standpoint of their capacity to solve the problems they were put forward to solve. A generalization of the falsificationist, progress-achieving methods of science – namely critical rationalism – can be put into practice in philosophy so that progress can be made in solving philosophical problems too.

Popper's passionate endorsement of cosmology, or natural philosophy, comes with a fierce condemnation of specialization and what Thomas Kuhn called 'normal science'. The natural philosopher should forego the spurious authority of the expert, and should do his best to communicate simply and clearly, without jargon and, as far as possible, without technicalities only comprehensible to specialists. Natural philosophy needs the love and participation of amateurs; it dies when it becomes the exclusive preserve of professionals.

Did Popper really give his primary allegiance to natural philosophy[1] as I have just characterized it? The following quotations from Popper, in addition to the two given above, show, I think, that he did.

> The belief that there is such a thing as physics, or biology, or archaeology, and that these 'studies' or 'disciplines' are distinguishable by the subject matter which they investigate, appears to me to be a residue from the time when one believed that a theory had to proceed from a definition of its own subject matter. But subject matter, or kinds of things, do not, I hold, constitute a basis for distinguishing disciplines. Disciplines are distinguished partly for historical reasons and reasons of administrative convenience (such as the organization of teaching and of appointments), and partly because the theories which we

construct to solve our problems have a tendency to grow into unified systems. But all this classification and distinction is a comparatively unimportant and superficial affair. *We are not students of some subject matter but students of problems.* And problems may cut right across the borders of any subject matter or discipline. (Popper 1963, pp. 66–67)

Genuine philosophical problems are always rooted in urgent problems outside philosophy, and they die if these roots decay. (Popper 1963, p. 72)

For me, both philosophy and science lose all their attraction when they … become specialisms and cease to see, and to wonder at, the riddles of our world. Specialization may be a great temptation for the scientist. For the philosopher it is the mortal sin. (Popper 1963, p. 136)

[T]he 'philosophy of science' is threatening to become a fashion, a specialism. Yet philosophers should not be specialists. For myself, I am interested in science and in philosophy only because I want to learn something about the riddle of the world in which we live, and the riddle of man's knowledge of that world. And I believe that only a revival of interest in these riddles can save the sciences and philosophy from narrow specialization and from an obscurantist faith in the expert's special skill and in his personal knowledge and authority; a faith that so well fits our 'post-rationalist' and 'post-critical' age, proudly dedicated to the destruction of the tradition of rational philosophy, and of rational thought itself. (Popper 1959a, p. 23)

The First World War destroyed not only the commonwealth of learning; it very nearly destroyed science and the tradition of rationalism. For it made science technical, instrumental. It led to increased specialization and it estranged from science what ought to be its true users – the amateur, the lover of wisdom, the ordinary, responsible citizen who has a wish to know … our Atlantic democracies cannot live without science. Their most fundamental value – apart from helping to reduce suffering – is truth. They cannot live if we let the tradition of rationalism decay. But what we can learn from science is that truth is hard to come by: that it is the result of untold defeats, of heartbreaking endeavour, of sleepless nights. This is one of the great messages of science, and I do not think that we can do without it. But it is just this message which modern specialization and organized research threatens to undermine. (Popper 1983, p. 260)

If the many, the specialists, gain the day, it will be the end of science as we know it – of great science. It will be a spiritual catastrophe comparable in its consequences to nuclear armament. (Popper 1994a, p. 72)[2]

2. DEMARCATION, METAPHYSICS AND UNITY

Popper's rediscovery, advocacy and celebration of natural philosophy is, in my view, of great importance, both intellectually and educationally. But it is, as I have already indicated, paradoxical.

Natural philosophy flourished in the sixteenth, seventeenth and eighteenth centuries, but then suffered a severe setback when Newton's ideas about scientific method became generally accepted, along with his contributions to physics. Newton famously declared, 'I frame no hypotheses' (Newton 1729, p. 547), and claimed to derive his law of gravitation from the phenomena, employing his rules of reason. Subsequently, natural philosophers – or *scientists* – sought to tread in Newton's footsteps, by deriving new laws from the phenomena by means of induction. Natural philosophers no longer needed, it seemed, to engage in debates about metaphysics, epistemology and methodology.[3] Newton had provided a definite method for scientists to follow, which undeniably worked. Natural philosophy became science. This splitting of natural philosophy into science on the one hand and philosophy on the other was reinforced by work produced by 'the philosophers'. Descartes and Locke struggled to make sense of the metaphysical view of the world of the new natural philosophy, and came up with Cartesian Dualism and the representational theory of perception. Their successors – Berkeley, Hume, Kant and others – struggling with the problems bequeathed to them by Descartes and Locke, produced work increasingly remote from science. Eventually, philosophy itself split into two non-communicating schools, so-called 'continental' and 'analytic' philosophy, both remote from science, and the very idea that modern philosophy had begun by trying to make sense of the metaphysics of physics was entirely lost sight of. Natural philosophy all but disappeared.

In view of this massive historical progression, Popper's attempted resurrection of natural philosophy is little short of heroic. Nevertheless, paradoxically, Popper's most famous contribution actually serves to maintain the traditional split between science and philosophy, and in this way serves to continue the suppression of natural philosophy. Popper makes clear near the beginning of his *The Logic of Scientific Discovery* that, in his view, the problem of demarcating science from metaphysics is *the* fundamental problem in the theory of knowledge (Popper 1959a, p. 34), a point often echoed subsequently: see, for example, Popper (1963), p. 42; (1972), pp. 29–31; (1974), p. 976; (1976), p. 79; and (1983), pp. 159–63. His solution, of course, is that theories that are scientific are empirically falsifiable: metaphysical and philosophical ideas, being unfalsifiable, are not scientific. That scientific theories are falsifiable is the key idea of Popper's philosophy of science. Inductivism is fiercely repudiated, but nevertheless the split between physics and metaphysics, stemming from Newton, is maintained. Metaphysical ideas can be, for Popper, meaningful, and may even play an important role in science *in the context of discovery*. But discovery is not rational; it is not 'susceptible' to 'logical analysis' (Popper 1959a, p. 31), and is not subject to scientific method. Metaphysics is not, for Popper, a part of scientific knowledge; it has no rational role to play within science (even though metaphysics may be pursued rationally, that is, critically, and may itself learn from science).[4]

If Popper's solution to the demarcation problem were basically sound, it would place a serious limitation on the scope and viability of natural philosophy, which is based on the *integration* of science and metaphysics. But it is not sound. It is quite fundamentally defective. Once this point has been appreciated, it becomes apparent that a new conception of natural philosophy is required, one that fully integrates science, metaphysics, methodology and philosophy of science in a way which is fully Popperian in spirit, even though it clashes with a number of Popper's views, as we shall see. Both the successes and the failings of Popper's rediscovery of natural philosophy can only be fully appreciated if one recognizes just how powerful – how powerfully *Popperian* – are the arguments in support of the fully integrated conception of natural philosophy I shall now briefly indicate. I here summarize an argument I have developed over many years: see Maxwell (1974); (1976a), chapters 5 and 6; (1984), chapter 9; (1993); (1998); (2002); (2004a); (2004c); (2005a); (2007); (2008); (2011b); (2013); and (forthcoming).

One of the great themes of Popper's philosophy is that we learn through criticism, through subjecting our attempted solutions to problems to critical scrutiny. Falsification is, for Popper, an especially severe form of criticism. This idea requires that assumptions that are substantial, influential, problematic and implicit be made explicit so that they can be subjected to critical scrutiny. If assumptions such as these lurk within science, implicit and unacknowledged, then these assumptions need to be made explicit within science, so that they can be criticized and, we may hope, improved. Just such assumptions do indeed lurk, unacknowledged, within science. They need to be made explicit so that they can be *criticized*.

Physicists only accept theories that are *unified*. That is, in order to be acceptable, a physical theory must be such that its *content*, *what it asserts about the world* (rather than its form or axiomatic structure), is the same throughout the phenomena to which it applies. Newtonian theory is unified because the same laws, $F = ma$ and $F = Gm_1m_2/d^2$, apply to all the phenomena to which the theory is applicable. A version of Newtonian theory which asserts that these laws apply up till midnight, but afterwards $F = Gm_1m_2/d^3$ applies, is disunified because different laws apply before and after midnight. Such disunified theories are never considered in physics. Physics only considers, and certainly only accepts, theories that are unified in the sense that the same laws apply to all the phenomena within the scope of the theory in question. In order to be *explanatory*, a physical theory must be unified in this sense. A theory that asserted that quite different laws applied for different phenomena might predict, but it would not explain. (Consider, for example, the extreme case of a theory which just consists of all the diverse empirical laws governing the diverse phenomena to which the theory applies. Such a theory would predict but, quite clearly, would not explain.)

Given any accepted (unified) physical theory, there will always be endlessly many easily formulatable, empirically more successful, but disunified rivals. (All physical theories are ostensibly refuted by some empirical phenomena; disunified rivals can easily be concocted to give the correct predictions for these recalcitrant phenomena. In addition, independently testable and corroborated hypotheses can be tacked on, to create empirically more successful theories.) Thus physics persistently accepts unified theories in the teeth of endlessly many empirically more successful (but disunified) rivals. This means that physics persistently, if implicitly, accepts a *metaphysical* thesis, to the effect that the universe is such that no disunified theory is true.

If physicists only accepted theories that postulate atoms, and persistently rejected theories that postulate different physical entities, such as fields – even though many field theories can easily be, and have been, formulated which are even more empirically successful than the atomic theories – the implication would surely be quite clear. Physicists would in effect be assuming that the world is made up of atoms, all other possibilities being ruled out. The atomic assumption would be built into the way the scientific community accepts and rejects theories – built into the implicit *methods* of the community, methods which include: reject all theories that postulate entities other than atoms, whatever their empirical success might be. The scientific community would accept the assumption: the universe is such that no non-atomic theory is true.

Just the same holds for a scientific community that rejects – that does not even consider – all disunified rivals to accepted theories, even though these rivals would be even more empirically successful if they were considered. Such a community in effect makes the assumption: the universe is such that no disunified theory is true.[5]

This assumption, however precisely interpreted (see below), is neither falsifiable nor verifiable. For, given any accepted physical theory, T, there will be infinitely many empirically more successful disunified rivals, T_1, T_2, … T_∞. The assumption in question, then, asserts 'not T_1 and not T_2 and … not T_∞'. This assumption cannot be falsified, because this would require that just one of T_1, T_2, … or T_∞ is verified, but physical theories cannot be verified. The assumption cannot be verified either, because this would require that all of T_1, T_2, … T_∞ are falsified, which is not possible since there are infinitely many theories involved.

Being neither falsifiable nor verifiable, the assumption is metaphysical. (For Popper, in order to be metaphysical, it suffices that the assumption is not falsifiable.) *But this assumption, despite its metaphysical character, is nevertheless such a secure part of scientific knowledge that endlessly many theories, empirically more successful than accepted theories, are rejected (or rather are not even considered) solely because they conflict with the assumption*. Popper's own requirements for intellectual integrity and rationality require that this usually implicit and unacknowledged metaphysical component of scientific knowledge be made explicit so that it can be criticized and, perhaps, improved. But this conflicts with, and refutes, Popper's proposed solution to the problem of demarcation. It leads, as we shall see, for wholly Popperian reasons, to a conception of science that integrates falsifiable theory and unfalsifiable metaphysics. It requires, furthermore, that the philosophy of science be an integral part of science itself. The upshot is natural philosophy, full-blooded to an extent that Popper could not envisage, upholding as he did, to the end, his demarcation criterion.[6]

If it were clear what the assumption 'all disunified theories are false' should be taken to be, the outcome of the argument, outlined above, would not be of much importance. What makes it of very great importance is that it is both unclear as to what the assumption should be and of profound significance for theoretical physics that a good choice of assumption is made. But before I can establish these two points I must first solve the problem of what it is for a physical theory to be unified or simple.

3. The Problem of the Unity or Simplicity of Physical Theory

It is widely recognized that when it comes to judging whether a physical theory should be accepted, its unity or simplicity is an important consideration in addition to its empirical success or failure. It is also widely recognized that the unity or simplicity of a theory poses a profound problem. There are two problems. First, what *is* unity or simplicity? Second, what rationale can there be for preferring unified or simple theories to disunified, complex ones?

Explicating what unity or simplicity *is* poses a problem because a unified, simple theory can always be reformulated so as to come out as horrendously disunified and complex, and vice versa, a horribly disunified, complex theory can always be reformulated so as to come out as dazzlingly unified and simple.

Richard Feynman (Feynman et al. 1965, vol. ii, 25, pp. 10–11) gives a beautiful example of the latter process. Consider an appallingly disunified, complex theory, made up of 10^{10} quite different, distinct laws, stuck arbitrarily together. Such a theory can easily be reformulated so that it reduces to the dazzlingly unified, simple form: $A = 0$. Suppose the 10^{10} distinct laws of the universe are: (1) $F = ma$; (2) $F = Gm_1m_2/d^2$; and so on, for all 10^{10} laws. Let $A_1 = (F - ma)^2$, $A_2 = (F - Gm_1m_2/d^2)^2$ and so on. Let $A = A_1 + A_2 + \ldots + A_{10^{10}}$. The theory can now be formulated in the unified, simple form $A = 0$. (This is true if and only if each $A_r = 0$, for $r = 1, 2, \ldots 10^{10}$.)

The reverse process can be performed with equal ease. Given any genuinely unified, simple theory, such as Newtonian theory, say, special terminology can always be defined such that, when the theory is formulated in this terminology, it comes out as horribly disunified and complex (as we shall see below).

The problem is to say what unity or simplicity *is*, and why it is important for science, given that it is so wholly dependent on choice of terminology.

This has long been recognized as a major problem in the philosophy of science. Einstein (1949, p. 23) recognized the problem and confessed he did not know how to solve it. There is now a vast literature expounding failed attempts at solving the problem: see Salmon (1989) and Maxwell (1998), pp. 56–68.

In *The Logic of Scientific Discovery*, Popper sought to solve the problem by identifying simplicity with falsifiability (Popper 1959a, chapter VII). But this proposed solution fails. Given a theory, T, one can easily increase the degree of falsifiability of T by adding on additional, independently testable theories, $T_1, T_2, \ldots T_n$ to form $T + T_1 + T_2 \ldots + T_n$. This latter theory is clearly of greater falsifiability than T. But, in general, it will be, not simpler or more unified than T, but vastly more complex and disunified. Thus simplicity cannot possibly be identified with falsifiability.[7] (This argument refutes not just Popper's theory of simplicity, but his basic doctrine of falsificationism. If $T_1, T_2, \ldots T_n$ are not just independently testable, but have also been empirically corroborated, then, according to Popper's methodology, $T + T_1 + T_2 \ldots + T_n$ should replace T, because the former theory is more falsifiable and its excess empirical content has been corroborated. But in scientific practice, quite properly, $T + T_1 + T_2 \ldots + T_n$ would never be considered for a moment, let alone be accepted. Falsificationism is straightforwardly refuted by proper scientific practice.)

Subsequently, in *Conjectures and Refutations*, Popper makes another suggestion concerning simplicity. He asserts:

> [A] new theory should proceed from some *simple, new, and powerful, unifying idea* about some connection or relation (such as gravitational attraction)

> between hitherto unconnected things (such as planets and apples) or facts (such as inertial and gravitation mass) or new 'theoretical entities' (such as field and particles). This *requirement of simplicity* is a bit vague, and it seems difficult to formulate it very clearly. It seems to be intimately connected with the idea that our theories should describe the structural properties of the world – an idea which it is hard to think out fully without getting involved in an infinite regress (Popper 1963, p. 241).

This gives an excellent intuitive feel for the idea of theoretical unity but hardly solves the problem, as Popper himself in effect admits in the above passage.

I now indicate how the problem is to be solved. All I do here is sketch the solution that I have expounded in much greater detail elsewhere (see Maxwell 1998, especially chapters 3 and 4; 2004a, appendix, section 2; 2004c; and 2007, chapter 14, section 2).

The decisive point to recognize (already hinted at above) is that the unity and simplicity of a physical theory have to do not with the *form* of the theory, its axiomatic structure or patterns of derivations, but with its *content*, with *what it asserts about the world*. In order to solve the problem we need to look not at the theory itself (which is what previously has been done), but at the world – or rather at what the theory asserts about the world. And the crucial requirement a dynamical physical theory must satisfy to be unified is that it is such that it asserts that, throughout the range of phenomena, actual and possible, to which the theory applies, the *same* laws govern the way these phenomena evolve in space and time.[8] A theory that asserts that one set of laws applies to one range of phenomena and a different set of laws applies to a different set of phenomena is, to that extent, *disunified*. And the greater the number of different sets of laws the theory postulates for different ranges of phenomena, the more disunified the theory is. This provides us with a way of specifying the degree of disunity of a theory. A theory that asserts that different sets of laws apply in N different ranges of phenomena (to which the theory applies) is disunified to degree N. For unity, we require that $N = 1$.

It is at once clear that the fact that theories can be reformulated, so that a simple, unified formulation becomes complex and disunified, and vice versa, is no longer a problem. As long as these reformulations leave the *content* of the theories in question unaffected (as the objection presupposes), they do not affect the degree of unity of the theory, as this has just been explicated.

There is a further refinement. Given a theory that is disunified to degree $N > 1$, the question can arise as to *how different, in what way different*, are laws in one range of phenomena from laws in another range of phenomena. Some ways in which sets of laws can differ, one from the other, can be much more dramatic, much more serious, than other ways. This gives rise to different *kinds* of disunity, some being much more serious than others.

Here are five different ways in which dynamical laws can differ for different ranges of phenomena, and thus five different *kinds* of disunity.

(1) T differs in N different space-time regions. Example: The disunified version of Newtonian theory indicated above, with $F = Gm_1m_2/d^2$ up to midnight tonight and $F = Gm_1m_2/d^3$ after midnight. Here, T is disunified to degree $N = 2$ in a type (1) way.

(2) T differs in N distinct ranges of physical variables other than position or time. Example: $F = Gm_1m_2/d^2$ for all bodies except for those made of gold of mass greater than 1,000 tons in outer space within a region of 1 mile of each other, in which case $F = Gm_1m_2/d^4$. Here, T is disunified to degree $N = 2$ in a type (2) way.

(3) T postulates N-1 distinct, spatially localized objects, each with its own unique dynamic properties. Example: T asserts that everything occurs as Newtonian theory asserts, except there is one object in the universe, of mass 8 tons, such that, for any matter up to 8 miles from the centre of mass of this object, gravitation is a repulsive rather than attractive force. The object only interacts by means of gravitation. Here, T is disunified to degree $N = 2$, in a type (3) way.

(4) T postulates N distinct forces. Example: T postulates particles that interact by means of Newtonian gravitation; some of these also interact by means of an electrostatic force $F = Kq_1q_2/d^2$, this force being attractive if q_1 and q_2 are oppositely charged, otherwise being repulsive, the force being much stronger than gravitation. Here, T is disunified to degree $N = 2$ in a type (4) way.

(5) T postulates one force but N distinct kinds of particle. Example: T postulates particles that interact by means of Newtonian gravitation, there being three kinds of particles, of mass m, 2m and 3m. Here, T is disunified to degree $N = 3$ in a type (5) way.[9]

Types (1) to (5) are to be understood as accumulative, so that each presupposes $N = 1$ as far as its predecessors are concerned.

These five facets of disunity all exemplify, it should be noted, the same basic idea: disunity arises when *different* dynamical laws govern the evolution of physical states in different ranges of possible phenomena to which the theory T applies. Thus, if T postulates more than one force, or kind of particle, then in different ranges of possible phenomena different force laws will operate. In one range of possible phenomena, one kind of force operates; in another range, other forces operate. Or in one range of phenomena there is only one kind of particle, while in another range there is only another kind of particle. The five distinct facets of unity, (1) to (5) arise, as I have said, because of the five *different* ways in which content can vary from one range of possible phenomena to another, some differences being *more* different than others.

Let me emphasize once again that the above five facets of unity all concern the *content* of a theory, and not its *form*, which may vary drastically from one formulation to another. One might, for example, split space up into N regions and introduce special terminology for each region so that Newton's laws look very different as one goes from one spatial region to another. Thus, for one spatial region one might choose to write d^2 as 'd^6', even though 'd^6' is interpreted to assert d^2. As one goes from region to region, the *form* of the theory, what is written down on paper, varies dramatically. It might seem that this is a theory disunified to degree N in a type (1) way – the most serious kind of disunity of all. But as long as *what is asserted, the content,* is the same in all spatial regions, the theory is actually unified in a type (1) way, with N = 1.[10]

It deserves to be noted in passing that this solution to the problem of what it means to say of a theory that it is unified or simple also solves the problem of what it means to say of a theory that it is *explanatory*. In order to be explanatory, a theory must (a) be unified and simple and (b) of high empirical content.

It also deserves to be noted that Popper quite explicitly demands that an acceptable physical theory must satisfy the first of the above five kinds of unity (with N = 1): see Popper (1959a), sections 13–15 and 79. What Popper did not appreciate is that an extension of this requirement of invariance with respect to space and time to include (2) to (5) as well goes a long way to solving the problem of simplicity or unity of theory. He comes closest to this, perhaps, in Popper (1998), chapter 7, but still does not, there, make the decisive point (see n. 8).

4. THE HIERARCHICAL VIEW AND SCIENTIFICALLY ESSENTIAL AND FRUITFUL METAPHYSICS

In Section 2 of this chapter we saw that persistent preference in physics for unified theories over empirically more successful, disunified rivals means that physics makes a persistent metaphysical assumption about the universe, namely that it is such that all disunified theories are false. But now we see that this assumption is open to a range of interpretations, depending on whether we interpret 'disunified' to mean 'disunified in a type (1), type (2) ... or type (5) way'. We have before us, then, five metaphysical theses, which I shall formulate as 'The universe is such that there is a true physical theory of everything which is unified (N = 1) in a type (r) way, with r = 1, 2, ..., 5'. These five theses become increasingly substantial, increasingly contentful, as r goes from 1 to 5. Let us call these five theses 'physicalism(r)'. If $r_2 > r_1$, then physicalism(r_2) implies – but is not implied by – physicalism(r_1). And even more substantial metaphysical theses are available, asserting that the universe is unified, or physically comprehensible (in the sense that *one* kind of physical explanation exists for all physical phenomena). An example is the thesis that the universe is such that the true physical theory of everything is unified in the very strong sense that it unifies matter and space-time into one entity. I shall call this thesis *physicalism*(6).

Which of these available metaphysical theses concerning the dynamical unity of the universe should be accepted by physics as a part of current theoretical scientific knowledge? *Some* such thesis *is* accepted, and *must be* accepted, as we saw in Section 2. It is enormously important, for the progress of theoretical physics, that a good choice of assumption is made. For this assumption determines what theories physicists accept and reject on non-empirical grounds; it also determines in what directions physicists look in seeking to develop new, better fundamental theories. In other words, the assumption is important in the contexts of both discovery and acceptance. If we are fortunate in making an assumption that is true, this will enormously help progress in theoretical physics. But if we make an assumption that is badly false, as seems all too likely, this will very seriously impede progress. Whatever assumption we make, it will be substantial, influential and problematic, all too likely to be false. It thus cries out to be made explicit and thus subject to critical scrutiny within science.

How do we choose? Two conflicting lines of argument lead to two very different choices. On the one hand, we may argue that that assumption should be accepted which has the least content which is just sufficient to exclude the empirically successful disunified theories that current methods of physics do exclude (although it is not easy to see what the assumption, chosen on these grounds, should be). On the other hand, we may argue that that assumption should be accepted which can be shown to be the most conducive to progress in theoretical physics so far. This latter line of argument is thoroughly Popperian in character. The whole point of criticism, for Popper, is to further the growth of knowledge. It makes perfect sense to accept, conjecturally, that assumption which seems to be the best from the standpoint of fruitfulness, its capacity to help promote the growth of scientific knowledge, and then subject it to sustained criticism from that standpoint.

How can we do justice to these two conflicting desiderata? The solution is to satisfy both by adopting not one, but a hierarchy of assumptions: see Figure 7.1.[11] At levels 1 to 6 the universe is asserted to be such that the yet-to-be-discovered true physical theory of everything is unified (N = 1) in the increasingly demanding senses of 'unified' spelled out in r = 1 to 6 above. As we descend the hierarchy, the metaphysical theses, versions of physicalism, become increasingly contentful, potentially increasingly fruitful in helping to promote scientific progress, but also increasingly likely to be false, and in need of revision. Associated with each version of physicalism there is a corresponding

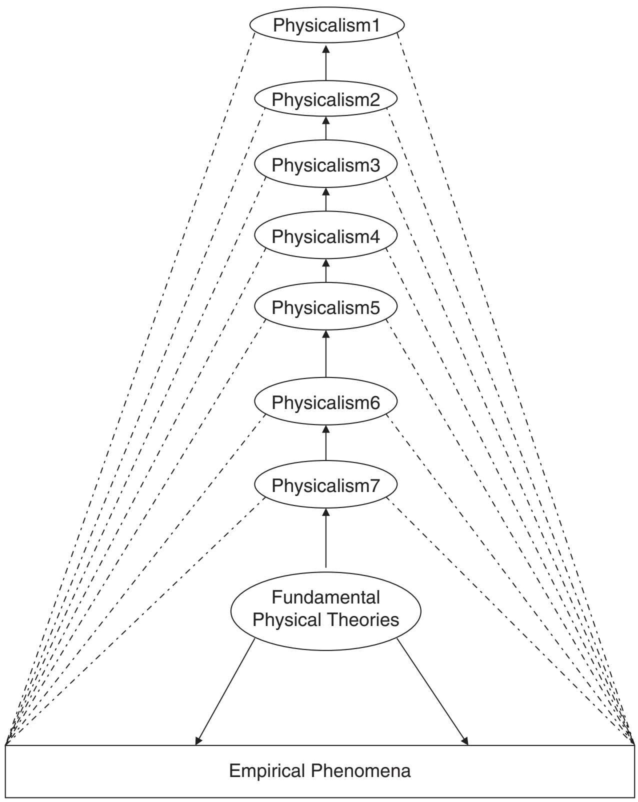

Figure 7.1 The Hierarchical View of Science.

non-empirical methodological principle, represented by dotted lines in the figure, which constrains acceptance of theses and falsifiable theories lower down in the hierarchy.

At level 7 there is an even more contentful, precise version of physicalism, very likely to be false, which specifies the kind of physical entities – or entity – everything is made up of. Examples of theses that have been presupposed at this level 7, taken from the history of physics, include the following. The universe is made up of tiny hard corpuscles which interact only by contact. It is composed of point-atoms which have mass and are surrounded by a rigid, spherically symmetrical field of force which is centrally directed. It is composed of a unified, self-interacting field. It is made up of a quantum field. There is only empty space-time, matter being no more than a kind of topologically complex quantum foam of empty space-time. Everything is composed of some kind of quantum string field in ten or eleven dimensions.[12]

At level 8 there are the currently accepted fundamental theories of physics, at present the quantum field theory of fundamental particles and the forces between them (the so-called standard model) and Einstein's general theory of relativity. At level 9 there are accepted empirical phenomena, low-level empirical laws.

The thesis at level 7 is almost bound to be false (even if physicalism(6) or physicalism(5) is true), just because it is so specific and precise. (The less you say, other things being equal, the more likely it is that what you say is true. 'Ultimate reality is not a chicken' is almost bound to be true of ultimate reality just because there are so many ways of not being a chicken.)

The grounds for accepting physicalism(6) are that this thesis is implicit in the non-empirical methods of theoretical physics, and is the thesis that is the most fruitful, the most conducive to progress in theoretical physics, at that level of generality. Other things being equal, the more nearly a new fundamental physical theory satisfies all six of the above requirements for unity, with N = 1, the more acceptable it would be deemed to be. Furthermore, all new fundamental physical theories, from Newton to today, have brought greater unity to theoretical physics, in one or other of the above five or six senses.

This hierarchical view accords perfectly with the spirit, if not the letter, of Popper's critical philosophy. The idea is to make explicit, and so criticizable and, we may hope, improvable, assumptions implicit in the (non-empirical) methods of physics.[13] The hierarchical view does justice to the two conflicting desiderata indicated above, as no view which specifies just one (possibly composite) metaphysical assumption can do.

So, to sum up, the basic idea behind this hierarchical view can be put like this. For science to proceed, and for the enterprise of acquiring knowledge to proceed more generally, an untestable, metaphysical assumption must be made about the nature of the universe. In order to meet with success, we need to make an assumption that is fruitful and true, but the chances are that the assumption we make will be false. Granted this, in order to give ourselves the best chance of making progress in acquiring knowledge, we need to make not just *one*, but a *hierarchy* of assumptions, these assumptions becoming increasingly insubstantial, and so increasingly likely to be true, as we ascend the hierarchy. We make those assumptions which seem to be implicit in our apparently most successful ventures at improving knowledge, and which seem to be inherently fruitful for improving knowledge, if true. The hierarchy, initially, simply makes explicit what is implicit in what seem to be our most successful efforts at acquiring knowledge. We then *revise* metaphysical assumptions, and associated methodological rules, in the light of which seem to lead to the most empirically successful research programmes, but in such a way that we keep such revisions as low down in the hierarchy of assumptions as possible. Only when efforts at acquiring knowledge seem to be meeting with little success do we actively consider more radical revisions higher up in the hierarchy. We conjecture that the top level 1 assumption in Figure 7.1 is true[14] and the bottom level 7 assumption is false. As we descend from 1 to 7, at some point we move from truth to falsity, and thus to an assumption which needs to be revised. Our hope is that as we proceed, and learn more about the nature of the universe, we progressively bring truth lower and lower down in the hierarchy. Criticism is concentrated where it is most likely to be fruitful, low down in the hierarchy. Furthermore, the framework of relatively unproblematic assumptions and associated methods, high up in the hierarchy, helpfully restricts ideas about how to improve assumptions low down in the hierarchy to just those most likely to be fruitful for progress. As our knowledge improves, assumptions and associated methods improve as well. There is positive feedback between improving knowledge and improving assumptions, and methods – that is, knowledge-about-how-to-improve-knowledge. This positive feedback between improving knowledge and improving knowledge-about-how-to-improve-knowledge is the sine qua non of scientific methodology and rationality. As science improves its knowledge and understanding of nature, it adapts its own nature to what it has discovered. The astonishing progressive success of science in improving our knowledge and understanding of nature owes much to the exploitation of this positive feedback, meta-methodological feature of the hierarchical view in scientific practice. (Even though the scientific community has officially upheld the orthodox view that there are no metaphysical assumptions implicit in the methods of science, fortunately its allegiance to this doctrine has been sufficiently hypocritical to make it possible to implement something close to the hierarchical view in scientific practice.)

5. METAPHYSICAL RESEARCH PROGRAMMES

This hierarchical conception of natural philosophy captures much of what Popper seems to have had in mind in writing of 'cosmology', 'great science' and 'metaphysical research programmes'. It is clear, however, that Popper did not adopt the view, and it is this, to my mind at least, which makes Popper's pursuit of natural philosophy paradoxical. Popper did not abandon his demarcation requirement, which one must do if the hierarchical view is to be accepted. Popper failed to solve the problem of simplicity, encapsulated in the above five facets of unity, and an essential ingredient of the hierarchical view. Popper argued for the causal openness of the physical universe, and for 'downward causation', especially in connection with his interactionist views concerning the mind-body problem: a universe with these features conflicts with physicalism(r), with $1 \leq r \leq 7$ and $N = 1$. Furthermore, Popper argued for the metaphysical, and hence unscientific, character of determinism; but physicalism(r) may be either deterministic or probabilistic, and these metaphysical theses are a part of current (conjectural) scientific knowledge, according to the above hierarchical view.

The key point, of course, is that for Popper the metaphysical theses of metaphysical research programmes are not a part of (conjectural) scientific knowledge. These unfalsifiable theses, for Popper, are upheld in the context of discovery, but not in the context of acceptance. According to the hierarchical view, by contrast, theses at levels 1 to 7 are an integral part of scientific knowledge, despite being untestable and metaphysical. Some of these theses are, indeed, more securely a part of knowledge than any testable physical theory, however well corroborated.[15]

Finally, Popper explicitly rejected the basic argument underpinning the hierarchical view. In Popper (1983), 67–71, he discusses 'silly' rivals to accepted theories – disunified rivals of the kind indicated above – and comments: 'Thus the belief that the duty of the methodologist is to account for the silliness of silly theories which fit the facts, and to give reasons for their *a priori* exclusion, is naïve: we should leave it to the scientists to struggle for their theories' (and their own) recognition and survival' (Popper 1983, p. 70). But this ignores that the 'silly' rivals in question satisfy Popper's own methodological rules, as spelled out in Popper (1959a), better than the accepted theories: these rivals are more falsifiable, not refuted (unlike the accepted theories), the excess content is corroborated, and some are strictly universal. All this holds, for example, if the accepted theory is taken to be T (Newtonian theory, classical electrodynamics or whatever) and the silly rival is taken to be $T + T_1 + \ldots + T_n$, discussed in Section 3 of this chapter. One can scarcely imagine a more decisive refutation of falsificationism. The sillier these silly theories are, the more severe is the refutation. If falsificationism failed to discriminate between a number of reasonably good rival theories even though physicists in practice regard one as the best, this might well be regarded as not too serious a failing. But falsificationism fails in a much more serious way than this; it actually favours and recommends a range of theories that are blatantly unacceptable and 'silly', thus revealing a quite dreadful inadequacy in the view. To argue, as Popper does, that these silly theories, refuting instances of his methodology, do not matter and can be discounted is all too close to a scientist arguing that evidence that refutes his theory should be discounted – something which Popper would resoundingly condemn. The falsificationist stricture that scientists should not discount falsifying instances (especially systematic falsifying instances) ought to apply to methodologists as well!

Popper might invoke his *requirement of simplicity*, quoted above, to rule out these silly rivals, but then, of course, the argument outlined above, leading remorselessly to the hierarchical view, kicks in.

My argument is not, of course, just that Popper blocked the approach to the hierarchical view with invalid arguments. It is, rather, that the hierarchical view succeeds in exemplifying Popper's most basic and finest ideas about science and natural philosophy. It does this more successfully than falsificationism. Popper holds that science at its best proceeds by means of bold conjecture subjected to sustained criticism and attempted refutation. What the above argument has shown is that this process breaks down unless severe restrictions are placed on the conjectures open to consideration – restrictions that go against empirical considerations. Such restrictions commit science to making unfalsifiable, metaphysical assumptions. This in turn requires – given Popper's basic idea – that science must make explicit and severely criticize these assumptions, from the standpoint, especially, of how fruitful they seem to be for scientific progress. In short, empirical testing requires metaphysical criticizing. The one cannot proceed rigorously (i.e. critically) without the other. The outcome is a much-strengthened version of Popper's conception of natural philosophy. Metaphysics forms an integral part of (conjectural) scientific knowledge. The scientific search for explanation and understanding emerge as absolutely fundamental.

Popper's failure to arrive at the hierarchical view had adverse consequences for what he had to say about a number of related issues:

metaphysical research programmes, scientific realism, quantum theory, science as the search for invariance, the incompleteness of physics in principle and the mind-body problem. A few words, now, about each of these issues.

Metaphysical research programmes are discussed, without the term being used, in a number of places (Popper 1990, pp. 1–26; 1994a, chapter 5; 1998, Essay 7; 1999, chapter 6), and are discussed explicitly in at least three places (Popper 1976, sections 33 and 37; 1983, section 23; 1982, sections 20–28). In the last listed source, Popper lists what he claims to be the ten most important, influential metaphysical research programmes in the history of physics: Parmenides's thesis that the universe is a homogeneous, unchanging sphere; atomism; the geometrization programme of Plato and others; Aristotle's conception of essential properties and potentialities; Renaissance physics of Kepler, Galileo and others; the clockwork theory of the universe of Descartes and others; the theory that the universe consists of forces (Newton, Leibniz, Kant, Boscovich); field theory, associated with Faraday and Maxwell; the idea of a unified field (Einstein and others); and indeterministic theory of particles associated with Born's interpretation of quantum theory. Popper comments on these programmes as follows (Popper 1982, p. 165):

> Such research programmes are, generally speaking, indispensable for science, although their character is that of metaphysical or speculative physics rather than of scientific physics. Originally they were all metaphysical, in nearly every sense of the word (although some of them became scientific in time); they were vast generalizations, based upon various intuitive ideas, most of which now strike us as mistaken. They were unifying pictures of the world – the real world. They were highly speculative; and they were, originally, non-testable. Indeed they may all be said to have been more of the nature of myths, or of dreams, than of science. But they helped to give science its problems, its purposes, and its inspiration.

These ten research programmes can be regarded as historically important versions of the hierarchical view, with levels 1 to 6 suppressed. Except that, rather surprisingly, Popper does not, here, characterize the research programmes as being made of *three* levels: basic metaphysical idea (plus associated methods); testable theory; and observational and experimental results. Popper stresses that metaphysical theories, even though not testable, can nevertheless be rationally assessed in terms of their capacity to solve problems, the fruitfulness for science being the 'decisive' issue. Popper also stresses (in line with the hierarchical view) that the search for unity is fundamental to science, to the extent even of declaring 'the fundamental idea of a unified field theory seems to me one that cannot be given up – unless, indeed, some alternative unified theory should be proposed and should lead to success' (Popper 1982, p. 194).

But, despite being 'indispensable for science', and despite helping 'to give science its problems, its purposes, and its inspiration', these 'unifying pictures of the world' are 'more of the nature of myths, or of dreams, than of science'. Popper's conception of metaphysical research programme overlaps with, but also sharply diverges from, the hierarchical view of physics I have indicated above (see also Maxwell 1998, chapters 3–5; 2004a, chapters 1–2 and appendix; 2007, chapter 14). The scientific status of metaphysics is quite different. And Popper's conception lacks the hierarchy of the hierarchical view, and thus lacks the explicit common framework within which competing metaphysical research programmes, of the kind considered by Popper, may be rationally developed and assessed. (The metaphysical theses Popper considers are mostly level 7 ideas, as far as the above hierarchical view is concerned.)

Popper goes on to sketch his own proposal for an eleventh metaphysical research programme: the universe consists of a unified propensity field (Popper 1982, pp. 192–211; see also 1990, pp. 1–26). Popper argues that this incorporates elements from all ten programmes he has discussed. It emerges from Popper's propensity interpretation of quantum theory, to which I now turn.

6. Quantum Theory

In *The Logic of Scientific Discovery* (first published in German in 1934) Popper made an important, though often overlooked, contribution to the interpretation of quantum theory. He refuted decisively the oft-repeated interpretation of Heisenberg's uncertainty relations, which holds that they prohibit the simultaneous precise measurement of position and momentum (and of some other pairs of quantum 'observables'). Popper argues that it is vital to distinguish *selection* and *measurement*. A *selection* is some procedure which, for example, screens off 'from a stream of particles, all except those which pass through a narrow aperture Δx' (Popper 1959a, pp. 225–26). A *measurement* is some procedure which determines the value of some quantum variable or 'observable' such as position, momentum or energy. And Popper goes on to point out that, whereas a selection can be used as a measurement, the reverse is in general not the case. We may use a Geiger counter to measure positions of electrons, but this does not provide a position *selection*.

The distinction that Popper has in mind here was further clarified by Margenau (1958; 1963), who used the term *preparation* rather than *selection*. A preparation is some physical procedure – some combination of screens with slits in them, magnetic fields, and so forth – which has the consequence that if a particle exists (or is found) in some predetermined region of space, then it will have (or will have had) a definite quantum state. A measurement, by contrast, actually detects a particle, and does so in such a way that a value can be assigned to some quantum 'observable' (position, momentum, energy, spin, etc.). And Margenau strongly reinforces Popper's point that a measurement need not be a preparation. Measurements of photons, for example, far from preparing the photons to be in some quantum state, usually *destroy* the photons measured! On the other hand, a preparation is not in itself a measurement because it does not *detect* what is prepared. It can be converted into a measurement by a subsequent detection.[16] Margenau paid tribute to Popper's contribution: see Margenau (1974), p. 757.

Popper (1959a, pp. 223–36) goes on to argue that Heisenberg's uncertainty relations place a restriction on what can be simultaneously *selected* (or prepared), but *not* on what can be simultaneously measured. Consider a stream of electrons moving in a horizontal direction only, with a definite momentum, there being zero motion in other directions. If now the electrons encounter a screen with a narrow horizontal slit in it of width Δx, the electrons which pass through the slit will be scattered, up and down. They will acquire a velocity, a momentum, in the vertical direction, up or down. Heisenberg's uncertainty relations prohibit the *selection* of electrons so that the outcome is electrons with both a precise vertical position and a precise vertical momentum. Furthermore, the smaller the slit Δx is, the greater the resulting scatter will be, and the greater the uncertainty in the resulting momentum of the electrons in the vertical direction. But none of this, Popper points out, prohibits the subsequent simultaneous *measurement* of position and momentum to any degree of accuracy. We may measure *position* subsequently, by means of a photographic plate, for example, and quantum theory places no restrictions on the accuracy of this measurement of position. But this is, simultaneously, a measurement of vertical *momentum*. From the position measurement, the distance between the two screens, the location of the slit in the first screen and knowledge of the horizontal momentum of the electrons, we can deduce what the vertical momentum of each detected electron is. The more precise we make the position measurements, the more precise the simultaneous momentum measurement becomes, and Heisenberg's uncertainty relations place no restrictions whatsoever on how precise these simultaneous measurements of position and momentum can be. The position measurements at the photographic plate which detect electrons simultaneously measure the momentum of the electrons in the vertical direction, and these measurements may be made as precise as we please by placing the photographic plate further and further away from the screen with the slit in it.

Popper (1959a, pp. 230–31) then argues, very effectively, that we need to be able to measure position and momentum simultaneously to a degree of accuracy well within Heisenberg's uncertainty relations *in order to test experimentally the scatter predicted by those relations*. This, to my mind, is the killer blow to the sloppy, customary interpretation of Heisenberg's uncertainty relations as prohibiting precise simultaneous *measurement* of position and momentum.[17]

The distinction, made by Popper in 1934, between selection and measurement, and subsequently elaborated by Margenau, is essential for a clear formulation of orthodox quantum theory, and ought to be an absolutely standard part of any introductory textbook on the subject. It is of far greater importance than Bohr's endlessly parroted idea that wave and particle are complementary, not contradictory, pictures of quantum systems. Whereas the former clarifies the theory, the latter merely obfuscates. Unfortunately, one can still find textbooks ignoring the former and solemnly expounding the latter.

I turn now to Popper's response to what is, in my view, the fundamental, and still unsolved, problem confronting quantum theory: the wave/particle dilemma. Quantum entities, such as electrons and atoms, seem to be both wave-like and particle-like, as revealed in the famous two-slit experiment. How can one have a sensible theory about the quantum domain when the basic entities of this domain seem to have such blatantly contradictory properties?

Orthodox quantum theory (OQT) evades this fundamental problem by being a theory merely about the results of performing measurements on quantum entities. Popper, appalled by the lack of realism of OQT (and even more appalled by the appeal, on some views, to 'the Observer'), developed his propensity idea in the hope that it would provide a probabilistic and realistic interpretation of quantum theory (QT): see Popper (1957); (1959b); (1982); (1983), part II, chapter III. Popper expounded his propensity idea as providing an interpretation of probability theory, but in my view it is best understood as a new kind of dispositional (or necessitating) physical property, like hardness, elasticity, mass or charge, in that it determines how entities interact, but unlike these in determining how entities with propensities interact *probabilistically*.[18] The unbiasedness of a die is an example of a propensity: it causes the die, when tossed onto a smooth table, to land with one or other face up with probability = 1/6. Popper conceives of this as a *relational* property between die and table (and manner of tossing).

Quantum entities, similarly, can, according to Popper, be regarded as having propensities to interact *probabilistically* with measuring instruments, in accordance with the predictions of QT. QT can be interpreted as a theory which specifies what these quantum propensities are and how they change. Electrons and atoms are, for Popper, *particles* with quantum propensities – non-classical relational properties between these entities and measuring instruments. The big difference between the die and the electron is that whereas the probabilistic outcomes of tossing the die are due to probabilistic variations in initial conditions (propensities being eliminable), in the case of the electron this is presumed not to be the case. Dynamical laws governing electrons are presumed to be fundamentally probabilistic, and not reducible to, or explainable in terms of, more fundamental deterministic laws. The apparent wave-like aspect of electrons is not physically real, but contains probabilistic information about an ensemble of similarly prepared, thoroughly particle-like electrons *subjected to certain kinds of measurement*. This idea receives support from the fact that the wave-like aspects of electrons are only *detected* experimentally via the wave-like distribution of a great number of particle-like detections, such as dots on a photographic plate.

Popper's key idea is that, in order to rid OQT of its defects, we need to take seriously the fundamentally *probabilistic* character of the quantum domain.[19] This idea seems to me to be of great importance, and still not properly appreciated by most theoretical physicists even today. But some of Popper's more specific suggestions are unsatisfactory. Popper's propensity interpretation of QT has been criticized for being just as dependent on *measurement*, and thus on *classical physics*, as OQT: see Feyerabend (1968). Popper replied that the propensity of the electron refers not just to measuring instruments, but to 'any ... physical situation' (Popper 1982, p. 71, n. 63).

But this response is unsatisfactory in two respects. First, the 'physical situations' in question are not specified, and second, there is no indication as to how they can be specified in simple, fundamental and purely quantum mechanical terms. The first failure means that Popper's propensity version of QT is either about quantum entities interacting with *measuring instruments* and thus at best a clarification of Bohr's OQT, or it is almost entirely open and unformulated (in view of the failure to specify the relevant 'physical situations'). The second failure means that Popper's propensity version of QT could not be an exclusively *micro-realistic* theory, exclusively about micro systems, in the first instance. Rather, it would be what may be called a *micro-macro realistic* theory, in that it would be about micro systems (such as electrons) interacting with macro systems, relevant macro 'physical situations' with propensities not reducible to the propensities of micro systems.[20] This second failure means that the kind of theory Popper envisages would be as disunified as OQT: some laws apply only to macro systems, and cannot be derived from laws that apply to micro systems. (This defect can only be overcome if QT can be interpreted as attributing propensities exclusively, in the first instance, to *micro-systems*: but it is just this which Popper rejects.)

The crucial issue, which Popper fails to confront, is simply this: What precisely are the physical conditions for probabilistic transitions to occur, what are the possible outcomes, and what probabilities do these possible outcomes have? During the course of expounding his eleventh, unified propensity field research programme, Popper does say:

> It is the interaction of particles – including photons – that is indeterministic, and especially the interaction between particles and particle structures such as screens, slits, grids, or crystals ... a particle approaching a polarizer has a certain propensity to pass it, and a complementary propensity not to pass it. (It is the whole arrangement which determines these propensities, of course.) There is no need to attribute this indeterminism to a lack of definiteness or sharpness of the state of the particle. (Popper 1982, p. 190)

The trouble with what Popper says here is that endless experiments have been performed with interacting particles, and with particles interacting with 'screens, slits, grids, or crystals', which seem to reveal that quantum entities do not interact probabilistically, and do seem to be smeared out spatially in a way that is entirely at odds with these entities being particles. The classic example of this is the two-slit experiment: the interference pattern that is the outcome (detected via a great number of particle-like detections) can be explained if it is assumed that each electron interacts with the two-slitted screen deterministically as a wave-like entity that goes through both slits, and then collapses, probabilistically, to a small region when it subsequently encounters the detecting photographic plate. But if the electron is a particle, and goes through just one slit, it is all but impossible to see how it can interact probabilistically with the screen in such a way as to mimic wave interference, 'knowing' somehow that the other slit is open.

Popper at this point appeals to Landé (1965) who in turn appeals to Ehrenfest and Epstein's (1927) attempted explanation of the two-slit experiment, based on an idea of Duane (1923). Duane's idea is that the two-slitted screen can only take up momentum in discrete amounts, and hence the electron can only be scattered by discrete amounts. But Ehrenfest and Epstein, in their original paper, admit that this attempted explanation is not successful. They conclude their paper with the words: 'It is, therefore, clear that the phenomena of the

Fresnel diffraction cannot be explained by purely corpuscular considerations. It is necessary to attribute to the light quanta properties of phase and coherence similar to those of the waves of the classical theory' (Ehrenfest and Epstein, 1927 p. 407). Duane, and Ehrenfest and Epstein, considered X-ray diffraction, but their conclusions apply to the diffraction of electrons as well. There is, of course, Bohm's (1952) interpretation of quantum theory, which holds electrons to be particles with precise trajectories; but Bohm's theory is *deterministic* and, in addition to particles, postulates the quantum potential, a kind of wave-like entity which guides the flight of the electron (all very different from Popper's propensity idea).

In order to implement Popper's idea properly, in my view, we need to take the following steps. First, we should seek to develop a *fully micro-realistic* version of quantum theory which attributes propensities to micro systems – to electrons, photons, and so on – and specifies precisely how these entities interact with one another probabilistically entirely in the absence of macro 'physical situations' or measuring instruments. Second, we need to recognize that quantum entities, possessing quantum propensities as basic properties, will be quite different from any physical entity associated with *deterministic* classical physics. It is unreasonable to suppose that quantum entities are anything like classical *particles*, *waves* or *fields*. Third, we need to specify *precisely*, in quantum theoretic terms, what the conditions are for probabilistic transitions to occur, what the possible outcomes are, and what their probabilities are. Probabilistic transitions may occur continuously, or intermittently, in time. If we adopt the latter option (which is what QT suggests), we should not be surprised if quantum entities turn out to be such that they become deterministically 'smeared out' spatially with the passage of time, until a probabilistic transition provokes an instantaneous localization. Elsewhere I have developed Popper's propensity idea in this direction, the outcome being a fully micro-realistic propensity version of QT which is, in principle, experimentally distinguishable from OQT: see Maxwell (1976b); (1982); (1988); (1994); (1998), chapter 7; (2004b); (2011c). According to this version of QT, probabilistic transitions are associated with the creation of new 'particles' or bound systems.

Popper argued for scientific realism tirelessly and passionately. Natural philosophy is hardly conceivable without realism, in that it springs from the desire to know and to understand the ultimate nature of the cosmos. Realism is required for explanation. A physical theory is only explanatory if the dynamical laws it specifies are *invariant* throughout the range of phenomena to which the theory applies. At the level of observable phenomena there is incredible *diversity*: only by probing down to the level of unobservable phenomena can invariance be discovered (as when quantum theory and the theory of atomic structure disclose invariance throughout the incredible diversity of phenomena associated with chemistry and properties of matter). But despite his passionate advocacy of scientific realism and the search for invariance, Popper also, at a certain point, turns about and opposes the whole direction of the argument. Popper supports scientific realism but not, as we have seen in connection with quantum theory, *micro-realism*. He holds that the 'fundamental idea' of some kind of 'unified field theory ... cannot be given up' (Popper 1982, p. 194), and argues for theoretical physics as the search for invariance in nature (Popper 1998, chapter 7), but then argues that invariance has its limitations, the physical universe is not closed, physicalism deserves to be rejected, and there is emergence of new physical properties not explainable even in principle in terms of the physical properties of fundamental physical entities, macro systems having physical properties not wholly explicable in terms of the properties of constituents, there being 'downward causation': see, for example, Popper (1972), chapters 3, 4, 6 and 8; 1982; 1998, chapter 7; Popper and Eccles 1977, part I). From the standpoint of the hierarchical view, all this is scientific heresy. It involves rejecting theses at levels 4 to 9 – the most scientifically fruitful metaphysical conjectures we possess.

7. THE PHYSICAL UNIVERSE AND THE HUMAN WORLD

How is Popper's ambiguous attitude to what may be called the scientific picture of the world to be understood? Popper is responding to what might be called the 'double aspect' of modern natural philosophy. On the one hand, it provides us with this magnificent vision of the universe: the big bang, cosmic evolution, formation of galaxies and stars, creation of matter in supernovae, black holes, the mysteries of quantum theory, the evolution of life on Earth. On the other hand, the implications of this vision are grim. If everything is made up of some kind of unified self-interacting field – everything being governed by some yet-to-be-discovered true theory of everything – what becomes of the meaning and value of human life, human freedom, consciousness, everything we hold to be precious in life? Science gives us this awe-inspiring vision and immense power on the one hand, and then takes it all away again by revealing us to be no more than a minute integral part of the physical universe, wholly governed by impersonal physical law in everything we think and do.

Popper believes that if the above picture of the world is correct, and some yet-to-be-discovered physical theory (whether deterministic or probabilistic) is true, the physical universe being closed, then everything that gives value to human life cannot exist (or perhaps could only be an illusion). Human freedom, creativity, great art and science, the meaning and value of human life, even consciousness itself would be impossible. There are, then, for Popper, powerful reasons for rejecting physicalism (with r = 4 to 7). It is metaphysical, not scientific. It is refuted by the obvious fact that theoretical scientific knowledge, not itself a part of the physical universe, can have an impact on the physical world. An obvious example is the explosion of the atomic bombs in Nagasaki and Hiroshima. These terrible physical events could not have occurred without the prior discovery of relevant physical theory. Thus Popper develops his interactionist approach to the mind/body problem: the world of theories, problems and arguments (World 3) interacts with the physical world (World 1) via human consciousness (World 2). The physical universe is open to being influenced by inventions of the human mind. There is emergence and downward causation, and propensities are to be associated with macro physical systems that cannot be reduced to the properties of constituent micro systems.

But all this needs to be contested. Physicalism *is* scientific. It is a part of (conjectural) scientific knowledge, as the hierarchical view makes clear. The scientific view is that the physical universe is causally closed. But this does not mean that physics is all that there is. Physics is concerned only with a highly specific *aspect* of all that there is: it may be called the 'causally efficacious' aspect, that which everything has in common with everything else and which determines (perhaps probabilistically) the way events unfold. Sensory qualities, experiences, feelings and desires, consciousness, meaning and value – all exist and are non-physical. Reductionism (the thesis that everything can be reduced to, or fully explained in terms of, the physical) is false, even though the physical universe is causally closed. As for Popper's argument that atomic explosions establish that World 3 theories can influence World 1 events, it is invalid.

What we need to recognize is that things can be explained and understood in (at least) two very different ways. On the one hand, there are *physical* explanations. On the other, there are what I have called elsewhere *personalistic* explanations – explanations of the actions of people in terms of intentions, beliefs, knowledge, desires, plans, feelings and so on, including the *content* of these things, the possible facts or states of affairs to which they refer.

A beautiful illustration of this distinction between physical and personalistic explanations is to be found in Plato's *Phaedo* 98c-99a. Socrates is in prison awaiting death. Commenting on his disappointment that Anaxagoras had nothing to say about the purposes or reasons underlying the world order, Socrates remarks:

> It was … as if somebody would first say that Socrates acts with reason or intelligence; and then, in trying to explain the causes of what I am doing now, should assert that I am now sitting here because my body is composed of bones and sinews; … and that the sinews, by relaxing and contracting, make me bend my limbs now, and that this is the cause of my sitting here with my legs bent … Yet the real causes of my sitting here in prison are that the Athenians have decided to condemn me, and that I have decided that … it is more just if I stay here and undergo the penalty they have imposed on me. For, by the Dog, … these bones of mine would have been in Megara or Boetia long ago … had I not thought it better and nobler to endure any penalty my city may inflict on me, rather than to escape, and to run away.

This passage is quoted by Popper (Popper and Eccles 1977, p. 170) to indicate the distinction between a physical explanation and an explanation in terms of 'intentions, aims, ends, motives, reasons and values to be realized' (Popper and Eccles 1977, p. 170) – what I am calling here a *personalistic* explanation. Popper assumes, in effect, that if physicalism is true, the physical universe is closed and a physical explanation of Socrates' movements in principle exists, then no personalistic explanation of why Socrates remains in prison can be viable. But what this overlooks is a view that I, and others, have defended according to which personalistic explanations may have real content and force even though physicalism is true. According to this anti-reductionist version of physicalism, personalistic explanations are compatible with, but not reducible to, physical explanations. This is a view that I have developed over a number of years: see Maxwell (1966); (1968a); (1968b); (1984), pp. 171–89 and chapter 10; and especially (2000); (2001); (2009); (2010); (2011a). For related ideas, see Taylor (1964), Dennett (1971; 1984; 1989), Nagel (1986) and Chalmers (1996).

It is almost a miracle that people (and animals, by extension) should be amenable to these two kinds of explanation simultaneously. This miracle is to be understood by an appeal to history, to evolution and to Darwin's theory of evolution.[21] If a purely physical explanation of an atomic explosion were in principle (not, of course, in practice) possible, it would explain merely by showing how one (highly complex) physical state of affairs follows from a prior state in accordance with fundamental physical laws. It would leave out what the personalistic explanation can render intelligible, namely what prior intentions, plans, knowledge and human actions led up to manufacturing and exploding the bomb. The physical explanation would describe all this *physically*, but with the experiential, personalistic aspect left out.

Popper's arguments may be valid when directed against the versions of physicalism he considers: radical physicalism or behaviourism, panpsychism, epiphenomenalism and the identity theory of U. T. Place and

J. J. C. Smart (Popper and Eccles, 1977 chapter P3). But they are not valid when directed against the anti-reductionist version of physicalism just indicated. No longer is it possible to argue that physicalism is not viable because it cannot explain the role that scientific discoveries can have in helping to bring about subsequent events, such as atomic explosions. Anti-reductionist physicalism makes it possible to explain such events personalistically, a kind of explanation that is fully viable, even though physicalism is true, essentially because it is compatible with, but not reducible to, physical explanations.

Popper's three-worlds, interactionist view may be thought to be, in some respects, heroic, in that it is very much at odds, as I have tried to indicate, with the scientific picture of the world. Interactionism amounts to postulating that tiny, poltergeist-like events occur persistently in our brains. It is a part of Popper's creed, of course, that the philosopher should swim against the tide of fashion, and should put forward bold conjectures that challenge current dogmas. All this is admirable.

What is less admirable, perhaps, is the way in which Popper ignored that anti-reductionist version of physicalism, indicated above, which constitutes a counterexample to his whole argument – the version of physicalism which holds that the physical universe, though closed causally, is not closed explanatorily, in that non-physical, experiential features of people and things exist and can be explained and understood personalistically, a mode of explanation compatible with, but not reducible to, physics. Personalistic explanations refer to the non-linguistic content of beliefs, conjectures and so on, to possible facts or states of affairs, in other words. These contents stand in for Popper's World 3 'theories' or 'propositions', but have none of the highly problematic, objectionable features of Popper's World 3 entities.

In order to see how this kind of view can do what Popper claims for his three-worlds view without employing anything like his quasi-Platonic, poltergeistic World 3 entities, consider the central candidate for a World 3 entity – the proposition. This can be regarded as a useful fiction. Beginning with unproblematic utterances and facts, we can arrive at propositions in the following six steps.

First, we consider not just facts but also possible facts, possible states of affairs, including non-existent facts, ostensible facts asserted by false statements. We pretend that possible facts exist – though, of course, those that correspond to false statements do not.

Second, we consider all utterances, however spoken or written, that are utterances of one and the same sentence – 'snow is white' for example, or 'Die Frau ist schön'. Even though a great variety of different sounds or marks on paper are made, we can nevertheless say that one and the same sentence is uttered or written down, just as long as these different sounds or marks do indeed correspond to the same sentence – the English sentence 'snow is white', let us say. Even if a person merely thinks the sentence, we can still say it is this sentence that is thought.

Third, in a way that is closely analogous to the above, we can consider all declarative sentences, or statements, in whatever form or language they may come, that assert the same possible fact, possible state of affairs, and declare that all these are the same proposition. Even though a great variety of different sentences (or strings of sentences) are, on different occasions, uttered, written down or thought by different people – in the same language or in different languages – nevertheless we declare these people to be asserting, or thinking, the same proposition. And just as many different noises or marks can be the same sentence without this meaning that the sentence is itself somehow an extra-linguistic (World 3) entity, distinct from its many actual expressions, so too the fact that the assertion of many different sentences on different occasions can all be the assertion of the same proposition does not mean that the proposition is some extra-linguistic (World 3) entity distinct from its many assertions on many different occasions by means of different sentences.

Fourth, we now imagine that propositions are precise and unambiguous in the sense that, given any proposition, there corresponds, unambiguously, a definite possible fact or state of affairs, asserted by the proposition, which must obtain if the proposition is to be true.

Fifth, we consider not just propositions that are in fact asserted or considered by someone on some occasion but, in addition, propositions, corresponding to some possible state of affairs, which could be asserted or considered.

Sixth, and finally, we consider, in addition, propositions – corresponding to possible states of affairs – that could never be uttered by anyone, because there are infinitely many of them (the consequences of a theory perhaps) or because it would take infinitely long to state just one such proposition.

As a result of taking these six steps, we have arrived at fictional entities, propositions, which do not exist but which it is very useful to pretend do exist. Propositions, in this sense, stand in for Popper's disembodied, poltergeistic, World 3 intellectual entities. Personalistic explanations of human actions, including those that refer to scientific theories being used to create new technology, can refer to propositions in the sense indicated.

It might be objected that personalistic explanations, interpreted in this way, appeal to fictional entities, to entities that do not exist. These explanations are therefore false, and thus not viable. I have four replies to this objection. First, many viable scientific explanations are false – in that they employ false scientific theories. Being false is not sufficient to render an explanation unviable. Second, those personalistic explanations which explicitly formulate the propositions employed in the explanation thereby ensure that these propositions do exist, as linguistically formulated statements. Third, personalistic explanations need, in the main, to refer to and use the content of propositions, rather than the propositions themselves – what the propositions assert to be the case, in other words. The content of a proposition may be perfectly real even though the proposition itself is a fictional entity, and thus something which does not exist in its own right. Fourth, many clearly viable personalistic explanations refer to the content of false beliefs. That there are no facts corresponding to these (false) beliefs does not render the explanations invalid.

Einstein once remarked: 'Knowledge exists in two forms – lifeless, stored in books, and alive in the consciousness of men. The second form of existence is after all the essential one; the first, indispensable as it may be, occupies only an inferior position' (Einstein 1973, p. 80). This, in my view, does better justice to what really matters than Popper's emphasis on 'objective knowledge' and 'epistemology without a knowing subject' (Popper 1972, chapters 3 and 4).

8. THE SIGNIFICANCE OF NATURAL PHILOSOPHY FOR EDUCATION

I conclude with a few words about the educational significance of natural philosophy.

Many scientists, and science teachers, regret the current 'flight from science' – the increasing tendency of young people today to choose subjects to study other than science. A number of remedies are tried, from science festivals to participatory science education. But there is one possible source for this current loss of interest in science that tends to be overlooked: pupils and students are given no opportunity to do natural philosophy.

Why is the sky blue? Where does rain come from? Why does the sun, every day, rise in the east, travel across the sky and sink in the west? Why does the moon shine? Why does the sun? What is everything made up of? How does space end? Where did everything come from, and how will everything end? How did people come into existence? What about animals, and plants? How do we see the world around us? What happens in our heads when we talk to ourselves silently, picture places we have visited, or think?[22]

Every child, and every student, from five years of age on should get the opportunity to ask, and to try to answer, questions such as these. They should get the opportunity to hear what their contemporaries think about these questions, and how one might go about choosing between different answers. When pupils have become actively engaged in pursuing natural philosophy, the suggestions of others can be introduced into the discussion. Democritus, Galileo, Newton, Faraday, Darwin can be introduced, not as authorities, but as fellow natural philosophers whose ideas deserve to be treated on their merits. Science, encountered in this way, as an opportunity to do natural philosophy, might gradually become what it ought to be – a vital part of our general culture.

The hope behind getting children to engage in natural philosophy is not, of course, that they will rediscover for themselves the path of modern science. The idea, rather, is that it is only if one has oneself struggled with a problem that one is in a position fully to enjoy, appreciate, understand and rationally assess the vastly superior attempted solutions of others. All too often science education amounts to indoctrination, in that one is informed of solutions without even being informed of what the problems were that led to the solutions, let alone being given an opportunity to think about the problems for oneself in the first place. Despite the influence that Popper's ideas have had on science education, it is still the case that science is taught as the acquisition of information and skills, rather than being what it ought to be – an opportunity to do natural philosophy.[23]

NOTES

1 Is it really appropriate for me to use the phrase 'natural philosophy' when it does not appear in the index of any of Popper's books, and Popper in relevant contexts in the main speaks of cosmology, or of great science? The problem with Popper's preferred term of 'cosmology' is that it is misleading, in that cosmology is now a recognized scientific discipline, alongside theoretical physics, astronomy and astrophysics. 'Natural philosophy' is much more appropriate, in that it alludes to natural philosophy as pursued by Galileo, Descartes, Hooke, Newton, Boyle, Leibniz and others of the seventeenth century, which intermingled physics, mathematics, astronomy, philosophy, metaphysics, epistemology, methodology and even theology. In any case, as Popper himself persistently reminds us, words do not matter. In at least one place, however, Popper does refer to natural philosophy. He writes: 'It is the great task of the natural sciences and of natural philosophy to paint a coherent and understandable picture of the Universe. All science is cosmology, and all civilizations of which we have knowledge have tried to understand the world in which we live, including ourselves, and our own knowledge, as part of that world' (Popper 1982, p. 1).

2 See also Popper (1982), pp. 172–73; (1983), p. 8; (1994a), pp. 109–10.

3 But natural philosophy is not yet quite dead. For a great contemporary work of natural philosophy, see Penrose (2004).

4 As we shall see, this attitude of Popper towards metaphysics did not really change, even later on in his life when he came to write about 'metaphysical research programmes'. Popper himself was quite explicit on this point: see, for example, Popper (1999), pp. 76–77.

5 This needs to be amended to read 'no *precise* disunified theory is true'. Unified theories entail endlessly many *approximate* disunified theories: the true, unified theory of everything (supposing it exists) will entail such true disunified theories as well.

6 I make no apology for suggesting improvements to Popper's philosophy. The highest compliment you can pay a philosopher is to suggest improvements to his work. It shows you take his problems, and his attempted solutions, seriously. The second highest compliment is to criticize. That shows that, even though you can't suggest improvements, you can at least suggest what those who seek improvements need to grapple with. Finally, merely to give an exposition of a philosopher's work is no compliment at all, but something close to an insult. It suggests the philosopher in question failed to make his thought clear. In the case of Popper, who was so supremely lucid, this would be the ultimate insult. Popper ought to have approved of the attempt to improve his ideas. He certainly thought progress in philosophy was desirable and possible – if only philosophers would abandon sterile meaning analysis, and instead learn from the way science makes progress, by proposing and critically assessing bold possible solutions to serious problems.

7 Popper puts forward two ways of comparing degrees of falsifiability of theories: by means of the 'subclass relation' and by means of 'dimensionality': see Popper (1959a), chapter VI for details. There are thus two theories of simplicity, corresponding to these two ways of comparing degrees of falsifiability. My refutation of Popper's identification of simplicity with falsifiability applies only to the subclass idea. It does not apply to the dimensionality idea. However Popper, quite properly, declares that if the two methods for comparing falsifiability clash, then it is the subclass method which must be accepted (Popper 1959a, p. 130). In the case of T and $T + T_1 + \ldots + T_n$, the former will, in general, be more falsifiable, and thus simpler, than the latter, if compared by means of dimensionality, but the reverse is the case if the two theories are compared by means of the subclass relation. Thus, since the verdict of the subclass relation is to be accepted if the two clash, Popper's account of simplicity commits him to holding that $T + T_1 + \ldots + T_n$ is simpler than T.

8 In a fascinating essay, Popper discusses the view that '*science is strictly limited to the search for invariants … for what does not change during change*' (Popper 1998, chapter 7, p. 154). But Popper does not here propose that a physical theory is unified or simple if what it asserts is invariant throughout the range of phenomena to which it applies. In other words, invariance is not exploited as providing the solution to the problem of simplicity. And a major theme of the essay is to express reservations concerning the search for invariants. Thus he says, 'though the search for invariants is admittedly one of the most important of all scientific tasks, it does not constitute or determine the limits of rationality, or of the scientific enterprise' (Popper 1998, p. 154).

9 This simplifies what I have spelled out elsewhere. In Maxwell (1998) and (2004a) I distinguish *eight*, rather than just five, kinds of disunity.

10 I have sketched an account of what it is for a theory to be *unified*, but have not said anything about *simplicity*. For that, see Maxwell (1998), chapter 4, section 16; (2004a), pp. 172–74. It is a great success of the theory that it sharply distinguishes the two notions of unity and simplicity. Another success is that the theory does justice to the important role that *symmetry principles* play in theoretical physics since Einstein. Symmetry is an aspect of unity: see Maxwell (1998), pp. 91–92, 94–95, 127–39, 262–64; (2004a), pp. 167–68; (2013), section 4.

11 What is depicted in Figure 7.1 is a specific version of a view – elsewhere called 'aim-oriented empiricism' – that I have developed over a number of years, some versions being more elaborate than others: see Maxwell (1974); (1976a); (1984); (1993); (1998); (2002); (2004a); (2004c); (2005a); (2013); and especially (2007), chapter 14. The view arose as a *modification* of Popper's falsificationism, made to make explicit, and so criticizable, within the context of science, metaphysical assumptions implicit in the methods of science – assumptions which falsificationism does not, and cannot, acknowledge.

12 Elsewhere I have suggested that physics today should accept, as its level 7 thesis, a doctrine I have called 'Lagrangianism'. This asserts that the universe is such that all phenomena evolve in accordance with Hamilton's principle of least action, formulated in terms of some unified Lagrangian (or Lagrangian density): for further details, see Maxwell (1998), pp. 88–9 and pp. 175–76, and references given therein. There are hints, however, in modern physics that Lagrangianism may need to be rejected: see Maxwell (1998), p. 89 and Isham (1997), pp. 94–95.

13 Popper (1959a, pp. 61–62 and 252–53) recognized that metaphysical theses have methodological counterparts and argued, in some cases, for the adoption of the counterparts. What Popper did not appreciate is that the argument works the other way around as well: where a methodological rule has a metaphysical counterpart, the metaphysical thesis needs to be made explicit within science so that it can be criticized and improved, this in turn enabling us to improve the counterpart methodological rule.

14 It may be, of course, that even the top level 1 thesis is false. This possibility is taken into account by more general versions of the hierarchical view that I have formulated and argued for elsewhere (Maxwell 1998; 2004a; 2005a; 2007, chapter 14), which take, as their top thesis, merely that the universe is such that we can acquire some knowledge of our local circumstances. Whatever the universe is like, it can never facilitate the growth of knowledge to reject this thesis! The version of the hierarchical view, sketched here, can be construed to be embedded in one or other of the more general versions of the view.

15 This difference has major implications for scientific practice. The metaphysical theses of Popper's metaphysical research programmes, being adopted in the context of discovery only, can suggest specific research programmes within physics, but cannot determine what testable theories are accepted and rejected. By contrast, the metaphysical theses of the hierarchical view, being adopted as a part of scientific knowledge in the context of acceptance,

constitute research guidelines for the whole physics, and help determine what testable theories are accepted and rejected (in addition to empirical considerations). The different status that metaphysical theses have, in the two views, means that these theses play substantially different roles in scientific practice.

16 The formalism of orthodox quantum theory seems to put all quantum observables on the same footing. In fact, the observable position is fundamental. Measurement of other observables, such as momentum, energy, spin, always involve a *preparation* – so that eigenstates corresponding to the observable in question can be associated with specific spatial regions – plus a *detection*, a position measurement, in one or other region. This combination of preparation and position measurement constitutes the measurement of the observable in question – momentum, spin or whatever. The point that all quantum measurements reduce to measurements of position is made by Feynman et al. (1965, p. 96); for a discussion, see Maxwell (1976b), pp. 661–63.

17 Popper recognizes, correctly, that Heisenberg (1930) holds that his uncertainty relations prohibit precise simultaneous measurement of position and momentum *as far as the future is concerned*, but not *concerning the past*. What Popper objects to is Heisenberg's view that when such simultaneous measurements are interpreted to be about the past, they are meaningless.

18 This is close to Popper's own view of the matter. Thus he says that the propensity view 'allows us to interpret the probability of a *singular* event as a property of the singular event itself, to be measured by a conjectured *potential or virtual* statistical frequency rather than by an *actual* or by an observed frequency' (Popper 1983, p. 359). In other words, Popper's propensity view can be regarded as a new application of the standard frequency interpretation of probability.

19 I may be overstating things a bit here. It is true that Popper does say in one place (Popper 1982, 98–99), 'if we do interpret quantum theory as a theory of physical propensities, then we can solve all those difficulties which have given rise to the Copenhagen interpretation'. Earlier, however, Popper's view was that in order to rid OQT of its defects, we need to take seriously the fundamentally probabilistic character of QT, leaving open the question of whether Nature itself is probabilistic or deterministic. Much of what Popper argued for, in connection with QT, is to be found in Popper (1959a) first published in 1934, when Popper supported determinism, long before the development of his propensity view (Popper 1957; 1959b). Thus, his view that QT is a statistical theory of particles, solving statistical problems, his interpretation of Heisenberg's uncertainty relations as statistical scatter relations, his rejection of wave/particle duality, antirealism and subjectivism – all these points are to be found in the 1934 edition of Popper (1959a). Even Popper's denial of the 'reduction of the wave packet' on measurement, as a real physical process, is independent of his propensity view. The chief function of the propensity view, it seems, is to clarify how the statistical theory of QT can apply to individual quantum systems and measurements.

20 David Miller's discussion of Popper's mature views about propensities would seem to indicate that Popper came to construe propensities quite generally in this micro-macro fashion: see Miller (this volume, pp. 237–40).

21 We require an appropriate version of Darwinism, however, one which gives a role to purpose and personalistic action in evolution, and holds that the mechanisms of evolution themselves evolve as evolution proceeds. In Maxwell (1984, pp. 269–75) I outlined a version of Darwinism I called *the generalized Darwinian research programme*: this holds that all life is purposive, and seeks to help us to understand how purposive life has evolved in a purposeless universe, purposive action playing an increasingly important role in the mechanisms of evolution with the development of evolution by cultural means, biological evolution gradually becoming human history. In Maxwell (2001), chapter 7 I elaborate this version of Darwinism further, and suggest how it might help us to understand the evolution of human qualities we especially value, such as sentience, consciousness, language, personalistic understanding, science, art and free will. In Maxwell (2010), chapter 8 I distinguish nine different versions of Darwinism: the first is purely mechanistic; subsequent versions give greater and greater roles to purposive action in evolution. I like to think that Popper would have approved – at least to some extent – given what he says about evolution in, for example, Popper (1994b), chapter 3. Popper there stresses 'the leading role played in evolution by behaviour and by behavioural discoveries … the development of new behavioural aims, preferences, and skills' (p. 59).

22 It may be objected that it is absurd to think that five-year-olds can produce answers to such questions. Not at all. Young children are obliged to be natural philosophers, in a way in which adults are not, in that they have to create a view of the world around them more or less from scratch. An indication of this is the insatiable curiosity of young children.

23 This theme is elaborated further in Maxwell (2005b).

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8 Metaphysics and Realism

Popper's theory of knowledge was resolutely fallibilist and opposed to all forms of dogmatism. Popper's metaphysics was resolutely realist and opposed to all forms of idealism. Is this not a paradox? Can you be both a realist and a fallibilist? Can you think that the aim of our inquiries is the truth about a world that exists (largely) independently of us and our inquiries and also be sceptical of any claim to have established the truth about that world? Has not the main threat to realism, down the ages, been scepticism?

In fact, the paradox, if there is one, lies elsewhere. Down the ages, defeating scepticism has been the chief motivation for idealist views. Thus, anti-scepticism and antirealism go together, and realism and fallibilism go together as well. Contrary to what might appear at first sight, Popper's critical realist metaphysics and his critical rationalist epistemology are natural bedfellows. Or so I shall argue.

Realism is, first and foremost, a metaphysical view. To be a realist about some entity or type of entity X is to think that X exists independently of ourselves and our doings, 'mind-independently' as it is usually put. Realism can be defended on three levels. First, there is common-sense or 'folk' realism, which stands opposed to philosophical idealism. Common-sense realists assert the mind-independent existence of rocks and rivers, mountains and trees – in general, of observable things. Second, there is scientific realism, which stands opposed to various instrumentalist or fictionalist or antirealist views about science. Scientific realists assert the mind-independent existence of electrons and genes, black holes and curved space-time – in general, of (some of) the unobservable things postulated by science. Third, there is Platonic realism, which stands opposed to various nominalist and physicalist views. Platonic realists assert the mind-independent existence of numbers and sets, problems and propositions – in general, of abstract or Platonic entities.

Popper was a realist on all three of these levels. He defended common-sense realism against the idealisms of Berkeley and Kant and (by implication anyway) their latter-day followers. He defended scientific realism against positivist and instrumentalist views. And in his later writings, he defended what he called 'World 3', a realm akin to (though not the same as) Plato's realm of forms or ideas. Popper was, then, a thoroughgoing realist in his metaphysics.

So far I have spoken of scientific realism as a metaphysical thesis, the assertion of the mind-independent existence of (some of) the unobservable things postulated by science. But Popper's scientific realism is, to begin with, anyway, an epistemological or methodological thesis. It is the thesis that the aim of science is truth. Now, it is possible to seek truth but never find it. It is possible that science is all false and that none of the unobservables postulated by science exist. Conversely, to be a metaphysical scientific realist is to think that science is not all false, that science has actually achieved its aim in this or that respect. Did Popper think that? His fallibilist or critical rationalist epistemology said that science aims at truth but can never be certain or sure that it has found it. But truth and certainty are not the same. Did Popper think that science not only aims at truth but also has sometimes found it, without ever finding certainty? I think that he did. But I admit that this is controversial. Popper's defence of epistemic scientific realism is better known than his metaphysical scientific realism. Worse, he occasionally seemed to be sympathetic to the conclusion of the so-called 'pessimistic induction' – that all scientific theories are false, and that (by implication) all scientific entities non-existent. Despite this, I shall be arguing that Popper was a metaphysical scientific realist as well as an epistemic one.

I begin with Popper's defence of common-sense realism. I then turn to his scientific realism, both epistemic and metaphysical. As we will see, common-sense and scientific realism are natural bedfellows. Once realism is accepted at the common-sense level, it is difficult to prevent it from creeping upwards to encompass the scientific level as well. For, after all, there is no sharp distinction between common sense and science, or between observables and unobservables.

It is different, however, with Platonic realism. As ordinarily conceived, Platonic entities stand outside the spatio-temporal and causal nexus of the world. Popper was aware of this. He tried to bring the Platonic realm down to earth, so to speak, with his evolutionary Platonism. Popper's evolving 'third world' was very different from Plato's eternal world of forms. Indeed, it is a moot point whether an evolving 'third world' is a Platonic realm at all.

1. THREE VIEWS ABOUT PERCEPTION

Where to begin? It is well to begin with perception and the senses. After all, the senses are our windows on the world. It is through the senses that we learn about the world around us. This leads to what is called *naive realism* about perception:

(NR) We perceive external objects directly and as they really are.

Notoriously, sceptics criticised this view, beginning with Pyrrho in ancient Greece. Sceptics distinguished appearance and reality. Things do not always appear to us in perception as they really are. We occasionally suffer illusions and even hallucinations, when appearance and reality do not match up. And because of the ever-present possibility of illusion or hallucination, we can never uncritically rely on the senses to tell us how things really are. Science joins forces with scepticism here. Science often explains why appearance and reality do not match up. For example, straight sticks look bent when half-immersed in water because of the law of refraction of light. Moreover, science teaches us that perception is not a direct process either, that we are removed in time and space from the objects we perceive. A star many light years away may have exploded and ceased to exist by the time we see it! To which philosophers add that a nearby tree might have been zapped out of existence by God by the time we see it!

So, science and scepticism show that naive realism about perception is wrong. What to do? What most philosophers did, for hundreds of years, was to try to preserve a dogmatic view of sense experience in the face of this two-pronged attack. They said that the senses are a source of direct and infallible knowledge after all. The senses tell us, directly and infallibly, about how things *appear* to be, about the appearances or representations or ideas or images or sense-data that objects produce in our minds or brains when we experience them. Thus there arose what John Locke called 'the new way of ideas', and what later was called the 'representative theory of perception' or 'sense-data theory'. I call it *idea-ism* (with a hyphen):

(I) We perceive appearances/ideas/images/representations/sense-data directly and as they really are.

In this way dogmatism regarding the senses was preserved in the face of sceptical and scientific attacks.

Idea-ism was a great mistake – one of the greatest mistakes in the entire history of philosophy. For it paved the way for *idealism* (with an 'l' instead of a hyphen). But before discussing that, let me point out that a third view of perception is possible. This third view preserves realism about perception but abandons dogmatism about perception. We might call it *critical realism*:

(CR) We perceive external objects indirectly and not necessarily as they really are.

This third view simply accepts the sceptical/scientific criticisms of naive realism. It is also, when you think about it, the common-sense view of perception. It is part of common sense that in perception we are not in 'direct contact' with the things we perceive. It is part of common sense that things do not always appear to us in perception as they really are. Naive realism was never a common-sense view; rather, it was a sceptic's whipping boy. As for idea-ism, that is no part of common sense either; rather, it is a dogmatist philosopher's invention. So, common sense, science and scepticism yield critical realism about perception. Dogmatism, on the other hand, yields idea-ism.

Popper (1972, pp. 63, 65) simply denied that ideas or sense-data exist:

> Almost everything is wrong in the common-sense theory of knowledge. But perhaps the central mistake is the assumption that we are engaged in what Dewey called the *quest for certainty*.
>
> It is this which leads to the singling out of … sense data or sense impressions or immediate experiences, as a secure basis of all knowledge. But far from being this, these data … do not exist at all. They are the inventions of hopeful philosophers, who have managed to bequeath them to the psychologists.
>
> I may perhaps mention in passing that [an] argument of Russell's against 'naïve realism', an argument which greatly impressed Einstein [1944, pp. 282–3], is unacceptable. It was this [Russell 1940, pp. 14–15]: 'The observer, when he seems to himself to be observing a stone, *is really, if physics is to be believed, observing the effects of the stone upon himself*. … Naive realism leads to physics, and physics, if true, shows naïve realism to be false. Therefore, naïve realism, if true, is false; therefore, it is false'.
>
> Russell's argument is unacceptable, because the passage which I have italicized is mistaken. *When the observer observes a stone, he does not observe the effect of the stone upon himself* (though he might do so, say, by contemplating a wounded toe), even though he decodes some of the signals that reach him from the stone. (Popper 1972, p. 65)

Of course, there are sensory stimuli or inputs to the sensory system – but we do not perceive these inputs. The inputs cause sensations – but we do not perceive sensations either (though we may introspect them, sometimes). What we perceive, when we perceive, are objects in the world. When an illusion occurs, we do not perceive an object as it really is. When a hallucination occurs, we do not perceive an object at all. But always, when we perceive, it is an external object that we perceive. (For more on Popper's realism about perception and the empirical basis of science, see my 2009.)

2. HOW IDEA-ISM TURNS INTO IDEALISM (1): SECONDARY QUALITIES

I said that idea-ism paves the way for idealism. Let me now explain that. Idea-ism is just a dogmatist view about the immediate objects of perception. It does not deny that external objects exist and have their natures largely independently of us and our perceivings. It merely asserts that we do not perceive external objects; rather, we infer their existence and their nature from what we do perceive, from the ideas that they produce in our minds. The inferences, or some of them, might be precarious. But there is nothing precarious about the foundation of them: we have direct and infallible knowledge of the ideas in our own minds. The sceptic is beaten.

But, clearly, this is only a Pyrrhic victory over the sceptic. The battle may have been won, and a bridgehead of certainty established, but at what a cost! An enormous gap has been opened up between our infallible bridgehead and the territory philosophers hoped to conquer from it. How to move beyond our solipsistic bridgehead into the wider world?

It is at this point that idealist metaphysical views, of one kind or another, start to look attractive. Their attraction is that they close that gap. The gap closes completely if I adopt the extreme version of idealism – solipsism. If the world is my dream, if all that exists is myself and my ideas, then my knowledge is complete as well as infallible. There is no wider world for me to make precarious inferences about. I know, infallibly know, all there is to know. Of course, no philosopher has been lunatic enough, or megalomaniac enough, to go so far as actually to adopt solipsism. (This might not be true. Bertrand Russell [1948, p. 196] says that Mrs Ladd-Franklin wrote to him saying she was a solipsist but was having trouble convincing other people! But if she was really a solipsist, what was she doing writing to Bertrand Russell?) Philosophers stop short of solipsism and go for less extreme idealist views.

The first step down the idealist road is to distinguish the primary from the secondary qualities of external objects, and to defend the subjectivity of the latter. Suppose colours, tastes, smells, sounds and the like exist only as ideas or sensations of various kinds in the minds of perceivers. Then sceptical worries about whether snow is really white, or sugar sweet, evaporate. Sceptical questions like 'Can we be sure that sugar is really sweet, just because it (sometimes) appears to us so?' cease to apply, assuming as they do that reality is tasty, coloured, and the like. When it comes to the secondary qualities, there is no gap between the appearances or ideas and reality. We infallibly know all there is to know about them.

Popper (1963, pp. 115–16) dismissed the doctrine of the subjectivity of the secondary qualities:

> Thus, we shall not … describe only the so-called 'primary qualities' of a body (such as its geometrical shape) as real, and contrast them … with its unreal and merely apparent 'secondary qualities' (such as colour) … the secondary qualities, such as colours, are just as real as the primary ones – though our colour experiences have to be distinguished from the colour-properties of the physical things, exactly as our geometrical-shape-experiences have to be distinguished from the geometrical-shape-properties of the physical things.

This critical realist view leaves room for scepticism. The fact that an object looks red, produces a colour-experience of redness or a redness-sensation, does not guarantee that it is red.

3. HOW IDEA-ISM TURNS INTO IDEALISM (2): BERKELEY

The doctrine of the subjectivity of the secondary qualities narrows the gap between appearance and reality, but it does not close it completely. A more radical and more famous gap-closing exercise is the philosophy of George Berkeley. Berkeley took Locke to be defending the subjectivity of the secondary qualities. (Locke actually had a different view, I think, one closer to common sense, and to science, and to scepticism. But no matter.) And Berkeley, who had the virtue of consistency if no other, saw that Locke's view was completely unstable. If we have only our ideas to go on, how can we say that some of them resemble real qualities of things while others do not? How can we even say that objects cause ideas to arise in our minds when we are never acquainted with these causes? Idea-ism and common-sense realism do not mix. The idea-ist bridgehead against scepticism only intensifies the sceptical problem of appearance and reality. As Berkeley (1710, part I, section 87) said,

> Colour, figure, motion, extension and the like, considered only as so many *sensations* in the mind, are perfectly known … But, if they are looked on as … images … [of] *things* … existing without the mind, then are we involved all in *scepticism*. We see only the appearances, and not the real qualities of things.… So that, for aught we know, all we see, hear, and feel, may … not at all agree with the real things … All this scepticism follows from our supposing a difference between *things* and *ideas* … It were easy to … shew how the arguments urged by *sceptics* in all ages, depend on the supposition of external objects.

Berkeley found a novel solution to the problem of scepticism: abolish the distinction between appearance and reality, get rid of the external objects, adopt a fully fledged idealist metaphysic.

But how fully fledged? Berkeley was no solipsist. He got rid of external objects, but he did not think that the world was his dream. There are other folk in it, too, like you and I. More important, it is not up to Berkeley or any of us what ideas or experiences we have. Vulgar critics of Berkeley, such as Samuel Johnson or Jonathan Swift, missed this point. According to Berkeley, God causes Berkeley's perceptions, and ours as well. The world is not Berkeley's dream, or anyone else's. It is more like God's dream, shared between us all. We are all continually subjected, in our experience, to collective God-induced hallucinations.

Berkeley's philosophy may have vanquished traditional scepticism regarding the senses. But despite his strenuous efforts to persuade us of the contrary, it flew in the face of common sense. It also flew in the face of science. According to Berkeley, the whole of physical science is false because there are no physical objects. Berkeley, being a great philosopher, saw this very well. Science is false, he said, but it need not be true to be good. It is a good and useful instrument for summarising regularities in our experiences. Thus, in Berkeley's philosophy, we see plainly how idea-ism leads to idealism and in turn to instrumentalism or antirealism about the sciences.

Few were convinced by Berkeley's philosophy. As David Stove (1991, pp. 102–03) put it,

> There was only one catch: … no one could believe the world-view to which those arguments of Berkeley led.… for the professional philosophers the great desideratum, after Berkeley, was simply this: a version of idealism which was not, like his, a proper object of general derision.
>
> It was precisely this which Kant appeared … to supply … he seemed to prove … that you could be an idealist without looking a complete fool.

4. HOW IDEA-ISM TURNS INTO IDEALISM (3): KANT

Which brings me to Kant, whose philosophy is not an object of general derision, but rather of general respect. Yet Kant's philosophy was also idealist, in a way, and motivated by dogmatism as Berkeley's was. Kant, like Berkeley, was an idea-ist. Unlike Berkeley and the empiricists, Kant thought that the ideas or appearances of experiences were not simply given to us, either by objects in the external world or by God. Instead, they were partly formed or constituted out of the raw data of sensation by the experiencing mind. This great insight, a permanent contribution to philosophy and the sciences of experience, gave Kant the clue to another problem.

Kant was convinced that mathematics (particularly geometry) and pure natural science (particularly Newtonian mechanics) represented absolutely certain knowledge about the world. The propositions of mathematics and pure natural science were synthetic or empirically true rather than analytic or 'true by definition'. How was such certainty achieved? Kant was roused from his dogmatic slumber by Hume's critique of inductive reasoning. We cannot get certain knowledge of the world by inductive reasoning from experience. So, we have this certain knowledge, and we cannot have got it a posteriori, by reasoning from experience. It follows that we must, somehow, have got it by a priori reasoning. Hence, Kant's big question: How is synthetic a priori certain knowledge possible?

Kant's answer to his big question ran to a big book, the *Critique of Pure Reason*. But notice that Kant's argument for synthetic a priori knowledge rests on the dogmatist assumption that we do have certain knowledge about the world. If we abandon this assumption, Kant's problem disappears. Hume's critique of inductive reasoning then merely shows that the laws of geometry and of pure natural science are synthetic a priori guesses or conjectures about the way the world is.

This is Popper's reading of Kant. Popper (1963, pp. 93–95) wrote:

> Mankind had obtained *knowledge*, real certain, indubitable, and demonstrable knowledge – divine *scientia* or *episteme*, and not merely *doxa*, human opinion.... Hume roused Kant to the realization of the near absurdity of what he never doubted to be a fact. Here was a problem which could not be dismissed. How could [we] have got hold of such knowledge? ... Thus arose the central problem of the *Critique* ... The question was inescapable. But it was also insoluble. For the apparent fact of the attainment of *episteme* was no fact.... Newton's theory is no more than a marvellous *conjecture* ... With this Kant's problem ... collapses ... Kant's ... solution of his insoluble problem consisted of what he proudly called his 'Copernican Revolution' of the problem of knowledge. Knowledge – *episteme* – was possible because we are not passive receptors of sense-data, but their active digestors. By digesting and assimilating them we form and organise them into a Cosmos, the Universe of Nature.... Thus our intellect does not discover universal laws in nature, but it prescribes its own laws and imposes them upon nature.
>
> This theory is a strange mixture of absurdity and truth.

Popper, ever a great admirer of Kant, goes on to spell out the truth in his philosophy, rather than the absurdity. But absurdity there is, idealist absurdity, similar to the idealist absurdity you find in Berkeley.

The Kantian absurdity is that our synthetic a priori certainties concern a Cosmos or Nature that is partly of our own making, a phenomenal world, to be contrasted with the 'noumenal world' of things as they are in themselves independently of our experience of them. This is a form of idealism. The phenomenal world is not exactly our dream, but our construct.

Kant, of course, denied that he was an idealist and composed a 'Refutation of Idealism', as he hopefully called it. The 'refutation' was simply a bare insistence that there is, as well as the phenomenal world, also a 'noumenal world', which lies behind, or causes, or 'grounds' the phenomenal world of sensory stimuli on which we have imposed our a priori laws. Kant insisted that we can know nothing of this noumenal world (apart from the fact that it exists).

Did this really refute Berkeley? Berkeley also had a noumenal world as well as a phenomenal world (putting it in Kantian terms). Berkeley's noumenal world consisted of God, who caused our ideas or sense-data. From a Kantian point of view, Berkeley's mistake was that he engaged in metaphysics (or theology) and assimilated the noumenal realm to the Christian God. Berkeley should instead have remained tight-lipped about the noumenal world, about which we can know nothing. But know-nothing-ism cuts two ways. Kant could not dismiss Berkeley's metaphysics as false, because that would be to know something about the noumenal realm. Kant could only dismiss Berkeley's metaphysics as unfounded. This is no refutation of (Berkeley's) idealism.

The foregoing assumes the 'two worlds' interpretation of Kant's philosophy. This is the interpretation that is assumed, and generalised, by Kant's constructivist followers, as we will see immediately. There is another, realist interpretation according to which there is only one world, and the expressions 'phenomenal world' and 'noumenal world' are just a metaphorical way of drawing attention to the fact that we may not experience the world as it really is. This is consistent with, indeed presupposes, the critical realist view of perception mentioned already. But now, Kant's fundamental problem disappears. Synthetic a priori certainties about the way the (phenomenal) world is turn into synthetic a priori guesses about the way the world is. Just as we can be wrong about a stick dipped in water being bent, though it appears so, so also we can be wrong about physical space being Euclidean, though it appears so. The latter is, of course, precisely what post-Kantian developments in mathematics and the sciences taught us. Not only does Kant's Copernican Revolution in epistemology collapse once the 'two worlds' interpretation is abandoned. His solutions to other problems, like the problem of free will and determinism, also collapse. But this is a long story.

The matter turns on perception, and on whether Kant is a critical realist about perception or an idea-ist. Kant talks of 'appearances', which seems to put him in the idea-ist camp. But the reason why Kant is an idea-ist runs deeper than this. It runs right back to Kant's attempt to prove that Euclidean geometry and arithmetic provide synthetic a priori certainties. This yields the idea that space and time are 'forms of sensibility'. Thus the spatio-temporal identity conditions of objects are cognition-relative features of objects. The objects of perception are spatio-temporal objects. So they must float free of the noumenal realm, whose things-as-they-are-in-themselves are not spatio-temporal. And we must view things-as-they-appear-to-us as distinct phenomenal things, appearances, or ideas, or sense-data. (Kant would object to the term 'sense-data'. His appearances are not data given to us; rather, we construct them out of the raw materials of sensation.) If the noumenal realm is not spatio-temporal, then things-as-they-are-in-themselves do not exist in space and time. Why assume that they exist at all? Why assume that corresponding to the empirical objects, the things-as-they-appear-to-us, there is an equivalent number of things-as-they-are-in-themselves? Could not the noumenal world be a single undifferentiated blob of pure being, not carved up into objects at all? Could it not be Berkeley's infinite spirit (which is also blessed with not being in space and time)? We do not know.

Berkeley and Kant agreed on one thing – that the phenomenal world was not a solipsistic world, but was rather intersubjective, and in that sense objective. Berkeley had a theological explanation of this. Kant had no explanation, and simply assumed that all humans inhabit the same 'phenomenal world'. In this respect the Kantian philosophy is a come-down from Berkeley's, just as phenomenalism (Berkeley *minus* God) is a come-down from Berkeley's philosophy. Contemporary post-Kantian philosophic wisdom has outgrown the assumption of a single 'phenomenal world'.

5. KANT GENERALISED – CONSTRUCTIVISM

Constructivists warm to the idea that things-as-they-appear-to-us are all that we know about, that they are partly constructed or constituted by our mental activity. But why assume that there is only one 'phenomenal world'? Why assume that things-as-they-appear-to-us (humans) are always and everywhere the same in certain fundamental respects? And why confine ourselves to perception? What about the way or ways in which we conceive the world, or theorise about it, or talk about it? Thus the dominant metaphysic of the age is Kantian idealism generalised and relativised to conceptual scheme, or linguistic scheme, or theory, or whatever. There is no unique world-as-it-is-experienced-by humans, or world-as-it-is-conceived-by humans. The world-of-the-Aristotelian differs radically from the world-of-the-Newtonian. The world-of-the-Inuit is not the same as the world-of-the-Kalahari bushman.

This gets really exciting if we drop human chauvinism and bring in non-human animals, too. The world-of-the-chimpanzee is different from the world-of-Albert-Einstein, and both are worlds apart from the world-of-the-honeybee.

Of course, all this Kantian talk about different worlds need not be taken seriously. We can see it just as a striking metaphorical way of drawing attention to the diversity of experience, thought and talk *about the world*. Many talk about different worlds in this metaphorical way, including Popper and I. But conceptual idealists or constructivists do take such talk seriously. For them it is not just a metaphorical way of speaking. They think it utterly naive to suppose that all experience, thought and talk is of one world that is (largely) independent of experience, thought and talk.

Suppose we do view 'different worlds' talk as mere metaphor. Suppose we accept common-sense realism about the observable bits of the world and reject Berkeleyan and other forms of idealism. Then the road to scientific realism lies open to us. For, after all, there is no principled distinction between common-sense realism and scientific realism, between the things we happen to be able to observe and those we cannot. Science grows out of common sense, and scientific realism grows out of common-sense realism. Or so Popper (1963, p. 115) saw it:

> Thus we … take all these worlds, including our ordinary world, as equally real; or better … as equally real aspects or layers of the real world.… It is thus mistaken to say that my piano … is real, while its alleged molecules and atoms are mere 'logical constructions' (or whatever else may be indicative of their unreality); just as it is mistaken to say that atomic theory shows that the piano of my everyday world is an appearance only.…

Others see things differently. They are led to see things differently by sceptical worries. They are led to see things differently, above all, by the problem of induction – the same problem that had roused Kant from his dogmatic slumber. This brings me to antirealism about science as an antidote to scepticism.

6. SCIENTIFIC ANTIREALISM VERSUS SCEPTICISM

The problem of induction centres on the fact that any general theory transcends experience. To use time-honoured examples, no experiences of white swans or black ravens, however numerous, can establish that *all* swans are white or *all* ravens are black. Genuine scientific theories transcend experience in two other ways than mere generality. First, they transcend experience in mathematical precision. Some theories are precise, all measurements more or less imprecise. Euclidean geometry tells us that the angles of all triangles add up to 180° – exactly 180° – not 180° give or take some margin to allow for errors of measurement. It follows that even if we set aside the problem of universality, and suppose *per impossibile* that we can measure the angles of all the triangles that have been, are, or ever will be, we still cannot be sure that the Euclidean proposition is true. One of the measured triangles might have angles that do not add up to 180°, but by an amount less than the unavoidable imprecision of the measurement, however precise that measurement is. Second, and more important, theories transcend experience in terms of observability. Scientific theories typically describe entities that are not observable, or not directly observable, at all. So even if the observations were complete, and completely precise, there would remain a gap between the observable realm and the unobservables science postulates to explain it. A theory will be false if the unobservables it postulates do not exist – even though it tells us nothing but the truth about observables.

This threefold gap (generality, precision, observability) between scientific theory and observation leaves ample room for sceptical worries. Philosophers and scientists worried about scepticism try in various ways to close the threefold gap. The only way to bridge the logical gap between experience and scientific theory is to water down the theory. And the way to do that is to go in for some kind of antirealist construal of the science. Much philosophy of science is engaged in this antirealist gap-closing enterprise.

The first to emphasise the threefold gap between theory and experience or experiment was the philosopher-scientist Pierre Duhem (see, e.g. Duhem 1954). As a scientist, Duhem was sceptical about the atomic theory. As a philosopher, Duhem saw that if the aim of physical theory was truth, then physics could never be certain from experience that its aim had been achieved. To defeat the sceptic, Duhem watered down the aim. The aim of physical theory is to 'save the phenomena', that is, to deliver the known experimental laws, in the simplest way we can. For Duhem there are only two legitimate ways to criticise a physical theory: you can point to some known experimental law that it does not yield; or, assuming that the theory does 'save the phenomena', you can point to a simpler way of saving them. If both criticisms fail, then (contrary to scepticism) we can be certain that the aim of physical theory has been achieved. Sceptical worries about the truth of the theory fall away. It does not matter that the theory may be precise and the experimental data imprecise – imprecise data can be saved by precise (and therefore simple) laws. It does not matter that the unobservable entities postulated by the theory might, for all its empirical success, not exist. The unobservables postulated by physicists are convenient fictions which exist (or need exist) only in the mind of the physicist. (Duhem evaded the problem of generality simply by assuming that the experimental laws, which it is the task of physical theory to save, are already themselves general.) Although Duhem was no Berkeleyan idealist, his instrumentalist philosophy of science was similar to Berkeley's. And it has been resurrected by the most prominent contemporary antirealist about science, Bas van Fraassen, who calls it 'constructive empiricism'. Popper never discussed van Fraassen's constructive empiricism, but I have done so in Musgrave (1999), chapter 5.

Then there is a Kantian stream of antirealist philosophy of science, *conventionalism*. This is hard to understand, but a silly example may help to begin with. Consider the absurd general principle, 'Nothing in the world is coloured purple'. Here is a way to render this principle immune from any possible refutation by observation: regard any observer who claims to have seen something purple as hallucinating! If we adopt this policy, then 'Nothing in the world is coloured purple' will be immune from sceptical attack and known for certain to be true. Its truth will be a matter of convention, not of trivial linguistic convention, but rather of a convention regarding proper non-hallucinatory experience. Getting closer to home, consider the law of conservation of matter. If a chemistry student claimed to have refuted this law in the laboratory, chemistry teachers would not be impressed. They would put the apparent refutation down to sloppy experimentation. It is a condition of a properly conducted chemical experiment that the weight of the materials you start with is the same as the weight of the materials you end up with. Thus no properly conducted chemical experiment can refute this conservation law. The law is immune from sceptical attack and known to be true. But its truth is a matter of convention, of the convention about what constitutes a properly conducted chemical experiment. Something like this was Poincaré's view of the status of Euclidean geometry. Poincaré was a great mathematician and knew of the invention of non-Euclidean geometry. Still, he thought we would always use Euclid's geometry within physics. This is because, if things ever go wrong with a system of physical theory that includes Euclidean geometry, it will always be simpler to modify other parts of the system and keep Euclid's geometry. So Euclid's geometry is immune from sceptical attack. It is immunised by the decision or convention never to renounce it in the face of recalcitrant experience, but rather to make compensating adjustments elsewhere.

Popper grew up, philosophically speaking, on the fringes of the Vienna Circle, and there he found ample evidence of these antirealist tendencies. The verifiability theory of meaning deemed all general statements meaningless pseudo-statements. It followed that theoretical science, properly understood, could not describe a reality lying beyond the reach of observation, nor could it explain what happens in the observable realm by describing unobservable entities and processes and stating laws governing them.

But if universal statements are not genuine statements at all, what are they and what role do they play in science? The idea was that these 'pseudo-statements' are actually more or less useful rules for deriving predictions about the next case from observations of previous cases. This 'inference licence' view of laws deprives theoretical science of any descriptive or explanatory function.

Some circumvented the traditional problem of induction, the problem of generality, by saying that scientists have no business asserting the truth of any generalisation. Rather, science confines itself to predicting only the next case of that generalisation. Observations of nothing but white swans cannot prove that all swans are white, but they can prove we that the next swan we observe will be white (or will probably be white). Thus Carnap (1950, p. 575), who concluded that general laws are 'not indispensable for making predictions' and that science might as well go from past instances of a general law to a future instance of the general law. Carnap admitted that it was 'expedient ... to state universal laws in books on physics, biology, psychology, etc'. But it was no part of physics, biology or psychology to assert such laws, since their inductive probability in Carnap's system was zero.

Then there were all sorts of attempts to narrow the gap between theory and experience by somehow reducing theory to experience: 'theoretical terms' must somehow be defined or reduced to observational terms. Percy Bridgman was appalled by the Einsteinian revolution in physics, and sought to 'render unnecessary the services of the unborn Einsteins' (Bridgman 1927, p. 24). Never again must we allow the concepts of science to outrun experience in the way Newton's had. Instead, we must give 'operational definitions' of theoretical concepts.

The Vienna Circle philosophers saw that Bridgman's operational definitions would not quite do, and tried to do better. For example, Carnap tried to do better with his 'reduction sentences'. The Vienna Circle philosophers were their own best critics. Hempel wrote a magisterial summary of all this (Hempel 1958), showing that all attempts to cut science down to size had failed. But what, according to Hempel, *was* the 'theoretician's dilemma'? The dilemma was that if the purpose of theoretical science is to predict phenomena, then we don't need scientific theories or 'theoretical concepts' at all! To which the answer is obvious: the purpose of theoretical science is not just to predict phenomena, but rather to explain them.

Thus Popper criticised all this from a realist point of view. Not that prediction is unimportant, for a scientific realist. That a theory about the unobservable bits of the world can successfully predict observable phenomena is the best evidence we can have that the theory has latched on to reality. But for a realist, successful prediction is a means to an end, not the be-all-and-end-all of the activity. For Popper it is emphatically not the case that 'the name of the [scientific] game is saving the phenomena' (to cite van Fraassen's [1980, p. 93] antirealist slogan).

There is a simple and striking way to bring out the difference here. Let T be some powerful, explanatory scientific theory which traffics in unobservable entities and processes in order to explain observable phenomena. And consider what I call the *surrealist transform* T∗ of T: 'The observable phenomena are *as if* T were true'. T∗ has the same predictive power as T does, or is observationally or empirically equivalent to T. If the name of the scientific game is saving the phenomena, T∗ will do just as well as T. And many of the sceptical worries about T evaporate if we switch to T∗. Could a theory yield nothing but true predictions about the observable and yet still be false because it postulates unobservables that do not really exist? Well, yes, realists have to agree that this sceptical scenario is possible. But this sceptical scenario does not matter if we confine ourselves in science to T∗ instead of T. Surrealist transforms have obvious appeal if you want to cut theoretical science down to size in order to avoid scepticism. No detailed reduction of theory to experience is required, like those the Vienna Circle philosophers tried, and failed, to provide. Surrealism is logical empiricism without tears.

You may think that surrealist transforms are silly, a philosophical trick. There is one real precedent for them, however. Philip Gosse was a nineteenth-century scientist and a fundamentalist Christian. How could the Biblical story that God created the universe and everything in it in about 4004 BC be reconciled with the growing geological evidence of the great age of the rocks and the fossils in them? Gosse had a great idea. God created the Universe in 4004 BC *as if* the teachings of geology and palaeontology were true, with fossils in the rocks, and so on. This is the main idea of Gosse's *Omphalos: An Attempt to Untie the Geological Knot* published in 1857, two years before Darwin's *Origin of Species*. 'Omphalos' is Greek for 'belly button', and the book begins with a discussion of whether Adam and Eve had belly buttons, which is a good question for a Biblical fundamentalist, when you think about it. (By the way, Gosse's transform is still going strong among creation scientists who, when push comes to shove, continue to adopt it.) Gosse's transform of geological theory was not quite a surrealist transform, of course, since it has a realist, theological component. Come to think of it, we can reconcile Berkeley's idealism with the teachings of physical science with 'God brings about our experiences *as if* physical science were true'. This, too, has a realist theological component.

Of course, surrealists pay a price. Realist science is explanatory, surrealist science is not. The two-sphere system of ancient astronomy gave a marvellous explanation of the apparent daily motions of the stars. It said that the stars move across the night sky in (arcs of) circles *because* they are fixed on an invisible sphere which rotates once a day about an immobile spherical earth located at its centre. You lose the explanatory force of this (but not its predictive force) if you say that the stars move across the night sky *as if* they were fixed on an invisible rotating sphere. Generally, if T is any theory which explains its phenomena, then 'The phenomena are *as if* T were true' predicts the same phenomena, but does not explain them.

Similarly, instrumentalism ('Theories are instruments for prediction') and conventionalism ('Theories are conventions') both, in their different ways, try to ensure that theories are certain. But again, the price paid is that theories lose their descriptive and explanatory functions. To preserve those, we need critical realism ('Theories are uncertain guesses about the way the world is, with which we explain features of it'). If we want to explain what we experience, we need theories that transcend experience. To have those, we must learn to transcend the horrors of scepticism, as well.

7. SCEPTICISM AND CRITICAL RATIONALISM

Sceptics have, down the ages, produced various more or less radical sceptical scenarios. Descartes imagined that we are all disembodied spirits fed experiences by an Evil Demon determined to deceive us in even the most familiar things. There are no tables or trees, and we have no bodies. The Evil Demon merely fixes it so that all our experiences are as if we have bodies and there are tables and trees. (Berkeley later said this sceptical scenario is how things actually are, and turned Descartes's Evil Demon into his Benevolent God!). In our materialist times, the scenario varies. Might we be disembodied brains kept alive in a vat of nutrient and hooked up to a supercomputer that feeds us experiences indistinguishable from those we actually have? You think you know that you weren't born yesterday? Might not the universe and all its contents have popped into existence (or been created) yesterday, with fossils in the rocks, and people with memories of their childhood and faded photographs and birth certificates to prove them? As for science, might not even an ideal scientific theory be false? By an ideal theory we mean a theory that saves all its phenomena, is maximally simple or elegant or whatever, satisfies to the highest degree any epistemic demands that our epistemology might impose.

What is the point of these scenarios? They are anti-dogmatist scenarios. They are meant to show that we cannot be absolutely certain that there is an external world, that we were not born yesterday, that science ever tells the truth. For to be absolutely certain that P, we must have eliminated all the relevant possibilities in which it is not the case that P. The sceptical scenarios depict possibilities that have not, and cannot be, eliminated. Ergo, say the sceptics, we cannot be absolutely certain that there is an external world, that we were not born yesterday, that science ever tells the truth. The sceptics are *absolutely right*. The philosophical industry devoted to showing that scepticism is contradictory or incoherent is quite misguided.

But the sceptical arguments, and the sceptical scenarios produced in support of them, do nothing to impugn realism, either common-sense realism or scientific realism. Sceptical criticisms are directed not against our claims about the world, but against dogmatism regarding those claims. For example, sceptics produce no criticism of the claim that there is a table in front of me. They only criticise the view that I can be certain that there is a table in front of me just because I seem to see one. Again, inductive sceptics produce no criticism of the claim that the sun will rise tomorrow. They only criticise the view that I can be certain that the sun will rise tomorrow just because it rose every day in the past. Or again, sceptics produce no criticism of the claim that the world was not born yesterday. They only criticise the view that we can be certain that the world was not born yesterday because of current traces of its past. Finally, sceptics produce no criticism of the claim that some successful scientific theory is true. They only criticise the view that the success of a theory guarantees its truth. The nature of sceptical criticism is often misunderstood. Once it is rightly understood, we see that scepticism is parasitic upon dogmatism. As Pascal said, 'Nothing fortifies scepticism more than that there are some who are not sceptics' (*Pensees*, No. 374; 1931, 102).

The scepticism that critical realists endorse is *certainty scepticism*, which is the thesis that all our beliefs are uncertain. There is another scepticism that critical realists need not endorse – *rationality scepticism*, the thesis that all our beliefs are irrational. What is the relationship between scepticism about certainty and scepticism about rationality? Scepticism about certainty will yield scepticism about rationality as well, provided we accept a further principle: that it is only reasonable to believe what is certain, what we can prove or justify. Call this the *justificationist principle*. We can accept scepticism about certainty, yet make room for rationality, if we reject this justificationist principle. This is what critical rationalists do. Critical rationalists say that it is reasonable to believe (accept as true, adopt as true, prefer as true) whatever has withstood our criticisms, our attempts to show that it is false. That a belief has withstood criticism does not make it certain, does not prove it, does not justify it. But it does justify our believing it, tentatively, for the time being. Or so Popper's critical rationalism, as I interpret it, proposes (Popper 1972, p. 82):

> To put it in a nutshell: we can never rationally justify a theory … but we can, if we are lucky, rationally justify a preference for one theory out of a set of competing theories, for the time being; that is, with respect to the present state of the discussion.

On this interpretation (which is admittedly controversial) there is a natural alliance between critical rationalism and critical realism. The upshot is a position that vindicates common-sense realist beliefs in tables and trees, without trying to make them certain. It also vindicates scientific realist beliefs in atoms and molecules, without trying to make them certain. On the one hand we have common sense, science and scepticism (that is, scepticism about certainty). On the other hand we have dogmatism, antirealist views about science, and philosophical idealism. This is the choice that Popper's philosophy presents us with. (For more on Popper's rejection of the justificationist principle and the problem of induction, see my 2004.)

8. 'THIRD WORLD' REALISM?

Popper was both a common-sense and a scientific realist. But was he also a realist about a Platonic realm of abstract entities? His doctrine of a 'World 3' of the objective contents of our thoughts makes it appear so. But appearances are, I shall argue, deceptive.

As ordinarily conceived, abstract entities do not exist in space or time and have no causal powers. There is an enormous gap between such abstract or Platonic entities and the entities of common sense and of science, all of which are spatio-temporal and have causal powers. Popper tries to close that gap and to bring Platonic entities down to earth, so to speak, with his 'evolutionary Platonism'. The entities of Popper's World 3 are not abstract in the traditional senses. They are supposed to exist in time, if not in space, and they are supposed to have causal powers. But both of these suggestions are problematic, to say the least.

That the entities of Popper's World 3 exist in time follows from his repeated insistence that they are produced or created or brought about by the invention of human language, with its descriptive and argumentative functions. Since human language is an evolutionary artefact, so are the entities it brings about. Before humans evolved, with their languages, there were no theories or arguments or problems. Put crudely, a theory does not exist in Popper's World 3 until somebody thinks it up.

Popper occasionally writes in a more Platonistic vein that suggests otherwise. For example, he says:

> [T]here are many theories in themselves and arguments in themselves and problem-situations in themselves which have never been produced or understood and may never be produced or understood. (Popper 1972, p. 116)
>
> Although man-made, the third world (as I understand this term) is superhuman in that its contents are virtual rather than actual objects of thought, and in the sense that only a finite number of the infinity of virtual objects can ever become actual objects of thought. (Popper 1972, p. 159)

But I agree with Brian Carr (1977, p. 216) that such passages, redolent of traditional Platonism, are lapses. Popper's real position is that the third world '*originates as a product of human activity*' (Popper 1972, p. 159), and it can hardly do that if its denizens exist already.

Popper even goes so far as to say that human beings created the archetypical denizens of the Platonic realm, the natural numbers. He writes:

> *Pace* Kronecker, I agree with Brouwer that the sequence of natural numbers is a human construction. But although we create this sequence, it creates its own autonomous problems in its turn. The distinction between odd and even numbers is not created by us: it is an unintended and unavoidable consequence of our creation. Prime numbers, of course, are similarly unintended autonomous and objective facts; and in their case it is obvious that there are many facts here for us to *discover*: there are conjectures like Goldbach's. And these conjectures, though they refer indirectly to objects of our creation, refer directly to problems and facts which have somehow emerged from our creation and which we cannot control or influence: they are hard facts, and the truth about them is often hard to discover. (Popper 1972, p. 118)
>
> Let us look at the theory of numbers. I believe (unlike Kronecker) that even the natural numbers are the work of men, the product of human language and of human thought. Yet there is an infinity of such numbers, more than will ever be pronounced by men, or used by computers. And there is an infinite number of true equations between such numbers, and of false equations; more than we can ever pronounce as true, or false.
>
> But what is even more interesting, unexpected new problems arise as an unintended by-product of the sequence of natural numbers; for instance the unsolved problems of the theory of prime numbers (Goldbach's conjecture, say). These problems are clearly *autonomous*. They are in no sense made by us; rather, they are *discovered* by us; and in this sense they exist, undiscovered,

before their discovery. Moreover, at least some of these unsolved problems may be insoluble. (Popper 1972, pp. 160–61; see also Popper 1994, pp. 19–20)

What are we to make of the idea that we create numbers by talking and thinking? It is false. It makes no sense to suppose that the number 7 popped into existence when somebody first counted up to 7, and did not exist before that moment. Thought and talk cannot create numbers, only number words (numerals) and number concepts. Nor can numbers be 'pronounced by men', though names of numbers can be. What thought and talk created, in short, is the theory of numbers. Whether any numbers exist is a further, metaphysical question.

As for autonomy, the unintended consequences of the creation of (the theory of) numbers, the consequences in question are *logical* consequences of statements about numbers, not (*per impossibile*) unintended *effects* of Platonic objects. As Greg Currie points out, 'discovery in mathematics is the discovery that something follows logically from something else' (Currie 1978, p. 421). Similarly, it is not natural numbers that create odd and even numbers, or prime numbers. Rather, it follows from the postulation that natural numbers exist, that odd and even numbers exist, or that prime numbers exist. We discover these unnoticed and unintended logical consequences, not the objects they are supposed to be about. The 'autonomy of World 3' can be understood entirely in terms of the autonomy of the notions of truth and of logical consequence. We do not need Platonic objects to understand it.

Are not *propositions* abstract Platonic entities? The idea that World 3 consists of propositions sits oddly with the idea that it is a product of human activity. Propositions, viewed as abstract Platonic entities, exist timelessly, and are not created at any particular time by anybody or anything. Popper says: 'We are workers who are adding to the growth of objective knowledge as masons work on a cathedral' (Popper 1972, p. 121). But as Popper himself stresses, a contradictory proposition logically implies any proposition whatever. This means that as soon as a contradiction got into World 3, so did every proposition whatever. 'It looks as though the masons who are building the grand cathedral of objective knowledge have not only completed it but also rather disastrously over-fulfilled their norms' (Cohen 1985, p. 4). This absurd consequence only follows from taking propositions as abstract or Platonic entities. Popper typically writes instead of the 'objective contents of thoughts [and, we might add, of linguistic inscriptions]', where thoughts and inscriptions are concrete 'second world' or 'first world' phenomena. To say that such phenomena have objective contents is just to say that whether what is thought or inscribed is true or false is an objective matter, and whether some other (actual or potential) thought or inscription follows from the first is also an objective matter.

Similar considerations apply to Popper's talk about *'The Causal Relations Between the Three Worlds'* (Popper 1972, p. 155). He does not really think that the third world causally interacts either with the physical world or with the mental world. First, he insists that the third world cannot interact with the physical world except through the intervention of the mental world. He says that 'the mind may be linked with objects of both the first world and the third world', and that '[b]y these links the mind establishes an *indirect* link between the first and the third world'. He continues:

> This is of the utmost importance. It cannot seriously be denied that the third world of mathematical and scientific theories exerts an immense influence upon the first world. It does so, for instance, through the intervention of technologists who effect changes in the first world by applying certain consequences of these theories; incidentally, of theories developed originally by other men who may have been unaware of the technological possibilities inherent in their theories. (Popper 1972, p. 155)

But what is doing the causing here, what is exerting 'an immense influence' on the physical world, is not the theories or arguments, but rather the 'graspings' of those theories and arguments by technologists. (And whether the theories grasped are true, or the arguments grasped valid, remains a separate question – false beliefs, and invalid reasonings, can do work in the world as well.)

Platonic objects are eternal and acausal. Popper's third world does not consist of such objects. It does not, I believe, consist of *objects* at all. It consists (if that is the right word) of objective properties of second- and first-world entities. If this is right, then Popper's realist metaphysics embraced common-sense entities, and scientific entities, but not Platonic entities. And his 'evolutionary Platonism' is really no kind of Platonism at all.

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DAVID MILLER

9 Popper's Contributions to the Theory of Probability and Its Interpretation

0. INTRODUCTION

It has more than once been remarked (by Gillies 1995, p. 103, for example, by Schroeder-Heister 1998, n. 1, by Keuth 2005, p. 166 and by Shearmur 2006, p. 270) that probability, the frequency interpretation in particular, is the most generously treated topic in *Logik der Forschung* (1935a), and axiomatic and formal issues in the theory of probability preponderate in the material added to later editions up to (1994) and to the English translation *The Logic of Scientific Discovery* (1959a). The declared purpose of the three-volume *Postscript* (1982a), (1982b), (1983), the original version of which was written between 1950 and 1957, was to introduce an alternative interpretation of physical probability, the propensity interpretation, and to elicit its significance both for quantum theory and for a new metaphysics of nature. Much other work, scattered through papers and discussion notes, the addenda to *Conjectures and Refutations* (1963a), and elsewhere, applied results in the theory of probability to methodological problems in science. The present chapter surveys this rich field of activity, and identifies and assesses its most signal achievements.

In (1935a) Popper's most pressing problem concerning probability was that 'in modern physics ... we still lack a satisfactory, consistent definition of probability; or what amounts to much the same, we still lack a satisfactory axiomatic system for the calculus of probability'; in consequence, 'physicists make much use of probabilities without being able to say, consistently, what they mean by "probability"' (proem to chapter VIII). Almost immediately, however, he began to distinguish the problem of axiomatization from that of definition or interpretation. Alongside his lengthy exposition of a novel variant of the frequency interpretation (*ibidem*), the text of (1935a) contained brief discussions of other interpretations of the term 'probability', such as the logical interpretation (§ 34) and the subjectivist interpretation (§ 48), according to which probability enters our deliberations only as a palliative for ignorance. In addition, chapter X set out to show that, contrary to the efforts of Reichenbach and of Keynes, in wrestling with the problem of induction, 'it is useless and misleading to employ the concept of probability in connection with scientific hypotheses' (1935b). Only in (1938) did Popper publish his first investigation into probability axiomatics proper. By then he had adopted the policy of treating probability theory as a formal discipline, and of using 'the word "probability" ... for all and only those meanings that satisfy the well known *mathematical calculus of probabilities*' (1983, Part II, § 1). This separation of formal and semantical (interpretive) issues will be followed here. It may be suggested in passing that the theory of probability is that scientific theory that is most painlessly understood along structuralist lines as a family of applications obeying a collection of set-theoretical constraints (an approach first recommended for all empirical theories by Suppes 1957, chapter 12). Many studies that appeal to probability take a wrong turn when they throw away the formal apparatus of probability spaces and talk about such poorly defined quantities as 'the probability of the emergence of life'. On this matter, see also Popper (1935a), § 67.

For the sake of brevity in what follows, a few technical and terminological points should be made at the start. Whether a is an event, a statement or some other item, I shall write p(a) for the *(absolute) probability* of a and p(a, c) for the *relative* (or conditional) *probability* of a given c. Absolute and relative probabilities always lie between 0 and 1 inclusive, and are related by the law $p(a, c) \cdot p(c) = p(ac)$, where ac is the intersection or conjunction of the items a and c. If $p(ac) = p(a) \cdot p(c)$, which Popper sometimes called the *special multiplication theorem*, then a and c are said to be *probabilistically independent*. Provided $p(c) \neq 0$, independence is equivalent to the identity $p(a, c) = p(a)$; it may be somewhat loosely expressed by saying that the occurrence or the truth of c does not affect the probability of a. This notation and terminology will be refined in § 3, but they suffice for the moment.

Popper's version of the frequency interpretation is the subject of § 1, and the successor propensity interpretation is the subject of § 2. In § 3 I explain briefly what is accomplished by Popper's axiomatizations of the probability calculus. In § 4 some comments are offered on the logical interpretation of probability, and on Popper's plentiful and diverse attacks on the doctrine of probabilistic induction. In conclusion in § 5 I venture a personal evaluation.

1. THE FREQUENCY INTERPRETATION OF PROBABILITY

The identification of the probabilities appearing in scientific theories with the corresponding relative frequency limits was advanced by Ellis and Venn in the nineteenth century, but first incorporated into a rigorous mathematical theory by Richard von Mises (1928) and by Reichenbach (1932). Mises proposed that a probability $\mathfrak{p}(\mathsf{a})$ can be attributed to a type of event a or *label* (such as the falling Heads of a coin, or the falling flat of a joke) only if events of type a are classified as members of a sequence called a *collective*, characterized by two fundamental axioms. One is the *axiom of convergence*, to the effect that, as the collective grows in length, the ratio $m(\mathsf{a})/n$ of the number $m(\mathsf{a})$ of occurrences of the label a to the length n of the sequence converges to a limit. Such limits exist, however, in regular (predictable) sequences, such as the sequence of night and day, to whose component events we are not inclined to attribute probabilities. A more serious difficulty is that there exist convergent sequences for which the traditional laws of large numbers of Bernoulli ([o] below) and of Poisson fail. An example given by Mises (1951), pp. 111f. (see Popper 1935a, § 63, n. 1), is the sequence of figures in the penultimate place in a table of square roots of successive natural numbers, calculated to a fixed number of places. In sufficiently far reaches of this sequence most segments of a given length have decidedly unrepresentative distributions. (Compare also the game 'Red or Blue' of Popper 1983, Part II, § 8.) Mises's *axiom of randomness* (or of *excluded gambling systems*) supplied the needed element of chance or disorder: the limiting frequencies of all labels must be unchanged under any method of selection (except those informed by knowledge of the label that in fact occurs) of a subsequence from the original sequence. For example, the relative frequency of Heads in a collective of Heads and Tails should be unaffected when we count only odd-numbered throws, or those preceded by three Tails. This axiom is obviously invalid for regular sequences. Given the identification of probability and frequency, the axiom of randomness is closely related to the assumption that distinct labels are probabilistically independent. For critical commentary on Mises's theory, and further references, see Popper (1935a), § 50; Nagel (1939), chapter II, § 4; Kneale (1949), §§ 32f.; Mises (1964); Fine (1973), chapter IV; Popper (1930–33/1979), Book II, fragment X; Miller (1994), chapter 9, §§ 2f.; Gillies (1995); Gillies (2000), chapter 5; Galavotti (2005), chapter 4, § 3; and Childers (2013), chapter 1, § 2.

The marriage of an axiom of lawfulness (the axiom of convergence) with one of lawlessness (the axiom of randomness) was from the start intensely scrutinized, and even suspected of inconsistency. There is a careful discussion of this controversy in Nagel *ibidem*, chapter II, § 5.3. Mises took the intimate combination of order and disorder to be a brute fact, not open to further analysis (Popper 1983, Part II, § 23), but for Popper it was *the fundamental problem of the theory of chance* to explain '[t]he seemingly paradoxical inference from the unpredictability and irregularity of singular events to the applicability of the rules of the probability calculus' (1935a, §49 and §64; the words 'seemingly paradoxical' were added in the English translation, 1959a, and a similar addition was made to later German editions of the book; but see also the Introduction to fragment X of Popper 1930–33/1979). His ingenious response was to strengthen the axiom of randomness in a demonstrably consistent way, and to derive a form of convergence from it. He was thus able to establish Bernoulli's theorem (1935a, §§61–64) without any explicit assumption of convergence. Popper faced another problem too, *the problem of decidability* (proem to chapter VIII, and §65), the problem that probability hypotheses, as customarily understood, are empirically neither verifiable nor falsifiable. In being unverifiable they are no different from other scientific hypotheses (§66). The predicament for Popper, who had earlier proposed falsifiability as a criterion of empirical science (*ibidem*, §6), was that they are also unfalsifiable: no observable sequence of Heads and Tails formally refutes a hypothesis such as $\mathfrak{p}$(Heads) = 1/2; a run of a million Heads is not impossible. Nor can any finite sequence refute a hypothesis of randomness in Mises's sense. These hypotheses can be made falsifiable, however, though still less verifiable, by being strengthened (see Popper 1974b, §29, especially n. 160). Popper's plan, not fully consummated in 1935 (Popper 1959a, §57, n. *1), was to specify a class of finite random sequences with relative frequency 1/2, for which convergence is easily proved, and only later to consider infinite random sequences. He then invited us to understand the hypothesis $\mathfrak{p}$(Heads) = 1/2 to imply that *from the beginning* the sequence of Heads and Tails is (approximately) *as random as possible*. To fix the degree of approximation at which the hypothesis is held to be refuted, a methodological rule is needed; but, Popper stressed, in this regard probability hypotheses are not greatly different from universal hypotheses. In (1935a), §68, he summarized his investigation concerning the decidability of probability hypotheses in physics: 'it is possible to frame the rule in such a way that the dividing line between what is permitted and what is forbidden is determined, just as in the case of other laws, by the attainable precision of our measurements'.

Popper's definition of finite random sequences was not wholly adequate (Ville 1939), and the matter was properly resolved only in the 1960s and 1970s with the computational-complexity approach to randomness of Kolmogorov and Chaitin (for reports see Fine *ibidem*, chapter V, Delahaye 1994, chapter 2, and, for more recent work on randomness, van Lambalgen 1990). The theory of collectives had earlier been put on a firm footing by Wald, who was led into the field by a talk given by Popper at Menger's *mathematisches Colloquium* in 1935 (Popper 1974a/1976, §20; 1983, Part II, §21), Copeland, Church and others. Some details are to be found in von Mises (1964), chapter 1, appendix 1; and in Popper (1983), Part II, §§21–23, which contains also a retrospective assessment of the successes and failures of the frequency interpretation.

2. THE PROPENSITY INTERPRETATION OF PROBABILITY

2.0. *Prospectus*

As noted earlier, the propensity interpretation of probability was the unifying theme of Popper's *Postscript* (1982a, 1982b, 1983), a work that, for the most part, was written in the 1950s. Like the frequency interpretation it interprets probability objectively, that is, as a feature of the world, but it is designed to make sense of the probability of single events. Popper offered this new interpretation as a valuable alternative to the subjectivist theory that goes back to Laplace (and even further), according to which probability enters only because we do not know enough. He claimed that it advances considerably our understanding of the role that probability plays in physics, especially quantum mechanics, and in other parts of natural science. On this matter, see Maxwell (2016), §6. In evolutionary theory Mills & Beatty (1979) have proposed a propensity interpretation of fitness, which is also probabilistic, but it has been questioned how close this proposal is to Popper's ideas (Drouet and Merlin forthcoming). Some writers have expressed the hope that the propensity interpretation of probability might play a central role in the social sciences; for example, in situational analysis (Runde 1996, §III), and in clarifying the relation between structure and agency (Williams 1999). Others (for example, Albert 2007) regard it as a useful way of involving objective probabilities in what is called *decision making under uncertainty*; that is to say, the calculation of expected utilities can be carried out using (estimates of) the propensities of the various outcomes under consideration, rather than the degrees of belief of the agent. If all these dreams come true, the propensity interpretation may provide an element of unity not only to the three volumes of the *Postscript* but also to the natural and social sciences.

Popper's first published exposition, the central argument of which was extracted from (1983), Part II, §20, announced the propensity interpretation as 'a revised or reformed statistical interpretation' (1957a, proem); and a few pages later he wrote that it 'differs from the purely statistical or frequency interpretation only in this – that it considers the probability as a characteristic property of the experimental arrangement rather than a property of a sequence' (*ibidem*, §2). This summary, which has misled commentators into crediting to Peirce the main ideas of the propensity interpretation, tells less than half the story, both philosophically and as an extended chapter in Popper's thought. The truth is that there are three sizable steps on the road from the frequency interpretation of (1935a) to the full-blown propensity interpretation of Popper's later writings (such as the Preface to 1982b). The first step, outlined in § 2.1 below, abandons the frequency interpretation's emphasis on collectives, and makes probabilities manifestations of dispositional properties imputed to experimental statistical arrangements. This step allows the frequency theory to give an unobjectionable account of unique single-case probabilities for some singular events. The second step (§ 2.2), which is the crucial move forward, gives primacy to single-case probabilities as singular dispositions or propensities, and relegates the resort to relative frequencies to the (not at all unimportant) level of empirical testing. The third step (§ 2.3) releases the single-case probabilities or propensities from reliance on anything akin to experimental arrangements, and begins to make intelligible the attribution of probabilities not only to repeated events but to unique events (such as the outbreak of World War II). It should not need to be said that none of the three steps is a necessary consequence of what preceded it. They are conjectures that have to be assessed in terms of the explanatory illumination that they provide. Each of them illustrates Popper's growing preparedness to formulate, and include within his philosophy, metaphysical hypotheses of a kind that in 1935 he was more than willing to push to one side (see nn. *3 and *4 to 1935a, § 71).

The section concludes with a few remarks (§ 2.4) on Popper's metaphysical picture (1990) of 'a world of propensities'.

2.1. *From Collectives to Experimental Arrangements*

An acknowledged shortcoming of the original frequency interpretation was its incapacity to make good sense of singular probabilities, especially those of unrepeatable events. Here it is both less general and more informative than the standard measure-theoretical approach due to Kolmogorov (1933), in which probabilities are routinely attributed to individual events; they are represented formally by the values of an additive set function, but this function is not given any substantial interpretation. Kolmogorov recognized that '[t]he concept of mutual *independence* holds … a central position in the theory of probability' (p. 8), but he shied away from endowing independence with any empirical interpretation, and it is a fair complaint (Mises 1964, p. 44) that '[m]ass distributions, density distributions, and electric charge are likewise additive set functions. If there is nothing specific in probability, why do we define "independence" for probability distributions and not for mass distributions?'

Mises calmly accepted the lacuna in the frequency theory, its inapplicability to single events, and denied that there is (for example) a unique number that can be called the objective probability of death of a named individual on the 15th of March next year (1951, pp. 17f.). Any calculation of such a probability, he held, is relative to the collective to which the individual is assigned; it may be greater among balloonists than among bassoonists. Popper agreed, and even acknowledged that the subjectivist interpretation may here be at an advantage, but he maintained that we can define '[t]he formally singular probability that a certain occurrence k has the property β — given that k is an element of the sequence α — ... [as] equal to the probability of the property β within the reference sequence α'. He added that 'although α is often not explicitly mentioned, we usually know ... which α is meant' (1935a, §71).

How can we know this? If a frequentist advises me that the probability is 1/2, or any other real number between 0 and 1, that I shall die on the 15th of March next year, I shall hardly know what to think. The answer has to be that there exist empirical sequences (though mortality data do not constitute good examples) for which the precise conditions of production are not important. By this I mean that there exist, at least at the microphysical level, empirically realizable conditions $\mathfrak{C}$ that, when maintained, produce distributions of frequencies, and so of probabilities, that are constant (within experimental error) and unalterable. We could say that in conditions $\mathfrak{C}$ no physically effective gambling system is known. A half-silvered mirror, a device that both transmits and reflects light, is a good example; the beam of individual photons that arrive at the surface of the mirror constitutes a sequence of repeated events under identical conditions, and the unflickering transmitted and reflected beams indicate that the relative frequencies of transmission and of reflection are stable. What is more, we know of no feasible procedure for selecting photons so as to change these frequencies.

Let us suppose, perhaps unrealistically, that an ordinary pin board (Popper 1967, thesis 8; 1982b, Introduction, §3) or Galton quincunx (Galton 1889, pp. 63–65; Mises 1951, pp. 169–71) is another example. It is uncontroversial that we can manipulate the conditions $\mathfrak{C}$ under which such devices operate (the colour of the incident light, the horizontal inclination of the quincunx), and obtain varying distributions of outcomes. What is important is that $\mathfrak{C}$ can often be controlled enough to produce a distribution that cannot be further varied. In the case of the quincunx it may well be that, if we could refine the conditions $\mathfrak{C}$ more narrowly, for example by a delicate adjustment to the funnel that delivers the balls to the board, we should obtain a different distribution, and eventually the same outcome on every occasion (that is, an outcome with probability 1). But it is an empirical fact that the exact conditions of operation of a physical device are commonly out of our hands (if only because of uncontrollable molecular fluctuations); there exist arrangements that, despite, and also thanks to, our best endeavours, produce non-trivial reproducible distributions of frequencies. It is this empirical fact that makes possible both statistical experimentation and gaming machines such as roulette wheels (Popper 1983, Part II, §28). In such cases a frequency theorist may, as Popper intimated, identify the singular probability $\mathfrak{p}(\mathsf{a})$ of an outcome of type a under conditions $\mathfrak{C}$ with the relative frequency of outcomes of type a in sequences generated by $\mathfrak{C}$; this value of $\mathfrak{p}(\mathsf{a})$ is absolute in the sense that no realizable refinement of $\mathfrak{C}$ can yield a different singular probability.

It was from the other direction, through the observation that a crude temporary change in $\mathfrak{C}$ may change drastically the singular probability $\mathfrak{p}(\mathsf{a})$ of a, though not its relative frequency, that Popper arrived at the idea that the probability of a single event a should be primarily referred to the conditions $\mathfrak{C}$ that generate a, rather than to any collective in which a happens to be resident (1957a, § 1; 1959b, § 3; 1983, Part II, § 20). If the conditions $\mathfrak{C}$ are repeated indefinitely and yield stable and immutable frequencies, then the corresponding singular probabilities can be defined in terms of them. But if the conditions $\mathfrak{C}$ are not repeated, or repeated only sparingly (the quincunx may rarely be used), it seems natural to resort not to empty or very short sequences, but to 'virtual sequences' of repetitions of $\mathfrak{C}$. In short, probabilities '*characterize the disposition, or the propensity*, of the experimental arrangement to give rise to certain characteristic frequencies *when the experiment is often repeated*' (Popper 1957a, § 1). That frequencies, and therefore probabilities, depend on the physical conditions had been recognized in the early 1930s (Popper 1930–33/1979, Book II, fragment X, § 7); Mises too had informally said as much (1951, pp. 14f.), although his official position (pp. 28f.) was always that probabilities are defined relative to collectives, and collectives are defined by the axioms of convergence and randomness. In the published discussion of Popper (1957a), Braithwaite even judged that 'Popper expresses the frequency theory in the same admirable way [as Peirce did] by using the term "propensity" to emphasize the similarity of a probability to a habit' (p. 78). For more on the extent to which Peirce may have anticipated some aspects of the propensity interpretation, see Settle (1974), § 2, and R. W. Miller (1975).

2.2. *From Dispositions to Propensities*

Although the distribution of probabilities of outcomes on an experimental set-up $\mathfrak{C}$ may indeed reflect the dispositional property of $\mathfrak{C}$ to produce the corresponding frequency distribution, and the probabilities may, in appropriate circumstances, also be understood to apply to single events, *a singular probability cannot reflect a disposition to produce frequencies* (among quite distinct events, actual or virtual), but only a disposition that can be exercised by the arrangement in a single case, or by sufficiently similar arrangements in sufficiently similar cases. In the present author's opinion, it is this introduction of singular dispositions with heterogeneous outcomes, which is far from compulsory, together with the assumption that the laws of the probability calculus are satisfied, that marks the true transition from the frequency interpretation to the propensity interpretation, and it is fitting to register the advance by a refinement of terminology: by using the term 'disposition' in the orthodox manner and reserving the term 'propensity' for those dispositions whose effect is restricted to the single case. This insistence on the single case has nothing to do with the question of whether or not the same distribution of propensities is active whenever, to preserve the example, a quincunx is used under the specific conditions $\mathfrak{C}$. That is a factual matter. It may be so, in which case we may say that the quincunx under conditions $\mathfrak{C}$ enjoys a constant distribution of propensities. What is crucial is that the outcome of each single case is itself singular, though perhaps with lasting effects. (Indeed, there exist more complex single-case propensities, for example, the propensity of a seismic tremor, or the propensity of an agent to hiccup, where a single occurrence itself establishes a propensity, usually diminishing, for the event to be repeated. Such sequences of events are typically not probabilistically independent, and for simplicity they are disregarded here.)

Shortly after the remark quoted at the end of §2.1, Popper wrote (1957a, §2):

> The main point … is that we now take as fundamental *the probability of the result of a single experiment*, with respect to its *conditions*, rather than the frequency of results in a sequence of experiments. Admittedly, if we wish to *test* a probability statement, we have to test an experimental sequence. But now the probability statement is not a statement *about* this sequence: it is a statement *about* certain properties of the experimental conditions, of the experimental set-up.

These two quotations straddle the dividing line between long-run and single-case interpretations of probabilities (Fetzer 1974; Giere 1973). According to the quotation from Popper (1957a), §1, a probability statement expresses the disposition of the experimental arrangement to generate frequencies over time. According to the quotation from *ibidem*, §2, it expresses the propensity of the arrangement at a single time to do something in a single case. Popper was sometimes not explicit about which interpretation he was endorsing, and on at least two occasions (1957a, pp. 88f.; 1990, p. 11) endorsed both simultaneously. We need not choose between them, of course, since both appear to have application in our world, in naturally occurring phenomena as well as in man-made experiments (1982b, Introduction, n. 63); but we do well to distinguish them.

In conformity with the proposed usage, familiar physical dispositions such as solubility and conductivity are propensities (the conditions of activation being understood); but the disposition to generate sequences with a given distribution is not a propensity. As for the single-case propensities introduced by Popper into the theory of probability, they are most naturally understood as propensities of the experimental arrangement to produce (to different degrees, or with different weights) one or other of the possible results that the arrangement permits. In a late lecture Popper wrote of the throws of a die that '*a tendency or propensity* to realize an event is, in general, *inherent in every possibility* and in every single throw' (1990, p. 11). It is misleading, however, to think of '*the propensity, or tendency, of a possibility*' as a propensity of the possibility '*to realize itself upon repetition*' (1967, thesis 8), as he sometimes put it, since the propensity in question is the propensity of the possibility to realize itself *here and now*. On a quincunx played under suitable conditions there may be a propensity, equal neither to 0 nor to 1, for a ball that strikes a specific pin on a specific occasion to continue in one direction, and some other non-extreme propensity for it to continue in another. (There are numerous animations of quincunxes available online.) At the surface of a half-silvered mirror there may be a propensity of 1/2 for each incident photon to be transmitted, and of 1/2 for it to be reflected. The quincunx ball takes only one path, of course, as does the photon; they succumb to only one of the propensities. Mellor (1971, chapter 4), who called 'chance' what is here called 'propensity', objected that traditional dispositions (such as solubility) are located in physical objects, and only derivatively in more general physical arrangements. To say that a die-throwing machine, together with a die thrown by it, has some propensity distribution 'would be like saying … that a grain of salt with a bucket of water might be soluble' (*ibidem*, p. 75). Yet it is plain that, in the case of a quincunx, the single-case propensities we are interested in should be credited to the equipment in its entirety, not to the balls alone, if only because the outcomes (which slot a ball comes to rest in) are defined only by the equipment.

Mellor claimed also that propensities of the usual stripe either take effect every time that identical conditions are repeated, or fail every time (and so have associated probabilities of either 1 or 0). He went on to dismiss the idea that propensities may be similar to human character traits such as generosity, because this idea 'needs analysis at least as much as chance does' (p. 69), and plumped for an interpretation (criticized in Fetzer 1981, pp. 115–19) that denies single-case propensities most of their individuality. The point that needs to be stressed (in opposition to Strevens 2006, pp. 34–36) is that propensities were not supposed by Popper to be dispositions of the usual stripe. He emphasized as well (1983, Part II, the end of § 20) that the propensity interpretation was not an outcome of

> the method of *meaning analysis* … [applied to] the word 'probability', … [but] *a new physical hypothesis* (or perhaps a metaphysical hypothesis), analogous to the hypothesis of Newtonian forces. It is the hypothesis that every experimental arrangement … generates propensities which can sometimes be tested by frequencies.

Whether we can make decent sense of such propensities depends on the vitality of our imagination. There may be some self-deception involved, but I think that I understand how, if offered a bowl of mixed fruit, I may have a rather strong inclination, short of compulsion, to choose a pitahaya, but may spontaneously choose a rambutan. Of course there is a dose of anthropomorphism here (Popper 1983, *ibidem*), but it is not quite where Mellor located it, and it need not be obscurantist. We must not forget that the source of an idea has little bearing on either its value or its validity.

It is easy to see that propensities, so understood, can assume non-extreme values only in an indeterministic world. If the physics of the quincunx is deterministic, as it may well be, then at each impact between ball and pin there is for each angle of deflection a propensity of either 0 or 1 for the ball to be so much deflected. The same is true for the half-silvered mirror if determinism rules there. But such a trivialization of propensities where determinism prevails does not mean that we must surrender single-case probabilities. As we have seen, stable and (in practice) unalterable statistics suffice for us to define such probabilities in terms of frequencies. Clark (2001, §§ 1f.; 1995, § 4) is quite correct to remark that the propensity interpretation has no role to play in classical statistical mechanics, but wrong to insist that 'the issue of determinism versus indeterminism really ought to be (is) irrelevant to an interpretation of probability theory' (2001, p. 275). The propensity interpretation, as already indicated, was intended as a physical hypothesis. If, like mass, energy, momentum and most other classical quantities, probability is part of the physical world, then some of what we want to say about it may well depend on what the world is like.

For similar reasons (and others), the objections raised by Eagle (2004) lack force against the theory of propensities that Popper advocates.

Ineffective too is the complaint of Rosenthal (2006) that an analysis of probabilistic propensities that avoids mention of frequencies is incorrect, while one that incorporates frequencies inevitably presupposes probabilistic ideas and is circular. Popper's interest in developing the propensity interpretation was (as noted) not to give an analysis of the concept of probability; since (1935a) he had held there to be many such concepts. What primarily concerned him were some of the outstanding philosophical problems of quantum mechanics, especially that posed by the two-slit experiment. In (1935a), § 76, he had offered a simple-minded explanation of why the so-called reduction of the wave-packet is a feature of every probabilistic theory. By providing an objective understanding of single-case probabilities, the propensity interpretation strengthened this explanation, and gave promise of resolving other supposed paradoxes. The principal point, ceaselessly stressed, was that '*it is the whole experimental arrangement which determines the propensities*' (1982b, § 18), and therefore the statistics. 'Thus … [an individual] particle will pass through only one of the slits, and in a certain sense will remain uninfluenced by the other slit. What the other slit influences are the propensities … not the particle itself: the propensities for reaching the one point or the other on the second screen.'

Even though statements about the propensities at work in conditions $\mathfrak{C}$ are not disguised statements about real or virtual frequencies, they can sometimes be tested by the frequencies displayed in repetitions of $\mathfrak{C}$. What are most amenable to testing are singular probability statements understood as statements about a constant propensity active each time some set $\mathfrak{C}$ of physical conditions is encountered. Such statements are straightforwardly testable (in principle) if we assume that the repetitions of the conditions $\mathfrak{C}$ are probabilistically independent; that is, that for any two events a, c in the sequence, $\mathfrak{p}(\mathsf{ac}) = \mathfrak{p}(\mathsf{a}) \cdot \mathfrak{p}(\mathsf{c})$. For according to the weak law of large numbers (Bernoulli's theorem), if in a long sequence of events generated by $\mathfrak{C}$, $\mathcal{K}_n$ is the set of all those segments of length n in which there occur m events with propensity p (and $n - m$ events with propensity $1 - p$), and the relative frequency m/n lies within some positive ε of p, then

(0) $\quad \mathfrak{p}(\mathcal{K}_n) \to 1$

as n increases without limit (Feller 1968, chapter VI, § 4, or any other good mathematical text on the theory of probability). Call an event with propensity p a *p-event*. Then (0) says that however small ε may be, for large enough n the propensity is near to 1 that a segment of length n will register a number m of p-events such that the fraction m/n differs from p by less than ε. For any ε, the value of this propensity may be calculated. In other words, the arrangement has an overwhelming propensity, if properly aroused, to generate a sequence of great length n with approximately $n \cdot p$ occurrences of p-events. As in the frequency theory (and in other theories of probability that aim to reach factual conclusions), we still need to adopt some form of Cournot's principle, that is, a suitably framed rule to permit us to ignore some (but not all) sufficiently minute probabilities. But with such a rule in our possession, the practicability of testing and refuting statements of propensity is assured. Bernoulli's theorem provides what Popper called 'a bridge from propensities to statistics' (1959a, §48, the starred addition to note 6).

There are stronger results that may be obtained using stronger forms of the law of large numbers, and the frequency and propensity interpretations exploit these theorems differently; see Popper (1983), Part II, §22 and §24. Of especial importance is the derivation of two results concerning infinite sequences of independent events: in almost all such sequences the limit of the relative frequency of each label tends to the probability of that label; and almost all such sequences are insensitive to any denumerable family of gambling systems. Here 'almost all' means all but a set of zero probability. In this way Mises's two axioms, of convergence and randomness, are explained, and 'one of the great riddles of the world', the fundamental problem of the theory of chance, is fully solved (*ibidem*, Concluding Summary 1982). The explanatory value of the conjecture that propensities obey the probability axioms is hereby established; and the often asked question of why they obey these axioms can be left on the file until a deeper explanatory theory comes along.

2.3. *From Repeatability to Uniqueness*

The final step in the development of the propensity interpretation as here depicted consists of the conjecture that singular propensities exist not only in repeatable conditions but throughout the physical world; and that, despite the absence of corresponding frequencies, these propensities also satisfy the calculus of probabilities. That is to say, every physical arrangement is endowed with a distribution of probabilistic propensities over the class of its future possibilities. This step amounts to more than a sweeping generalization of what was conjectured at step 2.2. For given that the propensities are no longer embedded in sets of repeatable conditions, we are obliged to reconsider what qualifies as a physical arrangement. The following extract from pp. 79f. of the published discussion of Popper (1957a) reveals some reluctance to venture further:

Ayer It is not at all clear to me what propensities are ascribed to. In the case of a die the situation is clear; but this is not so in the case of the horse race and I don't see how one is going to evaluate the probabilities here in terms of propensities.

Braithwaite I certainly shouldn't try to apply propensities to the horse-race situation. I should use it as an explanation of probability only as applied to what Popper calls 'experimental situations'.

...

Vigier I think the case of the horse race and the case where you have relative frequencies are two separate questions. The word 'probability' has meaning only in the second case ....

Popper (who did not attend that discussion) later remarked that 'what one might want to know in betting on a horse ... may be described as the propensity of that horse to do well in a race (as compared, of course, with its competitors)' (1967, thesis 8). In its use of the indefinite article this comment shows traces of a dispositional rather than a propensity approach to single-case probabilities. Even more is this so of the deflationary suggestions surveyed by Gillies (2000), pp. 119–25, that try to squeeze judgements of propensity out of objective statistics. But genuine single-case probabilities are not statistics, and if they are not tied to sets of repeatable conditions, they are not manifested in statistics either. The problem remains: What are such single-case propensities propensities of?

The propensity that a named individual will die on the 15th of March next year depends, doubtless, on his age, his state of health, the loyalty (and perhaps also the squeamishness) of his friends and much besides. That there are no theoretical limits (except those demanded by the special theory of relativity) to the circumstances that may bear on whether such a singular event happens is underlined by the butterfly effect described by Lorenz (Stewart 1989, pp. 139–42, or any other book on the theory of chaos). Almost anything within the light cone may be probabilistically relevant; that is, may affect the probability of the event's occurrence. It seems therefore that any propensity must be referred to the complete set of conditions, the state of the universe, obtaining at the time in question. Popper wrote in 1980: 'the propensity interpretation regards propensities as objective physical properties of the physical situation under consideration and, ultimately, of the whole physical world' (1982b, Introduction, §4; see also 1990, p. 17). For a closely similar sentiment, see Miller (1985), p. 19a. This fine-tuning of propensities does not imply that some propensities (such as those characteristic of experimental arrangements) may not be determined by a restricted set of circumstances; for '... there is a difference between being locally determined, which is a factual matter, and being locally defined, which is not. My tax inspector annually reminds me that my annual income is not locally defined, but includes anything I earn overseas; but in some years, as it happens, my income is locally determined' (Miller 1994, chapter 9, § 6).

'The main problem', opined Gillies (2000, p. 127), 'with the 1990s [in truth, 1980s] views on propensity of Popper and Miller is that they appear to change the propensity theory from a scientific to a metaphysical theory.' There is a modicum of truth in this complaint, since isolated statements about single-case propensities cannot be individually tested. Erdur (2006), p. 116, and Childers (2013), chapter 2, § 2.1, agreed with Gillies that the difficulty disqualifies this form of the propensity interpretation from playing a role in science. Galavotti (2005), chapter 5, § 3, called the failure of testability 'puzzling'. Yet the objection has been given undeserved weight (Humphreys 2005, p. 849b), as a comparison with classical mechanics shows. For individual statements about instantaneous (relative) positions, velocities, accelerations and forces are equally immune to testing, yet classical mechanics is testable (Miller 2002, § 7). We can test these statements *en masse*, by assuming that a body is at rest throughout an interval, or in uniform motion, or moves under some specified forces. The same can be said of propensity statements. It may be countered that in physics we have a multitude of laws that tell us how velocities, say, change with time (for example, the textbook classic of *motion in a vertical circle*), but nothing at all similar in the theory of probability. This too is an exaggeration. Urn models of the spread of contagious diseases may inform us not only about changing frequencies but also about changing propensities. This is precisely the field of the theory of stochastic processes (on which see also Popper's interesting remarks in 1930–33/1979, Book II, fragment X, § 7, on 'probability functions ... under continuously changing conditions'). In any case, the insolubility of the three-body problem indicates how distant empirically (rather than logically) statements of instantaneous velocity are in general from the (observed) trajectories. Propensity statements are surely harder to test than their mechanical counterparts, as emphasized by Guerrero (2004), p. 172. A theory that incorporates them is not on that account alone to be labelled metaphysical and banished from the precincts of science. Those (such as Hájek 2007, § 2.4) who find propensities not directly based in frequencies to be impenetrably occult should recall the obscurantist fists that, in the heyday of classical mechanics, were so often shaken at gravitational forces.

2.4. *The Emergence of Novelty*

Popper made substantial use of a theory of indeterministic propensities in some of his metaphysical speculations on the cosmos, on life, and on human freedom (Niemann 2014, §27). In 'A Metaphysical Epilogue' (1982b, chapter IV), which brings to a close the original text of the *Postscript*, he attempted 'to give a coherent view of the physical world which is no longer a strait-jacket for its physical inhabitants … [nor] a cage in which we are caught, but a habitat which we may make more habitable, for ourselves and for others' (*ibidem*, §27). On p. 9 of (1990), with only a passing reference to the ideas formulated thirty-five years before, he wrote of the propensity interpretation that 'it was only in the last year that I realized its cosmological significance. I mean the fact that we live in *a world of propensities*, and that this fact makes our world both more interesting and more homely than the world as seen by earlier states of the sciences.'

In Popper's opinion, the only serious form of the problem of determinism was 'the problem which arises from a physical theory which describes the world as … a *physically closed* system' (1966, §VII), since this doctrine, if true, 'destroys … the idea of creativity, … the idea that in preparing this lecture I have used my brain to create something *new*' (*ibidem*, §IX). Indeterminism, accordingly, 'is not enough' (*ibidem*, §X; 1973). We have to be able to take advantage of the indeterministic crevices, which means that, given that the physical world is, to a considerable extent, a world ruled by exceptionless force laws, we have to be able to introduce new forces into the world. It has already been noted that, from the start (1957a, §4), Popper compared propensities to Newtonian gravitational forces (not only because both are 'occult' and 'relational'). Genuine freedom, that is to say, requires an ability on our part to introduce new propensities. But propensities are weighted possibilities (1967, thesis 8; 1990, pp. 9–11) that are '*more than mere possibilities*, but tendencies or propensities to become real' (*ibidem*, p. 12); and 'zero propensities are, simply, no propensities at all' (p. 13). The problem of novelty is therefore how we, or indeed the world, can create new (non-zero) propensities.

Without, I hope, sounding facetious, I should like to say at this point that 'propensities are not enough'. *There have to take place from time to time events* (they are traditionally called chance events) *that have no propensity to occur* (Miller 1995, §4). To take an example offered by Miller *ibidem*: Popper would surely have agreed that before the beginning of life, and indeed for almost all of recorded and unrecorded history, there was no propensity at all, a zero propensity, for the *St John Passion* eventually to be written, even though this achievement was always abstractly possible; but by 1721, the propensity had become positive, but not yet unity. The conversion of a zero propensity for Bach's composition in the early universe into a positive propensity at some later date demands an intermediate event whose own propensity is zero, a chance event.

To see why, suppose that b describes the state of the world at a time *t*, and that the possible event a has a propensity at *t* that is neither 0 nor 1; that is, $0 < \mathfrak{p}(a,b) < 1$. Then there may occur an event c with non-zero propensity $\mathfrak{p}(c,b)$ such that the propensity $\mathfrak{p}(a, cb) = 0$; that is, the propensity of a in the new state of the world drops to 0. Does the occurrence of c therefore permanently extinguish the possibility of a after t? Of course it may do so: real possibilities are being extinguished all the time. What is at issue is how the process can be reversed; that is to say, how real possibilities can be created. If $\mathfrak{p}(a,b) = 0$, there can be no event c with non-zero propensity $\mathfrak{p}(c, b)$ such that $\mathfrak{p}(a, cb) \neq 0$. (This follows from the two axioms B0: $\mathfrak{p}(ac,b) \leq \mathfrak{p}(a,b)$ and B1: $\mathfrak{p}(ac,b) = \mathfrak{p}(a, cb) \cdot \mathfrak{p}(c,b)$ of the system $\mathbb{B}$ in § 3.) Yet if $\mathfrak{p}(c,b) = 0$, it may well be that $0 < \mathfrak{p}(a, cb) < 1$, even when $\mathfrak{p}(a,b) = 0$; it follows that if the chance event c were to occur in the world described by b, the event a would become concretely possible without necessarily becoming actual. It is far from clear how we are able to make capital out of such chance events, but to the extent that we introduce novelty into the world, we evidently do.

In the previous paragraph relative propensities of the form $\mathfrak{p}(a, c)$ have been slotted into the probability calculus with little comment. The propriety of this manoeuvre has been contested, when $\mathfrak{p}$ is understood to measure the strength of a causal link from c to a (as is suggested by Popper *ibidem*, p. 22, and by many others), on the grounds that causality has a temporal direction but the probability formalism has not; $\mathfrak{p}(a, c)$ is well defined even if the event c postdates the event a. This problem, known as Humphreys's paradox, has been much discussed (by Salmon 1979; Fetzer 1981, chapter 10; Humphreys 1985; Miller 1994, chapter 9, § 5; McCurdy 1996; Gillies 2000, chapter 6; Miller 2002; Humphreys 2004; Galavotti 2005, chapter 5, § 3; Milne 2005, § 8; Albert 2007; Belnap 2007; Drouet 2011; Childers 2013), chapter 2, § 2.3; and Lyon 2014), without any agreed resolution. Berkovitz (forthcoming) is a substantial survey of the literature on the propensity interpretation and on Humphreys's paradox in particular. There is unfortunately no space to consider the paradox further here.

In addition to the works already mentioned in this section, the books of Ackermann (1976), chapter 4, § II, O'Hear (1980), chapter VII, § 3, and Keuth (2005), chapter 8, § 5, and the papers of Kyburg (1974) and Bächtold (2006) contain critical discussions of Popper's various variants of the propensity interpretation, some diverging from the present account, as well as further references. Though sometimes dismissed, without argument, as 'half-baked' (Howie 2002, p. 221) or 'flawed' (Velupillai 2008, p. 159), the introduction of probabilistic propensities has been one of Popper's more respected innovations. There exist also quite disparate variants, for example the theory of Lewis (1981), which has the surprising consequence that it is not a factual truth that propensities conform to the probability axioms, but a necessary one. Lewis's subjectivist approach to objective chance has spawned an appreciable literature.

3. AXIOMATIZATION OF THE THEORY OF PROBABILITY

In (1935a), alongside his detailed treatment of the frequency interpretation (summarized in § 1 earlier), Popper made brief mention (§ 48) and brief use (§ 34) of another interpretation, called *the logical interpretation*, conceived of as some kind of generalization of classical deductive logic (see § 4 below). Much more widely noticed was the argument (§ 83) that this interpretation has nothing to do with the defining thesis of modern inductivism, the thesis that our most successful scientific theories, though not certain, are highly probable relative to the presented evidence (see § 4.1 below). In the introductory remarks to the new appendices added to (1959a) Popper wrote:

> *Does … the degree of acceptability or corroboration of a theory … obey the rules of the probability calculus?*
>
> I had answered this question in my book and my answer was '*No*'. To this some philosophers replied, 'But I mean by probability … something different from what you mean'. To justify my rejection of this evasive reply … the rules ('axioms') of the probability calculus had to be formulated …. For in order not to prejudge the issue whether degree of corroboration is one of the interpretations of the calculus of probability, this calculus had to be taken in its widest sense.

In one of the opening paragraphs of appendix *ii he added:

> Another of my motives [in 1938] … was that I wanted to show that what I had called in my book 'logical probability' was the logical interpretation of an 'absolute probability'.

These were his stated aims in launching the project of providing an abstract axiomatization of the calculus of probability. The central laws of this calculus, the addition and multiplication laws, were of course well known, but how they interlock was more mysterious. It was also unclear, to Popper and to others, in what way probability theory is a genuine generalization of deductive logic. These obscurities had not been fully resolved in the axiomatization given by Kolmogorov (1933), which anyway Popper was not aware of. With regard to the second problem, it turned out that it is not the calculus of absolute probability that delivers the needed generalization, but the calculus of relative probability. (A similar reform took place in the presentation of logic itself when the theory of proof gave way to the theory of deducibility. It should be noted, however, that although, within classical logic, deducibility is definable in terms of proof, via the deduction theorem, relative probability is not definable in terms of absolute probability.) To this problem Popper later gave a new and unanticipated solution.

It is open to question to what extent Popper's elegant axiomatic systems furnish illuminating answers to the philosophical questions that gave birth to them. But it would be wrong to suppose that the systems are of mathematical interest only. For an unintended consequence of Popper's work has been the emergence of the thesis, known as *probabilistic semantics*, that the theory of probability provides an alternative semantical foundation for propositional logic, alternative, that is, to the accredited semantics in terms of truth. The remainder of this section will be almost entirely dedicated to explaining the fundamental technical novelties of Popper's axiomatizations, but a subdued comment on probabilistic semantics will be made in conclusion (pp. 251f.).

Popper's (1938) system was, like Kolmogorov's, a system of absolute probability, which at that time, for reasons soon superseded, he preferred to relative probability as a primitive term. A more polished formulation is given in (1959a), appendix *ii. In such a system the relative probability $\mathfrak{p}(a, c)$ is defined only when $\mathfrak{p}(c) \neq 0$. This restriction was later removed – one benefit is explained at the start of appendix *ix of Popper (1959a) – and in the 1950s Popper began to publish a number of variations on a remarkable axiomatic system in which $\mathfrak{p}(a, c)$ is determinate for all elements a, c of a set $\mathcal{S}$ (even those elements that are interpreted as inconsistent). The fruits of much activity were gathered together in appendices *iv and *v of (1959a). Note that a number of significant divergences exist between these appendices and those numbered *iv and *v in later editions (from 1966 to 1994) of Popper (1935a).

We assume two operations on the elements of the set $\mathcal{S}$: a binary operation represented by concatenation, and a singulary operation represented by the prime ′. The system that we shall call $\mathbb{B}$, which is one of the simplest of the many closely related axiomatic systems that Popper finally proposed, consists of some theory of the real numbers, together with these six axioms:

A0 $\exists b \exists d\, \mathfrak{p}(a, c) \neq \mathfrak{p}(b, d)$

A1 $\forall b\, [\mathfrak{p}(a, b) = \mathfrak{p}(c, b)] \Rightarrow \mathfrak{p}(b, a) = \mathfrak{p}(b, c)$

A2 $\mathfrak{p}(a, a) = \mathfrak{p}(c, c)$

B0 $\mathfrak{p}(ac, b) \leq \mathfrak{p}(a, b)$

B1 $\mathfrak{p}(ac, b) = \mathfrak{p}(a, cb) \cdot \mathfrak{p}(c, b)$

C $\mathfrak{p}(a, a) \neq \mathfrak{p}(b, a) \Rightarrow \mathfrak{p}(a, a) = \mathfrak{p}(c, a) + \mathfrak{p}(c', a).$

In the later German editions of (1935a) there are some minor deviations; more tangibly, axiom C there takes the distinct form

$$\text{C}\lozenge \qquad \mathfrak{p}(a, a) \neq \mathfrak{p}(b, c) \Rightarrow \mathfrak{p}(a, a) = \mathfrak{p}(a, c) + \mathfrak{p}(a', c),$$

but everything said below about axiom C applies equally to C◊. The axioms in group A involve no operator on the domain $\mathcal{S}$ of elements. Nor does the definition of absolute probability

$$\text{AP} \qquad \mathfrak{p}(b) = \mathfrak{p}(b, a) \Leftrightarrow \forall c(\mathfrak{p}(a, c) \geq \mathfrak{p}(c, a))$$

(1959a, appendix *iv; see also Popper 1963a, addendum 2). The axioms B0 and B1 involve only the concatenation operator, and C only the prime. These axioms are, however, not definitions; they are *creative* (Popper 1963b) in that they enable proof of consequences involving neither concatenation nor prime. Note that there are no further algebraic axioms in $\mathbb{B}$ concerning the elements a, b, c of $\mathcal{S}$. It is not explicitly assumed here, as it standardly is, that the operations denoted by concatenation and prime behave like conjunction and negation, or that the domain has the structure of a Boolean algebra. These truths are embodied in theorems.

The following paragraph, which presupposes some knowledge of modern algebra, gives a flavour of what can be established in $\mathbb{B}$ and in related systems. We may prove from A2, B0, and B1 alone that the value of $\mathfrak{p}(b, b)$ is either 0 or 1 (Popper and Miller 1994, Theorem 0). When C is brought in, the probabilistic bounds

$$(1) \qquad 0 \leq \mathfrak{p}(a, c) \leq \mathfrak{p}(b, b) = 1$$

are derivable. In a helpful simplification of the dour derivations of Popper (1959a), appendix *v, Popper and Miller (*ibidem*, Theorem 4) showed that (1), added to B0 and B1, is more than enough to prove the following result: if A and C are concatenations of variables, and every variable that occurs in A occurs also in C, then $\mathfrak{p}(A, C) = 1$. As a consequence, each of the following identities

$$(2) \qquad \mathfrak{p}(aa, d) = \mathfrak{p}(a, d)$$

$$(3) \qquad \mathfrak{p}(ac, d) = \mathfrak{p}(ca, d)$$

$$(4) \qquad \mathfrak{p}(a(bc), d) = \mathfrak{p}((ab)c, d)$$

is derivable. Calling *probabilistically indistinguishable* those elements a, c of $\mathcal{S}$ for which $\mathfrak{p}(a, d) = \mathfrak{p}(c, d)$ for every d, and identifying indistinguishable elements, which it is the sole purpose of A1 to permit, we may derive from (2), (3), (4), that the operation denoted by concatenation is a meet (conjunction) operation, and that $\mathcal{S}$ has the structure of a lower semilattice whose ordering relation $c \lesssim a$ is defined by $\forall b[\mathfrak{p}(a, cb) = 1]$. By adding as an axiom the general addition law

D $$\mathfrak{p}(ac, b) + \mathfrak{p}(a \vee c, b) = \mathfrak{p}(a, b) + \mathfrak{p}(c, b)$$

(again, more than a definition of the join operation for which $\vee$ is shown to stand), we may extend this result to distributive lattices (Popper and Miller *ibidem*, Theorems 11–14). By reincorporating axiom C and returning to the system $\mathbb{B}$, we may extend the result to Boolean algebras (Popper 1959a, appendix *v, the formula immediately succeeding formula [100]; Popper and Miller *ibidem*, §4). One interpretation of Boolean algebra is as the Lindenbaum algebra of classical propositional logic. In this sense the calculus of probability is a generalization of logic, with the relation of deducibility $c \vdash a$ defined by the condition $\forall b[\mathfrak{p}(a, cb) = 1]$ or, more interestingly, by the identity $\mathfrak{p}(a, ca') = 1$ (Popper *ibidem*). It must be emphasized that deducibility of a from c is not adequately defined by $\mathfrak{p}(a, c) = 1$.

Note further that the zero element cc', a classical inconsistency, is an acceptable second argument (or conjunct in the second argument) of $\mathfrak{p}$, and that $\mathfrak{p}(a, (cc')b) = 1$ holds for every a, b, c. This is the probabilistic generalization of the classical law of *ex falsum quodlibet*. The familiar complementation law, $\mathfrak{p}(a, b) + \mathfrak{p}(a', b) = 1$, is therefore not unconditionally valid in this system; as B1, C and (1) indicate, it fails for precisely those b that are probabilistically indistinguishable from cc'. Some of the self-styled simplifications of the system $\mathbb{B}$ in the literature, for example that of Spohn (1986), forfeit this possibility without explicitly saying that they are doing so. Others (such as the axiomatization RP1–RP7 of Roeper and Leblanc 1999, p. 12) do not distort $\mathbb{B}$ in this way, but forfeit some of its elegant features. In this connection it is of some importance to distinguish the deductive theories that axiomatic systems axiomatize and the axiomatizations themselves. It is unsurprising that there are strong similarities among the theories of probability axiomatized by Kolmogorov (1933), Carnap (1950, 1952) and Popper, though Popper's theory is more general than the others. But as axiomatizations they are hardly similar at all. Indeed, among Popper's axioms only a variant of B1 is to be found in any of the other axiom systems. The axiomatic situation is poorly captured in the report that 'Carnap used essentially … [Popper's] axioms for his "c-functions"' (van Fraassen 1995, p. 372).

In (1959a), appendix *iv, Popper provided independence proofs for the axioms of $\mathbb{B}$, and showed that they remain independent even if it is assumed that $\mathcal{S}$ is a Boolean algebra with ordering $\lesssim$. This feature he named *autonomous independence*. He also contrasted the strictly formal nature of his axiomatic system with that of Kolmogorov, which assumes that $\mathcal{S}$ is a field of sets. Yet according to the Stone representation theorem, every Boolean algebra is isomorphic to some field of sets, and so no real generality is lost by assuming that $\mathcal{S}$ consists of sets. What was missed by previous authors, however, was the extent to which the Boolean structure of $\mathcal{S}$ is implied by axioms, for the most part transparent, of probability. In 1949, for instance, de Finetti, one of the century's most imaginative probabilists, published a set of probability axioms that have some resemblance to Popper's (Coletti and Scozzafava 2002, chapter 10, § 3). Yet he assumed explicitly that elements that can appear in the first argument of $\mathfrak{p}$ form a Boolean algebra, and something similar for the second argument (except that he did not countenance there the zero element cc′). There are similar assumptions in other axiomatizations that take relative probability as primitive, for example those of Hosiasson-Lindenbaum (1940) and Rényi (1955). What is lost may be judged by considering not only the weaker axiom systems already mentioned but the development by Miller and Popper (1986), § 2, of a system of probability axioms suitable for distributive lattices with zero. (It is part of a system $\mathbb{H}$ designed to perform for intuitionistic logic the service that $\mathbb{B}$ performs for classical logic.) To axioms A0, A1, A2, B0, B1 and D are added two axioms involving the constant f (for *falsum*):

F0 $$\mathfrak{p}(a, c) \leq \mathfrak{p}(c, f)$$

F1 $$\mathfrak{p}(a, a) \neq \mathfrak{p}(a, c) \Rightarrow \mathfrak{p}(a, a) = \mathfrak{p}(a, f) + \mathfrak{p}(f, c).$$

If they show nothing else, these less potent axiomatic systems show the virtue of avoiding unnecessary algebraic assumptions. The identification of the roles played by different assumptions is, after all, one of the main purposes of axiomatics (Popper 1959a, appendix ∗iv, the text between formulas (f) and (g)).

As mentioned above, the possibility of characterizing the logical connectives by probabilistic means has been exploited (by Field 1977, Leblanc 1983 and others) to develop a new semantics for logic (at least for propositional logic). Part II of Roeper and Leblanc (1999) provides a survey. Interesting as *probabilistic semantics* is, it deserves to be noted that it misrepresents Popper's own interpretation of what he had done, and this difference has led to some serious misunderstandings (by Leblanc and van Fraassen 1979, for example) concerning how probability and logic are related. Leblanc wrote of the result that every model of Popper's axiom system $\mathbb{B}$ is reducible to a Boolean algebra (*ibidem*, p. 264):

> The earliest theorem that probabilistic semantics boasts of is … a soundness theorem … roughly to the effect that if a boolean identity a = c … is provable … then $\mathfrak{p}(a, b) = \mathfrak{p}(c, b)$ for any statement b and any binary probability function $\mathfrak{p}$ meeting Popper's constraints [that is, satisfying $\mathbb{B}$].

But this is to read backwards Popper's claim to have established not the *soundness* of a *new* semantics for an *old* formulation of propositional logic, but the *completeness* of an *old* semantics for a *new* formulation of propositional logic: indeed, it establishes that the system $\mathbb{B}$ is strong enough to yield the probabilistic indistinguishability of the terms a and c, whenever a = c is an identity of Boolean algebra. For Popper, that is, Boolean algebra provides the semantics appropriate to a probabilistic theory of syntax, and not the syntax for which the theory of probability provides the appropriate semantics. (That Popper did not intend to construct a semantical theory has been appreciated by Stalnaker 1970, n. 8.) Both interpretations are permissible, but they ought not to be confounded. For further details, see Miller (2004).

4. THE LOGICAL INTERPRETATION OF PROBABILITY

4.0. Early Ideas

At the outset (§48) of chapter VIII of (1935a), the chapter on probability, Popper distinguished what he calls subjective and objective interpretations (Schroeder-Heister *ibidem*, §9.4). This distinction was continued in (1983), Part II, chapter I. He classified as subjective not only the subjectivist interpretation proper (Gillies 2000, chapter 4; Galavotti 2005, chapter 7) but also something that he called the *logical interpretation*. The former medley of views, often known now as Bayesian personalism or simply *Bayesianism*, share the idea that the value of $\mathfrak{p}(A)$ is particular to an agent and (to the extent that he is consistent) measures his *actual degree of belief* in the statement A. Bayesianism is associated mainly with the names of Ramsey, de Finetti, Good, Savage and their successors. But despite an extended critical treatment of 'the subjective theory' in the chapter cited, and in the succeeding chapter of (1983), Popper hardly mentioned these writers. His attack was directed largely at what he saw as a failure of any subjectivist position, according to which probabilities are continually updated, to explain the stability and testability of objective statistics. De Finetti's representation theorem is nowhere mentioned, even though some of Popper's examples, such as the game of 'Red or Blue' mentioned on p. 232 above, are directly relevant to it (Gillies *ibidem*, pp. 77–83).

The logical interpretation is often identified with the doctrine that $\mathfrak{p}(A)$ records not the *actual* but the *rational degree of belief* in A; or, better, $\mathfrak{p}(A, C)$ records the rational degree of belief in A *given* C (Gillies *ibidem*, chapter 3; Galavotti *ibidem*, chapter 6). Modern Bayesianism was the direct outcome of Ramsey's scepticism concerning the objectivity of even the non-numerical degrees of belief posited in Keynes's variant of the logical interpretation (1921). Popper too was sceptical, but like Keynes and like Waismann (1930), p. 9, who was influenced by von Kries (1886) and Wittgenstein (1921), and in some respects also like Carnap (1950), § 55B (who, however, identified the logical interpretation of probability with 'inductive logic'), he held that assertions of logical probability are 'about what may be called the "logical proximity" of statements' (1935a, § 48):

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

The connections between deducibility (derivability) and unit probability, and between inconsistency (contradiction) and zero probability, that are made in this early sketch may be contrasted with the more subtle probabilistic definitions of logical relations that the exigencies of the axiomatic approach forced Popper to formulate later (see § 3 above). The final sentence of this passage evidently implies also that $\mathfrak{p}(C, B) \leq \mathfrak{p}(A, B)$ whenever C is logically stronger than A in the presence of B. This much holds for any function $\mathfrak{p}$, defined on statements, that conforms to the axioms of $\mathbb{B}$. We may wonder if it is possible, within the bounds of logic, to say more about the values of the function $\mathfrak{p}$. If, as Popper hoped, the degrees of falsifiability of logically incomparable statements can sometimes be compared by reference to their dimension (1935a, § 38), then his remark that '[*t*]*he logical probability of a statement is complementary to its degree of falsifiability*' (§ 34) promises further guidance. Later he proposed implicitly (1983, Part II, § 15; 1959a, appendix *vii) and even explicitly (1957b, point 3) that we have to '*identify logical independence with probabilistic independence* (special multiplication theorem)', a proposal that, for all its intuitive appeal, will not do. For in all but the simplest languages the demand that (complete) logical independence imply probabilistic independence is demonstrably false. The implication can hold at most for the atomic statements of the language (as indicated by Wittgenstein 1921, ¶¶ 5.15–5.152), and for statements that are equivalent to atomic statements of equivalent languages. If A, C are atomic, for example, and probabilistically independent, then {A, A $\leftrightarrow$ C} and similar pairs are logically independent; and when $\mathfrak{p}(A) = \mathfrak{p}(C) = 1/2$, they are also probabilistically independent. But other statements that cannot be probabilistically independent stand in the relation of logical independence (Popper and Miller 1987, n. 2).

Persistently scathing about Carnap's (1950) project to fix the numerical values of the logical probability function by reference to the syntactic structure of rudimentary formal languages (1959a, Preface; §38, n. *2; appendices *viii and *ix), Popper adopted a more considered position not dissimilar to Ramsey's: in general '*there cannot be a satisfactory … [measure] of logical probability which is based upon purely logical considerations*' (1957b, point 3). He endorsed what he called 'the topological approach', comparing probability theory in this respect with [affine] geometry, where there are many interesting results that are independent of any spatial metric. Two notable examples from the theory of probability are examined below. A third example is the definition, in terms of logical probability, that Popper (1954) offered for *degree of corroboration*. Popper made it plain at the end of this paper that his definition is best understood not as a proposal for a numerical scaling of corroboration, but as a consistency proof for the desiderata for corroboration, and a proof thereby that, whatever logical probability may be, degree of corroboration is something else. (For a highly critical discussion, see Díez 2011.) It is a pity that there is no space here to inspect the definition, since 'the doctrine that *degree of corroboration … cannot be a probability* … [is] one of the more interesting findings of the philosophy of knowledge' (Popper 1959a, p. 394), and surely one of the more contentious. To Popper's central argument leading to this thesis we now turn.

4.1. Logical Probability Varies Inversely with Content

'Not for nothing', Popper wrote (1935a, §6), 'do we call the laws of nature "laws": the more they prohibit, the more they say.' And the more they say, the more possibilities they prohibit, and the more improbable they are, in any sense of 'probability' satisfying the system $\mathbb{B}$ (or the weaker systems mentioned). The probability of a statement, that is, varies inversely with its content. Popper proposed (1954, point 8, and elsewhere; see also Bar-Hillel and Carnap 1953, p. 149) that the absolute improbability $1 - \mathfrak{p}(\mathsf{H}) = \mathfrak{p}(\mathsf{H}')$ of a hypothesis H provides an adequate measure $\mathfrak{ct}(\mathsf{H})$ of its content. Here the prime $'$ stands for classical negation, as it did above. Other measures are possible, but this is the simplest. '*Since we aim in science at a high content*', he asserted many times, '*we do not aim at a high probability*' (1963a, chapter 11, §6). Declaring that only highly falsifiable hypotheses can be highly confirmed or corroborated, he added the more doubtful claim that the degree of falsifiability, or the *empirical content*, of an empirical hypothesis varies directly with its content (*ibidem*, addendum 1), and went on: '*Since we want a high degree of confirmation (or corroboration), we need* … *a low absolute probability*' (*ibidem*). These are the main planks of his thesis that in science we prefer improbable hypotheses to probable ones (see also 1959a, § 83, notes *2 and *3). It is a thesis that has provoked wide-eyed incredulity.

Disbelievers such as Carnap (1966), § 2, and Michalos (1971), chapter I, § 4, have objected that although $\mathfrak{ct}(\mathsf{H})$ varies inversely with *absolute* probability $\mathfrak{p}(\mathsf{H})$, this is not generally so for *relative* probability $\mathfrak{p}(\mathsf{H}, \mathsf{E})$, where E is the evidence. The inequalities $\mathfrak{ct}(\mathsf{H}) < \mathfrak{ct}(\mathsf{K})$ and $\mathfrak{p}(\mathsf{H}, \mathsf{E}) < \mathfrak{p}(\mathsf{K}, \mathsf{E})$ are compatible, provided that H and K are not comparable by logic alone (for if $\mathsf{H} \vdash \mathsf{K}$, then $\mathfrak{p}(\mathsf{H}, \mathsf{E}) \leq \mathfrak{p}(\mathsf{K}, \mathsf{E})$ for any E). Rosenkrantz, exercised by this 'probability–improbability conundrum', found solace in the assumption that 'in practice' we tend to pit against each other only hypotheses that are 'mutually exclusive' (1977, § 6). I do not know that Popper ever replied directly to this line of defence. But a quarter of a century before the cited criticisms were uttered, he had opposed the doctrine, which he ascribed to Keynes, that '[a] theory is regarded as scientifically valuable only because of the close *logical proximity* … between the theory and [accepted] empirical statements. But this means nothing else than that the *content* of the theory must go *as little as possible* beyond what is empirically established' (1935a, § 83; the italics were added in the translation in Popper 1959a). Both here and in the passage from (1935a) that was quoted in the third paragraph of § 4.0 above, Popper in effect assumed that the part of the content of a hypothesis H that goes beyond the content of the evidence E is inversely related to the value of the relative probability $\mathfrak{p}(\mathsf{H}, \mathsf{E})$. We shall question this assumption in § 4.2.

An extreme version of the thesis that we do not judge a hypothesis by how probable it is, even how probable it is relative to accumulated evidence, is encapsulated in Popper's claim that, if H is a universal hypothesis, then the only proper value for the absolute logical probability $\mathfrak{p}(\mathsf{H})$ is zero. This conclusion was acknowledged grudgingly by Carnap (1950), § 110F (though Zabell 2007, § VII.1f., suggests that it should not have come as a surprise) and by several Bayesians (see Jeffrey 1975a, p. 107). It implies that $\mathfrak{p}(\mathsf{H}, \mathsf{E}) = 0$ for any finite evidence report E (a similar conclusion is drawn in the fourth paragraph of § 2.4 above). Popper's attempted proof that $\mathfrak{p}(\mathsf{H}) = 0$ whenever H is universally quantified (1959a, appendix *vii) is one of those places (recorded in § 4.0 above) where he rashly assumed a tight connection between logical independence and probabilistic independence. Howson (1973) has explained why the proof is invalid. Indeed, the conclusion is in general false: Theorem 2.5 of Horn and Tarski (1948) shows that in a denumerable language of the kind supposed there exist absolute probability measures with positive values for all consistent statements. Gillies (1995), §4, later defended, by appeal to the Dutch book argument, Popper's thesis that universal hypotheses ought to be assigned zero probability (for a discordant consideration, see Rosenkrantz *ibidem*, chapter 6, n. 18). The resort here to an argument rarely found outside subjectivist treatments of probability may, however, not be the irony that Gillies feared, but a simple *reductio ad absurdum*. Indeed, this is the best way to look at all the qualitative or 'topological' results presented in this section. They all take the form: if the logical probability of a hypothesis is an important aspect of its assessment, then an undesirable result follows.

Scientific progress, according to Popper, typically involves the invention of universal hypotheses that at places revise (and therefore revoke) their predecessors, but also go appreciably beyond them; that is, of hypotheses that in some sense surpass in content competing hypotheses with which they are not logically comparable. In (1959a), appendix *vii, he recognized a difficulty due to what he called 'the fine structure of content': that if $\mathfrak{p}(H) = 0$ for every universal H, then all such hypotheses have the same measured content $\mathfrak{ct}(H)$ (defined as $1 - \mathfrak{p}(H)$). Despite ingenious attempts to soften this unwelcome result, including an unsuccessful explanation, in terms of their relative problem-solving ability, of how one hypothesis may qualitatively improve upon a competitor (1972/79, chapter 2, §8, and appendix 2(3)), he did not dispel the suspicion that if $\mathfrak{p}(H) = 0$ means that a universal hypothesis cannot be inductively confirmed (because it implies that $\mathfrak{p}(H,E) = 0$), then it means also that a new universal hypothesis cannot both emend and extend an old one. From Popper's perspective, the neatest solution to this difficulty (though he never resorted to it) is to allow the function $\mathfrak{p}$ to take values in a field that contains infinitesimals (whose reputation was salvaged by Robinson 1966). It is then possible to attribute infinitesimal probabilities of different orders to universal hypotheses with markedly different contents, and the thesis that a universal hypothesis H can never be confirmed by finite evidence E can be modified to say that $\mathfrak{p}(H, E)$, although sometimes greater than $\mathfrak{p}(H)$, is always itself infinitesimally small. 'The fact that Popper has not used them need not mean that infinitesimals are unPopperian' (Jeffrey 1975b, §IV).

The doctrine that the logical probability of a hypothesis is a measure of how well it is empirically confirmed or backed by evidence has had a rocky history in the last ninety years, but still survives. Keynes's version of the doctrine (1921) was criticized by Popper in (1935a), §83. The underlying thesis of Carnap (1950), stated in (3) of the Preface, was that the degree of confirmation of H by E should be identified with $\mathfrak{p}(H, E)$, but in §86 of the same book it was claimed that H is confirmed by E if and only if the degree of relevance of E to H, often called the degree of support of H by E, which is defined as $\mathfrak{s}(H, E) = \mathfrak{p}(H, E) - \mathfrak{p}(H)$, exceeds zero; that is, if and only if $\mathfrak{p}(H, E) > \mathfrak{p}(H)$. Popper (1954), point 6, had little difficulty in proving the misjoinder of these two claims, since a hypothesis H whose absolute probability $\mathfrak{p}(H)$ is low enough (but positive) may be confirmed by E yet have a lower degree of confirmation on E (that is, in Carnap's scheme, a lower relative probability) than a disconfirmed hypothesis K whose absolute probability $\mathfrak{p}(K)$ is high enough; that is, there may exist hypotheses H, K and evidence E such that

(5) $$\mathfrak{p}(H) < \mathfrak{p}(H, E) < \mathfrak{p}(K, E) < \mathfrak{p}(K).$$

For example, let H be the hypothesis that a fair die shows 6, E be the evidence that it shows an even number, and K be H′, the negation of H.

Extraordinary efforts were made (for example, by Michalos *ibidem*, chapter III, and Salmon 1975, § 2) to brush Popper's argument aside, and sometimes to ignore it altogether (Zabell 2007, § VII.4), but it is now generally agreed that he was right (see n. 4 to the Preface to the 2nd edition, 1962, of Carnap 1950, and text; Fitelson 2006, p. 393, and 2007, p. 479, note 11 and text). Indeed, in exalting 'the extremely fundamental and sometimes unnoticed distinction between degree of confirmation and degree of relevance', Salmon (1969, § 1) endorsed in variant vocabulary the thesis that Popper had advanced from the outset: that the *degree of empirical support* $\mathfrak{s}(H, E)$ (and hence Popper's degree of corroboration) enjoyed by a hypothesis H on an item of evidence E, here called the degree of relevance of E to H, are distinct from its *probability*, $\mathfrak{p}(H, E)$, here called its degree of confirmation. Many of those persuaded of the viability of inductive reasoning now agree that $\mathfrak{p}(H, E)$ is not a good measure of the extent to which E confirms H, and that some function that, like the support function $\mathfrak{s}(H, E)$, increases as $\mathfrak{p}(H, E)$ increases and as $\mathfrak{p}(H)$ decreases does the job better (see, for example, Jeffrey 1975a, pp. 112–16). It is often observed that $\mathfrak{p}(H, E) \geq \mathfrak{p}(H)$ whenever E is a logical consequence of H, so that $\mathfrak{s}$ to some extent captures the idea, so attractive to verificationism, that a hypothesis is supported by its consequences. What is less often observed is that the function $\mathfrak{s}$, unlike the function $\mathfrak{p}$, does not in any obvious way generalize the deducibility relation $\vdash$. Inductive logic, it seems, is not after all an extension of deductive logic. On this matter, see the revealing discussion in Salmon *ibidem*, and for other uncomfortable truths about $\mathfrak{s}$ and related functions (including Popper's measures of degree of corroboration), see Salmon (1975), § 3.

4.2. Probabilistic Support Is Not Inductive Support

In (1983) Popper and Miller purported to demonstrate that although a hypothesis H (for which $\mathfrak{p}(H) > 0$) can be supported by evidence E, in the sense that $\mathfrak{s}(H, E) = \mathfrak{p}(H, E) - \mathfrak{p}(H) > 0$, such support is wholly due to the existence of a deductive relation between H and E (namely, that their contents overlap), and disappears when they are, in the appropriate sense, deductively disconnected. (Some history of the result can be gleaned from Popper 1983, Part II, § 15, especially n. 2.) They argued, in particular, that that part of the content of H that properly goes beyond the content of E, called the *excess content* $\mathcal{E}(H, E)$ of H over E, *is never supported by* E: invariably $\mathfrak{s}(\mathcal{E}(H, E), E) \leq 0$. To understand the force of this claim we must look more attentively at the relativization of the function $\mathfrak{ct}$ introduced in the first paragraph of § 4.1 above.

The definition $\mathfrak{ct}(H, E) = 1 - \mathfrak{p}(H, E)$, which is stated explicitly by Popper in (1972), chapter 2, § VII, and chapter 9, § VII, is obvious enough, ploddingly generalizing from absolute to relative probability the singulary measure $\mathfrak{ct}(H)$ of content. But it is not possible simple-mindedly to take this binary function $\mathfrak{ct}(H, E)$ to measure the excess content $\mathcal{E}(H, E)$, since $\mathfrak{ct}(H, E)$ exceeds $\mathfrak{ct}(H)$ only when $\mathfrak{p}(H,E) < \mathfrak{p}(H)$. More particularly, the valid identity $\mathfrak{p}(H, E) = \mathfrak{p}(E \to H, E)$, where $\to$ represents the material conditional of classical logic, is equivalent to $\mathfrak{ct}(H, E) = \mathfrak{ct}(E \to H, E)$; and this, according to the suggested reading, would imply that the measure of that part of the content of H that is not contained in the content of E is equal to the measure of that part of the content of $E \to H$ that is not contained in the content of E. Yet *none* of the content of $E \to H$ is contained in the content of E. They are subcontraries and have no common elements (since logical truths, though consequences of all statements, are excluded from all contents). It follows that $\mathfrak{ct}(H, E) = \mathfrak{ct}(E \to H)$, and accordingly that $\mathfrak{p}(H, E) = \mathfrak{p}(E \to H)$. The invalidity of this identity has, however, been known for many years (see Reichenbach 1949, § 85, formula (9) and text). It can indeed be proved (Popper 1963a, addendum 3, formula (22) and text) that

$$(6) \qquad \mathfrak{p}(H, E) - \mathfrak{p}(E \to H) = -(1 - \mathfrak{p}(H, E))(1 - \mathfrak{p}(E)),$$

a result that has been refined in the lemmata to Theorem 3 of Popper and Miller (1987). According to (6), the 'conditional probability' and the probability of the conditional are equal only in cases that do not arise when H is a scientific hypothesis and E is the result of a severe test of H (for definitions of severity that also mention background knowledge B, here suppressed, see Popper 1963a, addendum 2, formulas (3) and (5)).

In (1972/1979), chapter 2, § 7, Popper suggested that $\mathfrak{ct}(H, E)$ can be understood as 'the class of all statements deducible from H in the presence of E, but not from E alone'. This suggestion, which indicates that the simple-minded proposal of the previous paragraph should have been restricted to H, E for which $H \vdash E$, is not vulnerable to the criticisms just outlined. But it does not determine $\mathcal{E}(H, E)$ in general, nor the best way of measuring it. A few years later Popper and Miller (1983) observed that $\mathfrak{ct}(E \vee H) + \mathfrak{ct}(E \rightarrow H) = \mathfrak{ct}(H)$, and indeed that the material conditional $E \rightarrow H$ is the unique statement that is subcontrary to E and, conjoined with $E \vee H$, which is the common content of E and H, is equivalent to H:

$$(7) \qquad (E \vee H) \vee (E \rightarrow H) \equiv \top$$

$$(E \vee H) \wedge (E \rightarrow H) \equiv H,$$

where $\top$ represents any logical truth. They accordingly identified $\mathcal{E}(H, E)$ with the material conditional $E \rightarrow H$, and its measure with $\mathfrak{ct}(E \rightarrow H)$. These identifications were not at all original, the first having been proposed by Hempel (1960), p. 465, for example, and the second by Bar-Hillel and Carnap (1953), p. 151. The identification of $\mathcal{E}(H, E)$ with the material $E \rightarrow H$ had also appeared in discussions of the application of the Ramsey elimination theorem to the problem of theoretical terms (Tuomela 1973, p. 59). But in this instance it fomented much dissent. Since $E \rightarrow H$ and $E \rightarrow (H \wedge E)$ are logically equivalent, an immediate consequence is that $\mathcal{E}(H \wedge E, E)$ is the same as $\mathcal{E}(H, E)$, which implies that $\mathfrak{ct}(E \rightarrow H)$ measures what Popper had in (1979) proposed to measure by $\mathfrak{ct}(H, E)$. It should be noted, however, that when used only to compare the excess contents of rival hypotheses H, K with respect to the same evidence E, the simple-minded proposal suffices. For provided that $\mathfrak{p}(E) > 0$, it follows from (6) that $\mathfrak{p}(E \rightarrow H) < \mathfrak{p}(E \rightarrow K)$ if and only if $\mathfrak{p}(H,E) < \mathfrak{p}(K,E)$, and hence that $\mathfrak{ct}(E \rightarrow H) < \mathfrak{ct}(E \rightarrow K)$ if and only if $\mathfrak{ct}(H,E) < \mathfrak{ct}(K,E)$.

In sum, $\mathfrak{ct}(E \rightarrow H)$ is a reasonable, though far from perfect, measure of $\mathcal{E}(H, E)$. What $\mathfrak{ct}(H, E)$ measures in general is anyone's guess. *A relative measure of content*, which $\mathfrak{ct}(H, E)$ doubtless is, is not perforce *a measure of relative content*. Mura (1990), § 3, and Mura (2008), § 3, offer an original discussion of the problem of measuring relative content, and of several other issues raised in this section.

The theorem of Popper and Miller, that E never positively supports (or is never positively relevant to) $\mathcal{E}(H, E)$, follows at once from the definitions of $\mathfrak{s}$ and $\mathcal{E}$, the identity (6), and some simple facts of the theory of probability. For

$$(8) \qquad \mathfrak{s}(\mathcal{E}(H, E), E) = \mathfrak{p}(\mathcal{E}(H, E), E) - \mathfrak{p}(\mathcal{E}(H, E))$$

$$= \mathfrak{p}(E \rightarrow H, E) - \mathfrak{p}(E \rightarrow H)$$

$$= \mathfrak{p}(H, E) - \mathfrak{p}(E \rightarrow H)$$

$$= -(1 - \mathfrak{p}(H, E))(1 - \mathfrak{p}(E)) \leq 0.$$

The result is valid for all H and E (and not only those H and E for which $H \vdash E$, as suggested by da Costa and French 2003, pp. 146–48). Unlike some stronger results, the claim that E never supports $\mathcal{E}$(H,E) holds good for most measures of support, including the alternatives to $\mathfrak{s}$ paraded by Fitelson (1999). A short proof from more general assumptions has been given by Mura 1990, n. 1 (see also Miller 1994, chapter 3, n. *b)*. The theorem is closely related to the celebrated results of Lewis (1976) to the effect that, on pain of triviality, there exists no conditional operator $\rightsquigarrow$ such that $\mathfrak{p}$(A, C) = $\mathfrak{p}$(C $\rightsquigarrow$ A) for all appropriate probability measures $\mathfrak{p}$. A proof has been given by Leblanc and Roeper (1990) that, within the system $\mathbb{B}$, a generalization of this identity implies not only that C $\rightsquigarrow$ A and C → A are probabilistically indistinguishable but that $\mathfrak{p}$ takes only the values 0 and 1.

The moral drawn by Popper and Miller was that support defined in terms of logical probability has no inductive aspect, since evidence E does not support, and usually undermines, every statement whose content goes beyond that of E. Many objections have been raised to this interpretation, most prominently the complaint that $\mathcal{E}$(H, E) has been misdefined. Popper and Miller (1987) went on to establish several related results and, especially in § 3, they replied to many criticisms. A survey of the literature is to be found in Rochefort-Maranda (2004), and a full bibliography is maintained by Rochefort-Maranda and Miller (2016).

5. A PERSONAL CONCLUSION

Having worked with Popper for many years on problems in the theory of probability, and partaken in many of his predilections, I am not the person to provide a fully disinterested assessment of what he accomplished in this field, and what his work presages for its future. The following summary, indeed the whole paper, is therefore confessedly partisan, although I hope that with regard to the past it accurately records what Popper said, and that with regard to the future it remains loyal to what he hoped.

Popper's contributions to the frequency, propensity and logical interpretations of probability, and to the axiomatization of the theory of probability, have been evaluated in §§ 1, 2, 4 and 3, respectively. The treatment of the frequency interpretation has, regrettably, been quite superficial, and Popper's imaginative incursion into this domain, especially his proposed solution to the fundamental problem of the theory of chance, still awaits sympathetic but critical adjudication. Not all his interventions there, or elsewhere, were successful. He made errors, some of which have been specified; and he overestimated at times the conclusiveness of some of the critical considerations that he adduced. This can hardly be gainsaid, given that the deployment of the theory of probability in explanations of how evidence and theory relate in science, and outside science, is as energetic as it ever was. The relentless war that Popper waged against Bayesian subjectivism, and other relics of the verificationist and inductivist traditions, is nowhere near its end, though Popper's work perhaps marks the end of the beginning. The battle is far from having been won, but the battlefield, on which many have struggled, against Popper, alongside him, and in defiance of both parties at once, has been irreversibly transformed.

Despite the continued openness of some of the problems concerning probability to which Popper devoted such efforts, at least two of his innovations are assured of a permanent place in the philosophy of science. One is the propensity interpretation, which may need further refinement in order to resolve difficulties raised in some of the works listed at the end of § 2 above, but, especially in its later and more acute version, will forever offer an eloquent description of the probabilistic features of the universe. The other resplendent jewel is his axiomatic treatment of the theory of probability, a little logical and mathematical triumph that is likewise of enduring value. Popper's critical rationalist philosophy, so precious to those of us who have been liberated by it, may be at risk from obdurate resistance and contumelious neglect, but his axiom systems for probability and the functions, often called *Popper functions*, that obey them, are here to stay.

Note: Throughout the paper, I have invariably used Popper's own translations of his works that were originally written in German. But in quoting from other authors, including Popper, I have discreetly brought the notation into line with that used elsewhere in the present chapter.

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FRANK JACKSON

10 Popper's Philosophy of Mind

Karl Popper's renown rests on his work in the philosophy of science and political philosophy, but he had firm views in the philosophy of mind, expressed most fully in his contribution to Popper and Eccles (1977) (see part I of this work, but also part III which is a series of conversations between Popper and John Eccles, the renowned neuroscientist). His views in the philosophy of mind have received comparatively little attention. There are a number of reasons for this. To start with, he was a dualist interactionist of a traditional kind (as was Eccles), and this has for some time been an unpopular position among philosophers of mind. Some version of materialism, the view that we minded creatures are nothing over and above very complex aggregations of the kinds of ingredients that make up the material world, is widely accepted. Secondly, Popper's terminology was non-standard: for example, in Popper and Eccles (1977), pp. 53–54, panpsychism and epiphenomenalism are classified as versions of materialism (more on what these theories are later in this chapter). This is of no great moment in itself, but it has dissuaded readers. Finally, his position on the mind was set within his metaphysics of three worlds: World 1, World 2 and World 3, set out, for example, in Popper (1972). He saw the recognition of the importance of World 3 as a key part of his contribution to the debate over the mind, and his three-worlds metaphysics is controversial, especially his views about World 3.

I think his views on the mind have been unduly neglected. His discussion of World 3 and its bearing on the mind-body problem bring out important issues for theories of mind. I will, however, be suggesting that World 3 does not support his anti-materialist views about the mind in the way that he thought it did.

I start by giving an unvarnished version of his theory of mind, a version that makes no reference to the three worlds. I then explain his three-worlds metaphysics as a preliminary to a critical discussion of the role of this metaphysics in his philosophy of mind. In the final section, I briefly consider Popper's discussion of the identity theory version of materialism, the version he thought of as the strongest version of materialism. I will be suggesting that his critique of the identity theory fails through a tendency to conflate the theory with, on the one hand, some kind of denial of the reality of consciousness, and, on the other, with epiphenomenalism.

The last thing I should mention in this introduction is a kind of meta-philosophical point one should bear in mind when considering Popper's views on the mind (as Jeremy Shearmur reminded me). Popper was very open to the idea that we might be surprised by future developments and be forced to reconsider matters we had thought closed. For just one example, although he was very critical of radical materialism – roughly, the view that denies the existence of mental states (see discussion later in this chapter for something less rough and for his criticisms) – he was careful to say that it just might turn out to be correct. Popper put his ideas about the mind with characteristic directness, but often they should be read with a caveat. The armed forces have research units dedicated to avoiding what is euphemistically called 'technological surprise'; perhaps Popper's caveat might be thought of as a reminder about philosophical and scientific surprise. Many philosophers of time had to do some radical re-thinking when the special theory of relativity came along. I think Popper had it in mind that something similar might, just might, happen in the case of the philosophy of mind.

1. POPPER'S VERSION OF DUALIST INTERACTIONISM: FIRST PASS

In common with most recent philosophers, Popper had little time for idealism: the view that tables, electrons, the planets, human bodies and the physical world more generally are some kind of mental construct. At the same time, he denied that pains, thoughts, desires and mental states in general were items in the physical world – states of the brain, the functional roles played by those states or anything along those lines; he denied, that is, the kind of position popular currently in the philosophy of mind. In consequence, he faced the question of how our minds are related to our bodies and especially our brains, and he took the position that mental states, though distinct from physical or material ones, causally interact with them. In particular, our bodily states and our mental states are distinct from one another, but on occasion cause and are caused by each other. Our pains cause bodily movements, and disturbances in our bodies cause pains; a book may cause thoughts, and in turn thoughts may cause a book; and so on. The obvious interaction point is somewhere in the brain, and this is why Popper was very interested in testing his ideas in discussion with Eccles.

His version of dualist interactionism was traditional in the style of Descartes (1641) in that it was not spelt out in dual attribute style. Some (e.g. Campbell 1970) who have rejected the materialists' view that mental states are nothing more than certain purely physical states of the brain have restricted themselves to insisting that mental reality involves extra properties that fail to appear in the physical inventory of the kinds of properties possessed by our bodies and brains. Mental states are brain states, but at least some of them are not 'purely physical' brain states. Typically, it is consciousness and those phenomenal mental states that seem to be especially closely connected with consciousness – aches, tickles, pangs, perceptions of colour and the like – that are said to have the 'extra' properties. But on dual attribute views, there is no suggestion that mental events or mental states involving consciousness are 'other-worldly' events or states; they are states of the brain (say), but with extra properties. Popper's view, however, was the stronger one that mental states are additional states, not brain states with additional features. Moreover, he held that although the phenomenon of consciousness was a good reason to deny materialism, the case against materialism did not rest on the phenomenon of consciousness. There is reason enough, he thought, from arguments concerning rationality, the self and cognitive mental states like the grasp of a scientific theory. Materialism cannot handle the 'feely' side of psychology, but equally, he held, it cannot handle, for example, rationality. If materialism were true, we could not be rational. Here he was in agreement (again) with Descartes.

The principal novelty of Popper's views on the mind-body problem lies in their location within his three-worlds metaphysics. This will be a focus in what follows, and we now turn to outlining this metaphysics.

2. POPPER'S WORLDS AND THE MIND-BODY PROBLEM

World 1 is the physical world – tables, bodies, brains, planets and so on: the world as studied in the physical sciences. World 2 is the mental world – ideas, pangs, perceptions, thoughts, selves and so on. In these terms, his dualist interactionist theory of mind holds that the items in World 1 and in World 2 are distinct but on occasion interact with each other. World 3 enters the picture in that it is a large part of his case for this view about Worlds 1 and 2. ('It is one of the central conjectures proposed in this book that the consideration of World 3 can throw some new light on the mind-body problem' [Popper and Eccles 1977, p. 47].) But what is World 3?

For Popper in Popper and Eccles (1977, p. 38), World 3 is 'the world of the products of the human mind, such as stories, explanatory myths, tools, scientific theories ... works of art'. Popper talks of *embodiments* of World 3 objects, an example is a book which expounds some given scientific theory. He points out that these embodiments will be World 1 objects and sometimes the conclusion from this point seems to be that World 1 and World 3 overlap, but other times it seems that the conclusion is merely that some embodiments of World 3 objects, not the World 3 objects themselves, are World 1 objects. The unclarity turns out to matter when we discuss the causal roles of World 3 objects. If embodiments of World 3 objects are themselves objects in World 3, there is no question but that some World 3 objects enter into causal interactions with World 1 and World 2. Books which expound theories, for example, interact with thoughts, bodies and printing presses. The only live issue will be whether *all* World 3 objects do. More on this later in the chapter.

What is in any case clear is that Popper holds that *some* World 3 objects are not World 1 objects. Some things we create are not physical things. When we create a proof in mathematics, we may well create a series of lines on paper giving the proof, and various things will be happening in our brains, but the proof itself, the thing we grasp when we understand the proof, is not the set of lines on paper and nor is it the happenings in the brain. It is not anything in World 1. Nor, he held, is it something in World 2. Our understanding of the proof is a mental state and so an item in World 2, but *what* we understand is not something mental. Or take the phenomenon of a theory being inconsistent. On Popper's view this is a property of an item in World 3. But the words on the pages of a book which expounds the theory – the World 1 embodiments – are not inconsistent per se; as we might say it, they are inconsistent only *under the intended interpretation*. Moreover, although a belief in the theory will be an inconsistent belief, this feature of an item in World 2 is parasitic on the inconsistency of the item in World 3. To take a simple example, why is it inconsistent to believe that there are finitely many primes? It is inconsistent because *what's* believed is inconsistent. The belief is inconsistent because what is believed – that there are only finitely many primes – is inconsistent.

So, on Popper's view, (1) there are World 3 objects; (2) some at least are neither World 1 nor World 2 objects – they are abstract objects, as he sometimes puts it; and (3) we create them – which is what makes them count as World 3 objects.

An important part of Popper's three-worlds metaphysics is that World 3 objects interact with World 1 objects. Atomic theory, a World 3 object, was part of the cause of the atom bomb, a World 1 object; and the motion of the planets, a World 1 phenomenon, was part of the cause of our theory of their motion, a World 3 object. Nonetheless, causal links between World 3 and World 1 always go via World 2. It was scientists' *grasp* and *thinking* about atomic theory that lead to the bomb, and it was astronomers' *observations* of the planets and their *calculations* that lead to our theory of planetary motion. World 3 and World 1 interact via the mind. Here is how it looks in terms of a famous myth. A falling apple, a World 1 event, caused Newton's belief in his theory of gravitation, a World 2 object, which in turn caused the theory itself, a World 3 object, but the apple could not have done the job without a mind, Newton's as it happens, playing its role. Going in the other direction, the theory of gravitation, a World 3 object, caused the embodiment of the theory in a book, a World 1 object, via the role of Newton's grasp of his theory, a World 2 object (the grasp, that is).

How does all this help with the mind-body problem? In four main ways, according to Popper: it tells us that radical materialism is false; that there is interaction between World 1 and World 2; that this interaction cannot be explained in purely materialistic terms; and it helps us to see how there might be interaction between World 1 and World 2 despite their being, on the dualist theory Popper espoused, distinct realms.

3. RADICAL MATERIALISM

By radical materialism, Popper meant the kind of behaviourism found in Quine (1975) and, on the reading of Gilbert Ryle as an analytical behaviourist, in Ryle (1949). This view denies the existence of mental states in the sense in which we all deny the existence of limps. Although it is true that some people limp, there are no such things as limps. To limp is to have a behavioural tendency, not to have something called 'a limp'. Likewise, say radical materialists, although it is true that people are in one or another mental state at one or another time, there are no mental states. To be in a mental state is simply to have one or another behavioural tendency. The position thus differs from the eliminativism of, for example, Paul Churchland (1981), concerning the propositional attitudes such as belief and desire (so called because they are attitudes to propositions: for example, the belief that snow is white is an attitude to the proposition that snow is white), where scepticism concerning the existence of beliefs, hopes and desires is grounded on the idea that they are or well may be the posits of a *false* theory. Eliminativism holds that it is false that we have beliefs; radical materialism (behaviourism, as it is more commonly called) allows that it is true that we have beliefs but insists that having a belief is not to have something called a belief in the way that having a hat *is* having something called a hat.

Popper rightly pointed out in Popper and Eccles (1977, § 18) that radical materialism rests on a now largely discredited philosophy of science that held that positing 'hidden' causes of experimental data was explanatorily empty, and in addition that it flew in the face of introspective experience – someone in serious pain finds it hard to take seriously the denial of the existence of their state of pain: Isn't the pain the cause of their behavioural tendency? But these are, of course, familiar criticisms of radical materialism. Popper saw his three worlds as making a new point against radical materialism through the way the three worlds highlighted the importance of interaction between World 3 and World 1, the interaction invariably mediated by World 2 on his view. Theories have changed our world, he noted. This is so obvious that examples are hardly necessary, but we gave one above when we mentioned the role of atomic theory in leading to the atom bomb; another example is the fact that the computer this chapter is being written on has, among its causal origins, a great deal of theory. But if the role of theories, World 3 items, in causing explosions and computers, World 1 items, is via World 2 – thinking about and believing the theories, for example – we had better, noted Popper, admit World 2 items, mental states, into our ontology.

We might quarrel about how much the worlds' apparatus adds to the last point. The idea that mental states are posits of a very successful theory is widely accepted. Many discussions in the philosophy of mind, especially those under headings like *functionalism* (the view that mental states play distinctive causal roles – pain, for example, is a causal intermediary between bodily damage and behavioural response) and *folk psychology* (the view that we all have a theory of mind that guides our ascription of mental states to people) highlight the fact that positing mental states as causal intermediaries is part of a highly successful explanatory theory, and that this implies that these posits have more than respectable scientific credentials for admission into our ontology. For example, a good way of explaining someone's behaviour is in terms of belief and desire. Why did Jones take an umbrella? Because she believed it would rain and desired to stay dry. But the success of explanations in terms of posited states is a good reason to believe in the existence of posited states. It follows that we have good reason to posit states of belief and of desire.[1] Nevertheless, surely Popper deserves credit for highlighting the important role of a certain class of mental states, those that mediate between theories and what they cause and what causes them. The way observation can lead to belief in some highly complex theory, which in turn allows the construction of something as complex as a computer, is very striking and makes very well the point that mental states play important explanatory roles.

4. THE INTERACTION BETWEEN WORLD 1 AND WORLD 2

Popper largely took interaction between the physical and the mental as a datum. But there is some criticism of epiphenomenalism, which is the view that mental phenomena are causally impotent by-products of brain activity. He points out that epiphenomenalism attracts trouble from evolutionary theory. How come mind evolved if it does nothing and has no biological function? He also notes the objection that epiphenomenalists cannot allow that their reflections, thoughts and memories play a role in causing them to be become epiphenomenalists.[2]

Popper's distinctive contribution was in the way he argued that his three-worlds way of looking at the issue of mind-body interaction helped us to understand how it could happen, and why materialism could not explain it. Popper's line of thought on the second question is, I think, reasonably clear, whether one agrees with it or not. It is not so clear why Popper thought that the three-worlds scheme helped to understand mind-body interaction. I will deal with this issue first and we will need to quote the key, and it seems to me unhelpful, passage from Popper and Eccles (1977, p. 48).

> If we admit the interaction of the three worlds, and thus their reality, then the interaction between Worlds 2 and 3, which we can to some extent understand, can perhaps help us a little towards a better understanding of the interaction between Worlds 1 and 2, a problem that is part of the mind-body problem.
>
> For we have seen that one kind of interaction between Worlds 2 and 3 ('grasping') can be interpreted as a making of World 3 objects and as a matching of them by critical selection; and something similar seems to be true for the visual perception of a World 1 object. This suggests that we should look upon World 2 as active – as productive and critical (making and matching). But we have reason to think that some unconscious neurophysiological processes achieve precisely this. This perhaps makes it a little easier to 'understand' that conscious processes may act along similar lines: it is, up to a point, 'understandable' that conscious processes perform tasks similar to those performed by nervous processes.

The reason this passage is unhelpful is that it says nothing to the two key questions about mind-body interaction for dualists who hold, as Popper does, that mental states are distinct from states of the brain. The first is the question of how something 'outside' the brain and the physical world gets to make changes to the brain. All the evidence is that the physical world is causally closed. The causes and effects of each physical event are themselves physical events. It is clear that Popper rejects this position; what isn't clear is how putting matters in the terms of his worlds framework helps him to respond to the challenge posed by the empirical evidence for the view that the physical world is causally closed. The second is the question of where the point or points in the brain where the changes from outside get initiated might be located. In fairness to Popper, it should be noted that his remarks are highly tentative; all the same, it is hard to see how his remarks get started as a way of thinking about these issues. We are left in the dark on how the three worlds picture might help with these two familiar questions for dualist interactionists. Popper does, rightly, make the point that it would be wrong to assume that there cannot be causal connections between very different kinds of thing. The problem for dualist interactionism should not be thought of as arising simply from its positing of such causal connections. But the two problems noted are the lack of empirical evidence for the kind of causal discontinuity in the physical world, and in the brain in particular, which would make it reasonable to posit some kind of causal influence on the brain from 'outside', and the lack of empirical evidence concerning the location of the putative interface between the brain and the mind conceived of as something quite distinct from the brain.

However, on the question of how the three-worlds picture reveals materialism's failure to adequately handle the interaction between World 1 and World 2, Popper's line of thought is easy to state and its appeal is easy to see. We noted earlier that Popper highlights the causal interactions between those mental states that involve World 3 objects and physical states, and that some of the most interesting interactions between World 1 and World 2 involve World 3. Now, as we saw, on one reading of Popper he allowed that some World 3 objects are also World 1 objects (a particular computer or book, for example), but, on any reading, his view is that very many World 3 objects are abstract; they are not World 2 objects and they are not World 1 objects. But in that case they are beyond materialism's ken, Popper held. It follows that the many important interactions between World 1 and World 2 that involve abstract World 3 items are ones that cannot be explained within materialism's framework. Here's a simple example to make the point concrete. A scientist's belief in relativity theory (a World 2 object, the belief that is) may cause her to modify the words she puts on the page of an article (a World 1 object). The role of relativity theory in this causal transaction falls outside materialism's purview because relativity theory is an abstract object.

The interest of this challenge to materialism is clear and its discussion calls for a separate section. But let me finish this section by noting a curious feature of Popper's thought about those World 3 objects which he insists are abstract. Popper consistently says that World 3 objects are things we create. In the case of computers and books – the embodiments of World 3 objects if they are indeed themselves World 3 objects – this is clearly the correct view to take. Some given book or computer does not exist until it is made. But in the case of the World 3 objects that he describes as abstract, the ones that are *not* their embodiments and are *not* our thoughts about them, the claim that we create them is surely false. If one insists, as Popper did, that there is a sense in which relativity theory is both distinct from anyone's belief in it – it is what is believed, not the believing, and distinct from any statement of it in the sense of some string of sentences on a page or a computer screen – it is what is stated in the statement, not the stating itself, then the only viable position on relativity theory so understood would appear to be that it is an abstract entity in somewhat the sense in which numbers and sets are often said to be abstract entities. But in that case, we do not create relativity theory. We may create statements of relativity theory and belief in relativity theory, but not relativity theory itself. In this sense, relativity theory did not come into existence when Einstein stated it; what came into existence were statements of it and people who believed it.

5. WORLD 3 AND MATERIALISM

There are three separate issues raised by the argument against materialism drawing on World 3 retailed in the previous section. All are important.

The first issue is what a materialist should say about our knowledge of World 3 objects (or anyway the abstract ones which are our focus here). Materialists make trouble for epiphenomenalists by asking how their theory can explain how we could know about mental states if they leave no causal traces in the world. The same question can be asked of materialists about World 3 objects. How do we know about them if they leave no traces? Materialists say three things in response. One is that, in some sense or other, abstract objects do not exist, but it is convenient to pretend that they do. There are no numbers, but it is good to pretend that there *are* numbers in doing science (or, if it comes to that, in getting the right change at the supermarket). A theory of this general kind is to be found in Field (1980). A second is that although playing a causal explanatory role in a successful theory is one good reason to believe in a class of entities, as we noted in discussing the failings of radical behaviourism, it is not the only reason. Postulating the existence of abstract entities like numbers and sets plays a key role in science and mathematics; there are parts of both that require us to state our theories in terms of them. Although the role played is not causal, it is valuable enough to warrant admitting abstract entities like numbers and sets into our ontology. This is what we learnt from, for instance, Quine (1969), when he showed us the limitations of virtual classes in the development of set theory. Finally and most importantly in the context of the debate between materialism and dualism, materialists say that dualists face the same problem. Abstract entities do not leave traces in the space-time world full stop, be it entirely material or not. Supposing that mental states were states of 'ectoplasm' would not help, as there would be no traces in the ectoplasm, argue materialists. I suspect that Popper would have demurred at this last step but this is, it seems to me and many, a hard thing to do. Of course, there is no special problem about saying that beliefs in numbers leave causal traces; the problem is with the view that numbers *as such* do. It is a pity that (to my knowledge) he does not address the issue directly.

The second issue is whether admitting abstract entities is *in itself* giving up on materialism. 'Isn't materialism the doctrine that everything that exists is material, and abstract entities are agreed by all not to be material?' The answer to this question is that it depends on what you mean by materialism. If materialism is a theory of *everything*, it cannot admit abstract entities; it will have to embrace some form of eliminativism or reductionism about them (see Papineau, 1993). However, if materialism is a theory of mind that insists that mind can be fully accounted for without going outside the ingredients needed in the physical sciences, then abstract entities can consistently be admitted *if* it turns out – and it likely does, say many materialists – that the physical sciences need them. This was J. J. C. Smart's (1963) position.

The third issue is how a materialist might explain the way that mental states that involve attitudes to items in World 3 can have causal effects in consequence of being such attitudes. This issue is a separate one from the issue of the existence of those items and the issue of our knowledge of them. Take relativity theory, thought of as the proposition or content of that theory, not as any particular embodiment of it; thought of, that is, as an abstract entity like a proposition or a number. And suppose, as is very plausible and was something Popper very much insisted on, scientists' belief in relativity theory has effects in World 1, on, for example, the design of certain rockets and what gets written on the pages of journals. You could be sure that relativity so thought of exists and is known about, while still wondering how standing in the relation of believing to such an entity could play a causal role in World 1. How does a belief's being the belief that *p* have effects in virtue of being the belief that *p*?

There is a huge literature on this topic, often under the heading of the causal efficacy of content.[3] Many hold that the right approach to the problem is to argue that standing in the relation to an abstract entity like a proposition should be thought of as a way of describing a property which is not in itself a relation to an abstract entity, and the debate then revolves around what property it is which is so described and understanding that property's causal role. But what is important in the context of whether we have a difficulty for materialism is that it is not at all obvious that there is a special problem for materialism here. Is it easier to understand how a state in ectoplasm's standing in a relation to an abstract entity can have causal effects in World 1 than it is to understand how a state of a material brain standing in such a relation can have causal effects in World 1? How could the switch from material to ectoplasmic instantiation help?

In sum, Popper identified a key question for the philosophy of mind arising from World 3. How should we understand the relation between World 3 and World 1? He saw it an especially pressing issue for materialism. I have been suggesting that we can divide the question into a number of separate questions and that when we do this, it is unclear that they are especially hard for materialists *as such* to answer. They are everyone's problems.

6. Popper on the Identity Theory of Mind

According to the identity theory, mental states are, literally, brain states. Each and every mental state is identical to some state of the brain. Popper approved of the identity theory's realism about mental states. If mental states are brain states, they exist. He approved of its recognition that mental states play causal roles. If mental states are brain states, then they are causes and are caused, because brain states are causes and are caused. And of course the identity theory is often touted as the theory of mind most in tune with scientific approaches to the mind. One might have expected that Popper would welcome it, and as noted, it seems he did regard it as the best version of materialism.[4] There seem to be two main reasons he rejected it.

One reason goes back to World 3 and the issues we have already traversed. A principal source of the identity theory is the conviction that World 1 is causally closed. Causal paths that contain World 1 events contain only World 1 events. If this is correct, then anyone who thinks of mental states as able to cause and be caused by World 1 events must think of them as themselves parts of World 1. Which parts? Given the great importance of the brain to mental functioning, the obvious answer is certain parts or events or processes or states of the brain. Items in World 2 are one and all items in World 1. One of Popper's reasons for rejecting the identity theory was that he thought that once you realise the importance of World 3 to what happens in World 1 via the intermediate role of World 2, you realise that we should reject the doctrine that World 1 is causally closed and that is to reject a key underpinning of the identity theory.

Popper's other main reason raises different issues. Popper thought of the identity theory as presented, for example, by Herbert Feigl (1967), as being very different from that as presented by, for example, Smart (1963) and David Armstrong (1968). Popper's reading of Feigl sees Feigl as offering a theory with historical links to Leibniz's and Kant's views about 'things in themselves' – the heading to § 54 in Popper and Eccles 1977 is *The Identity Theory After Leibniz: From Kant to Feigl*. Popper thought of Feigl's version of the identity theory of mind as a kind of dual attribute or dual aspect theory. Mental states are brain states with extra properties. These mental properties are revealed by 'the internal illumination' Feigl (1967, p. 138, cited by Popper) talks of; they are available from 'the inside', to subjective experience but not to brain science *qua* brain science. (For our purposes, it does not matter whether this is the right reading of Feigl, but perhaps we should note that Feigl's way of stating the identity theory does not definitely exclude this reading in the way that Smart and Armstrong's statements of the theory do.) Popper pointed out that it is very hard to square a theory of this kind with the theory of evolution if you accept that World 1 is causally closed. The issue is essentially the one we noted earlier in connection with epiphenomenalism: How come the mental properties evolved if they play no causal role in World 1? Popper noted that panpsychism might seem to avoid this difficulty because it affirms that all matter has a mental nature along with its physical nature, so that there is no question of explaining how mental properties evolved; they were there right at the beginning. But as he also noted, first, it is hard to believe that, for example, electrons have a mental nature, and, second, it would be absurd to hold that all matter possesses the kind of rich set of mental properties that we have – for example, electrons don't believe relativity theory. Accordingly, there would still be the problem of explaining the evolution of the rich set of mental properties we display.

Popper's view about the identity theory as presented by Smart and Armstrong, with their very clear statements that the identity theory should *not* be thought of as a kind of dual attribute theory, was that, at the end of the day, they were denying the reality of consciousness and phenomenal mental nature. Popper was convinced that to hold that mental states were nothing more than brain states, having just the properties a neuroscientist *qua* neuroscientist might attribute to them, had to amount to some kind of denial of the reality of conscious mental nature. Although these days a minority view, Popper's view has, of course, many adherents: they are the philosophers who insist that conscious phenomenal mental states, states like aches and pains and perceptions of colours, have properties that outrun the brain and attendant functional properties that are the subject of study by neuroscientists. Typically, the view is supported by the zombie argument or the knowledge argument, or some variant on them.

The zombie argument is that there is a possible being that is a physical duplicate of me, a neurological and functional property-by-property copy of me, that feels nothing: my zombie twin. But I have a conscious mental life and it does not. It follows that my conscious mental life is something additional to my physical and functional make-up, as the latter is something I share with my zombie twin. The knowledge argument is that a creature who has never seen colours – perhaps they are totally colour-blind, or perhaps they are confined to a black-and-white room with all information coming in to them in black-and-white form (no colour illustrations in books in the room, all monitors are black-and-white ones, their shirt is either black or white, etc.) – might know all there is to know about the physical and functional make-up of someone who sees colours without knowing the highly distinctive kind of experience they have. It follows, runs the argument, that there is more to know about the one who sees colours than is given in the physical and functional account of them.[5]

Interestingly, Popper did not draw on arguments like these. It seems he took the failure of the reductive kind of identity theory favoured by Smart and Armstrong to be pretty much self-evident. Here is a typical passage (Popper and Eccles 1977, p. 94), discussing Armstrong (1968), to give the flavour of Popper's discussion.

> I feel inclined to regard the identification of *unconscious* mental processes with brain processes as a very important conjecture. And although I am inclined to assume that even conscious processes somehow 'go hand in hand' with brain processes, it seems that an *identification* of conscious processes with brain processes is liable to lead to panpsychism. . . .
>
> Armstrong's theory may either be classified as a radical materialism with a denial of consciousness, and criticised as such, or it may be classified as a not quite outspoken form of epiphenomenalism, *as far as the world of consciousness is concerned,* whose significance it tries to minimize. In this case my criticism of epiphenomenalism as incompatible with the Darwinian point of view applies. (Emphasis in the original.)

Here we have an interesting challenge to the identity theory but from a perspective that does not find a place for a position that is avowedly realist about consciousness while identifying it with purely physical and functional properties of states of the brain. To use the illustration Smart and Armstrong sometimes appealed to, it turns out that lightning is identical with an atmospheric electrical discharge. There is, quite literally, just the one phenomenon picked out in two different ways. *Mutatis mutandis* for the mental and the physical was their idea. This is the kind of position which, although the subject of great debate, is perhaps the most popular one with current philosophers of mind. Reading Popper, one gets the sense that he found the idea that conscious experiences might be literally identical with neurological states or their physical and functional properties so counter-intuitive that he had to read the writings of philosophers like Smart and Armstrong as either being espousals of a radical materialism that denies the existence of consciousness, or as some kind of epiphenomenalism or panpsychism.

NOTES

1 For references and more detail, see Braddon-Mitchell and Jackson (2007).
2 See, e.g. Popper and Eccles (1977, pp. 72–74) and Popper and Eccles (1977, p. 75), respectively.
3 See, e.g. Jackson (1996) and McGinn (1989).
4 But see Popper's reservations about its name (Popper and Eccles, 1977, p. 82).
5 For full expositions of these two arguments, see nearly any text in the philosophy of mind and the references therein, e.g. Braddon-Mitchell and Jackson (2007) or Campbell (1970), where zombies are called imitation people.

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IAN JARVIE

11 Popper's Philosophy and the Methodology of Social Science

The scope and depth of Popper's philosophy of the social sciences is not much appreciated beyond the small circle of specialists in his ideas.[1] True, his name and simulacra of his ideas are bandied about, especially with regard to certain labels and controversies.[2] His actual ideas in this field, most of them beholden to his general philosophy and his philosophy of science, have extensive implications for our empirical endeavours – this is seldom realized or acted upon.[3]

After summarizing Popper's philosophy and methodology of the social sciences systematically, if somewhat artificially, I shall in this chapter unpack the background, detail and some of the controversies. Of all Popper's many contributions, his intervention in the centuries-old discussions about the nature of human rationality is, I contend, his most fruitful and original contribution to contemporary thought about how to grow our knowledge of society (see *The Rationality Debate*, later in the chapter). By taking little or no account of his contributions, mainstream discussions of rationality impoverish themselves.

Nature and convention. The phrase 'methodology and philosophy of social science' presupposes some sort of a contrast between the social and the natural sciences. This contrast goes back to before Aristotle (*Nicomachean Ethics*, 1134b26; trans. Williams 1869, ch VII, 7, pp. 162–63), who wrote:

> Some, indeed, there are who hold that all justice whatsoever is of this kind [conventional], inasmuch as that which exists by nature is – say they – unalterable, and everywhere alike holds equally good; as, for instance, is the case with fire, which burns here exactly as it burns among the Persians; while that which is just experience shows to vary.

This passage reports a debate that involved articulation of a strong contrast between the natural and the social. Taken seriously, the contrast has ramifications for our attempts to gain knowledge of each (Popper 1945, I, chapter 5, notes). Two rival philosophies of social science presuppose this contrast: naturalism and conventionalism. Naturalism favours applying 'the methods of physics to the social sciences' (Popper

1944–45/1957, I, p. 2), treating assertions referring to convention as mere place-holders *pro-tem* for an eventual explanatory reduction of all social phenomena to the laws of natural science. Most commonly this reduction is to be achieved via psychology, the science of the mind, and the reduction of that, in turn, via the brain, to biology, chemistry and physics. The principal exception is Marx, who deemed economics, not psychology, the basis of all social science. Conventionalism is rarely known by that name, more often it is labelled *Verstehen*, or 'interpretativism', or 'culturalism', or, by Popper, simply and most clearly, 'anti-naturalism'. The labels cover a range: from the strong position that almost nothing about the study of the social bears comparison to the study of the natural through to the weak position that grants the possibility of some sort of science of the social but one that is *sui generis*. As we will see, Popper holds a middle position: he views the method of conjectures and refutations as universal, and explanation by reference to rational action within given conventions as specific to the social sciences.

Neither naturalists nor conventionalists question the ancient distinction. Popper's philosophy opens a way to challenge it (and he offers a third alternative). I quote Popper (1945, 1, chapter 5, end of sec IV) on machines:

> (Even mechanical engines are made, as it were, not only of iron, but by combining iron and norms; i.e. by transforming physical things, but according to certain normative rules, namely their plan or design.)

A machine, then, whilst obedient to physical law, is constituted by means of conventions such as plan and design. Hence, it cannot be reduced to nature or to convention without remainder. It would seem to follow that nature and convention are interwoven in human social life in a manner that may be difficult if not impossible to disentangle. If one accepts Popper's view that the natural attitude is one of 'naïve monism' (all laws and regularities are natural), this entanglement is still more apparent. Popper (1945, 1, chapter 10, II; cf. op. cit., 1, chapter 5, II) thought this attitude is natural and only social breakdown could free us from it.

It helps also to consider language, since it plays a central role in Popper's account of all objective knowledge including science. Language stands as an insuperable obstacle to the nature/convention distinction. Is language natural? In many ways, yes, but not in all. Thus, Plato said (*Cratylus*), a name is arbitrary, but to have names is natural. There is here an equivocation about the contrast that opens the way for Hegel's view that to have conventions is natural, so that all conventions are in a sense natural, so that strictly speaking there are no conventions.[4]

This line of thought diverts discussion from the dichotomy to the question: In what way is language natural? It is natural in that it uses physical and biological aspects of the world to create and transmit sound, sounds in the range that humans are capable of producing. These sounds are used to express, to signal, to describe, to negate and so on. Language is, however, more than its physical and biological enabling conditions. The bewildering diversity of human languages – and, in many cases, their mutual unintelligibility – makes it obvious that language is also conventional. So language is a set of conventions that make use of our physical capabilities. This answer can be generalized so that it shows the error of Hegel's obliteration of all convention: though it is natural to make conventions, no convention is completely natural. This generalization does not fit at all comfortably with the polarized nature/convention distinction. The polarized dichotomy has to go. Language is also an institution or set of institutions that Popper takes, with Hayek, to be a paradigm of the unintended consequence of human actions. So viewed, language is reducible neither to the natural nor to the conventional, but combines elements of each.[5] This mixture, like all mixtures that do not fit the dichotomy, does not have a name, or an ancient lineage. This mixture nonetheless permeates all social life, and so it should permeate all social thought, but it does not, even though it does permeate all of Popper's social thought. It is one of the wellsprings of his later metaphysical division of the world into World 1 (the physical world), World 2 (the mental world) and World 3 (the world of objective contents of knowledge, and of social institutions) (Popper 1972).

Machines, language and other items of social life point to the conclusion that in writing of Popper's 'philosophy and methodology of social science' one already begs a question: the demarcation between the natural and the social and hence between the natural sciences and the social sciences is an issue for consideration *in* the philosophy of the social sciences and not one that is pre-scientific. As we have noted, some of the literature classified into this field contests whether there can be scientific study of social life at all; other writings describe a social science so idiosyncratic that use of the label 'scientific' seems perverse. Whether Popper was clear that his work constituted a criticism of the ancient dichotomy I am unsure. He first said he had never endorsed the classical dichotomy between nature and convention in 1974 (Popper 1974b, 2, p. 1116).[6] This should suffice for us, since all evidence indicates clearly that his philosophical work is inconsistent with the traditional, naïve contrast. That he was unbothered by the variability of convention is also clear: he did not fetishize certainty and he denied that it was offered by natural science. There were repeatable facts of social life as there were of nature; this was what made a science of the social possible at all (Popper 1944–45/1957, IV, 28).[7]

In his Scientific Method lectures at the LSE in the 1950s Popper classified the sciences not into natural and social, but by their aims or interests in questions theoretical, historical or applied (elaborating on Rickert and Poincaré).[8] There were examples of each in the natural and in the social sciences. A dominant interest in explanatory laws or mechanisms made for a theoretical science, such as physics or sociology. A dominant interest in initial conditions made for an historical science, such as geology or social history. A dominant interest in practical application or prediction made for an applied science, such as engineering, physical or social.[9]

Ontology or what there is. The social world, like the natural world, consists of many sorts of things. What we recognize as things, what classifications we make, and how we differentiate between things and classes is governed by our overall metaphysical point of view, what Popper (1945, 2, chapter 25, III; Jarvie 1960) in *The Open Society* calls 'general interpretations'. General interpretations or metaphysics are the untestable general presuppositions behind our ways of looking at the world (specific or singular interpretations are theories). Whilst criticizable, and sometimes fruitful or suggestive, such general interpretations, being compatible with all experience, are not empirically refutable. Some might be contradictories; more likely they are contraries or just differences of emphasis (or, as Popper has put it, points of view). Thus, the idea that humans seek freedom may give rise to an interpretation, says Popper, that inspires the writing of the history of freedom; yet a contrary alternative, that humans fear freedom, may give rise to the writing of a no less interesting history of humanity as shunning freedom. The merits and defects of such alternative interpretations can be debated. He also claims that much conventional political history is nothing but the history of political power, which in turn 'is nothing but the history of international crime and mass murder' (Popper 1945, 2, chapter 25, IV). But most humans are not criminals and their feelings about freedom may be ambivalent, so that the ideas behind the interpretation, Popper says, are not to be taken literally. Interpretations are nonetheless unavoidable: we cannot even describe without selecting, and once we select we interpret, and even use principles of interpretation, knowingly or not. Within our general interpretations we can individuate empirical facts by means of descriptions and seek out problems such as how might we resolve contradictions discovered between those statements of fact and our articulated expectations/folk claims.

The facts individuated in the social world include individuals, social institutions and traditions. To differentiate the latter two, Popper (1949, p. 52–53; 1963, p. 133) writes:

[W]e may say, perhaps, that we are inclined to speak of institutions wherever a (changing) body of people observe a certain set of norms or fulfill certain *prima facie* social functions (such as teaching, policing, or selling groceries) which serve certain *prima facie* social purposes (such as the propagation of knowledge, or protection from violence, or starvation), while we speak of traditions mainly when we wish to describe a uniformity of people's attitudes, or ways of behaviour, or aims or values, or tastes. Thus traditions are perhaps more closely bound up with persons and their likes and dislikes, their hopes and fears, than are institutions. They take, as it were, an intermediate place, in social theory, between persons and institutions.

Institutions are undesigned (the majority), designed or mixed. Examples of the (more-or-less) undesigned would be natural languages, the family, the old path through the forest. Examples of the (more-or-less) designed would be businesses, laws, government, clubs, universities, theoretical systems, literary forms, musical works and the paved new path through the forest. Such a distinction uses ideal types. As is obvious from the examples themselves, most institutions, whether they began without or with design, end up as mixed cases like machines, like the townscapes just grown and later designed.[10] Another distinction is more important to Popper. Individuals are concrete things; persons are not quite; personalities are even less. The other objects of social science, notably institutions and traditions, are variously characterized as 'abstract objects', '*theoretical* constructions', 'models' used to interpret our experience and explain it. They are not less real for all that, if only because they are bound to feature in our construal of the situation in which we act (Popper 1944–45/1957, IV, 29), so that in any case we may ignore the designed aspect of one institution and the spontaneous aspect of another (cf Boland 1982/2003). These abstract objects are to be cashed out in concrete terms of acting individuals, 'their attitudes, expectations, relations' etc. Popper's wording in this crucial passage is far from clear and gives hostage to the idea that he is an ontological individualist, not just a methodological one. How exactly individuals are concrete and institutions and traditions are not is hard to discern, yet he uses it to reject Hayek's idea that we can 'directly' observe the social world. 'Attitudes, expectations, relations' would seem to involve interpretation and hypothesis. Those impressed by Hume might not wonder if individuals, too, were hypothetical explanations of experience, hence abstract or theoretical. It is then no wonder that methodological individualism is one of the topics on which there has been lots of commentary. I will add to this only two points that I think need to be borne in mind. One is that Popper, like Hayek, opposes holism in moral and political deliberations: terrible things were done to real people in the name of collectivities. Second, this leads to the idea that human purposes and lives should not be sacrificed for collective and social purposes, rather the other way around. Methodological individualism is a choice freighted with moral and political considerations. The primary moral consideration, according to Popper (1945, 1, chapter 5, n. 18), is our moral responsibilities.

Values, Scientific and Extra-Scientific. Something further needs to be said about the role of values in the social sciences in particular. Popper's views diverge radically from the received views. He does not, for example, claim that natural scientists and natural science are utterly value-free or fully objective. His theory of objectivity is social: to the degree that it exists, objectivity resides in the institutional framework of science that is designed to check those biases of individuals that infect their scientific results. And we should accomplish this by fashioning scientific institutions so as to expose to criticism all scientific claims, including criticism of bias of all kinds. The underlying reason for rejecting the idea that the sciences should or can be value-free is the obvious point, noticed by Weber, that pursuing the truth and avoiding bias are themselves values. So the issue is not about the value 'value-freedom', but about whether one set of values, scientific values, is not confused with the rest, with the extra-scientific ones. Whilst admitting that scientific values such as truth, relevance, simplicity and so forth are deeply anchored in extra-scientific value judgements, including religious ones, Popper (1961/1992, pp. 73–75) sees it as the role of the critical institutions of science to check any tendency to confuse the two sets of values in the assessment of scientific results.[11]

Something also needs to be said about political values (not a full-blooded treatment of Popper's politics – for that, see Jeremy Shearmur's contribution [Chapter 13] to this volume). Various commentators have argued that Popper's methodology of the social sciences carries clear political implications. His sceptical emphasis on the limits to our knowledge, his insistence that even the most well-intentioned actions have unanticipated, unwanted and possibly self-defeating consequences, and the moral insistence that there is hardly ever any excuse for experimenting with people's lives all converge on a rather conservative politics: attempts to engineer social change should be modest and strongly hedged. Yet both *The Poverty of Historicism* and *The Open Society and Its Enemies* embrace the phrase and the thought of 'social engineering'. It made uncomfortable critics as diverse as Hayek (in correspondence) and Rush Rhees (1947). On the one hand, there was the epistemologically and methodologically sceptical Popper, stressing our limitations. On the other, there was Popper the young Viennese radical and social worker fired with the cause of social reform. When *The Poverty of Historicism* was republished in 1957, it was no wonder it was subject to attack by the then–New Left, since it provided an armoury of arguments to show that their agenda could not be carried out and that even trying to do so was morally questionable. It is also noteworthy that when Popper moved from the democratic welfare state of New Zealand, which he admired, to the United Kingdom, where a newly elected socialist government had begun building a welfare state, he inserted into the revisions of *The Open Society* a note explicitly warning of socialist abuse of power. He argued that the Labour government had taken powers to direct labour 'without compelling need' because of failure to grasp that expanding the discretionary economic powers of the state was a step down the road to abuse of power (Popper 1945, 2, chapter 17, VII and n. 28.[12]) There was, then, to the very end, a clear and laudable conflict between the Popper who hoped for social improvement and the Popper whose philosophy of the social sciences implied that enhancing the power of the state, even for such improvement, was only warranted under 'compelling need'. To resolve this conflict, according to Popper, is possible only stepwise, judging it carefully and seriously and critically case by case.

Nominalism. Popper was a consistent and unremitting methodological nominalist. That is to say, he excluded from science and relegated to the domain of general interpretation all inquiries into what a person or individual really or essentially was, into how an institution really differed from a tradition, or into any inquiry into the final catalogue of all the furniture of the world. He condemned all such inquiry as barren 'methodological essentialism' and criticized it as stemming from the desire for certainty and hence as fundamentally anti-scientific. The quest for certitude is particularly pernicious as it slides into verbal discussions. These characteristically start from questions such as 'What is so-and-so?' or, more clearly, 'What is so-and-so really/truly/essentially?' In *The Open Society* Popper laid methodological essentialism squarely in the lap of (Plato and) Aristotle and, with Russell, he echoed the traditional Enlightenment view that Aristotle had been a dead hand on the growth of science for almost 2,000 years. Popper's methodological nominalist vision of scientific knowledge envisaged it as in a constant state of flux and growth. Entities that are one day postulated in order to explain may be replaced another day. Popper views language, concepts and even postulated entities as tools that we use in the process of conjecturing and trying to refute in our struggle to increase our knowledge of the world. Hence, how we answer the question 'what is so-and-so' depends on our theory of so-and-so, and hence a theory change leads to a conceptual change, and with no finality in science there are no final entities for the final concepts to depict and no final concepts for the methodological essentialist to reach. The entities, persons, institutions and traditions discussed above are the common-sense ontology Popper deployed in his own first-order attempts at explanation – for example, his explanation of the social phenomenon of science – together with those deployed by social scientists whose work he had learned from. He rejected some of the ontology of Marx on both scientific and moral grounds. He accepted much of the ontology of economics on the same grounds.

Popper combined his methodological nominalism with a robust ontological realism, not quite so straightforward a position in regard to the social world as to the natural world. The social entities postulated by common sense and in some cases by social science are experienced as real, indeed almost as physical. We can work on them, he said, as we can work on pieces of furniture. In an unpublished lecture he says: individuals come into the world not naked, but 'clothed' in social settings; they act in social situations; they are surrounded not only by physical things but by social institutions; and he stresses that social institutions are experienced by us as if they were physical things. They are experienced by us like physical hurdles.[13] Popper (Popper Archives, Hoover Institution Archives 12–1 'Ethical and Methodological Individualism') wrote:

> The rule of the road is sometimes developed into a hurdle on some, let us say one-way streets or motorways or streets where there is a hurdle between the left side and the right side. But we experience it as a hurdle, even if it isn't a hurdle. So institutions are experienced by us as if they were physical things.

Much commentary implies that Popper was an ontological individualist, that is, someone who denies the reality of social things, traditions, institutions, social forces. As we have seen, his own texts are partly responsible, denying that social institutions are concrete things like crowds of people, seeing them, rather, as abstract models – yet they are experienced as if they were (physical) obstacles (Popper 1994b, p. 167). His denial of the collectivist claim that undesigned social entities are in possession of aims or goals or the ability to act on their own is much more salient and less confusing (Popper 1945, 1, chapter 7, n. 23; Popper 1963, pp. 341, 350).

Method. Method and metaphysics are closely linked. Popper consistently tried to show that metaphysical claims were important not least because they favoured different methodological recommendations. His own explicit rule that he practised systematically was to try to translate intractable metaphysical ideas into discussible methodological rules, and to discuss them while staying with common-sense metaphysics whenever possible.

There is, according to Popper, only one method of inquiry. It is the same for social science or any other attempt to gain knowledge of the world. It is the effort to be clear, to focus on problems and to treat solutions as occasions for criticism and hence for learning. As David Miller has elegantly phrased it, '[O]ur conjectures have to be criticizable if they are to deserve to be entertained; for critical argument is the sole control that we have over our meditations and our dreams' (Miller 1983, p. 11). Popper's view of the quest for knowledge is that it is very much like groping in the dark. It is when we touch or bump into something that we know it is there, not before (Popper 1972, p. 360). But a touch or a bump may give only local or partial information. We may now know that there is something, not mere thin air, in front of us, but the touch or bump may mislead us when we conjecture what it is that we are encountering. It remains the case that the touch or bump or further touches and bumps are the only check on our speculations. This is Popper's famous Socratic doctrine of the *via negativa* which says that we learn by correcting our errors, from refutation of our conjectures, from touching and bumping into the real; in any case not from confirmations, since they can be spurious, and they often are. Methodologically, social life is no different from physical nature. Our descriptions create problems because they are not consistent with expectations or with one another. Only if we can find ways to bump them against reality can we get any purchase on the world.

Methodological inquiry must be driven by practical concerns, Popper contends. Notice his use of the word 'practical' – not 'technological' or 'applied' – meaning deriving from genuine first-order research (Popper 1944–45/1957, III, 19). Popper refers here to such things as the marginalist revolution in economics, which was driven by research needs. Another example, taken from my PhD research with Popper (Jarvie 1964), would be the fieldwork revolution in anthropology, driven by the criticism that the previous preparation for anthropology – classical education and a speculative temperament – was no substitute for the study of distinct unfamiliar societies. From the refuting corrections provided by fieldwork there flowed further methodological discussions of the relations between fieldwork, theory and speculation about the history of social life. The practical concern helps methodology to make our research more fruitful, more intellectually far-reaching by confronting it with the way things are in the social world. Popper always privileges getting at the truth of things as the aim of science. In context this view is clearly a sideswipe at the rather tormented and fanciful debates about the metaphysics of the social and the historical to be found in nineteenth-century German philosophy (Popper 1944–45/1957, III, 19).[14]

> The more fruitful debates on method are always inspired by certain practical problems which face the research worker; and nearly all debates on method which are not so inspired are characterized by that atmosphere of futile subtlety which has brought methodology into disrepute with the practical research worker. It should be realized that methodological debates of the more practical kind are not only useful but also necessary. In the development and improvement of method, as of science itself, we learn only by trial and error, and we need the criticism of others in order to find out our mistakes; and this criticism is the more important since the introduction of new methods may mean a change of a fundamental and revolutionary character.

Query: How do these words apply to Popper's own work? In other words, was he a philosopher standing outside the practical arena of the research worker? More specifically, were his strictures about historicism, methodological individualism, rationality, unintended consequences, the Oedipus effect and so on cases of "futile subtlety"? The answer I suggest is 'no': Popper's reflections on these matters emerge from his engaging in first-order as well as philosophical reflections on society and its problems, and his efforts to assess various proposed remedies partly by the methodology they invoke (Jarvie 1999, 2001a, 2001b). Indeed, in his youth as a social worker and teacher Popper was keen to understand how society worked but found that the most fashionable explanations in his circle, Marxism and psycho-analysis, were uncritical. *Logik der Forschung* is among other things a social theory of science (Jarvie 2001b) and *The Open Society* is among other things an attempt to explain fascism as a token of 'the perennial revolt against freedom'.

An illustration of Popper's use of metaphysics to forge rules of method is his short discussion of what he calls 'the conspiracy theory of society' (1945, 1, chapter 14; 1948; 1949). The phrase has entered the language, though seldom in ways informed by Popper's intentions. The conspiracy theory is his label for the metaphysical view that a satisfactory explanation of a social phenomenon consists in the identification of the person or persons who have planned and conspired to bring it about. Sometimes this is phrased as 'who benefits?', with the implication that those who benefit may well have arranged what happened. Somehow, perhaps by hearing the phrase but not reading the original, the view is abroad that Popper thinks conspiracies do not happen. Popper did not say that conspiracies were phantoms or that we should not use them in our explanations. His point was different: disclosure of a conspiracy is insufficient for a satisfactory explanation. More is needed: testability;

for example, by what means was the conspiracy consummated? Without that the conspiracy theorist is committed to the dogmatic view that the social stuff is malleable. Hence, if the conspirators do not succeed, the explanation ready to hand is a counter-conspiracy. Popper (1945, 2, chapter 14) does not view society as so easy to mould:

> Social life is not only a trial of strength between opposing groups – it is action within a more or less resilient or brittle framework of institutions and traditions and it creates – apart from any conscious counter-action – many unforeseen reactions in this framework, some of them perhaps even unforeseeable.

This point is made in the course of discussing the deficiencies of psychologism and affirming 'the autonomy of sociology'. Popper (*Ibid.*) used the conspiracy theory of society to illustrate how a common way of thinking underestimates 'the unwieldiness, the resilience or the brittleness of the social stuff, of its resistance to our attempts to mould it and to work with it'. He grants that society is full of conspiracies, of people and groups trying to achieve specific outcomes. There are so many conspiracies, though, that it is obvious that most of them fail. To explain the few that succeed, then, we have to specify the situation, the conditions, the actions and the inaction that permitted success. Conspiracies are not their own explanation. A successful conspiracy still needs social explanation (this point eludes Pigden 1995; cf. Keeley 1999; Clarke 2002). Popper here made a major contribution to how we should think about society, about the intractability of the 'social stuff'. He made it in a context of criticizing the deficiencies of psychologism and affirming the autonomy of sociology.

Explanation. According to Popper as well as according to Descartes, William Whewell, J. S. Mill and perhaps already Aristotle, an explanation is a deductive link between statements. In expounding his view in *The Poverty of Historicism* Popper (1944–45/1957, IV, 28) paraphrased and quoted from the German of *Logik der Forschung*. The problem to be explained is formulated as a statement of fact. That statement is explained when it is deduced from some other statements. Trivially, any statement can be deduced from itself, or from a contradiction, or from a conjunction of it and any other statement. Hence, this criterion of explanation as deduction is too easy to satisfy. Deduction is, then, a minimal or necessary condition for explanation. Without it there can be no criticism. When we specify further, we leave the formal realm. We want a *satisfactory* explanation, one that does not simply reiterate itself, contradict itself or tack on a conjunction. Drawing from Popper's methodology of natural science, one could specify that the primary criterion of satisfactoriness would be that the explanation be empirical. And, as Popper explained in his *Logik der Forschung*, the empirical is the empirically testable, namely the empirically refutable. A testable explanation that has been repeatedly and severely tested is more satisfactory still, and its having stood up to tests – if this is the case – makes it even more so (Popper 1944–45/1957, IV, 28).[15]

The two social scientists that Popper most admired were Plato and Marx. (The severity of his criticism of them and the vulgar confusion of criticism with censure have together managed to mask this obvious fact.) In his lengthy commentaries on their work that take up the bulk of *The Open Society* he finds some of their explanations satisfactory, that is, worth criticizing, which is not to say that he finds their claims to be true. Explanations have to be at least in principle deductions, and Popper demands that the premises of the deductions should in the social sciences meet conditions that are both steeper and simpler than in the natural sciences. I shall come to these presently.

Background remarks. Popper never laid his thought out as a system. His ideas are highly integrated and develop somewhat over time. Yet to set them out as a system, or in the manner of a textbook, or as I have done, is to do them violence, albeit unavoidably.[16] Popper's ideas on the philosophy of the social sciences, as with all his ideas, arose in the course of working on problems. There is a received view, partly subscribed to by Popper himself, that he was a philosopher of physics who, after publishing his major methodological work *Logik der Forschung,* turned to consider its application and modification to the social sciences. This makes him sound like a systematic philosopher. Chronological evidence for this view is that the earliest draft of this first major work on the social sciences, 'The Poverty of Historicism', was read at Brussels in early 1936, and at the London School of Economics – that is, subsequent to the publication of *Logik der Forschung* in late 1934. Counter-evidence is that what that paper tried to show was 'how "historicism" inspired both Marxism and fascism' (Popper 1974, I, sec. 24). In other words, he started working on a first-order problem that was then current: the intellectual and other errors that lay behind the totalitarianisms of his time rather than the second-order academic exercise of extending his philosophy. No doubt he made use of, even corrected and modified, his work on the methods of the natural sciences. But the aim was problem solving, not system-building.

The received view needs contextual correction in three ways. The first and most important is that the methodology of *Logik der Forschung* is in part a social proposal. I have compared it to a proto-constitution for the Republic of Science (Jarvie 2001a, 2001b; cf. Stokes 1998, pp. 4–7). As noted above, in *Logik der Forschung* Popper was already thinking about science socially. The second context in which to place the received view is that Popper was from childhood surrounded by social scientists amongst his family and family friends. He had closer contact with social scientists than with physicists. This continued when he did social work, became a schoolteacher, did research in educational psychology and acted as a political activist. The third context puts all this together and views Popper as a social scientist of sorts. He put forward some very interesting ideas for political thought and social and political reform before *The Poverty of Historicism* was given its final revision for journal publication (Jarvie 1999).

The first set of social problems that he consciously tackled concern the evaluation of the scientific and political claims of Marx. As early as 1920, by his own account, and certainly by 1928, he seems to have tried to write about Marx, sifting the true from the false, and the tenable from the untenable. Nothing survives of this effort except a letter to Hayek which says that the systematic summary and critique of Marx's work in *The Open Society* is its culmination (Popper to Hayek, 14 March 1944, Popper Archives, Hoover Institution Archives 305–13; cf. Hacohen 2000, p. 326, n. 142).[17]

He focuses on *historicism* as the central error presupposed by many of those, progressive and reactionary, who participated in the social and in political debates of his time. Historicism is a cluster of views the core idea of which is that social and historical sciences aim to predict the future much as astronomy predicts eclipses. Such predictions were claimed to be possible only on the basis of some law, rhythm, pattern or trend of human destiny. The search for such laws, rhythms, patterns or trends is the method of 'historicism'.[18] One way to think of historicism in Popper's sense is that it is essentialism with regard to history. That is, it seeks the essence or the moving force of history, or, more specifically, of historical periods, the ethos of an age, the spirit of the age: the *Zeitgeist*. He sees this clearly as he traces essentialism back through Aristotle to Plato, and finds there the characteristic combination of progressive language and reactionary essentialism that was so familiar from twentieth-century nationalist and totalitarian political discourse.

The Poverty of Historicism was not Popper's first publication on the philosophy of the social sciences. That place goes to the much humbler 'What is Dialectic?' of 1940. It was a swingeing critique of the ideas behind a popular buzzword. It ends with a plea that we not seek the laws of the movement of history, but rather study the critical methods of science. 'The Poverty of Historicism' aimed at (1) controverting what Ryle called 'the juggernaut theory of history' in its (a) naturalist and (b) anti-naturalist versions (Ryle 1947). Popper was the first clearly to distinguish 'naturalism' and 'antinaturalism', these being his labels for versions of the juggernaut theory that did (e.g. Marx) and that did not (e.g. Hegel) claim scientific status for themselves.[19] (2) Another aim was to commend some methodological approaches to social science over others.[20] Popper was pro-naturalist in that he endorsed the unity of scientific (or critical) method: there were repeatable social facts (or regularities) that it is the task of the social sciences to explain. He qualified his naturalism not because of traditional arguments from meaning or the uncertainties introduced by the human factor, but because the social sciences had to cope with the Oedipus effect and to utilize the rationality principle (Jarvie 1982).

The Oedipus effect. Oedipus kills his father and sleeps with his mother mainly because, in horrified reaction to the prophecy that he will do so, he goes into exile with the result that he is aware neither of who he is, nor of who his parents are, or that he is destined to fulfill the prophecy. Put simply, knowledge, prediction or even prophecy can play a part in bringing about subsequent developments or in precluding them. This aspect of human social formations has no parallel, it seems, in natural systems such as formations of planets or rocks, or in chemical processes, and such like. In human society, information can influence the situation to which the item of information refers.[21] The Oedipus effect itself is as ancient as the Oedipus myth, of course. What Popper said about it is new: the effect complicates explanation in the social sciences because account must be taken of such a feedback loop. Also, incidentally (and in the wake of others, including Poincaré), Popper makes use of the Oedipus effect in his argument that the future of humanity cannot be predicted because it depends upon the growth of knowledge (including predictions), and that this makes it unpredictable in principle.[22]

In brief, it is not the Oedipus effect but its integration into the philosophy of the social sciences that is Popper's contribution. The effect itself is allowed for by building it into the description of the situation and of the agent's appreciation of the situation. Politicians know, for example, that public opinion polls can affect the public opinion they are polling. The buyer and seller know that information about market conditions is a significant part of market conditions. Agent ignorance of, or uncertainty about, information and its effects are at the core of some of the exercises of game theory (see below).

The Oedipus effect may complicate social explanation; *the rationality principle*, by contrast, simplifies it. There being an element of rationality in most social situations, it is possible to model social actions and interactions using a few simple assumptions and to treat the resulting models as approximations, just as the physicist does. (The influence of Max Weber is obvious, though complicated, as Hacohen [2000, pp. 471–76] indicates). The models can be constructed on the assumption of complete rationality (action towards clear goals under perfect knowledge) and then research can estimate the deviation of the actual behaviour of people from the predictions derived from the model. The closer the two are, the more has been explained by the model; the further apart the two are, the closer to a refutation of the model. Popper held this rational element of explanation to be the most important difference between the methods of the social and the natural sciences (Popper 1944–45/1957, IV, 29).[23] It is worth noting that both neoclassical economics and game theorists proceed in this manner. A serious objection from Popper's point of view would be the pervasive instrumentalism – even conventionalism – used to defend the models (see Boland 1982/2003; Popper 1994b). Theories and models are attempts to describe the world – to treat them instead as simply means of generating predictions was to embrace a stultifying method.

Neither of the differences with natural science – the Oedipus effect and the rationality principle – licensed the rampant historicism of the social sciences. In criticizing this doctrine one of Popper's most cogent arguments focuses on trends. His discussion of trends cuts a swathe through much social science. A trend, he argues, is produced by initial conditions. Any change in those conditions can result in alteration, even reversal, of the trend. It is this fact which encourages intervention in social life. If trends are seen as independent of initial conditions then they are 'absolute' trends. Absolute trends can never be falsified because they amount to the existential claim that there exists such and such a trend: they are metaphysical or interpretative. The historicist claims that we are subject to absolute trends. This is prophecy, not science.

The Open Society and Its Enemies is primarily a work of social philosophy that also delves into the methods of sociology and economics as part of a critique of Plato and of Marx. Popper suggested that Plato's essentialism distorted the history of political thought by posing questions that invited authoritarian answers. In a long and exemplary exercise in the sociology of knowledge, Popper argued that Plato had a partially hidden totalitarian agenda that much of the educated élite of Western Europe swallowed. About Marx, Popper suggested that whilst for the most part his heart and his values were in the right place, his absorption of the historicist method from Plato and Hegel vitiated his scientific writings in ways that contradicted the values he was supposedly endorsing.[24] Where *The Open Society and Its Enemies* goes beyond 'The Poverty of Historicism' as a work on the philosophy and methodology of social science is in its more generalized treatment of scientific method and its concession of the importance, even inescapability, of metaphysical presuppositions, especially general historical interpretations. To accommodate this change Popper developed a new theory of rationality: an idea is rational to the extent that it is open to criticism. This was his major breakthrough.

In *Logik der Forschung* Popper had attempted, among other things, to characterize empirical science in a way that brought out its contrast with psychoanalysis and Marxism, albeit mostly implicitly (Popper 1963, chapter 1, opening paragraphs). When writing *The Open Society*, his emphasis became more historical, especially drawing out similarities between his own views and what he took to be Socrates' teaching about inquiry and intellectual honesty as distinct from that of Plato's *Republic* and later works. This led him to generalize his view of the value of criticism from the purely logical and empirical to any and all sorts of rational arguments. Ideas could also be assessed from the point of view of their internal logical consistency, their consistency with other assertions already held, their adequacy as solutions to the problems that they came to solve, and their capacity to stimulate new ideas, to help to see new connections and more. Each of these kinds of criticism he used in his examination of Plato and Marx and the problems they had tackled. To capture this move to a broader critical point of view rather than the narrower falsificationism, Popper favoured the label 'critical rationalism'.

A central plank of *Logik der Forschung* is that the mandatory social cooperation of scientists is what confers objectivity on their results. The traditional accounts of objectivity – the purified mind, the unbiased observer, the pure language of observation – are dispensed with. It is the well-designed and well-maintained institutions of science that enable it to make contact with reality. Popper's sights were set on avoiding the view of individual experience as the foundation of science. He similarly opposed John Stuart Mill's view that explanation in the social sciences should be limited to laws of human psychology, a position he labelled 'methodological psychologism'. Despite his strictures, methodological psychologism, like historicism, remains rampant. Current social explanations continue to favour psycho-analysis, or humans modelled as rational calculators, or endless and pointless philosophical discussions of beliefs, intentions, even, in philosophy, of 'collective intentions'. The problem here is well illustrated by Gorton's (2006) monograph on Popper. Gorton insists that the expulsion of psychology to the extent possible, in which Popper follows Weber, impoverishes the situational approach. He complains that when Popper looks at irrational factors, he tries to treat them as rational. (Popper claims that this is what Freud did.) As an alternative, he discusses Jon Elster's idea on the explanation of revolutions, claiming that it is situational and that it is enriched by the inclusion of psychology.[25] This is confusion, since Elster does not embrace methodological psychologism. What he does is enrich his model of the revolutionary actors by general features that he calls 'everyday Kantianism' (= universal cooperation is better than universal defection) and 'everyday Calvinism' (= by acting on the symptoms one can treat the cause). Popper would approve of this if and when adding these claims to a theory adds new possible tests to it. The main psychological element Elster brings in is that of 'magical thinking', the belief that some thought, action or utterance will itself bring about your aim. As this is a variant of wishful thinking, and as Popper has employed that notion, Gorton's case fails. The hypothesis that a group of actors in a situation are prone to wishful thinking at times increases testability. This then would be a structuring factor in the social scientist's situation and one of the most obvious discrepancies between the situation to be explained as it is assessed by the actors and as it is found to be by the social scientist.

By 1945, then, Popper had written extensively about society, the social sciences of history, sociology, political science and economics, as well as about the philosophy of the social sciences. He was about to take up an appointment at a world centre of the social sciences, the London School of Economics (LSE). Yet for the next fifty years only a handful of papers dealt with the social sciences. Some of the handful, it is true, are very important. In order of their appearance, the very important ones are: 'Prediction and Prophecy and their Significance for Social Thought' (1948), 'Towards a Rational Theory of Tradition' (1949), 'Public Opinion and Liberal Principles' (1955), 'The Logic of the Social Sciences' (1961), 'Models Instruments and Truth' (1963) and 'A Pluralist Approach to the Philosophy of History' (1967). In my view, the main emphasis should be on the second and fourth of these, since they contain the most new material, and so they are discussed below. 'Models, Instruments, and Truth' is perhaps the most discussed after its eventual publication in part in 1969 and in whole in 1994. For it was in that paper, addressed to the Harvard Economics Department, that Popper tried to clarify his notion of the rationality principle, of the logic of the situation, and to show why the use of the idea of models as approximations need not be instrumentalist.

The problem Popper sets himself in 'Towards a Rational Theory of Tradition' is what can the rationalist reply to conservative traditionalists such as Burke and Oakeshott. Can the rationalist handle tradition? Was the radical anti-tradition attitude of the Enlightenment correct? Contrary to the Enlightenment tendency, Popper willingly conceded the importance of tradition. He distinguished traditions from institutions, as we have seen, but then linked them by conceding that institutions need to develop traditions for their smooth working and continuity. In later writings he would say that the institutions of democracy needed traditions, but that the traditions could not develop without the institutions. Their growth required time, stability and vigilance. Traditions are, then, something that can be articulated and hence compared and criticized. Noticing the presence and significance of a rationalist tradition going back at least as far as Socrates (in 1958 he extended it to Thales or to his immediate disciples), Popper rejected radical anti-traditionalism as inconsistent. He argued that there is a reason why we need tradition. It is a need for order and predictability without which we become anxious and terrified.[26] Jeremy Shearmur taxes Popper with indulging in psychologism here, although Popper is pointing to a feature of acting in very many social situations (but decidedly not all; hence no psychologism is implied): that acting in them is scarcely possible without some minimal expectation of stability (Shearmur 1996, p. 74–75; Gorton 2006, p. 132, n. 4). No formal set of institutions can encode all the many ways people should act in order to keep them functioning adequately. It is tradition, uniformity of attitudes, ways of behaving, default aims or default values or tastes that guide people when the handbook, so to speak, leaves it up to them. Whether this paper is wholly consistent with the social philosophy of *The Open Society* has been questioned (Shearmur 1996, chapter 3; Stokes 1998, pp. 49–50). Its major significance for the philosophy of the social sciences is that it presents Popper the social scientist at work. He offers a rational theory of tradition that treats tradition as an ideal type, reconstructs various typical situations and decodes their logic.

'The Logic of the Social Sciences' was prepared for delivery in 1961 at a conference in Germany where the doyen of the newly revived Frankfurt School of 'Critical Theory' would confront the inventor of critical rationalism. It was a paper intended to contrast Popper's own views about the social sciences with what he knew of the neo-Marxist views of T. W. Adorno and his followers. In an effort to control and focus the discussion, the paper is divided into twenty-seven theses, a suggestion and a comment. The paper draws on Popper's general epistemology and philosophy of science, on his criticisms of Marxism and Hegelianism and on the moral imperative to argue for a modest, critical and truth-seeking social science that uses situational logic. It consolidates and clarifies Popper's views on many matters regarding the social sciences. On the final page Popper insists (echoing 1945, 1, chapter 7, n. 23) that institutions do not act, only individuals act. Hence 'the general situational logic of these actions will be the theory of the quasi-actions of institutions' (Popper 1961, p. 104).[27] A curious sequel to this story are the attempts by Shearmur, Stokes and Fuller to make a case that critical theorists (specifically Habermas and Adorno) have parallel views to those of Popper and that there are possibilities for mutual cross-fertilization (Shearmur 1996, p. 168; Stokes 1998, chapter 8 and Chapter 12 in this volume; Fuller 2003, chapters 13 and 14). These exercises, however intriguing, should not divert attention from the huge differences, in particular the failure of critical theorists to confront the deficiencies of essentialism and historicism and to abandon them in a clear manner and in clear prose. Popper considered these criteria the acme of intellectual responsibility.

Consider Stokes's chapter (1998, chapter 8). Stokes looks at what he sees as convergences, overlaps and similarities of emphasis. Hardly surprising (as all opponents share background assumptions). Yet this is an uncritical approach. It leads Stokes to endorse claims of Habermas and Apel that there is a fatal flaw in Critical Rationalism that they can overcome with their transcendental/pragmatic alternatives. These claims are presumptuous: the dispute over the consistency of Critical Rationalism is clear and simple; offers by the obscurantists and obfuscators Habermas and Apel to transcend it are difficult to take seriously; Popper always aimed to highlight and sharpen disagreement and difference, not agreement and overlap. We learn from exploring the former. The problem common to both Popper's *Open* Society and the Frankfurters concern the explanation of the rise of modern totalitarianism. Popper and the Frankfurters shared disappointment in Marxism and little more. However, here some sharp divergences begin. The Frankfurters clung to historicist and utopian elements of Hegel and Marx in order to treat fascism as resulting from capitalism. Liberal society was friendly to capitalism and so they viewed liberal society as part of the problem. Popper discarded much more of Marxism, including talk of 'capitalism', which he thought quaint (Popper 1945, 2, chapter 18, III and n. 9). He suggested as a starting point that liberal society, with all its flaws, is still the best yet devised. Adorno resisted, pointing to Auschwitz, i.e. Nazism (Adorno et al. 1976, p. 120). Adorno, a privileged intellectual honoured in liberal society (in the United States and West Germany alike), refused to stand up for liberal society (shades of Weimar). Adorno thus exemplifies Popper's fundamental charge of the complicity of intellectuals in the triumph of fascism. It is little wonder, then, that in a final essay in the English edition of the Positivist Dispute volume (Adorno et al. 1976) Popper described the Frankfurt School as 'irrationalist' and 'intelligence destroying' and dismissed most of its content as high-sounding trivialities.

Controversy of this fervour swirled around Popper and his ideas on almost every front. From those in the philosophy and methodology of social science I shall pick out only three. Others – the philosophy of history; Marx's methods; principles of interpreting Plato and other texts; the testability of psychoanalysis – must succumb to limitations of space.

Methodological individualism. The philosophical debate over methodological individualism was initiated in 1952 by Popper's LSE colleague, J. W. N. Watkins (1952), in 'Ideal Types and Historical Explanation'. The cudgels were taken up by assorted philosophers of history, Marxists, Hegelians, and so on, as well as proponents of neo-classical economics. Watkins was soon qualifying his position, which was by no means identical to Popper's (for correctives, see Agassi 1960, 1975; Wisdom 1970). Anthologists try to balance both sides, thus concealing that the majority came down squarely in favour of methodological individualism (because of the votes of so many economists). Yet the debate was conducted largely by proxy. Popper did not participate in the debate, nor did he write anything further on the subject beyond his scant original remarks in 'The Poverty of Historicism' and in *The Open Society and Its Enemies.* The strongest and the longest of these reads in full (Popper 1944–45/1957, IV, 32):

> methodological individualism … the quite unassailable doctrine that we must try to understand all collective phenomena as due to the actions, interactions, aims, hopes, and thoughts of individual men, and as due to traditions created and preserved by individual men.

Popper (1944–45/1957, IV, 29) connects the imperative to his methodological nominalism:

> [T]he task of social theory is to construct and to analyze our sociological models carefully in descriptive or nominalist terms, that is to say, *in terms of individuals*, of their attitudes, expectations, relations, etc.

In *The Open Society* the idea is only referred to in one place, where it is contrasted not with methodological psychologism as in 'The Poverty of Historicism' but with 'methodological collectivism': 'it rightly insists that the "behaviour" and the "actions" of collectives, such as states and social groups, must be reduced to the behaviour and to the actions of human individuals' (Popper 1945, 2, chapter 14).[28]

In some places Popper refers to Hayek on the matter, showing his usual deference and gratitude to the older man and failing to highlight any discrepancies.[29] There is one other respect in which Popper commended methodological individualism that is almost never discussed: the moral dimension. To what was already said I add the following. If we grant that collectives act in ways that are quite irreducible to the actions of individuals, then a certain fatalism is permitted: we are creatures of forces greater than ourselves and these forces will drive towards their outcome no matter what we do. This claim is for Popper one of fatalism or despair: there is nothing we can do to change our fates. If there is nothing we can do, then we are not responsible. If we are not responsible, we need do nothing. Popper considered such reasoning highly immoral. He sometimes said it was our moral duty to be optimistic, to consider ourselves responsible and therefore to try to assume that possibly we can make a difference, so that we might and should act responsibly (Artigas 1999).

The debate over methodological individualism turned time and again to the reality of social wholes, social forces and the like. Looking back, it is not surprising that Popper neither intervened nor offered commentary in any of his papers or later printings of his books. For as I have tried to make clear from the outset, the sorts of things that comprise the social, the problems of their study, and the best way to explain them were matters that Popper did not wish to settle a priori. His comments about the Oedipus effect, for example, and about the logic of the situation were procedural: they were the result of looking at how the social sciences proceed and the kinds of special problems they encounter; they were not deductions from a priori principles. Popper had never embraced ontological individualism or ever confused it with methodological individualism.[30] As to his attitude to the extreme methodological individualism of neo-classical economics, game theory, rational choice theory and their likes, we can only speculate. Given his desire to emphasize a technological orientation for the social sciences he may well have taken the view that if such models were of some technological value then, like any false models that work, they were not to be disdained. Given his strong arguments against any kind of psychologism, and against ontological individualism, both of which are rife among those who work with these ideas, he may equally well have dismissed them as wrong-headed.[31]

Popper's own vision of what methodological individualism amounts to is best captured, I believe, in his account of the logic of the situation and the unintended consequences of human action. The arguments from fruitfulness and morality lead to something like a methodological rule not to halt the explanatory project until it reaches a typified individual acting rationally in a typified situation. Social explanations that fail to indicate how the problem to be explained stems from the goal-directed actions of individuals are unsatisfactory. I write 'indicate' because Popper would not demand that the explanation be carried through to the individual level, only that this could be done in principle and that it should be tried. This is also his attitude to deductive explanation: the explanatory deduction is seldom if ever fully spelled out, as that would require a fully formalized system. Once we grasp the scope of what Popper means by the logic of the situation we can see that his methodological individualism does not rule out the wholes and social forces that other thinkers believe to be at work. Finding ways to turn them into testable assertions is what renders them serious contenders for scientific status.

Following Bernard Mandeville, Adam Smith, Max Weber and others, Popper saw the primary task of the theoretical social sciences as that of explaining central social problems by viewing some phenomena as the unintended consequences of action. These comprise what Hayek following Adam Ferguson called 'the results of human action but not of human design' in frameworks consisting of both consciously designed institutions and those that have just grown. Such explanations cannot but refer to wholes such as traditions, institutions, structures and structural relations. The demand that Popper's philosophy makes is that we know in principle how to decode the holist shorthand into individual actions should we need to do so in order to test its components. Popper loads almost all of the social, cultural as well as biological, chemical and physical conditions that surround the individual into the situation part of logic of the situation. This brings us to rationality.

The Rationality Debate. Perhaps the least discussed of Popper's ideas on the philosophy of the social sciences are those where he tried to offer a sketch of the most fruitful kinds of explanation. Following Hayek, he states bluntly that the vast majority of social institutions are not designed, and this includes important aspects of society at large. They grow out of patterns of human practice, the path through the forest and the like. Persons are born into a dense surround of such institutions, traditions and arrangements. It follows that every action of the person is constrained in some ways as well as facilitated in others. Surrounded though they are, persons are not necessarily aware of the extent of that surround, and hence able to anticipate how their actions will reverberate through it. For this view Agassi chose the felicitous name 'institutional individualism' (Agassi 1975). Popper borrows examples from economics: you want to get the best price for what you sell, but the very offering of it on the market may marginally contribute to the lowering of the price. Hence your offer of sale has unintended (as well as undesired) consequences. This argument from our inevitable ignorance is utilized to endorse a cautious attitude to social reform and to show the futility of long-range social planning.

But what of rationality? Popper adds little to the core notion of instrumental rationality found in Max Weber, namely an action is rational if it is oriented towards a goal, and is appropriate in the light of the total situation facing the actor (Popper 1994b). We can call this actor-rationality. The social scientist sometimes has the advantage over the actor in judging actor-rationality. The declarer of war thinks that the opponent has weapons of mass destruction, or is close to developing them. The social scientist, the historian say, knows that the declarer was mistaken on this point. What is the historian's rational assessment? Clearly that the declarer of war may have acted rationally *given the appraisal of the situation he worked with*. Some would like to argue that the historian can second-guess the declarer and say that, because there were no weapons of mass destruction, the declaration of war was irrational. Suppressed premise: to be mistaken is to be irrational. This is not the way the rationality principle is deployed by Popper. If the social scientist were able to gain a better grasp of the actual situation than was available to the actors, then the explanation would deem them rational but mistaken (see Popper's reference to Churchill's discussion of the failure of a team of military leaders in 1994b, pp. 178–79). Mistakes are not necessarily culpable and certainly do not in general indicate irrationality.

To avoid the presumption of general imputations of irrationality, I argued together with Agassi that Popper's model of actor-rationality could be extended to the social scientist, and that this produced a clear model of *degrees of rationality* (Jarvie and Agassi 1967). We did not deny that attributions of irrationality were common. We cited Sir James Frazer, Sir Raymond Firth and Sir Edward Evans-Pritchard as anthropologists who argued that, because there was no magical causation, the actions of magicians were in some way less than rational. Our case against this had a negative and a positive side. The negative side was that most of the knowledge, including scientific knowledge, that human actors claimed was false; and that awareness of such corroborated knowledge as existed at any particular time was very limited. There was the danger of presumption in restricting rationality too narrowly. The positive side was that rationality should be seen as a matter of degrees but not in the traditional way as a range between two poles. Rationality was not, we argued, to be granted only to impossible situations of perfect knowledge and unanimous judgement – classical rationality. Rationality was, rather, a capacity to suit action to situation and that rationality was lacking wherever the *best available* information was not taken into account. That the best information might be mistaken should not direct one to withdraw rationality from the actors, but rather to show how the actual state of affairs, now known to the historian or social scientist, explains the outcome as unintended consequences of rational action. A war undertaken to stem the spread of weapons of mass destruction could be judged rational. The non-existence of the weapons of mass destruction does not invalidate that judgement if the best information available was used. Rather, it helps to explain why, for example, the declarers of war were embarrassed. Either the best information was flawed or their interpretation was flawed. Politicians hate to admit mistakes.[32] They hate it so much that there are no generally accepted ways of doing it without adverse consequences. Bureaucrats also hate to admit mistakes. Those who admit them are open to the charge of incompetence. With these over-demanding notions of rationality fixed in the minds of élites and public alike, no wonder rational debate about the lessons of war or of anything else is so rare. Inspired by Popper and by Bartley, Agassi and I added critical rationalism to our degrees of rationality. The best information available is the minimum for rationality; the best information available by some rationally discussible and discussed criterion is more rational; and the best information available combined with the best criterion available is the highest rationality. Critical rationalism seemed to us the best account of what information was the best (and why).

At this point I redeem my claim at the beginning of this chapter that one of Popper's most original contributions to philosophy of the social sciences is his rehabilitation of rationality. Two recent writers, Baert and Gorton, claim that Popper has close affinities with rational choice theory. We are told that Popper chose Von Neumann and Morgenstern's *Theory of Games and Economic Behavior* as shipboard reading for his trip from New Zealand to England, yet I have never seen any evidence that he gained enlightenment from it (Simkin 1993, p. 188). Gorton praises Elster's approach, which is strongly influenced by game theory in that he allows rationality only to calculating instrumental actions. Actions constrained by norms are non-rational for Elster but rational for Popper. And magical thinking is, according to Elster, quite irrational. For Jarvie and Agassi, by contrast, there can be rational magical action based on rational magical thinking: magical options may all be low on rationality, yet some may be less rational than others; Frazer ignored this obvious fact. If you think a certain rain dance can bring rain and end the drought, it is entirely rational to do the rain dance. Since it is a hypothesis that dancing can cause rain, and since hypotheses are tentative, it behooves the social scientist not to consign the actions of the rain dancers to irrationality.[33]

The traditional or classical view of rationality sees it as a human capacity that allows us to avoid error. Error in any form, then, be it scientific or superstitious, is traditionally judged irrational. As truth is one, it seems that every problem of rational action in specified circumstances must have a unique solution or a unique set of equally acceptable solutions. This theory of rationality forces one to judge incomplete any description of a situation not given to such a unique solution. The hunters of paradoxes in game theory seek descriptions of individuals in complete sets of situations that are not amenable to such unique solutions. These exercises fly in the face of the obvious: freedom from error is impossible, and to meet its end the exercise needs a criterion for when a situation is complete and when it is not (Watkins 1970). Popper effectively scraps this entire way of treating the rational/irrational. For one thing, as mentioned, he points out that that specialist in the irrational, Freud, gives us a rational model of irrational action. True, irrational conduct rests on error, but given the error, Popper says, the conduct can be rational and may be so in just the way Freud explained it. For another, Popper did not think scientific error – that is, the history of science – was a tale of irrationality. Far from it. He considered science in its ideal type to be the highest form of rationality. His originality lay in exporting rationality from the mind to the attitudinal, from the computational to the institutional. The rationality of science, he held, was embodied in the institutions and traditions that encouraged criticism and negatively sanctioned dogmatism. Dogmatism, then, is less rational than engaging in critical discussion; it is irrational even more so than, say, getting flustered when driving (to use Popper's own example).

Relativism and the myth of the framework. One of the debates into which Popper did intervene several times was with relativists. He castigated them in *The Open Society and Its Enemies* for moral irresponsibility. He was a moral realist who held that our moral knowledge and our moral sensibilities grow ontogenetically and phylogenetically (1974b, pp. 1158–59; cp. Shearmur 1996, chapter 4; Hacohen 2000, pp. 511–20). Relativism about knowledge was of course anathema to someone whose main interest was promoting the growth of knowledge. Like Russell, he thought common-sense or folk knowledge was mostly out of date science.

In 1961 he added an addendum to *The Open Society and Its Enemies*, 'Facts, Standards, and Truth', subtitled 'a further criticism of relativism' (Popper 1945, 2, Addendum to 1962 and later editions). He claimed that since demands for criteria are bound to lead to scepticism and relativism, a fallibilist view of them is imperative, and adding to it absolutism about truth avoids both dogmatism and authoritarianism. In 1965 he wrote for the Festschrift for P. A. Schilpp 'The Myth of the Framework', not published until 1976, and later the keynote essay for his eponymous book (1994a). This offered both a transcendental critique of the idea of incommensurable or mutually unintelligible frameworks and a vigorous affirmation that difficulties of cross-cultural communication do exist but are often overcome, always partially, always tentatively. As Popper considered all communication as fraught with difficulty, he had to reject the view of communication across frameworks as always and inevitably forced to break down. Bartley once went so far as to sum up Popper as holding that we never know what we are saying. That is, words are crude instruments both to deploy and to decode (Bartley

1987, pp. 432–40). Like Gellner, Popper regarded relativism as one of the major modern *trahisons des clercs* (Gellner 1992). Since according to Popper it was rationality, the capacity to communicate, argue and learn that constituted the unity of humanity, there was a moral duty to combat relativism.

The most virulent form of relativism in the late twentieth century went under the label of postmodernism, repudiating the Enlightenment as such on the basis of some excesses committed in its name. Hacohen has argued rather cogently that Popper already diagnosed and overcame all the problems posed by postmodernists to modernity, including, one might add, a testable hypothesis as to the social source of such resistance to change. In doing so he illustrated the ability of the Enlightenment tradition to be rectified and renewed. He offered resources to deal with postmodernist claims about knowledge, rationality, progress and so on. 'He urged a continued fight for a better future. Where there is struggle there is hope. This is Popper's rejoinder to postmodernism' (Hacohen 2000, p. 551) that preceded its appearance.

In various places, some of them noted above, Popper expressed great ambivalence about the social sciences. Oftentimes he echoed the disparagement common amongst natural scientists that the social sciences were, to say the least, underdeveloped (they have not yet found their Galileo, he says on the first page of the book *The Poverty of Historicism*). Yet whether he realized it or not, he was a social scientist himself, and the social democracy endorsed in *The Open Society and Its Enemies* would not be possible had we no knowledge of society and were it not possible to grow it (Jarvie 1999). On his own principle of attributing the logically stronger and more criticizable view to an author, his disparaging remarks are better ignored. His ambivalence is quite easily resolvable: he abhorred the pretentiousness of some social scientists and welcomed the sense of balance that others introduced into the life of democratic society. The hope for ever more knowledge of society and its workings is much needed for effective social reform; and equally for criticizing reformist proposals with sympathy. Popper's theory of knowledge, however, is one that cautions us to be watchful for mistakes and overconfidence, and absolutely never to sacrifice human lives on the altar of our claims to know.

NOTES

1 *Readings in the Philosophy of Social Science* has only four index references to Popper, all of them cursory, and no extract from any work of his. It does list some of his major works in its bibliography (Martin and McIntyre 1994). *The Blackwell Companion to Social Theory* contains a chapter on

'The Philosophy of Social Science' that does not refer to Popper, not even under 'Further Reading' (Outhwaite 2000). *Philosophies of Social Science. The Classic and Contemporary Readings* contains a five-page extract from Popper on the problem of induction. The editors state that the conditions Popper requires for a serious scientific theory are not met in the social sciences (Delanty and Strydom 2003, pp. 19–20). In the best of a less than satisfactory bunch, *The Blackwell Guide to the Philosophy of the Social Sciences*, Popper is mentioned in connection with methodological individualism, falsification, and classed as an 'analytic philosopher' (Turner and Roth 2003). These few paragraphs offer no overall picture of his work. Indeed the main treatment of him is consigned to an article in a section of the book called 'Pasts', strongly implying that he is no longer current. The chapter on Popper in Baert (2005) is written with a puzzling lack of scholarly detachment. Controversial issues are treated as settled and what are said to be contradictions turn out not to be so. It is to be hoped that the circulation of Shearmur (1996), Stokes (1998), and now Gorton (2006) will do something to rectify the situation. See also note 14.

2 A valiant attempt to do Popper justice is the second part of Simkin 1993. (See also Boland 1982 and second edition 2003; Wettersten 1992; Shearmur 1996, chapters 2, 3; Stokes 1998, chapter 5; Gorton 2006). Simkin's effort is somewhat vitiated by a tendency to treat all of Popper's ideas as a whole rather than to see them as responding to problems and changing under the impact of criticism and new ideas. Simkin also slights Popper's occasional efforts to do social science: 'Popper, when he wrote *The Poverty of Historicism*, had also thought there were some causal laws in social science but the examples he gave were both trite and unconvincing. He has since come to agree with the view of Wicksell, Hayek, Hicks and Samuelson that economics lacks causal laws' (Simkin 1993, p. 172). No reference is given for the second sentence. As to the first sentence, it is hard to see how Popper's examples could be at once trite and unconvincing. If Simkin was alluding to the examples Popper gave in 1957 (orig. 1944–45) III 20 then I think he is far too dismissive.

3 In Jarvie (1999) I try to show where Popper put his own ideas into practice by actually doing social science.

4 Lichtheim notes that Hegel gave indirect encouragement 'to the notion that legal and political institutions are to be regarded as the outcome of slow "organic" growth from custom and usage' (Lichtheim 1967, p. xxviii).

5 Jeremy Shearmur points out that Hayek took the stronger view that the results of human action but not of human design, forms of spontaneous order, are an emergent rather than a combination (see Hayek 1967, chapter 6).

6 In the chapter devoted to nature and convention in *The Open Society* (5) Popper writes of the distinction as 'difficult to make and to grasp', yet of the 'need' to make it in order to approach social phenomena in the spirit of scientific investigation.

7 On my reading, already in his 1934 monograph *Logik der Forschung* Popper rejects the naturalist and the conventionalist views of science and offers a new one (Jarvie 2001a, 2001b). That he there presented science as a social fact makes a rejection of the classical dichotomy almost unavoidable. His

new view presents science as a social (rather than individual) phenomenon, thereby destroying the very basis of the classical dichotomy, the idea that science is proof and so the natural is obligatory and the conventional is arbitrary. In his Replies to Critics he rejects the identification of 'by nature' with 'in truth', or convention with 'falsely' or 'in fiction' and even of 'by convention' with 'by arbitrary convention' (Popper 1974b, 2, p. 1116).

8 He seems to have been drawing on a lecture given at Alpbach in 1948 and published, in German only, the following year. It first came out in English in his (1972) as an appendix, 'The Bucket and the Searchlight: Two Theories of Knowledge'.

9 This idea appears, not fully worked out, in Popper 1944–45/1957, IV, 28, 30. It is elaborated in 1972, Appendix, The bucket and the searchlight.

10 Descartes has an interesting discussion of how paths used by many become more usable than direct routes, and that townscapes that accumulated higgledy-piggledy get improved by the need to rebuild or the intervention of planning. His aim, of course, is to draw an analogy with the edifice of knowledge. See the beginning of Part Two of the *Discourse on Method*.

11 On the value implications of methodology in general and the kinds of worlds excluded, see the brilliant paper Gellner contributed to the Lakatos memorial volume (Gellner 1976). Mariano Artigas argues compellingly that, for Popper, science itself is a moral enterprise (Artigas 1999).

12 The note first saw the light of day in the first American edition of 1950, and then in the second (1952) and subsequent English editions.

13 One suspects that Popper meant something more like 'obstacle', since hurdles are things one tries to get over, and he clearly means here intractable constraints.

14 That, I take it, is why Popper writes of 'our speculative inclinations (which, especially in the field of sociology proper, are liable to lead us into the region of metaphysics)' (Popper 1944–45/1957, III, 20). Metaphysics is to be avoided, or disciplined, it seems, because it can distract us from the scientific.

15 Some analytic philosophers, including Uebel (see note 28 to this chapter) and Ruben, who both taught at the LSE after Popper's retirement, hold that deductions do not explain, referring to some arguments of Wesley Salmon (1990, esp. pp. 46–51). Their errors include confusing deduction as a necessary condition of explanation with deduction from a satisfactory theory and initial conditions as a sufficient condition; and trying to introduce to methodology questions of time, relevance, and so on that are part of interpretation and theory. They are, it goes without saying, trying to save induction.

16 Besides developing over time, his ideas are subject to criticism and then modification. At times Popper candidly plays this up; at other times he seems unaware that this is happening (Jarvie 2001b, p. 30).

17 Patrick Baert (2005, pp. 64–65 and elsewhere) and many others have made much of Popper's alleged unfamiliarity with the social sciences. He himself made conflicting claims on the matter. Contrast what has just been said in the text about his social circle and his letter to Hayek with the remark in *The Poverty* (1944–45/1957, IV, 29) that when he wrote *Logik der Forschung* he knew 'next to nothing about the social sciences'. Yet in *The Open Society* (1945, 2, chapter 14, n. 11) he recalls having in 1924 private

discussions about Marxism with Karl Polanyi. In his autobiography (1976a, p. 113) he writes: 'I had for a long time been thinking about the methods of the social sciences.'

18 Although Popper chose his labels carefully, this one has not taken. Indeed it has come, over time, to be used to refer to exactly the doctrine he distinguished it from, namely explanation by reference to the predilections and interests prevailing in a particular historical period. He labelled this 'historism'. His terminological innovation evoked verbal quibbles that he had hoped to avoid. One verbal innovation that has taken, though the fact is seldom noted, is his replacement of the traditional philosophical term 'realism' (or 'Platonism') by 'essentialism'. He protested in his autobiography that he is not given credit for this (1974a, 1, p. 157, n. 7). A glaring example of such oversight is George Pitcher's *The Philosophy of Wittgenstein* which has an entire chapter crediting Wittgenstein with 'The Critique of Essentialism' (Pitcher 1964).

19 To the claim that he is reworking the long-standing nomothetic/ideographic distinction it should be noted that 'anti-naturalism' is a residual category.

20 See Jarvie 1982 and Shearmur 1996 for complementary detailed examinations of the text.

21 Working at almost the same time, and certainly in mutual ignorance, Merton came up with a similar idea to which he gave the less felicitous and less general label of the 'self-fulfilling prophecy' (Merton 1936, 1948).

22 Popper developed the argument more fully in his 1950 paper and the 1957 preface to the book version of 1944–45. Popper 1973 is also germane.

23 Hacohen 2000 pp. 471–76 explores the intellectual background and the extent to which Popper followed and resisted following Weber, who he insisted was infected with historicism.

24 In 1965 Popper says that under the impact of evidence from the Marx-Engels correspondence he changed his mind about Marx's good intentions. This is of only incidental interest. Marx in private may have been self-promoting rather than selfless. By Popper's own principle of interpretative charity it makes a stronger case to attribute goodwill to Marx (Popper 1945, 2, Addendum II to 1965 and later editions).

25 '[T]he situational analyses that Elster produces are superior to the kind recommended by Popper because Elster's incorporate psychological mechanisms' (Gorton 2006, p. 132, n. 9).

26 See also Stokes 1998, pp. 48–50.

27 When the volume in which the paper appeared was translated to English, Popper insisted on adding a general commentary on the whole meeting and its subsequent German publication that more or less says that it exemplifies his claim that Critical Theory was irrationalist (1976b).

28 Thomas Uebel says that Popper has not shown that social science *must* be done his way (Uebel 2003, p. 82). He seems to have been misled by Popper's intensifiers such as 'rightly', 'unassailable', 'must try' in the quotations in the text. But if methodological essentialism is to be eschewed and methodological nominalism commended, and if the link between methodological nominalism and methodological individualism is clear, the force of these qualifiers is quasi-logical, something like 'it follows that'. Admittedly this is a subtle point. Not so Uebel's question as to whether Popper required

that 'the concepts and laws of all social sciences be reducible to individual psychology?' (Uebel 2003, p. 78): Popper devoted a whole chapter to opposing the idea that it is (Popper 1945, 2, chapter 14), sharply and effectively criticizing it as the methodological myth of the original contract. Uebel also states (loc cit.) that Popper 'opposed ontological holism' (he did not) and was an ontological individualist (he was not). Giving great weight to the work of Otto Neurath leads him to an unbalanced treatment of Popper by making too much of the overlap of their ideas. 'The important question here is: are Popper's charges against Neurath correct?' (Uebel 2003, p. 79). Uebel's paper is called 'Twentieth Century Philosophy of Social Science in the Analytic Tradition'. Popper was not an analytic philosopher by Uebel's own criterion, and it is simply lazy to class logical positivists into that category (see Gellner 1959). If anything, analysts should be classified as (degenerate, scholastic) logical positivists (Wisdom 1963). Uebel's paper and its appearance in a 2003 reference book is a striking illustration of my claim in the first sentence of this chapter that Popper's philosophy and methodology of social science is not well known.

29 On the differences between, and the mutual influence on, Popper and Hayek, see Shearmur 1996; Caldwell 2004, 2005.

30 This blunder, confusing methodological individualism with ontological individualism, is easy to commit. An early item of mine led both Lars Udehn and Mario Bunge to try to tar me with that confusion in perpetuity (Jarvie 1959; Udehn 2001; Bunge 2004). I take comfort in the fact that Robert Nozick made the same blunder in his single discussion of the doctrine of methodological individualism. It requires, he writes, 'that there be no basic (unreduced) social filtering processes' (Nozick 1974, p. 22). 'Filter' is Nozick's opaque term for any invisible-hand mechanism, hence he is saying that methodological individualism cannot be reconciled with invisible hand, i.e. unintended consequence, explanations. In his (1996), p. 127, Jeremy Shearmur endorses this and says it proves methodological individualism 'defective'. This stems from a narrow misreading of the recommendation of methodological individualism in a passage on p. 136 (IV, 29) of 1944–45/1957, in which institutions are described as abstract, or theoretical, and hence have to be tested, that is analysed '*in terms of individuals*, or their attitudes, expectations, relations, etc.' that is to say, since institutions are unobservable, tests in the social sciences have to refer to individuals. This is obvious, and an effort of Popper to concede to psychologism as much as he could. This concession is far from an admission, since neither expectations nor relations are less abstract or theoretical than institutions. There is some evidence of strain in this passage. A principle of interpretative charity is to attribute to an author the logically strongest and most criticizable position that fits the text. Ontological individualism does not fit the text since it is explicitly repudiated. As Popper (1945, chapter 7, n. 23), Agassi (1960), Wisdom (1970), and I (Jarvie 1972) have asserted, methodological individualism consists mainly in the denial of aims to social wholes. It does not require us to begin or end all explanation in the motivation of individuals. The explanatory model is one of a typical individual in a typified situation. Aims or goals, and hence the initiation of action, go to individuals – typified in the generalizing social sciences and named or described in the historical ones. Individuals always act in situations, which

for the most part they did not create. The situation is unpacked into at least two components: the situation as the actor assesses it, and the situation as it is. To take Shearmur's example (since Nozick gives none), the success of a small business may be affected by the current rate of interest or a change in it, and hence the outcome cannot be explained by the motives of individual agents (Shearmur *ibid.*). This way he reads methodological individualism as a reduction of social explanation to the 'specific motives of individual agents'. This is to read it ontologically – it cannot handle certain kinds of entities, filters, emergent products of the actions of other individuals that have a 'thing-like' character. Popper did not view methodological individualism as a reduction. It is fully realized in a situational analysis in which any amount of 'thing-like' entities can enter an account regardless of whether they and/or their effects are noticed by the actors or not. The main question is whether such accounts are testable.

31 Lars Udehn offers a balanced treatment of this in his book (2001) and his article (2003); see especially the final two paragraphs of the latter.

32 'We all have an unscientific weakness for being always in the right, and this weakness seems to be particularly common among professional and amateur politicians' (Popper 1944–45/1957, III, 24).

33 The usual move by anthropologists – to put rain dances in a category of symbolic actions – betrays a condescending attitude, contrary to the intention behind it. All actions have symbolic aspects, but none is purely symbolic; moreover, their symbolic character does not explain why they are performed. More often than not, it is incidental, and their symbolic accomplishments are unintended consequences.

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GEOFFREY STOKES*

12 Popper and Habermas: Convergent Arguments for a Postmetaphysical Universalism

As a result of their robust engagement in the 'positivist dispute' or 'Positivismusstreit' of the 1960s, the philosophies of Karl Popper and Jürgen Habermas are often considered to be in irreconcilable conflict.[1] Divided over issues in epistemology, social science methodology and on political ideology, Popper and Habermas seemed to share little common ground. During this debate Habermas (1976a; 1976b) criticised Popper's arguments about scientific method and castigated his decisionism and fideism regarding the choice or adoption of values, especially those of reason or rationality. Popper's dismissive comments about the Frankfurt School (1992b, pp. 82–95; 1994, pp. 78–81) would appear to have put the issue beyond doubt. Nonetheless, there is a recurring strand of interpretation that points to similarities and even a 'reconciliation' between the significant 'ideas' of the two philosophers. These critics suggest that Popper and Habermas may not only address mutual problems and themes, but also hold in common a number of values and assumptions.[2] Furthermore, there are strong indications that since the 1960s, the philosophies of Habermas and Popper have become closer, even in regard to the epistemological and methodological solutions to such problems.

My aim is to revisit this problem of interpretation in the light of the later works of Popper and Habermas. The chapter therefore does not canvass the whole range of issues relevant to a comparison of the work of Popper and Habermas. Nor does it raise the many criticisms that could be made of their work, either from within or outside their respective traditions. Instead, it draws out the parallels and convergences in their thinking. It concentrates on their universalism and the role that *problem solving* comes to play in both their philosophies of knowledge, ethics and politics. In particular, I consider the implications of their work for democracy and political critique.

Together Popper and Habermas lay the foundations for a post-metaphysical universalism in *epistemology and philosophical method*.

* I am grateful to Jeremy Shearmur, Marina Cominos, Roland Axtmann and Alex Naraniecki for their critical appraisals of versions of this chapter.

By this I mean that both provide arguments that contribute to the transformation of philosophy from its foundationalist role to one that is more fallibilist and dependent upon understanding the universalities of human practice, especially with regard to questions of reason and rationality. That is, a postmetaphysical project seeks to integrate particular forms of empirical knowledge into philosophy while retaining the latter's capacity to reflect on and formulate universal insights into epistemological and ethical problems.[3] This chapter also offers a brief explanation of the sources of these convergences with reference to the work of Immanuel Kant, and briefly evaluates their philosophies against a competing, critical tradition of Enlightenment thought.

1. CONTEXT: THE POSITIVIST DISPUTE IN GERMAN SOCIOLOGY

The positivist dispute had its origins in a conference of the German Sociological Association held in Tübingen in 1961, at which Popper presented a paper on the logic of the social sciences (1976a [1962]) and Adorno (1976c) was invited to provide a reply. Over the next decade, the debate attracted participants – advocates of critical rationalism and those of critical theory – who addressed core issues in the philosophy and methodology of social science. The participants held different views about what it meant to be 'critical' in social science, and how these claims could be supported. At issue were the nature and foundations of criticism and rationality. The debate therefore ranged over a wide variety of topics, but especially the philosophy of science, the rational foundations of knowledge and ethics, as well as the implications for politics and society. These debates had their origins in a longer trajectory of issues that had preoccupied Marx and Weber, as well as Kant and Hegel before them.

Although the critical theorists located their intellectual origins in Marx's critique of political economy, they shifted the focus of critique away from that of historical materialism. The critical theorists subscribed to a Marxism that had relinquished its dependence upon the proletariat as a bearer of revolutionary change and that concentrated more on the problems of consciousness, that is, ideology, philosophy and culture. Engaging in 'ideology critique', they directed attention not only to the dominant ideas of capitalism and fascism, but also to Soviet socialism (e.g. Marcuse 1971).

The first generation of critical theorists included German philosophers and social theorists such as Max Horkheimer, Theodor Adorno and Herbert Marcuse.[4] Their concept of critique extended beyond the criticism of philosophical and theoretical statements, however, to embrace that of social, political and economic systems. The normative force motivating critical theory was its commitment to human emancipation, by which was meant liberation from all forms of oppression and domination. But the associated practical tasks were those of explaining society, politics and economy in ways that enabled their transformation, according to defensible norms.[5] Such a program led inexorably to investigation into the theories of knowledge and methodologies that could provide the best explanations.

Critical theory's primary method is that of internal or 'immanent' critique. Such critique aimed to show how, with reference to capitalism, for example, the ideology of individual freedom masked systematic oppression and inequality. But critical theorists, such as Adorno, had become pessimistic about whether their critical method could deliver on its early promise to uncover the utopian, and thereby revolutionary, potential in culture (Benhabib 1986, p. 174). Jürgen Habermas, who attended university after World War II and worked for Adorno between 1956 and 1959 (Habermas 1986c, p. 150), was a member of a second generation of Frankfurt School theorists that included Albrecht Wellmer and Claus Offe, among others. Habermas absorbed the larger project of critical theory and sought to give it stronger philosophical foundations across a wide array of problems. Like Marcuse, he adopted a more positive philosophical and political outlook, along with a more democratic and reformist approach to social change (Heller 1978, pp. 54–55).

Critical rationalism, the name that Popper (1992a [1976], pp. 115–16) had given to his broader philosophy, also incorporated philosophy of science and social science, as well as political thought. By the 1960s, Popper was well known both for his innovative falsificationist philosophy of scientific method and his criticisms of Marxism in *The Poverty of Historicism* (Popper 1961) and the *Open Society and Its Enemies* (Popper 1966a; 1966b). (Versions of these two books had been published in English during the 1940s.) Popper's critique of Marxism was directed towards not only the more traditional theory of historical materialism, but also its claims to being founded upon scientific method. Popper was acutely aware of the social and economic problems of capitalism, but he considered that the holistic and utopian ideas characteristic of some versions of Marxism would, if put into practice, more likely make conditions worse. Popper also aimed to show the serious limitations of Marxist claims to social science. Critical rationalism was therefore understood as anti-Marxist and thereby a leading liberal competitor to critical theory.

Freedom came to be a guiding value for Popper, and he understood this in familiar liberal individualist terms, such as that manifest through political freedom, and freedom of thought and speech. Nonetheless, although he supported a negative view of freedom as freedom from various concrete constraints, when it came to knowledge, he also proposed a positive view. He saw the open society, for example, as setting free 'the critical powers of man' (1966a, p. 1) for intellectual self-realisation. This became Popper's (1992b [1961], p. 137) meaning for Kant's phrase 'emancipation through knowledge'. Nor did his wariness of state domination prevent him from espousing positive programs of social and economic reform that sought to overcome identifiable evils and improve the capacities of individuals to exercise their freedoms. Popper thus also endorsed freedom from economic exploitation.

Although Popper had come under the influence of Marxist and social democratic ideas throughout his university studies and political activism in Vienna, he later rejected Marxism, largely because of its dogmatism and what he saw as its advocacy of violence (1992a [1976], pp. 33–34). Popper was concerned with, and active in, social reform, but towards the end of the 1920s, he became preoccupied with problems of scientific method as they applied to natural and social science. In this context he argued with and against members of the Vienna Circle of logical positivism, a number of whom, such as Carnap and Neurath, were socialists. Popper's political works and later commentaries reflected strong social democratic and social liberal principles, though the liberal emphasis upon freedom of thought was always strong. In the Tübingen discussions, Popper referred to himself as a 'liberal' (Dahrendorf 1976, p. 129).

At the conference, Adorno's formal response (1976c, pp. 105–22) hardly engaged with Popper's paper, and indeed parts of it expressed agreement with a number of his arguments, though tempered by a misreading of others. Nonetheless, Adorno (1976a; 1976b, pp. 68–86) reversed this conciliatory tendency in his later contributions. He sharpened his differences with reference to issues, such as the transferability of logic and falsifiability to social science, and argued in such a way as to present Popper as a positivist of the kind that he had criticised and rejected.

Adorno's subsequent lack of intellectual generosity led to distortions of Popper's arguments and their implications. One difficulty was that Adorno had interpreted Popper's theory of problem solving more narrowly than had been intended, or that could reasonably be warranted by Popper's statements. Thus Adorno missed what, in my judgement, was a significant development in Popper's work. Furthermore, by undertaking an immanent critique of Popper, Adorno (1976a, p. 59) portrayed the distance between them in stark political form:

> Popper advocates an 'open' society. The idea of such a society is contradicted, however, by the close[d] regimented thought postulated by his logic of science as

a 'deductive system'. The most recent form of positivism fits the administered world perfectly.

This claim sheds light on one of the major differences between the two schools of thought. First, the critical theorists saw critical rationalist concepts of reason as too narrow. That is, reason was modelled on the ideal attitudes, behaviour and methodology of natural scientists, and was primarily an attribute of individual thought and action. As a purely cognitive concept, whose objectivity (allegedly) lay solely in formal logic, it appeared to give unqualified support to instrumental rationality, and was therefore inherently unreflective and uncritical of the society in which it was practised.

Second, the concept of reason at the heart of critical rationalism could not be satisfactorily argued for or 'grounded'.[6] Popper had retreated from such a task and was both unable and unwilling to provide a coherent set of arguments for adopting reason, or for preferring one concept of reason over another. The critical theorists, however, saw a larger role for reason, first as an attribute of the wider society and economy, as in the Weberian concept of 'rationalisation', but not limited to empirical analysis. They sought a concept of reason whose justification could be located both in abstract philosophy and within the practical life-world of thought and action. For social science, the concept of reason was not only to be applied to the clarification of statements about social reality with a view to explaining it; it should also have the capacity to supply a normative critique, and suggest ways of transforming social reality. Such a critique would be possible, it was argued, because the critical theorists could provide a rational foundation for reason.

Habermas's role in this debate was to shore up Adorno's stand, but from a slightly different direction. In his first contribution, he (Habermas 1976a) argued against what he called the 'analytical theory of science' and for 'dialectics', which he portrayed as reaching beyond the exercise of deductive logic and that took account of a 'totality' such as the 'whole' of a system under analysis. In so doing he touched upon motifs such as hermeneutics and cognitive interests that were to occupy him in the coming years. He also briefly canvassed the idea of the essential 'pre-understandings' (Habermas 1976a, pp. 154–56) that make possible the necessary attitudes of scientists, their practices and scientific progress. Thus Habermas foreshadowed his later arguments on transcendental deduction as a method of 'founding' or justifying the value of rationality. It is arguable that Habermas undertook an ideology critique, the result of which was to suggest that Popper's formal arguments contributed to the larger process of societal rationalisation in which the advocates of scientific method were unaware of how it contributed more to domination than enlightenment.

Habermas's second contribution expanded upon these themes in a response to Hans Albert who had taken on the role of standard-bearer for critical rationalism. In this paper, Habermas showed great familiarity with Popper's early work[7] and, faithfully applying an internal critique, often used Popper's own observations and arguments against him. The main point of difference was that Habermas (1976b, p. 213) argued we ought to, and could, justify rationally the decision to take up the critical method: 'Certainly it cannot be justified in the sense of deductive proof but it can in the form of supporting argumentation.' For Habermas, this required giving attention to the very assumptions or preconditions underlying and motivating the critical attitude. Developing this line of thought, he (Habermas 1976b, p. 215) wrote:

> As a makeshift, we can conceive of criticism … as a process which, in a domination-free discussion, includes a progressive resolution of disagreement. Such a discussion is guided by the idea of a general and unconstrained consensus achieved amongst those who participate in it.

This idea is both an empirical fact of social life and a 'transcendental' precondition. Habermas's mention of 'domination-free discussion' and the requirements of communication gestures towards a central (liberal) principle of Popper's, namely freedom of discussion. It also prefigures Habermas's later arguments about communicative action and discourse ethics.

For Popper, much of the discussion was arid and unproductive, based as it was on misinterpretations of his work and expressed in obscure language. He taunted the critical theorists by 'translating' into 'plain language' one of Adorno's statements about totality – selected as significant by Habermas (1976b, p. 131) – in such a way as to ridicule the arguments. Dismissing Habermas, Popper (1976b [1970], p. 297) writes: 'Most of what he says seems to me to be trivial; the rest seems to me mistaken.'

Nonetheless, Popper (1976b [1970], p. 291) did point to a crucial distinction between his social theory and theory of method.

> And it is a fact that my *social theory* (which favours gradual and piecemeal reform, reform controlled by a critical comparison between expected and achieved results) contrasts strongly with my *theory of method*, which happens to be a theory of scientific and intellectual revolutions.

Thus Popper marked out what he saw as the political difference between the 'revolutionary' tendency of critical theory and his own 'evolutionary' liberalism. Furthermore, he differentiated more clearly between his philosophies of science and social science. A philosophy of social science must not only assume all the determinants of the physical world; it must also take account of the social world. It is here that the method of 'situational logic' played a vital role, though important questions of interpretation and understanding remained underdeveloped. Although Popper was dedicated to theorising the 'unity of science', the working out of such a theory in practice demonstrates significant differences between the methods of natural and social science.[8]

For all Popper's efforts at clarity, an important ambiguity remained about the nature of rational criticism and the role of deductive logic. In his Tübingen lecture, Popper (1976a, pp. 98–101) gave prominence to the role of deductive logic. Because of its significance as a 'theory of the validity of logical inferences', Popper (1976a, p. 98) saw deductive logic as '*the* theory of rational criticism [my emphasis]'.[9] In so doing, he provided ammunition to his critics who latched onto the statement as evidence of a legacy of logical positivism. He thus undermined the tendency in his arguments to understand deductive logic as only *one* instrument in the armoury of rational criticism. In the same paper, for example, Popper (1976a, p. 98) wrote: '[T]he method of science consists in the choice of interesting problems and in the criticism of our always tentative and provisional attempts to solve them.'

With this broader understanding of science, deductive logic is largely applicable to the theoretical process of theory formation and test design, not to the wider dialogical process of discussing and determining scientific problems within scientific traditions. Nor would it necessarily be applicable to the discussion of metaphysical arguments that could have the potential to become scientific, the importance of which he had come to recognise later (e.g. Popper 1982, pp. 210–11). Indeed, in his later works, Popper develops a theory of science that gives greater attention to the less formal aspects of rational criticism.[10] It is such a project that gradually intersects more with that of Habermas.

2. POPPER, HABERMAS AND UNIVERSALISM

At its most general, what connects Popper and Habermas is their quest for universal philosophical theories and precepts. From an epistemological perspective, neither accepts the tenability of relativism or scepticism, in any of their forms. At the heart of their universalism is the formulation of rational procedures for addressing significant problems of epistemology, ethics and politics, and the proposing of certain minimalist criteria of epistemological progress. In reaching their proposals, Popper and Habermas, to varying degrees, draw upon common and complementary philosophical *methodologies* and *methods*. These include rational reconstruction, internal and external critique, and even arguably transcendental deduction. In these processes they use the traditional techniques of analysis and argument, guided by the epistemic values of logic, coherence and consistency. Such strategies are relatively unproblematic. The more interesting and significant point, however, is that both draw upon the empirical theory of evolution to formulate an account of human beings as 'problem solvers'.

2.1. Universalist Criteria and Procedures: The Role of Evolution

Crucially, Popper and Habermas make recourse to certain kinds of empirical knowledge in order to support, confirm or 'validate' their philosophical arguments. In particular, both look to evolutionary theory and, for Habermas, certain kinds of knowledge about moral development. I will develop this argument sequentially with reference to Popper and then to Habermas.

2.1.1. POPPER. From the 1960s, Popperian epistemology became the 'theory of problem-solving, or, in other words, of the construction, critical discussion, evaluation, and critical testing, of competing conjectural theories' (1972c, p. 142). According to Popper (1972b, p. 199), a theory is only rational if it is an attempt to solve real problems, and if it can be examined critically. This is only possible in relation to a given 'problem situation'. Rational criticism, however, also entails inquiry into whether the theory is true or false, or nearer to the truth than another theory (Popper 1983, pp. 24–25). Nonetheless, testability is now only viewed as 'a certain kind of arguability: arguability by means of *empirical* arguments, appealing to observation and experiment' (Popper 1983, p. 161).

The Darwinian theory of evolution, as Popper interprets it, provided further non-philosophical grounds for adopting the method of trial and error criticism. Within the biological process of natural selection Popper (1984, p. 239) saw a model of the growth of knowledge comparable to that which he had devised for science and philosophy.[11]

> 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 (Popper 1972c [1981], p. 261).

He concluded that there was an actual, not just metaphorical, continuity between the lowest to the highest organisms in that all knowledge grows by means of problem solving.[12] By recognising both the analogies and the continuities between the world and our knowledge of it, Popper opened the way towards an *explanation* of the growth of knowledge as part of an 'evolutionary' epistemology. What we see emerging is an external empirical confirmation of Popper's early intuitions about the significance of 'problems' and 'problem solving'. In his later years, this motif becomes even more evident in his talks and writings.[13] It is arguable further that despite his avowed intentions earlier in his work (e.g. Popper 1972a [1959] [1934], pp. 50–53), this evolutionary tendency is evidence of a light and unacknowledged resort to naturalism in his writings.[14]

A number of practical issues arise, however, in the application of insights from biological evolution to human life and knowledge. In this regard, Popper qualifies his Darwinism by noting that among the 'higher' evolutionary forms of human life not all problems are those of survival (1972c, p. 244). Furthermore, human beings may be consciously self-critical in their attempt to solve problems and allow their hypotheses to die instead (1972c, p. 248). He also points out that often the outcome for human knowledge is different to that of biological evolution (1972c, p. 262). Where biological evolution results in greater differentiation and specialisation of species, the evolution of 'pure' scientific knowledge often results in greater integration into unified theories, such as in physics.

Furthermore, problems do not emerge fully formed; they are the subject of interpretation, debate and discussion in the context of the literature and traditions of a field. Even empirical problems are determined through the lens of one's values; what may be problematic to one group may not be to another because of different value priorities. In the Popperian tradition, one could envisage that the process of delineating problems would also be a fallible exercise in which the arguments would be contestable. This indeed would be part of the task of 'rational reconstruction' in which he engages to clarify contesting philosophies and arguments.

2.1.2. HABERMAS. A concern to 'validate' a philosophical thesis with reference to empirical processes may be seen in Habermas. In an earlier work, Habermas made a case for testing his philosophical hypotheses in what he terms the 'reconstructive sciences' or 'universal pragmatics'. As he presents it, philosophers seek an empirical reconstruction of the cultural evolution of our moral experience or 'pre-theoretical' knowledge. In this project, transcendental arguments furnish 'reconstructive hypotheses' for empirical research (Habermas 1990, p. 16). As in other empirical inquiries, the propositions are not only arguable and testable, but the conclusions are also open to contest. Habermas's (1990, p. 117) discourse theory, for example, is 'dependent upon, *indirect* validation by *other* theories that are consonant with it'. Nonetheless, Habermas (1993, p. 94) is vitally aware how the 'indeterminacy of discursive procedures' and limits to information, among other factors, can prevent consensus emerging on such matters. In this way, Habermas demonstrates his commitment to fallibilism.

Habermas's strategy of argument is also adopted in one of his later books, *Truth and Justification* (2003), in which, like Popper, he draws upon the theory of evolution. In his argument for a 'weak naturalism' (Habermas 2003, p. 28), he observes the 'continuity between nature and culture', and that 'the natural evolution of the species can be conceived as the result of "problem solving"', which is analogous to our own (fallible) learning processes. Unlike Popper, Habermas is pressed in this direction by his awareness of the pragmatist tradition of philosophy. 'For pragmatists,' Habermas (2003, p. 26) writes, 'cognition is a process of intelligent, problem solving behavior that makes learning processes possible, corrects errors and defuses objections.' Becoming aware of error may provoke a learning process that may be either unconscious or self-consciously chosen. As we shall see later, the recognition of error becomes part of Habermas's argument for realism and a non-epistemic concept of truth in natural science, as well as for fallibilism and objectivism more generally in science and ethics.

Interestingly, Habermas (2003, p. 29) appropriates the concept of 'emergence' for understanding the evolution of 'sociocultural forms of life'. Although Popper put great store by the concept of emergence in biological evolution and in discussing propensities, he never applied it to social and political life, where it may have yielded promising results.

For both Popper and Habermas, the focus on problem solving performs a number of philosophical functions. It confirms fallibility as a fundamental condition and presupposition of human existence, which then is also given a prescriptive role in the pursuit of knowledge, ethics and politics. An adherence to fallibilism will remind us of not only of the limits of our knowledge, but also the imperative to try to improve knowledge. Understanding human behaviour as a form of problem solving provides a way of both explaining the growth of knowledge and prescribing norms for epistemology.

The recourse to evolution plays an important role for both Popper and Habermas. It provides an empirical basis, albeit a fallible one, for their universalism. As such, it constitutes a light or 'weak' turn to naturalism that complements other philosophical arguments. The aim of this kind of argument is to be able to make universal claims that go beyond the assumptions and practices of a particular community or era, and therefore avoid the dangers of contextualism and historicism. Problem solving is an unavoidable condition of human life, upon which we can then provide the basis of normative recommendations. To try and ignore problems, or avoid solving them, has serious consequences for individuals, societies, polities and species.

The key epistemological questions remain: How do we know when we have solved a problem or solved it better than another? And are there any general epistemic criteria available for a problem-solving approach?

2.2. *Criteria for Solving Problems*

For both philosophers, fallibilism complicates all efforts to provide an answer to the question of how one knows whether a problem is solved or not. Both Popper and Habermas recognise that there are no firm decision procedures or definitive criteria of success, or failure. Possible exceptions would be those 'errors' that result in the death of a human being, the destruction of a polity or loss of a species. Given these assumptions, both Popper and Habermas recommend certain rational procedures *and norms* – epistemic and non-epistemic – for dealing with problems. The better the quality of the procedures, however, the more confidence we can have that we have made the best judgement, 'for the time being'. What do Popper and Habermas recommended as guidelines for such procedures?

2.2.1. POPPER. In Popper's account, rational criticism ought to proceed in four stages similar to biological problem solving. The first step is to delineate the problem for which the theory is offered as a solution. This requires analysis of the intellectual context or problem situation. The next step is to propose a tentative theory or series of theories as a solution to the problem. The key stage occurs with the attempt to eliminate errors among the competing theories. Proceeding by whatever practical or theoretical means are appropriate; the aim is to isolate and eliminate contradictions within a theory and in its relationship to the problem it was designed to solve. The difficulties raised by the elimination of errors constitute a new problem. Popper (1972c, pp. 242–43) represents this process by his tetradic schema: $P_1 \rightarrow TS \rightarrow EE \rightarrow P_2$, where '*P*' signifies a problem, '*TS*' a tentative solution and '*EE*' stands for error elimination. Thus problem solving by means of trial and error is an essential process by which humans learn and their knowledge grows.[15]

Although Popper supplies a variety of familiar precepts for undertaking criticism, when compared to his early epistemology based on science, the normative criteria for epistemological progress and the accompanying methodological prescriptions are much less specific. For example, Popper (1992a, p. 132) argues that we must not only attempt to discern and solve problems, but also generate new problems. With his proposal (1972b, p. 184) that the highest form of intellectual creativity and rationality has become the discovery of a new problem, Popper provides a general criterion of progress.

What remains important, however, is the rigour with which solutions to a problem are tested and criticised. Popper considers a problem may be said to be solved only after the most thorough public discussion, argument and criticism. But this too must meet the criteria of objectivity, understood as intersubjectivity. In an important footnote to his *Logic of Scientific Discovery*, Popper (1972a, section 8, n. *1), following Kant, points out that 'inter-subjective *testing* is merely a very important aspect of the more general idea of inter-subjective *criticism*, or in other words, of the idea of mutual rational control by critical discussion'. Popper therefore recognises that all inquiry, and problem solving, is a social and collaborative process that operates as a form of 'control' on intellectual rigour and quality.[16]

The social core of objectivity resides in the 'public character of scientific method' (1966b, p. 218). Only if scientific theories, experiments and their results are made public can the scientific community continue its tradition of free critical discussion. Critical dialogue is ultimately governed by widespread recognition of the importance of experience as the arbiter. Furthermore, 'experience' itself has a particular quality in that it has no existence outside a system of methodological rules. 'Experience', it may be said, is constituted or governed by methodological rules that are essentially social. In summary, objectivity depends upon cooperation, public communication and mutual criticism among scientists; it has nothing to do with an individual's impartiality. Popper (1972a [1934], pp. 109–10) uses the metaphors of the juries, jurors, rules of procedure and verdicts to explain the rule governed process of reaching a decision. These metaphors also imply that the resultant outcome is a form of provisional, consensual agreement.

For Popper, any judgement upon the truth or falsity of a theory is a decision resulting from its critical discussion by a scientific or philosophical community. Originally, Popper argued this solely with reference to decisions about the acceptance or rejection of 'basic statements' in science (1972a, end of section 30). Yet, it would be difficult to argue against extending such norms to other parts of his philosophy where the topic of concern was *not* the acceptability of 'basic statements'. If we rationally reconstructed the logic of Popper's arguments, as he often does with those of other philosophers, it is arguable that a similar process would and should apply to judgements about the identification and resolution of 'problems'. That is, such judgements could and should also be decided by *consensual agreement* among the relevant individuals, scientists or citizens. Such decisions may later be revised as the scientific, philosophical or political consensus changes, as has occurred historically in science.

Although Popper argues that discussion over difficult problems is always possible, he expresses two kinds of reservations about the outcomes. First, he (Popper 1994 [1976], p. 37) is somewhat pessimistic about participants in such discussions reaching agreement. He is adamant, however, that we do not need to agree on a 'common [intellectual] framework of basic assumptions' to engage in a 'rational and fruitful discussion' (Popper 1994 [1976], p. 34). Nonetheless, Popper (1994 [1976], p. 35) does differentiate this requirement from another crucial one, which 'may indeed be preconditions for a discussion, such as a wish to get to, or nearer to, the truth, and a willingness to share problems or to understand the aims and the problems of someone else'. Similar views are evident in the *The Open Society and Its Enemies*, where Popper (1966b, p. 225) gives content to the rationalist 'attitude' he upholds with the words: '[W]e must recognize everybody with whom we communicate as a potential source of argument and of reasonable information.' But these and other statements remain at the level of a moral appeal about what is required to engage in communication, rather than providing sustained argument for it.

Second, Popper (1972b, p. 359) raises the more difficult example of conflict over 'ends', which he does not believe can generally be solved by arguments and discussion. Nor does he expect that such discussions should aim for, or will result in, agreement or conclusive arguments. In response to both points, it should be emphasised that consensual agreement does not require such conclusiveness or unanimity among a group. It simply requires that everyone who participates in a decision consents to it on understanding that, whereas disagreements might remain, it is the best that can be achieved for the time being. Such agreements are not as unusual in social, political and scientific life as Popper seems to believe.

In science, for example, a rational reconstruction of the logic of decision making at the higher level of theory acceptance would generally support such an argument. This is what Thomas Kuhn (1970) was arguing when he introduced the concept of a paradigm and sought to explain how shifts occurred between paradigms. As Kuhn's 'Postcript 1969' shows, the process of conversion to a new paradigm is a complex one that, among other things, requires deliberation, which is based on the application of rules (p. 195) and 'good reasons' (pp. 199, 204), not irrational decision or arbitrary commitment.

Popper's illustrative comments on the dilemma confronting someone who wants to argue against violence are instructive for the strategy of argument deployed in this chapter. Popper (1972b [1963], p. 359) does not think that arguing with someone who admires violence is necessarily a waste of time: 'If he is willing to listen to your arguments without shooting you, then he is at least infected by rationalism, and you may, perhaps, win him over.' Here Popper again refers to a pre-existing commitment to the value of discussion. This is precisely the opposite of the views of the young member of the National Socialist Party he (Popper 1994, p. xiii) reported once meeting, who said: 'What, you want to argue? I don't argue; I shoot!' Preconditions, presuppositions and 'attitudes' are therefore vital, and part of his arguments for rationalism. Indeed, it would appear that critical rationalism could not operate in practice without them.

With Popper's reference to crucial 'preconditions', however, we can recognise the kinds of 'presuppositions' that Habermas (e.g. 1976b [1964]. p. 219) writes about and investigates from another perspective. Furthermore, if we were to draw again on the method of rational reconstruction, it is arguable that the logic of Popper's diverse statements on the presuppositions of rationalism commits him to the method of transcendental deduction that Habermas deploys in support of a discourse ethic.

Interestingly, an example of this kind of argument can be found in the transcript of a lecture Popper gave in New Zealand (c. 1940) on the subject of Kant's categorical imperative and impartiality.[17]

> Suppose that we propose the categorical imperative to someone who replies that he is only willing to accept it as an ultimate moral principle if we can justify it. We can then tell him that he already accepts it tacitly, because the desire for justification implies the desire to treat the problem rationally, and rationality implies impartiality, and this is what the categorical imperative requires. (Hoover Institution Archives, Popper Archive, Box 366, Folder 14: 5)

Nonetheless, as Habermas and others would say, Popper 'does not go far enough' in exploring the nature and value of this kind of argument.

2.2.2. HABERMAS. Similar to Popper (and Kant), Habermas (1973, p. 168; 2003, pp. 16, 30) conceives of 'objectivity' as 'intersubjectivity'. This theme is pursued in at least four ways. The first is in his sociological concept of the 'public sphere' (1989 [1962]). This concept is given normative force in his idea of the 'ideal speech situation' (1970), and developed further in his argument for a 'discourse ethic' (1990). Finally, he returns to it in recent discussions of, and revisions to, his theories of truth and rightness (2003).

To paraphrase Popper on science, Habermas seeks to 'establish the rules, or the norms by which humans are guided when they engaged in communication, criticism or argument'. Habermas proposes a meta-ethic called a 'discourse ethic' that allows for the rational 'justification' of values. The discourse ethic involves principles of participation and power, as Habermas (1990, p. 66) explains: 'Only those norms can claim to be valid that meet (or could meet) with the approval of all affected in their capacity *as participants in a practical discourse*.'

In his earlier work, Habermas thought the discourse ethic was a suitable way of deciding claims to truth in science *and* normative rightness in ethics.[18] Habermas (1973, p. 169) considered that 'the truth of propositions is not corroborated by processes happening in the world but by a consensus achieved through argumentative reasoning'. Ideally, such procedures provide the basis on which a provisional agreement may be reached by all those involved. As we shall see below, he comes to differentiate between the non-epistemic concept of 'truth' and the epistemic discourse of 'ideal assertability' (2003, p. 38). Habermas is pressed to this conclusion by a thoroughgoing awareness of the fallibility of both knowledge statements and the reigning consensus about them.

In proceeding this way, Habermas significantly modifies his previous discursive and contextualist concept of truth that relied solely upon articulating the ideal conditions for argument. In portraying the cycle of truth acceptance and its problematisation, Habermas's account (2003, p. 40) parallels that of Popper (1972a, p. 111) and his analogy of driving piles into a swamp. Habermas's (2003, p. 41) somewhat wordy summary of his revised position strengthens this observation:

> For the concept of learning produces the legitimating connection between knowledge and rational knowledge acquisition for participants in argumentation. But it does not endow their discursively justified beliefs with the infallibility of certainties of action. Insofar as knowledge is justified based on a learning process that overcomes previous errors but does not protect from future ones, any current knowledge remains relative to the best possible epistemic situation at the time. Even the agreement reached by way of a 'constructive' justification that convincingly terminates a discourse for the time being yields knowledge that is fallible and subject to improvement.

Like Popper, Habermas puts a strong emphasis on the provisional nature of any consensual agreements reached by this method. What is distinctive about Habermas's approach, however, is both his greater stress on and provision of detail on the ideal normative and political requirements of his procedures for solving problems.

For both philosophers, all judgements about problems and solutions are subject to interpretation and critical discussion among a community of inquirers who then reach agreement, until better knowledge, theories or tests come along. Thus any judgement is always fallible and provisional. Whereas this ideal procedure may not always be realised in practice, it does offer guidelines for how knowledge claims ought to be treated. Whereas Popper gives greater methodological guidance on how we are to go about problem solving, Habermas provides a better – that is, sharper – account of the ideal conditions under which decisions or agreements could be reached.

Yet, these arguments look very much like a retreat from universalism towards relativism, albeit of a democratic and more inclusive kind. For the cynic, knowledge claims would simply be subject to determination by whoever had the expertise and rhetorical power to influence it.[19] From another perspective, it is arguable that the more inclusive the process, the more likely the results could be distorted by those without the expertise to assess the reliability of the techniques, testing and evidence. What both Popper and Habermas trust will save them from this fate are their theories of realism and truth, where there is another convergence in their thinking.[20]

2.3. *The Role of Realism and Truth*

Both Popper and Habermas accept the necessity of realism for maintaining a concept of truth that is distinct from, but linked to, the process by which we arrive at true, confirmed or justified knowledge. Whereas both see the role of seeking 'consensual agreement' in knowledge claims, they reject consensus and coherence theories of truth.

2.3.1. POPPER. Popper's realism is a metaphysical conjecture about the nature of the world and our ways of knowing it.[21] It accepts that there exists a real physical world independent of us that can be known by our senses and discovered by science (Popper 1982, p. 2). Realism provides the metaphysical underpinning for Popper's *epistemology* and *methodology*. It provides a kind of premise and set of limits for what is epistemologically or methodologically possible (Popper 1974, p. 966). An epistemology that sets itself the task of discovering true explanations can hardly do without a conception of objective reality that is the equivalent of ultimate truth, and against which one can judge the adequacy of one's explanation. Popper needs realism and a theory of truth in order to avoid the pitfalls of relativism and scepticism.

Given his claim that he considers all criticism to be criticism of a theory's claim to be true, Popper also requires a theory of objective truth. Here Popper draws on the work of Alfred Tarski. Tarski's achievement was to overcome certain paradoxes in the concept of truth, such that, in Popper's view, we could now unashamedly *speak* of theories as being true or false (1972c, p. 316). Popper acknowledges that this theory does not provide any epistemological guidance; it merely reinforces our common sense notion of truth (1966b, p. 371). That is, Tarski's theory does not provide a criterion of truth and, according to Popper, we must not ask for such a criterion because it is simply unavailable (1972c, p. 318). This does not, however, prevent us from having some intuitive idea of truth, and using it as a regulative idea to guide us in our search for knowledge (1966b, p. 373).

As we have noted, however, judgement upon the adequacy, meaning, truth or falsity of a theory is largely a decision resulting from its critical discussion by a scientific or philosophical community. Rather than being problematic, Popper regards the distinction between truth as a regulative ideal, and the procedures for ascertaining truth or its equivalents as essential. Requiring that we engage in a search for truth is the crucial normative complement or counterweight to a social process of inquiry that could be diverted towards less worthy objectives, such as the quest for power or instrumental success. The search for truth is intended to keep the social process and the participants in it honest.

2.3.2. HABERMAS. Habermas too becomes a realist, and he makes a similar distinction between a theory of truth and procedures for seeking it. Habermas draws his insights, not from Tarski, but from the pragmatist tradition of philosophy, epistemology and politics. Following Hilary Putnam's arguments on pragmatic realism, but invoking 'induction', Habermas (2003, p. 34) writes:

> The presupposition of a world of objects that exist independently of our descriptions and are nomologically connected plays the role of a synthetic a priori for inductive scientific practice and indeed for any empirical theorizing.

In this later work, Habermas (2003, p. 251) discards his earlier views that equated truth with what would be accepted as justified in 'an ideal speech community'. He now advocates a 'non-epistemic concept of truth' that will do justice to our practical common-sense intuitions about reality. That is, he wants to separate the ideal way in which we determine truth claims, discussed above, from our understanding of the pragmatic 'reality' of truth itself.

Instead of invoking a correspondence theory of truth, however, he develops an argument based upon the distinction between action and discourse. There is a truth or certainty to be found in the pragmatic assumptions essential to action in everyday life, which is distinct from the intersubjective and fallible discursive methods by which we decide truth claims. Habermas (2003, p. 38) therefore now warns against assimilating 'truth' to 'ideal assertability'.

Habermas sets his argument in the pragmatic context of problem solving in everyday life, where, for the most part, we have few doubts about truth. Habermas (2003, p. 39) explains:

> We don't walk onto any bridge whose stability we doubt. To the realism of everyday practice, there corresponds a concept of unconditional truth, of truth that is not epistemically indexed – though of course this concept is implicit in practice.

The example here is intended to support a concept of truth distinct from the fallible epistemic discussions and decisions about it. In this respect, Habermas shares much with Popper (1972c, pp. 10–11) who regularly points out how our everyday practical wisdoms, such as 'bread nourishes', are refuted. For Habermas, when our expectations and dogmas are challenged, a process of critical reflection and discourse about 'justification' begins. This amounts to a search for truth, in which reasons are given and decisions made in dialogue with others. Like Popper, Habermas is now essentially a critical realist.

Both Popper and Habermas now have come to the view that we must retain or recover a way of speaking about objective reality and truth that is separate from, but linked to, our fallible methods of arriving at knowledge of reality and truth. Given the strong intersubjective requirements of Popper and Habermas, presumably something stronger than just individual honesty is needed to ensure that epistemic values are maintained. Both consider somewhat similar political and institutional solutions.

2.4. *Epistemological Politics: The Case for Democracy*

The message, explicit and implicit, from both Popper and Habermas is that we must develop the best possible institutional frameworks for promoting criticism, argument and discussion for protecting 'the public use of reason'. The main guarantee that the force of the better argument prevails, rather than violence, lies in democratic institutions and the rule of law. But there are important differences in how these are understood and advocated.

2.4.1. POPPER. Popper (1976a, p. 95) has commented that objectivity 'depends, in part, upon a number of social and political circumstances which make ... criticism possible'. Central to the maintenance of objectivity are the institutions of the *open society* and democracy that promote particular ideas such as the critical tradition, social values such as toleration of free discussion, and practices (of communication) such as publications and conferences (1976a, p. 96). Such institutions, including those of the state, impose the necessary 'mental discipline' and values of critical thought upon the individual researcher (1961, pp. 155–56). Popper (1961, p. 154–55) writes: 'Ultimately, progress depends very largely on political factors; on political institutions that safeguard the freedom of thought: on democracy.'

Given all that it is intended to protect, however, Popper's political theory of democracy remains relatively underdeveloped and uncritical. It may be described as a liberal minimalist theory oriented primarily towards the election of representative governments. In this view, democracy is simply a political technique, and thus another form of instrumental action. There is not the same emphasis upon participation and deliberation as there is in his philosophy of natural science, and it is essentially a form of democratic elitism. Regular elections are valued simply as a method of registering political concerns and for changing governments.[22] Critical discussion is important, but only because it provides part of the context for political democracy, of which not too much should be expected. If we were to rationally reconstruct Popper's ideas, however, it is arguable, and of course contestable, that the logic of Popper's philosophy requires the kind of deliberative democracy to be found in Habermas.

2.4.2. HABERMAS. Habermas elaborates a sociological concept of the public sphere that he develops into a normative theory of deliberative democracy. He sees both the idea and the actuality of a 'public sphere', situated between, and relatively separate from, both civil society and the realm of the state, as a central feature of modernity. As Thomas McCarthy (1989, p. xi) has portrayed it, the public sphere is an arena of social life in which 'critical public discussion of matters of general interest is institutionally guaranteed'.

On Habermas's account (1989, p. 4), the ideal of transparency of communication and argument was central to this conception of the bourgeois public sphere.[23] For Habermas (1989, p. 102), and following Kant, the historical emergence of this public sphere was characterised by private individuals engaging in 'rational-critical public debate'. This practice was often initiated in response to absolutist rule and authoritarian forms of government. Habermas's (see e.g. 1989, pp. 222–35) early project was historical in that it documented the paradox of democratisation. The more the liberal constitutional state transformed into a social welfare state, the more the capacity for critical publicity declined and the tendency for publicity to promote manipulation and acclamation increased.

This perceived historical domination of instrumental rationality, among other things, led Habermas to explore the theoretical foundations of alternatives, namely communicative action (1984; 1987a). By such means, Habermas (1987a, pp. 391–96) attempted to recover the potential for critique and emancipation in modern societies. His analysis laid the foundation for thinking further about alternatives to force and violence and resulted in a project to recover and revise democracy. Habermas (1992a, p. 446) explains the link:

> [The] 'political public sphere' is appropriate as the quintessential concept denoting all those conditions of communication under which there came into being a discursive formation of opinion and will on the part of a public composed of citizens of the state. This is why it is suitable as the fundamental concept of a theory of democracy whose intent is normative.

Steering away from democracy in its instrumentalist, liberal form as a method of forming and changing governments, and criticising republican democracy's overly rigorous requirements for citizenship, Habermas formulated a theory of *deliberative* democracy (Habermas 1996a; 1996b). Deliberative democracy, however, also evolved out of a project to provide the rational foundations for ethics, and it is Habermas's discourse ethics that delivers the normative foundations for a deliberative democracy. As Habermas (1996a, p. 25) presented it,

> [D]eliberative politics should be conceived as a syndrome [sic] that depends on a network of fairly regulated bargaining processes and of various forms of argumentation, including pragmatic, ethical, and moral discourses, each of which relies on different communicative presuppositions and procedures.

At its most general, deliberative democracy 'refers to the idea that legitimate law-making issues from the public deliberation of citizens' (Bohman and Rehg 1997, p. ix). This contrasts with liberal democracy that relies primarily upon procedures for aggregating preferences and tends to limit deliberation to various political and judicial elites.[24]

The ideal of deliberative democracy is premised upon a more radical form of political equality than that envisaged by the more minimalist liberal democracy. Habermas specifies that all those who have an interest in an issue of morality and law ought to be allowed to engage in public debate to influence the decision. He writes: 'Only those actions are valid to which all affected persons could assent as participants in rational discourses' (Habermas 1996b, p. 459).

The ideal model for a deliberative democracy derives in part from Habermas's (2003, p. 260) universal requirements for determining the validity of moral judgements, which are 'assessed in terms of how inclusive the normative agreement is that has been reached among conflicting parties'. Inclusiveness, however, is not just a quantitative matter; it is characterised by the commitment to mutual understanding. It requires participants to try to mutually take 'one another's perspectives' (Habermas 2003, p. 260). A deliberative democracy encourages the kinds of procedures necessary for determining and deciding complex moral, cultural and political conflicts.[25]

In terms of problem solving, it is arguable that a deliberative democracy has a number of advantages over liberal democracy. First, it facilitates better the recognition of problems. Second, it suggests universalist procedures for reaching agreements on a variety of problems. In this way, it also enables a powerful capacity for the transformation of perspectives, knowledge, morality and political institutions. Such a critical and self-critical process is definitive of an open society and the antithesis of a closed one. But criticism is not just of inherent value; it is a crucial resource for human survival. The transformative power of criticism within a deliberative democracy is essential for responding effectively to changes or indeed crises in the natural and social world.

Such democratising projects are generally confined to the nation state. Implicit in Popper and explicit in Habermas (e.g. 1996b, pp. 514–15), however, is a more radical option. Deliberation need not be limited to citizens and institutions within the nation state. Given his Kantian influences, Popper is not just a moral universalist (see Shearmur 1996, p. 101); he holds cosmopolitan values, as demonstrated in his regular references to the 'rational unity of mankind' (1966b, p. 225), the 'unity of mankind' (1966b, pp. 232; 246) and the 'unity of human reason' (1966b, p. 239). In his brief reflections on international politics (1966a, p. 113) he recommends internationalist strategies such as the creation of international organisations to prevent international crime and conflict.

Habermas too is a Kantian universalist, and is more of a thoroughgoing cosmopolitan who has developed his ideas in more detail, especially with reference to global human rights and supranational institutions such as the European Union.[26] It is therefore evident that deliberative reasoning can be required of citizens *and* their international representatives, whether these are governments or non-governmental institutions. Deliberative democracy may assist in the formation of a new ethical community of global citizens that would encourage the overriding civic unity and principles of good citizenship that many nation states seem to have lost. For the purposes of this essay it is important to see that deliberative democracy can provide a global approach, not only to solving global and local problems, but also to safeguard institutions that can secure conditions for progress in knowledge.[27]

4. KANTIAN SOURCES OF CONVERGENCE

How might this apparent convergence be explained? At its most general, when one considers the broad intellectual traditions that Popper and Habermas both share and oppose, it would not be surprising if they came to similar conclusions on at least a few issues. Both work in the broad tradition of the Enlightenment and understand the links between reason and freedom, autonomy and democracy. In their opposition to authoritarianism and dogmatism, whatever their source or nature, they generally reject the use of violence to achieve intellectual or political objectives (e.g. such as to maintain religious, political or intellectual authority). Opposition to authoritarian political ideologies, such as fascism and Stalinist communism, eventually came to provide the impetus for much of their intellectual work.

Popper and Habermas are both rationalists and objectivists who consider crucial the epistemic values of criticism and fallibilism. Neither sees much value in philosophy that is confined to 'puzzle solving', or that is preoccupied with the analysis of language or meaning. Philosophically, Popper and Habermas also repudiate scepticism and subjectivism, and of course, relativism. Both are also systematic thinkers – in a minimalist sense of the term – in that, over time, they have developed philosophies that examine a wide array of interrelated problems in epistemology, methodology, ethics and politics. Where possible, they have also tried to preserve some coherence between the various parts. Nonetheless, there remain many differences of argument, in the way they use certain terms, and especially in their modes of expression. They also diverged radically in their critical assessment of the character of contemporary capitalist societies and polities.

Over the course of their lives, Popper and Habermas shared a political allegiance to the European left and both distanced themselves from classical Marxism. In his adolescence, Popper began as a Marxist and communist, or at least a 'fellow traveller'. On his account (Popper 1992a, pp. 33–34), he abandoned Marxism and turned towards social democracy. Whereas Popper identified himself as a liberal from the 1950s, social democratic principles and policies appear as recurring themes throughout his published and unpublished writings. For example, in a number of places Popper goes well beyond conventional liberal theory in his views on socialisation (2008 [1947], pp. 103–05), (2008 [1946],

p. 125–26), worker's participation (2008 [1973], p. 295) and nationalisation (2008 [1974], p. 307).

As we have seen, Habermas's early intellectual life was influenced by Marxism, albeit within the revisionist tradition of the Frankfurt School of critical theory. His Hegelian language with its references to 'dialectics' reflected the radical, 'totalising' critiques of capitalism and technical domination that sanctioned its overthrow, and would have coloured his view on the merits of piecemeal 'reform'. By at least 1986, however, Habermas had espoused a fallibilistic view of socialism that shared much with Popper's political thought. In answer to a question about the relevance of socialism, Habermas (1986b, p. 92) said: '[S]ocialization, as a project which is capable of self-correction, undertaken on what could be called a fallibilistic basis, is as essential today as ever.' As he formulated his theory of communicative reason, his discourse ethic, and a theory of deliberative democracy, Habermas drew more on the philosophy of American pragmatism, and also leaned further towards social democratic principles (e.g. Habermas 1991 [1990], p. 45). There is also a corresponding diminishing emphasis upon the larger project of emancipation. For some critics, therefore, Habermas's abandonment of his early political radicalism has been interpreted as a retreat to liberal constitutionalism, with its more narrow concepts of freedom.

Popper and Habermas read widely, and many intellectual influences are both clear and acknowledged in their writings and interviews. A veritable scholarly industry would be needed to sort out the intellectual lineages for every aspect of their work. Nonetheless, the philosophical connection between the two are often more direct. For example, Habermas's frequent references to fallibilism indicate a debt to Popper. While still critical of what he sees as Popper's decisionism in regard to his support for rationalism, Habermas (1986a, p. 50) has said:

> . . . I have learned an enormous amount from Popper. He is one of the great figures in that admirable process of self-criticism through which analytical theory of science has matured and cast off its dogmatism. . . .

In the context of the major themes in this essay, it is arguable further that the stronger, common thread throughout their work is their joint intellectual debt to Immanuel Kant.[28] Popper and Habermas see themselves as reworking Kantian problems in the historical era of modernity. Popper (1972b, p. 27), for example, regarded his philosophy as putting 'the finishing touch to Kant's own critical philosophy'. He praises Kant's call for self-liberation or self-emancipation through knowledge (1992b [1961], pp. 137–38), seeing it as a spur to being able to criticise one's own ideas, and therefore a 'powerful enemy of fanaticism'

(1992b [1961], p. 149). Throughout his works, Habermas regularly turns to Kant as a starting point for discussion, often connecting his own views to Kant, as well as defending, extending or revising that philosopher's arguments (e.g. inter alia Habermas 1989, pp. 102–18; 2003, pp. 83–130).

In further support of this interpretation, it is notable that Popper and Habermas draw upon similar passages from Kant that refer to the importance of reason, the practices of reasoning, the public use of reason and its role in promoting peace. Both Popper and Habermas regard Kant's essay 'What is enlightenment?' as a vital source of intellectual and political inspiration. Kant urged people to think for themselves and their enlightenment required freedom, by which he means 'freedom to make *public use* of one's reason in all matters.' Kant (1970b, p. 55) elaborates:

> The *public* use of man's reason must always be free, and it alone can bring about enlightenment among men; the *private use* of reason may quite often be very narrowly restricted, however, without undue hindrance to the progress of the enlightenment. But by the public use of one's own reason I mean that use which anyone may make of it *as a man of learning* addressing the entire *reading public*.

Popper (1972a [1934], section 8), for example, cites with approval Kant's *Critique of Pure Reason* in reference to his concept of objectivity, understood as intersubjectivity, and how it may be distinguished from subjective belief or conviction. Habermas (1989, p. 108) too quotes the same passage (Kant 1963, p. 645):

> The touchstone whereby whether we decide whether our holding a thing to be true is conviction or mere persuasion is therefore external, namely, the possibility of communicating it and finding it to be valid for all human reason.

Thus we see the foundation of all 'rational-critical debate' is the public testing of knowledge claims, with a view to determining a general public consensus (Habermas 1989, pp. 107–08). Throughout their engagement with various philosophical arguments, over different theoretical terrains, the return to Kant remains constant.

5. PHILOSOPHICAL SIGNIFICANCE

This analysis has relevance for understanding the evolution of philosophy in the late twentieth and early twenty-first centuries. It demonstrates the resilience of a major optimistic strand of critical thought in the Enlightenment tradition, and marks it out against other more ambivalent or pessimistic strands that are evident, for example, in the works of Nietzsche and Foucault, amongst others.[29] This latter tradition sets great store by criticism and critique, but generally of a different type than that to be found in the later works of Popper and Habermas.

In an essay written shortly before his death, Michel Foucault, for example, reflects upon Kant, the Enlightenment, the nature of a critique, as well as the public use of reason and the limits to reason. Central to his understanding of the Enlightenment is the practice of problematisation:

> … the thread that may connect us to the Enlightenment is not faithfulness to doctrinal elements, but rather the permanent reactivation of an attitude – that is, of a philosophical ethos that could be described as a permanent critique of our historical era. (Foucault 1991a, p. 42)

One vital element of this philosophical ethos is what he calls a 'limit attitude': 'Criticism', Foucault (1991a, p. 45) writes, 'indeed consists of analysing and reflection upon limits.' Foucault's enterprise is inherently a political one in that reflection upon limits is in the service of understanding and fostering freedom. He tends to focus on how freedom is circumscribed or confined by various techniques of governing, or as he would call it, 'governmentalization' (Foucault 2003 [1997], pp. 264–65). This emancipatory tendency is tempered, however, by his strong opposition to 'all projects that claim to be global and radical' (Foucault 1991a [1984], p. 46).[30]

Foucault's method is historical and sociological rather than expressly philosophical, though it is commonly argued that the empirical and the philosophical are inextricably linked. One of his goals was to demonstrate not just the particular trajectories in the evolution of knowledge, which he formulates as 'discourses', but how the latter become almost autonomous. For Foucault, discourses exercise a particular form of power over individuals, contributing to their very formation, nurturing certain capabilities, but also setting limits to them. Foucault extends his thinking about discourses into a theory of truth, of which only a bare sketch can be given here. Foucault regards affirmations of truth as contestable discourses in regimes of power, and writes (1991b, p. 74):

> 'Truth' is to be understood as a system of ordered procedures for the production, regulation, distribution, circulation, and operation of statements.
>
> 'Truth' is linked in a circular relation with systems of power which produce and sustain it, and to effects of power which it induces and which extend it. A 'regime' of truth.

In both Popper and Habermas's work we may therefore observe the convergence of philosophical arguments into just such a 'regime of truth', with its overtones of dogmatism. From this Foucauldian perspective, the results of their philosophising could be considered an epistemic discourse that exerts specific controls over those who adopt its precepts. Despite its claims to universality, this regime could be portrayed as simply one contestable form of discursive power among others.

In one sense, this sociological depiction is accurate; Popper and Habermas are indeed formulating normative guidelines for the determination of knowledge claims. Nonetheless, their proposals are not confined to a particular discipline or field of technical expertise, of the kind, such as medicine or criminology, that Foucault studied. Their guidelines are at a second-order level and intended to apply to all fields of knowledge. What may weaken the claim that they have established a 'regime' in an authoritarian sense, as opposed to an authoritative one, is that both their epistemic guidelines, and the results that are reached by them, are fallible. Whatever 'truths' are produced even at this philosophical level are provisional – that is, non-dogmatic – until they are supplanted.

An associated question must also be asked, however, as to whether there are any plausible epistemological norms available from within this alternative critical tradition. Because Foucault is profoundly aware of the effects of epistemic discourses, he rejected this option and tried to avoid such a normative project. His critique arises from within a social and historical theory that celebrates resistance to the dominant regimes and discourses of truth as they have evolved in government, the humanities and social sciences. Although Foucault offers his *methodologies* of archaeology and genealogy as ways of investigating, explaining and understanding such empirical processes, he gives no epistemological argument for why these would offer better knowledge outcomes than any other. He provides political reasons for their practical value, but he gives no *epistemological* reasons why we should give his analysis any higher status or credibility than any other theory. Just as important, he does not explain how his methodologies would not constitute or contribute to their own regimes of truth. Yet, without such reasons and explanations, Foucault remains locked into an epistemological and normative relativism.[31]

For both Popper and Habermas there remains a vital role for *normative* epistemology. For them, the criteria of progress in knowledge cannot be reduced to the instrumental or strategic considerations of power. Nonetheless, both Popper and Habermas are alert to the historical evolution of knowledge, and the need to revise it in the light of past mistakes. It is this possibility for human error that is a dominant motif in their work. Like Foucault, their philosophical and political projects are bound by awareness of the limits to the claims that can be made about knowledge. In a later work, Habermas (2008 [2005], p. 6) talks about a 'fallibilistic but nondefeatist post metaphysical thought [that] differentiates itself … by reflecting on its own limits – and on its inherent tendency to overstep those limits'. For such reasons, both Popper and Habermas are thoroughgoing fallibilists, for whom the conduct of criticism and critique is central.

Although committed to the ideals of freedom, truth and progress in knowledge, Popper and Habermas do not see them as embedded in history, or as delivering 'meta-narratives' in the ways commonly portrayed by postmodern or poststructuralist theorists (e.g. Lyotard 1979). Nor would they accept a sociology of science that represents all rational discourse about truth as a form of domination, and that accords little or no substantive and effective role for rational, critical argument in determining knowledge claims. For these reasons they would reject Foucault's arguments.

Both Popper and Habermas are critical realists (and rationalists) who rely on a concept of truth, discursive argument and fallibilism as part of a wider process of problem solving. They remain among the foremost philosophers in the optimistic traditions of the Enlightenment. From different directions they have pushed the boundaries of epistemology beyond its long-standing concerns to achieve certainty through rigorous processes of justification. Whether they have been entirely successful in their project to establish a normative epistemology has been the subject of a vast literature, and cannot be considered here.[32] My more limited aim has largely been to recast the longer legacy of the positivist dispute and demonstrate how, over time, Popper and Habermas have come to share a number of fundamental philosophical tenets.

6. CONCLUSION: THE CASE FOR UNIVERSALISM

This chapter has pointed to the various components of a postmetaphysical universalism to be found in the work of Popper and Habermas. For epistemological, ethical *and* political reasons, Popper and Habermas are committed to the universal values of freedom, toleration, mutual respect and inclusivity. The discussion has also pointed to a universal *unity of method* common to Popper and Habermas. Although their universalism is supported by various types of philosophical analysis, it also draws on arguments derived from, or inspired by, evolution. Both Popper and Habermas refer to biological evolution to demonstrate how human beings are essentially problem-solving creatures who learn by trial-and-error criticism. Their normative philosophies are thus informed by empirical and naturalistic considerations.

It becomes evident both that fallibility *is* an ineradicable feature of the human condition and that fallibilism *ought* to be a central element of epistemology. In such a way, both Popper and Habermas understand that although reason and rationality cannot be understood outside practical contexts – evolutionary, cultural and institutional – those who adopt such concepts also commit themselves to reflect critically upon the context in which they arise, and other contexts. That is, reason and rationality enable and encourage the *problematisation* of the environments or situations in which they are practised. To borrow from Putnam (1983, p. 234) via Habermas (2003, p. 221; see also p. 85), reason is both immanent and transcendent.

From these foundations, Popper and Habermas derive universal procedures for arriving at judgements on claims to knowledge about empirical truth and normative rightness. These procedures provide the means by which solutions to problems may be proposed. In this way, both Popper and Habermas offer proposals on how to maximise the problem-solving capacity of human beings. But the requirement of intersubjectivity in these procedures extends beyond individual morality and epistemic communities, to require external political and institutional protection. In formulating their concepts of the open society and the public sphere, both Popper and Habermas come to advocate democracy as essential for protecting and facilitating effective problem solving.

Explicit in Habermas and implicit in Popper, however, is a theory of *deliberative* democracy that replicates and extends the universal meta-values and procedures essential to progress in science and ethics. At the political level, deliberative democracy enables a non-violent and inclusive approach to solving problems of complexity and change. Building upon already existing communicative predispositions and practices, deliberative democracy provides a universal means for communities, societies and polities to engage in peaceful forms of critical self-reflection and decision making.[33]

From this perspective, the ensuing political task is to extend these 'universal' norms and practices to the global level and seek to establish the institutional protection of intersubjectivity and non-violent problem solving across the world. Such a project would give universal effect to the Kantian cosmopolitanism[34] evident in the work of Popper and Habermas. It would also continue the larger evolutionary process of improving our capacity to transform ourselves, and maintain our survival as a species.

NOTES

1 See Adorno et al. (1976 [1969]) and the early analyses in Frisby (1972; 1974).
2 The early observers of this tendency included, in chronological order, Radnitzky (1970), Wilson (1976; 1981), Heller (1978), Ray (1979), Hesse

(1980), Giddens (1985), Thompson (1984/85). More recent commentators include Shearmur (1996), Stokes (1998) and Hacohen (2000).
3 See Habermas (1992b) and Yates (2011, p. 35).
4 See the analytical history by Held (1980).
5 See Horkheimer (1982).
6 As a general principle, Popper rejected the quest for ultimate justification. But this is different from being able to provide convincing reasons and a cogent argument for a particular standpoint, or as Popper would prefer, to defend it against strong criticism. For example, Popper never developed a strong meta-ethic, as Habermas did, that would allow him to determine and address ethical problems.
7 He also criticised the arguments of Bartley (1964) who had attempted to overcome Popper's concessions to fideism and irrationalism.
8 See e.g. Stokes (1997).
9 Another exposition of the role of logic in rational criticism appears in Popper (1972c [1981], pp. 304–18).
10 See the discussion in Stokes (1998, pp. 126–28).
11 Darwinian themes are evident in the *Logic of Scientific Discovery* where Popper (1972a, p. 108) writes: 'We choose the theory that best holds its own in competition with other theories; the one which, by natural selection, proves itself the fittest to survive.' See also (1961), pp. 133–34. See also ter Hark's (2004) account of Popper's earliest engagement with evolutionary epistemology.
12 See Popper (1983, p. xxxv), and also Popper (1994 [1975], p. 2) where he writes: 'From a biological or evolutionary point of view, science, or progress in science may be regarded as a means used by the human species to adapt itself to the environment ...'.
13 There are strong affinities here with the work of the pragmatist John Dewey (1980 [1929], p. 103), who wrote: 'The first step in knowing is to locate the problems which need solution.'
14 On Popper's naturalism, see also Naraniecki (2014).
15 See also Dewey (1980 [1929], p. 101), who writes: 'There is nothing that a scientific mind would more regret than reaching a condition in which there were no more problems.'
16 See here the arguments in Stokes (1998, pp. 170–72) and Jarvie (2001).
17 I am indebted to Jeremy Shearmur (1996, p. 95) for alerting me to this source, though he expressed some doubts about the authenticity of the document.
18 In an Addendum (1961) to *The Open Society and its Enemies*, Popper (1966b [1945], pp. 384–86) too drew parallels between scientific and ethical reasoning.
19 For a persuasive account of the importance of a particular kind of rhetoric to the success of deliberation, see O'Neill (2002).
20 Shearmur (2007) offers an alternative political proposal that addresses the institutional weaknesses of Popper and Habermas' epistemology.
21 Although there is no explicit treatment of the topic in *The Logic of Scientific Discovery*, Popper acknowledges elsewhere that a 'robust' realism 'permeates' the work (1983, p. 81).
22 See Stokes (2006) for an argument about how Popper departs from the usual model of democratic elitism and realism with a theory of social democracy.

23 Habermas (1989, pp. xvii–xviii) also distinguishes between the liberal model of the 'bourgeois' public sphere, the 'plebeian' form that is characteristic of the Chartist and European anarchist movements and 'plebiscitary-acclamatory' forms characterised by modern dictatorships. See too Habermas's (1992a) later reference to specific weaknesses in his analysis.

24 Nonetheless, most deliberative theorists envisage democracy being conducted within a liberal constitutional framework and under the rule of law in which particular rights are protected.

25 From the Popperian perspective of negative utilitarianism, it could be argued that deliberative democracy would be more valuable in gaining agreement on those most urgent forms of suffering and concrete problems that confront society. Rehg (1996, p. xvi) sees this kind of understanding also present in Habermas's work.

26 See e.g. the essays in Habermas (1998; 2006).

27 For one ambitious attempt at this, see Dryzek (2006).

28 I am indebted to Jeremy Shearmur for suggesting that I develop this line of argument, the origins of which appear briefly in his book (Shearmur 1996, pp. 96–97 and 165–66).

29 The latter strand would also include the critical theorists Horkheimer (1996 [1946]) and Adorno and Horkheimer (1979 [1944]).

30 Yet, it should be noted that late in his 'political' life, Foucault (2002 [1984], pp. 474–75) advocated universal human rights and a concept of international citizenship as a way of bringing the abuse and suffering of men and women to the attention of governments.

31 See Habermas's critique (1987b, pp. 238–65 and 266–93) of Foucault. But see also Norris (1994) for an account of the tensions in Foucault's thought and how he tried to reconcile them in his late works.

32 Many of the chapters in this 'companion' canvass strengths and weaknesses in Popper's philosophy. For a collection of wide-ranging critiques of Habermas's philosophy, see Freundlieb, Hudson and Rundell (2004a), and especially the overview essay by these authors (2004b, pp. 1–34).

33 This is not to claim that deliberative democracy assumes only one form, or that it is without any theoretical or practical difficulties. Critical debates over this revision to democratic theory have proliferated since the 1990s, as have many successful deliberative democratic 'experiments'.

34 See e.g. Kant's (1970a) 'Idea for a universal history with cosmopolitan purpose'.

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JEREMY SHEARMUR

13 Popper's Politics and Political Thought

Popper wrote quite extensively on political themes, although, with the exception of his *Open Society*, most of his writing took the form of lectures and relatively occasional pieces.[1] His political thought may usefully be understood in three ways. First, there is his own personal political trajectory. Second, there are issues from his theory of knowledge and his more general philosophical ideas which are pertinent to his politics. Third, there are his more specifically political theories. I will discuss these in turn, prior to a concluding section in which I turn to criticism and will thus raise some problems concerning Popper's political philosophy. For reasons of space, I will not discuss Popper's critical work on Plato or Marx.

1. POPPER'S POLITICAL TRAJECTORY

In his *Unended Quest*, Popper has told us that as a child he was deeply concerned about human suffering. He was, then, influenced by some writings from the pre-World War I pacifist movement. After World War I, in his teens, he was influenced by Marxism, and, as Bartley (1989) has documented, he worked as a volunteer in the offices of the Austrian Communist Party. As Popper has often discussed, he was stirred from his dogmatic slumbers by the impact of a demonstration, which the Austrian communists had organized, and which led to the death of several demonstrators (Hacohen 2000; see also Hacohen, Chapter 2 in this volume). This shook his confidence in his support for Marxism, and led to a reaction not just against Marxism but also against rationality. The effect seems to have been, on the one side, an espousal of Kantian ethics on a basis of something like existential choice, in which Kierkegaard was an influence (Bartley 1989; Hacohen 2000)[2] and, on the other, a development of his interests in the contrast between the standards that operated in science and the basis on which Marxism, Freud and Adler claimed intellectual respectability for their own ideas.

After giving up on Marxism, Popper still remained a socialist. He joined a Social Democratic organization, taught for an educational movement associated with the social democrats (the *Kinderfreunde Bewegung*; Popper 2008, pp. 31 and 104–05), and was known as a socialist by, say, Rudolf Carnap, who was himself a socialist. Nevertheless, he seems over time to have shifted from socialism as well, not least because of his growing concern with the problem of bureaucracy under socialism, and his disagreement with the tendencies within Austrian socialism that he was to criticize as 'historicism'. One can see the outcome, in terms of the kinds of political views which were manifested in his *Open Society*. The language of the first edition expresses these particularly clearly, in terms of its hostility to laissez faire capitalism.

Popper's viewpoint was ethically individualistic, and he cared passionately for the freedom and well-being of each individual. (One might note, here, what Popper called 'protectionism'. This concern was influenced by Kantian ideas and has much in common with the ideas that Pettit [1997] and Skinner [1998] have recently revived as 'republicanism'.[3] Popper stresses the need for the protection of not just individual liberty but freedom of the individual from economic exploitation, and all this is, distinctively, the proper duty of the state.) Popper joins Marxism in a critique of laissez faire capitalism, and is scathing about conservative disregard for the well-being of the poor. But he stresses the idea that responsibility for society, and for what will happen in history, is ours, as distinct from there being any kind of historical inevitability, or *telos* to history. He argues for the need for us to take active responsibility for the improvement of the human condition. His view was that while history has no intrinsic meaning, it is open to us to try to give it one, by way of our moral and political endeavours. These were to be pursued by way of piecemeal social reform, in which we learned through trial and error.

One other dimension to Popper's political development is also of interest. It relates to his contacts with Leonard Nelson's 'Fries School'. Because this is less familiar, I will explain it in more detail (but see also Hacohen's contribution to this volume). The reader of Popper's *Logic of Scientific Discovery* may recall Popper's discussion there of what he called 'Fries' Trilemma', which is a problem about the justification of knowledge claims. The Fries in question was Jakob Friedrich Fries (1773–1843), who developed a form of naturalized Kantian epistemology and who is also known as an opponent of Hegel. Popper's concern with Fries came about as a result of his friendship with the philosopher Julius Kraft who was himself strongly influenced by Leonard Nelson (Popper 2008, chapter 1). Nelson was a philosopher with particularly wide interests who exerted a strong personal influence over a number of people with whom he was in contact. He died tragically young. Before his death he had set out to champion ideas that he found in Fries's work.

In addition, he developed a distinctive line of argument in political philosophy, which in some respects represented a self-conscious updating for his own times of certain themes from Plato's *Republic*. In particular, he argued – in a manner that influenced Popper's own later discussion of the 'paradoxes of democracy' – that there was something paradoxical about espousing, say, the ideal of justice and also saying that one was a democrat. The point here is that the latter view seemed to commit you to favouring whatever it was that a majority favoured, whether or not it was just. Nelson advocated an alternative to democracy, in the form of an organization that, after the model of the Roman Catholic Church or the Army, was dedicated to internal promotion of its leadership on the basis of how well it served and developed the ideals to which the organization was committed (Nelson 1928). Nelson's ideas were not just theoretical: an organization was set up to foster them. Popper had extensive discussions with Julius Kraft about philosophical issues, and Kraft was a member of this organization. At one point Kraft and another senior member of the organization, Willi Eichler, made a formal attempt to recruit Popper into membership of it.[4] Popper did not accept, and he has written that arguments that he used in defence of democracy against Eichler played a 'great role' in his *Open Society*.[5]

Let me return to the issue of the way in which Popper thinks that we should set out to improve society. Popper repudiates (as 'essentialist') Marxist ideas about there being structural limitations on the ways in which society is open to change. But at the same time, he is a fallibilist, and is strongly aware of the fact that our attempts at social reform typically produce unintended consequences, to which he thinks that it is vital that we should pay attention. He also thinks that we may attain theoretical knowledge of the limitations of our capacities, and that those who want to improve society should consider these. (This was similar to his reaction to Hayek's argument about limitations on our knowledge imposing limits on our abilities to run a planned society (Popper, 1945, volume 1, chapter 4, n. 9).) Popper's view, as I shall explain in more detail in the next section, seems to me best understood in the following manner. Government should uphold, and itself be restricted by, the 'protectionist' ideas referred to above. It should then attempt to address what members of the society take to be the most pressing social problems. Its attempts to do this are fallible, and will typically generate undesirable unintended consequences. All this needs to be subject to critical feedback from all citizens, while government itself should be replaceable through elections. (This lies at the heart of democracy for Popper, and it is striking that, in his old age, he was critical of the idea of proportional representation because he considered that it would serve to undermine our ability to get rid of an unsatisfactory government; see

Popper 2008, chapter 41.) The way in which we should proceed was by means of what he called 'piecemeal social engineering'. He contrasted this tentative approach to the remedying of social problems, subject to critical feedback, with what he called 'utopian social engineering'. Popper was, in this context, highly critical of themes that he discerned in both Plato and Marx, and his arguments exhibited some interesting parallels with Hayek's critical discussion of social planning in his *Road to Serfdom*, although what lies behind them is rather different.

It is important to note that for Popper the 'piecemeal' character of such social engineering is not a matter of scale. However, he disagrees with the idea that society can be operated on as a 'whole', and insists that our efforts at social reform take seriously our fallibility and our need to be able to learn. Thus, for example, in his correspondence with Rudolf Carnap, just after World War II, Carnap raised the question of whether or not Popper was still a socialist. Popper indicated that while he would not describe himself in such terms, he was still open to experiments with the socialization of the means of production, and positively favoured it in respect of monopolies that could not be broken up.[6]

Popper had completed the text of his *Open Society* prior to reading Hayek's *Road to Serfdom*,[7] but he was subsequently in correspondence with Hayek about his *Open Society* which Hayek was able to place with his own publishers, Routledge, and in connection with the publication of 'The Poverty of Historicism' in the journal *Economica*, of which Hayek was the editor. The final section of Popper's 'Poverty of Historicism' was rewritten to include discussion of some of Hayek's work on the methodology of social science (although as the earlier versions of the manuscript seem to be missing, it is a matter of conjecture just what changes Popper made). They were, however, in agreement about the defects of 'historicism' by which they meant ideas about there being inevitable tendencies in history. There were many other points of similarity between the viewpoints of *The Open Society* and *The Road to Serfdom*.

As I have indicated by reference to Popper's correspondence with Carnap, Popper was in some respects clearly more sympathetic to socialism than was Hayek. From correspondence with Ernst Gombrich, who had played a major role in attempting to place *The Open Society* with a publisher on Popper's behalf, it was clear that they both felt a certain distance from, at the very least, the enthusiastic reception that Hayek's work had received on the political right (Shearmur 1996a, chapter 1). There was, however, some room for ambiguity here, in that Hayek, at the time, was himself more in at least emotional sympathy with socialist ideals than one might imagine from his later work (Shearmur 1996b). Hayek himself expressed some concern about the kind of political reception that his *Road to Serfdom* had received, and went out of his way to urge his publishers to advertise his work to the political left (Shearmur 2006a). In addition, the role that Hayek accorded to state activity in *The Road to Serfdom* and his discussion of 'planning for freedom' were clearly ideas with which Popper could feel himself to be in agreement. Nonetheless, Popper recognized that there were differences between Hayek's political views and his own. It is striking, however, that Hayek was himself gradually to move to a more distinctively classical liberal perspective, and to repudiate certain aspects of *The Road to Serfdom* as, presumably, too interventionist (compare the 'Preface 1976' in Hayek 2007). Popper, for his part, was more critical of Hayek's views in private discussions with him than he was in public or in correspondence with Hayek. For example, in letters to his friend Colin Simkin, Popper criticized Hayek's economics as utopian.[8]

I have stressed some of the differences between Popper and Hayek, just because, subsequent to the publication of *The Open Society*, Popper might be seen as moving a little closer to him. In the second full edition of *The Open Society*, Popper toned down some of the language that he had used in criticism of what he had initially called laissez-faire capitalism, something for which Hayek had taken him to task. He also emphasized more strongly that government activity should not be discretionary in its character. In addition, in the period following World War II, two developments took place.

As Hacohen (2000) brings out so well, *The Open Society* is a product of lessons that Popper thought should be drawn from his experience of interwar Austria for the period of postwar reconstruction. His main criticisms of Marxism – the text of his *Open Society*, his *Poverty of Historicism* [1944–45] (see Popper 1957) and his criticism in 'What Is Dialectic?' [1937] (see Popper 1963, chapter 15) – were thus formulated before or during World War II. He was, however, also obviously active during the period of the Cold War. His delivery of 'Prediction and Prophecy' at an International Congress of Philosophy in 1948 (see Popper 1963, chapter 16) brought him into critical contact with Soviet philosophers. Popper was gradually led to formulate his own ideas about the character of the Cold War (see Popper 2008, chapter 27). Also, while stressing that it was far from perfect and in need of reform, he argued that the society in which he was living was better than any other society that had existed to date. Such ideas were also expressed in the course of critical reactions to the resurgence of interest in Marxian-influenced views, in the late 1960s and early 1970s (see Popper 2008, chapter 31).

In political terms, Popper could be said to have become slightly more conservative and sympathetic to classical liberal perspectives as time went by, and examples may be given of the casual expression of views which contrast with the radicalism of his *Open Society*. It is also clear that one would not hear from the elderly Popper the degree of sympathy for some of the socialist concerns that he expressed to Carnap in 1947. Nonetheless, it does not seem that there was any systematic change in his viewpoint. In a letter to his friend Bryan Magee in 1974, when the latter had just become a Labour Member of Parliament, Popper made a startling suggestion to the effect that the government might take a 51 per cent shareholding in all publicly quoted companies as an alternative to their paying tax (see Popper 2008, chapter 34). Although he was possibly more favourable towards markets in his old age than he was in *The Open Society*, he regularly stressed the idea that a state-provided legal system was necessary for a free market,[9] and he was highly critical of what might be called 'market fundamentalism'. In addition, in reflecting on the changes that were needed in the former Soviet Union, he gave pride of place to the need for a legal system and the rule of law (Popper 2008, chapter 45). Finally, in what was perhaps the most headline-catching work on political themes of his old age – his criticism of the effects of television and his proposals for governmental controls to limit it – his particular concern was the effects that violence might have (see Popper 2008, chapter 48). This idea expresses his fear that such exposure to violence might undermine our sensitivity to the suffering of others, a concern for which, as *Unended Quest* (Popper 1976) indicates, went right back to his childhood.

2. EPISTEMOLOGY AND POLITICS

If one is concerned with cross-fertilization between Popper's more general philosophical ideas and his political thought, a key theme is his fallibilism. In addition, there are obvious links between his ideas on the philosophy of social science, his ideas about explanation in *The Logic of Scientific Discovery* and his criticism of historicist themes in both Plato and Marx.[10] In this section, however, I discuss two perhaps less obvious issues. I start with one preliminary point about Popper's *Open Society*. It is important, when reading this work, to bear in mind the character of the theory of rationality that Popper held at the time at which he wrote this book.

In this context, one needs to note one significant issue concerning Popper's *Logic of Scientific Discovery* [1934] (Popper 1959). When writing that work, Popper held various metaphysical views (for example, about scientific realism as an aim of science, and about truth). Yet he did not argue for them as such but, instead, either avoided referring to them or adopted the procedure of treating metaphysical ideas as if they were methodological proposals. The reason why he proceeded in this way is clear enough, as he later explained in his *Objective Knowledge* (Popper 1972, chapter 2, note 9). Although he held various metaphysical theories when he was writing *The Logic of Scientific Discovery*, he did not have at hand a theory of their rational defensibility. Accordingly, while he referred in passing to having such ideas, he did not wish to make anything in his argument depend on them. Instead, he worked with methodological proposals (which he thought were open to argument), or alternatively stressed our need simply to make decisions between alternative views.

For example, while he clearly favoured a realist view of the aims of science, and later was to go on to offer arguments for it, in *The Logic of Scientific Discovery* he avoided appealing to such a view. Rather, on the one hand, he offered a characterization of his own view of the aims of science without actually invoking realism. On the other hand, the crucial issue of the aim of science was left as a matter open to individual decisions. In effect, those who disagreed with Popper on the aims of science were left free to choose them, and in consequence to adopt a different methodology of science, and further to recommend that we should follow different methodological rules.

My reason for mentioning all this is that when Popper wrote *The Open Society*, his views on the scope of reason were much the same. This is made clear by the way in which he takes Whitehead to task for offering ambitious metaphysical views, without being able to offer a theory of the rationality, or of the progress, of metaphysics (Popper 1945, chapter 24, section v). This was something that Popper there did not himself attempt to furnish. However, Popper *later* offered such a theory in his *Postscript* (which dates from the early 1950s; see Popper 1982–83), and in material extracted from it published in (Popper 1963, chapter 8, part 2).

It is important to bear this in mind when reading *The Open Society*, for the fact that Popper held a limited view of the scope of reason serves to explain several features of that work. Consider the 'decisionism' with regard to value judgements which one finds there. Popper repeatedly stresses that ethical issues are matters for our own decision. This gives an almost existentialist flavour to the book. It contrasts, however, with the book's great moral seriousness, and also with what, as I have argued elsewhere, are indications of moral realism expressed in some of Popper's footnotes (Shearmur 1996a) – something which comes out more clearly in some later unpublished work.[11] Popper does say, in *The Open Society*, that our decisions are not arbitrary, and writes about the importance of a rational attitude, and of a willingness to learn from others. But he does not explain – and at the time, it is not clear to me that he could have explained – by what our moral decisions are constrained.

The absence of a general theory of rationality also serves, I believe, to explain the lack of engagement with Plato on issues of metaphysics and epistemology. This, in turn, has the consequence that Popper's critique of Plato (and Heraclitus) in many respects rests upon drawing out what he takes to be politically unacceptable consequences from their work,[12] rather than on a more direct engagement about the unacceptability of the philosophical ideas from which these consequences are drawn. In this respect, the philosophical style of *The Open Society* is in certain ways at odds with Popper's later work, in which he had no inhibitions about the explicit discussion of metaphysical and epistemological views.

Let me now turn, however, to two ideas from *The Open Society* that merit more specific discussion regarding the relationship between Popper's political thought and wider themes in his philosophical writings.

First, there is Popper's theory of 'negative utilitarianism'. Popper's theory may usefully be understood by way of a parallel with his theory of 'the empirical basis' in his *Logic of Scientific Discovery* (see Shearmur 2006b, 2006c). In that work, Popper developed an account of the testability of scientific theories in terms of an open-ended intersubjective consensus as to what is the case; one that focused more specifically upon the behaviour of observable, macro-level objects. The notion here was that people may favour – and, indeed, see the world in terms of – different, contrasting theories. These, Popper urged, could best be tested if they were held accountable, not to statements about the content of individuals' subjective experience, but instead to claims about the behaviour of relevant aspects of the world, issued on the occasion of the undertaking of tests, concerning which there was (at least currently) no disagreement. These were, thus, claims about the world which were intersubjectively acceptable to people who held different substantive theoretical ideas. Popper's views here have been widely criticized, but I have suggested (Shearmur 2006b) that none of the criticisms that have been advanced against them are effective. (For a somewhat differing interpretation of his views, see Andersson, Chapter 5 in the present volume.)

Let me now turn to Popper's parallel ideas about an agenda for political action. In his *Open Society* these ideas are typically formulated as 'demands'. In Popper's mature thought, it would be possible to couch these more as claims which could be intersubjectively appraised (as opposed to 'justified'), but while Popper, with this issue in mind, shifted his terminology to speak instead of 'proposals', he does not elaborate on such an account of how they are to be assessed.[13] Nonetheless, in *The Open Society* Popper does offer an account of a procedure through which the agenda for public policy might be set. He gave a more explicit account of what was involved in 'Public and Private Values', which is likely to date from some time between 1944 and 1946 (see Popper 2008, chapter 13). In this paper, Popper poses a problem concerning what he terms utopian political ideas (which include socialism, liberalism and utilitarianism). He calls them utopian because, while they inspire people's approach to politics, he argued that there was no rational means at hand for resolving what was at issue between them. In response to this situation, Popper offered the following suggestion: one might try to discover what it would be possible for people who held such different views to mutually agree stood in need of remedy. What one has here is a suggestion in some ways reminiscent of later suggestions by Rawls (1993) and Sunstein (1995). (They have, in different ways, argued that we might focus upon what there is in common between different [reasonable] perspectives.)

However, Popper's approach differs from theirs, because it is focused upon what people find *un*acceptable and suitable as an object for governmental initiative. It thus seeks to find something like the highest negative common factor between the views in question. The focus upon what is unacceptable is something upon which agreement is more likely to be possible than it might be if the focus was upon what people found most desirable. In addition, in his *Open Society*, Popper gave at least an indication that it might be possible for people to learn morally rather than being trapped within their particular systematic moral framework. He referred to the way in which a character from Shaw's *St Joan* recanted (obviously rather too late) when he discovered what his demand that Joan of Arc should be burned alive actually meant (Popper 1945, chapter 24, section III).

It might, in principle, be possible to see if Popper's various 'demands' in the text of his *Open Society* could be defended by way of recourse to such a theory, but this is not something that Popper himself undertakes. Alternatively, I have elsewhere suggested that his ideas about the protection of the autonomy of the individual might be defended by way of another theme in Popper's work. In *The Open Society*, he refers to the significance of critical input from anybody, drawing a contrast – that he illustrates with, for example, quotations from Pericles and Burke – between the fact that only some may be able to suggest policy measures but anybody may be able to criticize them. (One might look at this in terms of each person potentially having knowledge relevant to their appraisal, which is likely to be specific to their own particular social situation.) In this context, Popper refers to the Kantian theme of the 'rational unity of mankind' (Popper 1945 chapter 24, section I), which is an idea that, as Popper develops it, fits well with his own interpretation of rationality in terms of openness to criticism.

Now, as J. S. Mill argued in his *Subjection of Women*, if someone is beholden to others, they may not be able to make a critical input of this kind. Further, Jeremy Waldron (1993) has drawn our attention to the way in which historically, in the 'republican' tradition, women and others considered dependent were not accorded a political voice, as it was argued they did not possess the relevant kind of autonomy. Waldron further suggests, however, that if we are committed to the democratic participation of everyone, then this argument may be reversed, and furnish, instead, the basis for according them the material support they need to enjoy such autonomy. Clearly, if we value criticism – as we should from the point of view of the discovery of truth, or of relevant kinds of social learning – this, in itself, would offer us a Popperian argument as to why we should accord people autonomy. Such a concern fits well with the broadly Kantian resonance of Popper's 'protectionism', and thus serves as an independent line of argument for according people the kinds of 'protection' that Popper favours. While Popper shows no signs of taking the argument in this direction himself, one could further suggest that there is a certain parallel here with a theme in Habermas (see Stokes's contribution to the present volume). For one might suggest that there is an epistemological argument drawing upon the conditions required for the rational appraisal of any substantive ethical proposal, for the autonomy of each individual (see Shearmur 1996a, 1996b). On this basis it might be claimed that the individual autonomy required for the appraisal of specific ethical claims should be accorded priority over the content of any such specific ethical claim, and thus almost to have an a priori status (relative to the appraisal of other specific moral claims)![14]

The second line of argument relates to Popper's ideas about toleration, and to what he wrote about 'The Myth of the Framework' (in Popper 1994). As I have explained, Popper's views shifted from his position in *The Logic of Scientific Discovery* and *The Open Society*, where he did not have a theory of the rational appraisal of non-empirical (and non-methodological) ideas, to the view that such things could be appraised on the basis of intersubjective criticism, relative to their ability to solve the problems towards which they are directed. This, as I have suggested, opened in principle the possibility that *The Open Society* could be reinterpreted in such a way that its decisionism – something that might seem to give it a slightly irrationalistic, almost existentialist air – could be eliminated. At the same time, it is important to note that while Popper's mature approach might be understood as a full-fledged critical rationalism that could be applied in principle to any subject matter, Popper was himself rather modest concerning the ways in which such ideas might be applied.[15] More generally, while

Popper's later approach to social and political issues extended his 'critical rationalism',[16] he does not develop these ideas as expansively as later do Habermas[17] and some proponents of deliberative democracy, to whom Popper's ideas, however, also offer some important correctives. To explore this, let us look briefly at Popper's work on two related themes.

First, there is his concern with 'the myth of the framework'. This term, to my knowledge, was first introduced in Popper's work in the course of his critical reflections on Kuhn in *Criticism and the Growth of Knowledge*.[18] There he stresses, against Kuhn, that while presuppositions, frameworks and traditions play an important role in science, he does not think that rationality must be limited by them. Instead, Popper thinks that learning is possible wherever people accept the fallibility of their own ideas and understand that they might have something to learn from other people. At the same time – and this plays a major theme in his paper 'The Myth of the Framework' – Popper argues that we should not exaggerate what we will learn through such encounters. He retells the story, from Herodotus, of the confrontation engineered by King Darius between Greeks who burned their dead and another group who ate theirs. Popper stressed that while one might expect – if the people were, indeed, willing in principle to learn – they might learn something, there was no reason to suppose that they would come to consensus (consensus being understood here as tentative agreement – of the kind that he was expecting in respect of basic statements and his 'negative' agenda for government).

Popper develops a similar point in the context of a discussion of toleration (Popper 2008, chapter 37). Here he gives the striking example of discussions between Albert Einstein and Niels Bohr on metaphysical and epistemological issues relating to science. These represented exchanges from different perspectives and they did not lead to consensus. But Popper argued that each participant learned from them. If such a thing occurs – as in the case of Bohr and Einstein – at the level of metaphysical issues relating to science, one might surely expect that, while learning can take place, we should have even less reason to expect the achievement of consensus in areas where interests also play a significant role. Accordingly, the hopes of some theorists of deliberative democracy may seem overly optimistic. It is perhaps worth recalling here Popper's argument that the frustration of over-optimistic expectations about what reason may achieve can lead people to irrationalism. Popper, however, is surely correct in his emphasis on the idea that discussion may nonetheless be fruitful if we accept our fallibility and are open to the possibility of learning from others.[19] Yet, there would seem to be a potentially useful role for Popper's arguments from 'Private and

Public Values' for generating an agenda for governmental action in the face of persisting, and deep, disagreements.

3. ISSUES FROM POPPER'S TREATMENT OF POLITICS

The broad ideas reviewed in the previous section explain the more general approach that is developed in Popper's work. They serve to provide the context within which we may consider some of his more specific suggestions. There are many more such ideas than can be fully covered here, but I would like to place emphasis upon three. First, there is his resolute opposition to political utopianism. Second, there is his critique of ethical collectivism. Third, there is his approach to the understanding of democracy, in which his arguments on 'the paradoxes of democracy' and his critique of the question 'who should rule?', are particularly important.

Popper is among the most wholehearted critics of utopianism. On epistemological grounds he takes issue with the claims to knowledge that typically inform utopian projects. He also argues that social action generates unintended consequences, such that the problem situations which utopian planners address typically change during the course of their activities, in ways that may call the relevance of their utopia into question. Popper also stresses the difficulties of forming a consensus about issues of values and priorities, and our inability to resolve such disagreements rationally. In the face of this, a determination to impose a utopian plan amounts to the imposition of one's wishes on others, by force if necessary. Popper is also critical even of the idea that we should be guided by the explication of an ideal (here there is a contrast with Hayek 1967, chapter 12). As we have seen, Popper argues that a better procedure would be to spell out a more modest program, to address those problems which we can agree to be most pressing,[20] leaving issues of the good life, and the pursuit of divergent ideals, to private initiative.

Second, Popper, as I have mentioned, is a resolute ethical individualist. By this I do not mean that he favours the idea that individuals should purse their self-interest against the interests of others. Rather, he is a trenchant critic of the idea that the state, rather than its individual citizens, should be accorded ethical primacy. He also criticizes the idea that it is acceptable for individuals to be sacrificed to the achievement of particular social goals. This is not least because they may well die before the goals are realized, or the goals may become irrelevant as a consequence of changes in our circumstances. Popper here also offers a useful piece of analysis, in which he argues that proponents of collectivism, from Plato onwards, have typically been guilty of trying to pass off what is best seen as a form of collective self-interest as if it were moral, by dichotomizing the options before us as being between what they favour and individual self-interest. Popper, however, argues that we need to distinguish between self-interest and altruism at both an individual and a collective level. Here he wished to speak in favour of individual altruism and to distinguish it from collectivism. Popper's point is striking and it is also of contemporary relevance. For the sleight of hand that Popper detected among collectivists of his own day – passing off group self-interest as if it were the moral alternative to individual self-interest, and demanding that we treat what is simply group self-interest as if it were moral – is alive and well among some contemporary feminists and champions of identity politics and communitarianism.

Third, there are Popper's ideas about democracy. Here one might say that Popper took off from Leonard Nelson's problems about democracy but moved in a very different direction. Popper was in agreement with Nelson that there was something paradoxical about a view of democracy which would allow it to set any agenda, even, say, that a dictator should rule. Popper's own reaction to this idea was to argue that it was problematic to claim that a key agenda for politics was who should rule. Instead, he argued that we should substitute for it the question: 'How can we so organize political institutions that bad or incompetent rulers can be prevented from doing too much damage?' (Popper 1945, chapter 7, section I.) One could then see Popper's own ideas, which we have reviewed earlier, as constituting his attempt to provide an answer to his question.

4. CRITICISM

Popper's work on political themes is both important and interesting. In the light of the contributions made by himself and Hayek, to say nothing of interesting work by Oakeshott and within the Marxist tradition and the Frankfurt School, it seems to me ludicrous to write of political philosophy as if it was something that needed to be reinvented in the late twentieth century by John Rawls. That being said, I think that there are certain respects in which Popper's work is open to criticism, and stands in need of modification, although my discussion here can only be very brief.[21]

First, I think that his opposition to essentialism – of which he makes use to argue against the view that there are structural constraints upon what we can achieve politically – is defective. Essentialism, on Popper's account, included many different strands such as criticism of the idea that there are essences behind the use of the use of some term such as 'the state', criticism of ultimate explanation and criticism of the view that we can, epistemologically, gain certain knowledge of ultimate explanations, intuitively. All these seem to me in order. (Although there is a sense in which an action may have a meaning which can – albeit fallibly – be grasped in such a way.) But I think that Popper made the mistake of throwing out the structural baby with the essentialist bathwater. In consequence, some of the points on which, for example, he was critical of Marx for seeking explanations beneath the surface of things could be answered perfectly well in terms of Popper's own later scientific realism. This position, for which he was willing to accept the description 'modified essentialism',[22] allows for (tentative) knowledge not just of phenomena and regularities but also of structures and dispositions. It would, here, seem to me plausible – as, say, a range of political theorists have argued, from Marxists to Hayek[23] – that our actions give rise to structures which have a reality of their own, and may then serve to constrain our subsequent actions, and what we can achieve in politics. If this is the case, *some* of Popper's dismissal of 'essentialism' is too quick.

Such a 'modified essentialism' applied to the social world also offers a useful perspective from which to look at some issues raised by Hayek, which seem to me to point to some difficulties for Popper's approach to politics. For while he does not himself put the issue in such terms, one can see Hayek as having argued that a modern, market-based society has certain structural characteristics which will produce consequences that we, as citizens, may find problematic. Hayek argues that there are certain institutions, for example the price system and a system of law of a distinctive character, which we cannot do without in such a society. He also brings out the way in which such things, in their turn, typically generate consequences which we are likely to find unattractive, but which we cannot remove while these structural features are in place. This does not mean that nothing can be done. If we know what we are doing, we may be able to alleviate, rather than resolve completely, some of the problems. Alternatively, if we simply try to change these features, we may do a lot of damage without accomplishing what we wanted. For example, we may not be able to retain the benefits that we receive from membership of such a society, while at the same time getting rid of those things that we found objectionable.

To what am I referring? David Hume, in his early reflections on the kind of system of justice that was needed by a commercial society, already noted that it may result in assets being taken from those who seem deserving and given to the undeserving.[24] Hayek himself has argued that any such society is likely to feature inequalities of wealth that we may feel are unjust, and will not reward those who might seem to merit it.[25]

Now, if such points are correct, they would seem to me to pose a problem for Popper's approach and, indeed, for any political system that is strongly responsive to even a negative agenda generated politically by way of its citizens' preferences. For it is possible that citizens might wish both for the benefits of an extended, market-based society and for a society that exemplifies ideas of 'social justice'. That is, people might want to receive the benefits of living in an extended, market-based society while also demanding that everyone receive remuneration which fits their intuitions about what is just, without understanding that they cannot, in fact, have both of these things at the same time. That there may be *some* such problems is prefigured in Popper's work. Consider his various comments about 'the strains of civilization' and about the problems of life in an 'abstract society'. Popper himself thus highlighted things which people might strongly desire but which he thinks cannot be achieved within a free and open society.

Why, however, did I refer to these arguments as posing a problem for Popper's political thought? It is because while Popper recognizes that there may be such issues, it is not clear that he offers us an adequate way of dealing with them. At best, he would seem to suggest that such knowledge is something of which people need to be aware when undertaking piecemeal social reform.[26] The problem here – which the cynic might see as Plato's revenge – is that it is not clear how good, fallible, but possibly quite technical knowledge is to acquire an entrenched position within a Popperian Open Society. Clearly, if such ideas could, somehow, be constitutionally entrenched, or be safeguarded by a well-trained Supreme Court, open to arguments in the public sphere but insulated from populist political pressures, things might work very well. But that a country should have such an institution would seem a sheer matter of historical good fortune. Alternatively, it is possible that a powerful, educated elite, embedded in a culture that is open to such ideas, might equally resolve the problem. It is worth noting here that Popper's quotations from Burke and from Pericles,[27] to which I referred in the context of his ideas about 'the rational unity of mankind', also suggest that not everyone can initiate ideas about public policy. This point suggests that in principle, Popper's idea of an Open Society may not be completely averse to the idea of there being such an elite.

The problem, however, is that it is not clear why constitutional arrangements, or the views of a social elite, will in fact embrace the knowledge in question. The anti-authoritarian strand to Popper's fallibilism, and what, over time, looks like a steady (and in many ways welcome) breakdown of what one might call natural authority in Western societies,[28] may make it difficult for people with relevant knowledge to play a significant social and political role. It is striking, say, that in the United States, which of all Western countries exhibits such tendencies in the highest degree, there is, alongside a high degree of sophistication, a tendency towards moral panics. In the United States, the consequences of such idiocy are largely kept under wraps because of the strong social entrenchment of the Constitution. But populist moral panics are a powerful force, and have, for example, started to make their mark on the process through which Republican presidential candidates are selected in the United States, and there would seem every reason that it could become really dangerous in the face of some of the structural vulnerabilities of an open society.

Third, there is a respect in which Popper, perhaps one of the most resolute critics of utopianism that there has ever been, might himself be criticized as utopian. What I have in mind is as follows. Popper offered us an approach which points to certain tasks that he considers should be discharged by politics and the state. But he does not go on to discuss how (or, indeed, whether) this can in fact be accomplished. I am certainly not criticizing Popper for not having tackled this task himself. For while he wrote extensively on political themes, his time was heavily committed to a host of other pressing projects in a whole range of areas within philosophy. There is, however, a sense in which Popper's political thought (and closely related issues, such as his critique of Kuhnian 'normal science'[29]) seems to me to fall down, by virtue of the fact that it accords significant roles to politicians and public servants, which it is not clear that they do, or could, play. Let me give a few examples.

Popper sees the central concerns of a rational politics as involving the discovery of those issues upon which there is a consensus that action needs to be taken, and then the pursuit (within a framework of the protection of the individual) of various initiatives to try to address these problems, controlled by critical feedback from the population. By contrast with this, politicians typically feel that they can never admit to having made a mistake (Popper, in one of the lectures of his old age, pictures an ideal polity as being one in which politicians compete against one another on the hustings, in terms of how many of their own mistakes they have detected. This, while charming, surely should serve to convey that there is something highly problematic about Popper's view). As to politicians, one might also wonder just what role they should, in fact, have in the kind of politics that Popper has described. He has – not unreasonably – pointed to a key role being played within a democracy by the ability of an electorate to get rid of its rulers without a revolution. But a reader of Popper's work more generally might equally ask: 'Isn't his view of a good system of learning, one in which people allow their ideas to die in their stead?' It is not enough that we can get rid of politicians; we need, also, to be able to get rid of their (and public servants') bad ideas.

Now, it is striking that, in an undated note about Ralf Dahrendorf's book, *The New Liberty*,[30] Popper responded warmly to an idea that Dahrendorf described. This was an 'Office of Technology Assessment', in which social and natural scientists examined the results of government policy and compared them with their intended purposes. It is not surprising that Popper should have welcomed this idea. But in the light of these remarks, we might, fairly, comment that Popper's view of politics – the discovery of problems, the assessment of possible responses to them and then the assessment of the actual consequences of the policies – looks technical rather than political. Given, especially, that what plays a role in the generation of problems (and, presumably, agreement on what should count as negative feedback) is consensual in its character, it is not clear that politics in the usual sense has any role to play in the matter at all. While (as I suggest below) dialogue may have a role to play, it is not clear what role there is for politicians.

The problem, I think, runs deeper. For it is not clear how, in the light of literature on how it operates, the public service could function in a manner which fits Popper's ideas about what it should do.[31] At the electoral level, politicians typically speak (and have little option but to speak) to the interests and prejudices of their electorates. While interests also play a major role in the process of the actual formation of policy, much of it takes place without significant public scrutiny.[32]

What of deliberation? Although it does take place, not least in some sections of the mass media and the 'public sphere' more generally, in my view the best example of a deliberative *chamber* is probably the modern British House of Lords. The members – who are now typically appointed for life,[33] on the basis of a successful career in one or other sector of society, but who, by virtue of accepting appointment, signal the end of further political ambition – treat one another with respect. A chamber, such as the House of Lords, with people appointed to it who combine a knowledge of and a concern for particular sectors within society, but whose members are committed to deliberation rather than simply pressing particular sectional interests, seems to me to offer the best model for deliberation in politics. The offices of particular members of such a chamber might be staffed in such a way that the staff can assist members to selectively take up and develop issues that are the concern of members of the public (but which do not involve their interests – which would, as at present, be looked after by regular members of Parliament).

If one considers issues of deliberation more generally, then Habermas's *Structural Transformation of the Public Sphere* seems to me of particular importance (Habermas 1991; Calhoun 1993). Habermas went beyond the kind of call for the desirability of deliberation that one finds in Kant's 'What is Enlightenment?' to a consideration of practical issues concerning its institutional embodiment. Habermas traces the way in which deliberative institutions emerged as a by-product of commercial activity in, for example, coffee houses. He further described the way in which the otherwise desirable widening of democracy led to the emergence of a machine politics of interests, which served to limit the effectiveness of deliberation. This work, it seems to me, poses an important problem for Popper's politics, namely that it should lead us to think in institutional terms about the kinds of arrangements that would serve to realize the (important) functions to which Popper has drawn our attention.

In Shearmur (1996a), in addition to raising certain of these problems, I suggested a programmatic response to them, which is that Popper could usefully have moved closer to classical liberalism than he did. First, and most obviously, market arrangements create powerful incentives for people to admit failure, and to learn from their mistakes, of a kind that one does not readily find among politicians and public servants. (Clearly, Popper himself favoured a good measure of economic redistribution, e.g. by way of limiting 'economic exploitation'; and there could be ways of doing this – e.g. by way of a 'negative income tax' – which would limit the degree to which government itself had to operate a welfare system.[34])

Second, to the degree to which we could limit the scope of politics, by way of having activities which are currently undertaken by government operate on a commercial basis, it might make politics function better as a forum for deliberation. At present, our representatives seem to spend so much of their time giving approval to technicalities of legislation that there is little time for the discussion of issues of principle.

Third, Popper's own account places ideas about the good life, and concerns about which there is no consensus, into the private sphere. Insofar as such matters go beyond what individuals can do on their own or with friends, it implicitly calls for a realm within which such activity can take place. But Popper does not discuss this, let alone consider by what institutional means such things may occur. But there is an aspect to classical liberalism which seems to me underappreciated, despite the 'utopia' section of Nozick's *Anarchy, State and Utopia* (1974). It is relevant here for two reasons. Before turning to it, however, I should note that Popper was, in passing, critical of the ideas that I am going to discuss, because they have the features of what might be called small-scale, experimental social holism. Popper's criticism, however, seems to me to fall with the aspects of his criticism of essentialism with which I have taken issue earlier in the chapter.

What, then, is the idea that I have in mind? It is that, within a classical liberal polity, it would be possible for people, on a consensual basis, to try out, experimentally, ideas about a good society or a good life, upon the desirability of which there was not a wider social consensus. Just because of the risk of fanaticism if such people were to isolate themselves completely, a useful real-world model here would be the (open, not gated) Disney Corporation town of Celebration (see Shearmur 2002), rather than some kind of total community. Celebration was designed to try to realize a particular style of life, by means of a combination of private planning and regulation. It was (initially) administered by the company who constructed it, who were able to make adjustments if problems emerged. People chose to live there because they favoured the town's ideals; they were consulted, but did not run the town: arrangements were more like those of a resort hotel than anything political. The facilities of the town were – like any other town – open to visitors, and those living in it typically worked outside the town.

Such arrangements would seem to me to have two advantages. First, they suggest a response to some of the problems discussed earlier. For they would offer people a choice between joining or not joining a designed community, the rules of which might allow for the regular consultation of residents but not for popular political control. Residents would, of course, be free to move elsewhere. The owners would have every incentive to make sure that things functioned well, subject, however, to legal agreements that they had made with residents, and which would be adjudicated by courts outside the community in question.

Second, such proprietary communities could also – from the perspective of other people – function as experiments in living. One could thus see what it would be like if people tried living in a particular way. And, as in fact occurred in the planning of Celebration, lessons could be drawn from other cases for the design of new social experiments, or the modification of old ones. Such arrangements would thus neatly exemplify the Popperian theme of learning by trial and error, but again in a setting that combines consent and commerce rather than politics.[35]

5. CONCLUSION

All told, Popper is an interesting and important political philosopher. While his work was written in a simple and direct manner, there are insights to be found in it which are of lasting significance. Further, the interest of his work is strengthened by its interconnections with Popper's wider philosophical ideas. Attention to these, however, also serves to pose some interesting problems. These occur in relation to specific elements in his political thought and, as I have argued in another place (Shearmur 1996a), to its political thrust. They also raise wider issues concerning the scope of reason, on which Popper's views changed subsequent to his writing *The Open Society*.

In my personal view, while a number of writers have written about Popper's political thought, it seems to me that his work deserves to be treated in much greater depth. (I would include my own work in such criticism.) Malachi Haim Hacohen has shown us, in his interesting treatment of the early Popper, just how much there is to be understood when Popper's work is seen in context. This, in its turn, means that there is much work to be done in both the exploration and the critical appraisal of Popper's work. Popper's work in political philosophy – as, indeed, in so many other areas – has played a relatively marginal role in the development of the wider field of social and political philosophy since he wrote. In my view, contemporary work is the poorer for the fact that this is the case.

NOTES

1 In this chapter, I will discuss Popper's writings only insofar as they are available in English. This means that I will not consider his early work on education, despite the fact that Hacohen (2000) and Wettersten (2005) have suggested that it may be of importance for understanding certain of his early political concerns. See also Hacohen's discussion in the present volume. My reason for this is, in part, my inability to handle adequately material that is available only in German; in part, because I do not feel confident about my ability to address the problem situation of discussions of Austrian education in the interwar years.

2 See also Popper's draft of his Autobiography (Popper Archive, Hoover Institution Archive, 134.11), section VIII.

3 See Popper (1963, p. 350) and also Boyer (2005) for a particularly useful brief discussion.

4 I heard this story from H.-J. Dahms, who interviewed Popper about this matter, and also consulted records in the archives of the Nelson organization; the argument of Popper (1945, chapter 7) was apparently initially developed against Nelson. For a discussion of Popper's and Nelson's political ideas, see Dahms (2006).

5 See on this Popper's letter to Paul Branton (undated, but in response to a letter sent to Popper on 21 June 1982) in the Popper Archive, Box 263.1, General Miscellaneous Correspondence 1982.

6 See, notably, his letter to Carnap of January 6, 1947; see now Popper (2008, chapter 9).

7 He had Ernst Gombrich, who was trying to get the book published, add a note at the end indicating when the text was completed, to implicitly indicate that it had been written prior to his seeing (Hayek 1944).

8 See Popper to Colin Simkin, 12 September 1989 and 6 August 1990 (Popper Archive 563.1); in this, Popper – apropos of the Popper/Hayek correspondence in the Hoover Institution – comments that they had extensive oral

discussion on points upon which they disagreed, and that such disagreement was better handled orally. One criticism to which Popper referred was of the utopian character of Hayek's economic (as opposed to his political) ideas. Popper is also more strongly critical of Hayek in notes on his work (e.g. as held in the Popper Library at Klagenfurt) than he is in correspondence.

9 For example, in his letter to Simkin of 12 September 1989.

10 That is, in respect of his view that our ability to predict depends on initial conditions as well as on laws.

11 It is particularly clear in the lectures that he gave at Emory University in 1956. I have discussed these in Shearmur (2004); see also Shearmur (2009).

12 Which makes his treatment in some ways read oddly like that of 'vulgar Marxism'.

13 Popper, in an early revision of *The Open Society*, indicates that he is adopting the terminology of 'proposals' from a paper by L. J. Russell published in 1948 (see Popper 1963, chapter 5, n. *3), while in his Addendum I to *The Open Society*, 'Facts, Standards, and Truth: A further criticism of relativism (1961)', he goes a long way to draw parallels between his epistemology and moral theory. However, Popper indicates, in his response to a critical piece on his work by Rev. Michael Sharrett (24 October 1974 University of Klagenfurt Popper Library, Manuscripts Verschiedenes 68) that he is conscious of not having addressed these issues in any systematic way. For fuller discussion, see Shearmur (2009).

14 There is a paper in the Popper Archive, 'A Non-Psychological Justification of the Categorical Imperative' (Popper Archive 366.14), which explores something close to this line of argument. But as I discuss in Shearmur (1996a, chapter 4, n. 27), there are some reasons to question its authorship.

15 Popper himself was reluctant, say, to see it applied to theology, but this aspect of Popper's work was taken further in Bartley (1984).

16 Popper himself also emphasized the significance of a non-rational commitment to reason; but for reasons that I have offered in Shearmur (1996a), this seems to me something that can be avoided.

17 The later Habermas, it seems to me, moves very close to Popper (see on this Stokes's contribution to the present volume), or, where he fails to do so (e.g. in respect of the justificationist strand in his work), he would I think have benefitted from doing so. At the same time, his approach – and the kinds of 'deliberative democracy' which have been influenced by it – seem to me to have overlooked some issues about the limitations of critical reason, set out in Popper's work, from which they might also have benefitted; see the text following this note. (They could also, I believe, have gained from reading what Hayek had to say about the limits of rational decision making in Hayek [1944], too, but that raises different issues.)

18 See Popper (1970). They did not occur in the transcription of the remarks that Popper made at the conference; see 'Professor Popper's Contribution to Kuhn-Watkins Discussion', Popper Archive 75.5.

19 Such discussion may, obviously, include discussion about the objectivity of the procedures that we have been using, to date; compare, in this context, Popper's social theory of scientific objectivity in Popper (1945) – something that can clearly be extended to discussion in general.

20 There was a slight oddity here about Popper's both holding this view and – in other respects – standing in his *Open Society* somewhat on the political Left. He was, for example, taken to task on this point – e.g. that it would make it unclear that, say, the government should be in the business of the provision of playing fields and libraries – by the moderate Conservative, Sir Edward Boyle, in his contribution to Schilpp (1974), and Popper was happy to grant him the point. It is also interesting that, for example, in a draft of his Sonning Prize Lecture, 'For a Better World' (1973) (First version of Popper's Sonning Prize Address. University of Canterbury Archive, G. E. Roth Box 8 [Popper-related material], University of Canterbury, Christchurch, New Zealand; now in Popper 2008, chapter 32), Popper advocates the governmental funding of experimental residential schools for young people.

21 The views that I set out here are controversial – as are, indeed, aspects of the interpretation of Popper's ideas that I have offered earlier. For some contrasting treatments, see, for example, James (1980); Carey (1986); Williams (1989); Magee (1995); Stokes (1998); Salamun (1999); Hayes (2001, 2009); Boyer (2005); Sassower (2006); Parvin (2010); Benesch (2012); Naraniecki (2013). For a critique of my interpretation of Popper's politics, see Eidlin (2005) and my response (Shearmur 2005).

22 See, for example, his 'Three Views Concerning Human Knowledge', in Popper (1963); see, however, his 'Agassi on a Modified Conventionalism', in Schilpp (1974, volume II, p. 1115) for some discussion.

23 Hayek did not present his own work in such terms; but this seems to me the most fruitful way in which it may be understood.

24 For a useful discussion of these aspects of Hume's work, see Hayek (1967, chapter 7).

25 For Hayek's argument, see Hayek (1973, pp. 56, 59–60, 64). For a discussion, see Shearmur (1996a, 1996b).

26 Compare Popper's reaction to Hayek's arguments about knowledge-based problems concerning social planning in Popper (1945, volume 1, chapter 9, n. 4).

27 See Popper (1945), volume 1, the page opposite the 'Preface to the First Edition', and the beginning of chapter 1.

28 Cf. Sampson (1984) and Popper's own theme of a 'fatherless society'.

29 Which I have not discussed in this chapter, but I have discussed it extensively in Shearmur (1996a). See also, in this context, Fuller (2004).

30 Dahrendorf (1975). For Popper's note, see Universität Klagenfurt Bibliothek, Karl-Popper-Samlung, Original Manuscripts, 604 (now in Popper 2006, chapter 36).

31 My treatment here is, of necessity, over-brief for reasons of space. But a measure of the problem that a Popperian politics faces with regard to the public service is, I think, conveyed if one compares the accounts of how governments currently function in Pressman and Wildavsky (1984) and Wilson (1991), with Popper's expectations of how governments might function.

32 Cf., in this context, the literature in political science on neopluralism; for discussion and references, see Shearmur (1984).

33 Current ideas for the replacement of which by an elected body seem to me to threaten its usefulness as a deliberative body.

34 There are wider problems about the case for redistribution and also the compatibility between government-operated 'piecemeal social engineering' and individual liberty, which I will not pursue here. There is, in this area, a sense in which Popper's political thought leaves unexplored important normative questions, but this is not an issue which I can discuss further here, for reasons of space.

35 Clearly, if people were determined to set up some form of utopian political community, I would not wish to stop them; but the track record of such arrangements, when they are not religious in their character, is not, I understand, a happy one.

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Index of Names

Terms formed from the name of a person are listed under the person.

Ackermann, R., 246, 261
Adey, G., 314, 316, 347
Adler, A., 3, 40–2, 63, 352
Adorno, T., 22, 27, 301–2, 314, 316, 319–23, 345, 347–8, 350
Agassi, J., 7, 27, 40, 63, 303, 305–7, 313–15, 373
Akeroyd, F., 167–8
Albert, H., 136–7, 141, 323
Albert, M., 234, 246, 261
Almäng, J., 141
Amoroso, R., 101
Anaxagoras, 196
Anaximander, 171
Anderson, P., 348
Anderson, R., 267
Andersson, G., i, vii, ix, 7, 16–17, 125–42, 359
Anon, 62–3
Antunes, R., 101
Apel, H.-O., 302
Apelt, E., 63
Aquinas, T., 99
Aristotle, 188, 217, 284, 290, 294, 296, 314
Armstong, D., 280–2
Arndt, A., 34
Artigas, M., 304, 311, 314
Axtmann, R., 318
Ayer, A., 54, 61, 63, 242–3, 265

Bach, J., 245
Bächtold, M., 246, 261
Baert, P., 307, 310–11, 314
Bain, A., 75, 99
Bar-Hillel, Y., 254, 259, 261
Bartley, W., 12–13, 27–8, 37, 64, 67, 101–3, 135, 141, 169, 307–9, 314, 316, 346, 348, 350, 352, 372, 374–5
Bauer, O., 35, 64
Bayes; Bayesianism, 16, 112–13, 116–17, 123–4, 252, 255, 261–2, 264
Beatty, J., 234, 264
Belke, I., 33, 64
Belnap, N., 246, 262
Benesch, P, 373–4
Benhabib, S., 320, 348–9
Bennington, G., 349
Berkson, W., 15, 26–7
Berkeley, G., 90, 93, 174, 208, 213–14, 216–18, 220, 222–3, 228
Berkovitz, J., 246, 262
Bernoulli, D., 232–3
Bernstein, E., 46
Bernstein, R., 348
Blackburn, R., 349
Blumberg, A., 54, 64
Bohm, D., 194, 205
Bohman, J., 337, 348
Bohr, N., 191–2, 362
Boland, L., 288, 298, 310, 314
Boole, G., 249–52, 263
Borchert, D., 267
Borger, R., 317
Borkenau, F., 38, 64
Born, M., 188
Boscovich R., 188
Boyd, R., 109, 123
Boyer, A., 261, 371, 373–4

Boyer, J., 33, 64
Boylan, T., 267
Boyle, E., 373
Boyle, R., 201
Braddon-Mitchell, D., 282
Bradie, M., i, vii, ix, 17, 143–69
Braithwaite, R., 237, 243, 265
Branton, P., 371
Braunthal, A., 67
Bretenbach, B., 31, 64
Bricmont, J., 262
Bridgman, P., 221, 228
Brodbeck, M., 317
Brouwer, L., 226
Bühler, C., 84, 99
Bühler, K., 5, 45, 47–9, 52, 64, 68–9, 74–5, 83–4, 94, 96–7, 99
Bunge, M., 124, 265, 313, 315, 348
Burger, E., 45–6, 64
Burke, E., 300, 360, 366
Burton, J., 375
Butler, S., 162, 167–8

Cajori, F., 207
Caldwell, B., 2, 27, 313, 315, 374
Calhoun, C., 349, 368, 374
Calvin, J., 300
Campbell, D., 137, 141, 147–9, 151, 168
Campbell, K., 271, 282
Carey, G., 373–4
Carlson, G., 264
Carnap, R., 3, 46, 53–4, 56–8, 60–1, 63–4, 67, 93, 104, 106, 111, 116, 221, 228, 250, 253–7, 259, 261–2, 264, 268, 321, 353, 355, 357, 371
Carr, B., 226, 228
Catton, P., 141–2, 229
Chaitin, G., 233
Chalmers, D., 197, 205
Chaplin, C., 91
Childers, T., 232, 244, 246, 262
Church, A., 233
Churchill, W., 306
Churchland, P., 273, 282
Cioffi, F., 317
Claassen, E., 101
Clark, P., 240, 262
Clarke, S., 294, 315
Coelho, C., 101
Cohen, L., 227, 229
Cohen, R., 141, 229, 315, 376
Coletti, G., 251, 262
Cominos, M., 318
Compton, A., 76, 101
Conant, J., 132, 141
Copeland, A., 233
Copernicus, N., 215–6
Cournot, A., 242
Craig, E., 263
Creath, R., 268
Creutzfeldt, O., 169
Cronin, C., 349
Currie, G., 28, 227, 229
Cziko, G., 165, 168

D'Agostino, F., 11–12, 27
da Costa, N, 259, 262
Dahms, H.-J., 27, 41, 52, 55, 64–5, 67, 371, 374
Dahrendorf, R., 321, 348, 368, 373–4
Darius, 362
Darwin, C. ix, 15, 17, 25, 73, 76, 81, 85–6, 88–9, 99, 102, 143–55, 157–69, 197, 201, 205, 222, 281, 325–6, 346
Darwin, E., 167
Davidson, D., 138–9, 141
de Finetti, B., 251–2
De Grieff, P., 349
De Groot, A., 96, 102–3
De Staël, G., 31
Deák, I., 36, 64
Dedekind, R., 71, 96, 99
Del Castello, M., 65
Delanty, G., 310, 315
Delahaye, J.-P., 233, 262
Democritus, 201
Dennett, D., 197, 205
Descartes, R., 85, 87–8, 120, 174, 188, 201, 223, 271, 282, 294, 311
Devitt, M., 109, 123
Dewey, J., 105, 211, 346, 348
Dews, P., 348
Díez, J., 254, 262
Diogenes Laertius, 99
Disney, W., 370
Dollfuss, E., 61
Dowe, P., 267

Dreisinger, N., 64
Drouet, I., 234, 246, 262
Dryzek, J., 347–8
Duane, W., 193–4, 205
Dürr, D., 262
Duhem, P., 63, 132, 141, 219–20, 229
Duncker, K., 81–2, 99

Eagle, A., 240, 262
Earman, J., 106, 123
Eccles, J., 6–7, 20, 26, 28, 85, 98–9, 101–2, 159–61, 165, 169, 195, 197–8, 207, 269–71, 274–5, 281, 283
Eddington, A., 41–2
Eger, A., 32
Ehrenfest, P., 193–4, 205
Eichler, W., 354
Eidlin, F., 373–4, 376
Einstein, A., 3, 9, 17, 41–2, 50, 53, 61–2, 64, 91, 122, 146, 150, 171, 178, 184, 188, 200, 203, 205, 211, 218, 221, 229, 277, 325, 362
Eisler, E., 374
Eisler, H., 374
Elizabeth, Empress, 129
Eldredge, N., 164, 168
Elstein, M., 41
Elster, J., 299–300, 307, 312
Elvee, R., 102
Engels, F., 312
Epstein, P., 193–4, 205
Erdur, M., 244, 262
Euclid, 41, 53, 70, 216–20
Evans-Prichard, E., 306

Fadrus, V., 47, 64
Faraday, M., 188, 201
Farias, M., 101
Faubion, J., 348
Feigl, H., 54, 56–7, 64–5, 69, 280, 282, 317
Feller, W., 241, 262
Ferguson, A., 305
Fetzer, J., 238, 240, 246, 262
Feyerabend, P. ix, 10, 141, 192, 205, 262, 315
Feynman, R., 177–8, 204–5
Field, H., 251, 262, 277, 282
Fine, T., 232–3, 262
Firth, R., 306
Fitelson, B., 257, 260–2
Fleming, D., 65
Forber, P., 107, 123
Foucault, M., 341–4, 347–8, 350
Frank, P., 61
Frazer, J., 306–7
Freed, J., 350
Freed, L., 350
Freeman, E., 316
French, S., 259, 262
Fresnel, A.-J., 193
Freud, S., 1, 3, 40–2, 64, 299, 308, 352
Freudenthal, G., 348
Freundlieb, D., 347–8
Friedman, M., 53, 65, 268
Fries, J., 5, 55, 63, 65–6, 126, 141, 353
Frijda, N., 103
Frisby, D., 27, 314, 316, 345, 347–8
Fuchs, A., 33, 65
Fürstenberg, F., 316
Fuller, S., 301–2, 315, 373–4
Fultner, B., 349, 351

Gabbay, D., 264, 268
Galavotti, M., 232, 244, 246, 252, 261–3
Galilei, G., 188, 201, 309
Galton, F., 152, 236, 263
Geiger H., 189
Gellner, E., 309, 311, 313, 315
Gerschenkron, A., 67
Ghirardi, G., 262
Giddens, A., 346, 348
Giere, R., 112, 117, 123, 238, 263
Gillies, A., 230, 232, 243–4, 246, 252, 256, 261, 263
Glaser, E., 33, 65
Godfrey-Smith, P., i, vii, ix, 16, 104–24
Goldbach, C., 226
Goldman, A., 107, 123
Goldschmidt, R., 161
Gombrich, E., 92, 99, 355, 371
Gomperz, H., 15, 48–9, 60, 65, 68–9, 94
Good, I., 113, 123, 252
Goodman, N., 13, 27
Gorton, W., 299–301, 307, 310, 312, 315

Gosse, P., 222, 229
Gould, S., 154, 164, 168
Gregory, R., 94, 100
Grübl, R., 32
Gruender, F., 264
Guerrero, P., 244, 263
Guizot, F., 31
Guttenplan, S., 283
Gutting, G., 350

Habermas, J., viii, 22–4, 31, 301–2, 318–51, 361–2, 368–9, 372, 374
Hacohen, M., i, vii, ix, 2–3, 5, 15, 26–7, 30–69, 94, 96, 296–7, 308–9, 312, 315, 346, 349, 352–3, 356, 371, 374
Hahn, H., 54, 67
Haller, R., 54, 65
Hahlweg, K., 168
Hansen, T., 5, 15, 27–8, 30–1, 37, 52–3, 63, 65–7, 94, 102
Harré, R., 101
Hartmann, S., 268
Hájek, A., 105, 123, 244, 263
Hájek, P., 264
Hamilton, W., 203
Hautmann, H., 38, 65
Hayek, F., i, x, 2, 27, 286, 288–9, 296, 303, 305, 310–11, 313, 315, 354–6, 363–5, 371–6
Hayes, C., 373–4
Hegel, G., 75, 168, 285–6, 296, 298, 301, 303, 310, 315, 319, 340, 350, 353
Heisenberg, W., 61, 189–91, 204, 206
Held, D., 346, 349
Heller, A., 320, 345, 349
Helmholtz, H., 53
Hempel, C., 61, 104, 106, 111–12, 116–17, 123, 221, 229, 259, 263, 267
Henkin, L., 263
Henninger, J., see Popper, Josefine
Heraclitus, 171, 359
Herodotus, 362
Hertz, P., 161, 168
Herz-Kremenak, E., 102
Hesse, M., 345, 349
Hicks, J., 310
Hicks, R., 99
Hilferding, P., see Milford, P.
Hohengarten, W., 349
Holleis, E., 33, 65
Hooke, R., 201
Hooker, C., 168
Horak, R., 34, 65
Horkheimer, M., 319, 346–7, 349
Horn, A., 255, 263
Horster, D., 348
Horton, D., 261
Horwich, P., 112, 123
Hosiasson-Lindenbaum, J., 251, 263
Howie, D., 246, 263
Howson, C., 11, 28, 116, 123–4, 255, 263
Hubert, R., 35, 66
Hudson, W., 347–8
Huey, R., 161, 168
Hull, D., 104, 124
Hume, D., 4–5, 12–13, 56, 79, 90, 93, 174, 215, 288, 365, 373
Humphreys P., 244, 246, 261, 263–4, 266
Hurley, R., 348
Huxley, J., 96, 100

Ingthorsson, R., 141
Isham, C., 203, 206

Jackson, F., i, vii, 20, 123, 269–83
James, R., 373–4
James, W., 85, 100
Jarvie, I., i, vii, x, 11–12, 21–2, 27, 29, 31, 64–6, 68, 141–2, 261–2, 267, 284–317, 346, 349, 351, 374–6
Jeffrey, R., 255–7, 263–4
Jennings, H., 5, 73, 87, 97, 100
Jerne, N., 77, 81, 100
Jessop, T., 228
Joan of Arc, 360
Jørgensen, J., 70
Johansson, I., 141
Johnson, S., 214
Joja, A., 263
Joyce, J., 85, 100

Kant, I., 5, 7, 31, 46, 48, 54–5, 69–73, 85–6, 93–5, 125, 127, 147, 149, 174, 188, 208, 214–8, 220, 280, 300, 319, 321, 329, 331, 336, 338–42, 345, 347, 349–50, 352–3, 359–1

Keeley, B., 294, 315
Kelly, K., 120, 124
Kelsen, H., 68
Kepler, J., 188
Keuth, H., 230, 246, 263, 267
Keynes, J., 230, 253, 255–6, 263
Kierkegaard, S., 352
Király, B., 64
Kitcher, P., 106–7, 124
Kneale, W, 232, 263
Köhler, Eckehart, 262
Köhler, Elsa, 75, 100
Körner, S., 28, 207, 265
Kolb, F., 67
Kolmogorov, A., 233, 235, 247–8, 250, 263–4
Kotarbiński, T., 61
Kraft, J., 5, 55, 57, 63, 66–7, 69, 353–4
Kraft, V., 3, 52–4, 56, 66
Krassnigg, A., 101
Kronecker, L., 226
Külpe, O., 75
Kuéss, G., 35, 66
Kuhn, T., ix,8, 104, 118, 124, 141, 172, 206, 315, 330, 349, 362, 367, 372, 374
Kyburg, H., 246, 263–4

Ladd-Franklin, C., 212
Lagrange, J.-L., 203
Lakatos, I., i, ix–x, 10–11, 14, 28, 141, 206, 311, 315, 375
Lamarck, J.-B., 161–2, 167
Laplace, P.-S., 234
Landé, A., 193, 206
Laudan, L., 109, 124
Laurence, R., 35, 66
Lawrence, F., 349
Le Pore, E., 141
Leblanc, H., 250–1, 260, 264, 266
Lenhardt, C., 349
Leibniz, G., 188, 201, 280
Leinfellner, W., 262
Leite, A., 101
Levinson, P., 315
Lewis, D., 246–7, 260, 264
Lichtheim, G., 310, 315
Lindahl, B., 97–8, 100, 102
Lindenbaum, A., 250
Lloyd Morgan, C., 75, 100
Locke, J., 90, 174, 210, 213
Lorenz, E., 243
Lorenz, K., 73–4, 78, 96, 100
Loui, R., 264
Lube, M., 95
Luce, A., 228
Lyon, A., 246, 264
Lyotard, J.-F., 344, 349

Mach, E., 5, 33, 63, 70–1
Macdonald, G., 141–2, 229
MacKay, D., 98–100
Madden, P., 261
Magee, B., 357, 373, 375
Mandeville, B. de, 305
Mannheim, K., 98, 100
Marcuse, H., 319–20, 350
Margenau, H., 189–91, 206
Martin, M., 309, 315
Marx, K., 1, 3, 22–3, 30, 39, 41–3, 46, 55, 81, 168, 285, 291, 293, 295–6, 298–9, 301–3, 312, 319–21, 339–40, 348, 350, 352–7, 364–5, 372
Massumi, B., 349
Maus, H., 316
Maxwell, G., 262, 267
Maxwell, J., 188
Maxwell, N., i, vii, ix–x, 9–10, 13, 18, 170–207, 234, 264
Mayr, E., 86, 100, 154–5, 164, 168
McCarthy, T., 336, 349
McCurdy, C., 246, 264
McGinn, C., 282
McGuinness, B., 267
McHenry, L., 207
McIntyre, L., 309, 315
McMullin, E., 109, 124
Medawar, P., 85, 89, 100, 102
Mejer, J., 102
Mellor, H., 239–40, 264
Melnechuk, T., 100
Menger, C., 46
Menger, K., 61, 233
Merlin, F, 234, 262
Merton, R., 312, 315
Michalos, A., 255, 257, 264
Milford, K. x, 27, 29, 31, 64–6, 68, 141–2, 261–2, 267, 315, 351, 375–6
Milford, P., 46, 66
Mill, J. S., 31, 51–2, 66, 294, 299, 361

Miller, D., i, vii, x, 5–6, 9–10, 12–14, 19, 26–9, 31, 64–6, 68, 95, 134, 139, 141–2, 204, 230–68, 292, 315, 351, 375–6
Miller, R., 237, 264
Mills, S., 234, 264
Milne, P., 246, 264
Mises, L. von, 39, 68
Mises, R. von, 232, 234–7, 242, 265
Moisil, G., 263
Monod, J., 86, 100
Monroe, M., 91
Moore, G., 29, 267, 376
Morgan, C. Lloyd, see Lloyd Morgan
Morgenstern, O., 307
Moser, S., 101
Motte, A., 207
Mura, A., 259–60, 265
Musgrave, A., i, vii, x, 10–12, 14, 18–19, 28, 115, 123–4, 131, 135–9, 141, 208–29, 375

Nagel, E., 61, 232, 265
Nagel, T., 197, 207
Naraniecki, A., 318, 346, 350, 373, 375
Nash, L., 141
Nelson, L., 5, 15, 31, 48, 54–6, 63–4, 66, 69, 353–4, 364, 371, 374–5
Neurath, O., 3, 16, 39, 42, 54, 57, 61, 64, 66, 105, 126–7, 313, 321
Newton, I., 9, 41–2, 122, 154, 159, 174–5, 178, 180–1, 184, 187–8, 201, 207, 214–5, 217, 221, 240, 245, 273
Newton-Smith, W., 123–4
Nicholsen, S., 349
Nicod, J., 111
Nietzsche, F., 341
Niemann, D., 102
Niemann, H-J, 95, 102–3, 245, 265
Nisbet, H., 350
Norris, C., 347, 350
Notturno, M., 28, 102, 169, 207, 229, 316, 350, 375
Novalis, 17, 27
Nozick, R., 313–15, 369, 375

O'Gorman, P., 267
O'Hear, A., 168–9, 229, 246, 262–5, 375
O'Neill, J., 346, 350–1
Oakeshott, M., 300, 364
Occam, Ockham, W., 119, 124
Oddie, G., 27–8
Oedipus, 293, 297–8, 304
Offe, C., 320
Oster, P., 44
Outhwaite, W., 310, 316

Paley, W., 166
Papineau, D., 278, 282
Parmenides x, 99, 102, 171, 188, 207
Parusniková, Z., 141, 229, 376
Parvin, P., 373, 375
Pascal, B., 224, 229
Passmore, J., 315
Pavlov, I., 78–80, 100
Peirce, C., 105, 115, 124, 234, 237, 264
Penrose, R., 202, 207
Pericles, 360, 366
Perona, A., 315
Petersen, A., i, vii, x, 5–6, 15, 26, 69–103, 207
Petruccione, F., 262
Pettit, P., 353, 375
Pigden, C., 294, 316
Pitcher, G., 312, 316
Place, U., 197–8
Plato, 1, 18–19, 48, 55, 85, 188, 196, 198, 208–9, 225–9, 285, 290, 295–6, 298–9, 302, 312, 350, 352, 354–5, 357, 359, 363, 366
Platt, J., 107, 124
Poincaré, H., 53, 63, 65–6, 220, 287, 297
Poisson, S., 232
Polanyi, K., 312
Pollard, J., 169, 350
Popper, A., 32
Popper, D., 32, 37
Popper, Jenny, 32
Popper, Josefine, 47, 52
Popper, S., 32, 35, 43, 68
Popper-Lynkeus, J., 34, 64, 68
Porter, B., 261
Pressman, J., 373, 375
Pribram, K., 96, 103
Protagoras, 99
Pryce, M., 207
Psillos, S., 120, 123–4
Putnam, H., 334, 345, 350

Quarton, G., 100
Quine, W., 118, 124, 273, 278, 283

Rabinow, P., 348, 350
Radnitzky, G., 103, 169, 314, 345, 350
Ramsey, F., 252–4, 259
Rawls, J., 360, 364, 375
Ray, L., 345, 350–1
Rehg, W., 337, 347–50
Reichenbach, H., 61, 115, 124, 230, 232, 258, 266
Reiss, H., 349–50
Rényi, A., 251, 264, 266
Rescher, N., 168, 267
Rex, J., 348
Rhees, R., 289, 316
Rickert, H., 287
Ringer, F., 54, 68
Ritter, J., 98, 103
Rivadulla, A., 261, 263
Robinson, A., 256, 266
Rochefort-Maranda, G., 260, 266
Roeper, P., 250–1, 260, 264, 266
Rose, N., 348
Rosende, D., 261
Rosenkrantz, R., 255–6, 267
Rosenthal, J., 241, 267
Roth, G., 373
Roth, P., 310, 317
Ruben, D., 311
Runde, J., 234, 267
Rundell, J., 347–8
Russell, B., 5, 28, 139, 142, 211–12, 229, 290, 308, 316
Russell, L., 372
Ryle, G., 273, 283, 296, 316

Salamun, K., 373, 375
Salmon, M., 267
Salmon, W., 13, 28, 123–4, 178, 207, 246, 257, 267, 311, 316
Sampson, G., 373, 375
Samuelson, P., 310
Sassower, P., 373, 375
Savage, L., 252
Scheichelbauer, B., 35, 66
Schelling, F., 350
Scheu, F., 37–8, 68
Schiff, J., see Popper, Jenny
Schiff, M., 32, 68
Schiff, W., 60
Schilpp, P., 28, 101, 141, 147, 153, 168, 205–7, 229, 266–7, 308, 316–7, 350, 373, 375
Schlesinger, K., 32
Schlick, M., 3, 49, 52, 54, 56–9, 61, 68, 71, 96, 103
Schmetterer, L., 67
Schmidt, J., 349
Schmidt, R., 169
Schmitt, F., 100
Schönberg, A., 43, 374
Schopf, T., 168
Schorske, C., 32, 68
Schroeder-Heister, P., 230, 252, 261, 267
Schrödinger, E., 80, 103
Scozzafava, R., 251, 262
Segre, M., 65
Seiler, M., 68
Settle, T., 237, 267
Selz, O., 5, 29, 31, 45, 48–50, 63, 75, 96, 103, 169, 351
Shaffer, M., 207
Shakespeare, W., 88–9
Shand, J., 29, 267, 376
Sharrett, M., 372
Shaw, G., 73, 103, 360
Shearmur, J., i, vii–viii, x, 1–29, 67, 95, 139, 142, 230, 261, 267, 270, 289, 301–2, 308, 310, 312–14, 317–18, 338, 346–7, 350–76
Sherrington, C., 85, 103
Sigmund, K., 67
Silverman, P., 35, 68
Simkin, C., 307, 310, 317, 356, 371–2
Sinervo, B., 161, 168
Skinner, Q., 353, 376
Smart, J., 198, 278, 280–3
Smith, A., 305
Smith, M., 123
Soares, P., 101
Sober, E., 119, 124, 168
Socrates, 66, 196–7, 292, 299, 301, 374
Soen, B., 207
Spencer, H., 101, 153
Speidel, M., 261
Spohn, W., 250, 267
Stachel, J., 62, 67
Stadler, F., 34, 41, 48, 54, 64–5, 67–8

Stalin, J., 339
Stalnaker, R., 252, 267
Stanford, K., 107, 120, 124
Stebbins, G., 161, 169
Stewart, I., 243, 267
Stokes, G., i, vii–viii, x, 1–29, 95, 295, 301–2, 310, 312, 317–51, 361, 372–3, 376
Stone, M., 251
Stove, D., 214, 229
Streatfield, R., 168
Strevens, M., 240, 267
Strydom, P., 310, 315
Stumpf, C., 94, 103
Suárez, M., 207
Sunstein, C., 360, 376
Suppes, P., 231, 263, 266–7
Suttner, B. von, 66
Svennerling, C., 141
Swift, J., 214
Swinburne, R., 264

Tarski, A., 61, 255, 263, 334
Taylor, C., 197, 207, 315
ter Hark, M., 15, 26–7, 29, 52, 63, 65, 68, 96, 103, 147, 169, 346, 351
Thales, 301
Thompson, J., 346, 351
Thorpe, W., 86, 103
Travena, A., 207
Trotter, W., 229
Tuomela, R., 259, 267
Turner, B., 316
Turner, P., 28, 67, 350, 375
Turner, S., 310, 317

Udehn, L., 313–14, 317
Uebel, T., 311–13, 317
Urbach, P., 116, 123–4

Valdés-Villanueva, L., 264
Van Fraassen, B., 123–4, 220, 222, 229, 250–1, 263–4
Van Lambalgen, M., 233, 264
Van Reijen, W., 348
Veber, M., 207
Velupillai, K., 246, 267
Vigier, J.-P., 243, 265
Ville, J., 233, 267
von Kries, J., 253, 264
von Neumann, J., 307

Wächtershäuser, G., 86–9, 97, 103
Waismann, F., 56, 68, 253, 267
Wald, A., 233
Waldron, J., 361, 376
Wartofsky, M., 315
Watkins, J., i, 10–14, 16, 27, 29, 112, 124, 134–5, 142, 165, 169, 303, 308, 317, 372
Weber, M., 21–2, 289, 297, 299, 305, 312, 319, 322
Weisberg, M., 117, 124
Wellmer, A., 320
Westerståhl, D., 264
Wettersten, J., 15, 26–7, 29, 31, 52, 68, 310, 317, 371, 376
Whewell, W., 294
Wicksell, K., 310
Wiener, P., 141
Wildavsky, A., 373, 375
Williams, D., 373, 376
Williams, M., 234, 268
Williams, R., 284, 314
Willis, W., 169
Wilson, A., 264
Wilson, H., 345, 351
Wilson, J., 373, 376
Wisdom, J., 303, 313, 317
Wittgenstein, L., 54, 57, 94, 103, 253, 268, 312, 316
Wolpert, D., 97, 103
Woods, J., 268
Worrall, J., 28

Xenophanes, 171

Yates, M., 346, 351

Zabell, S., 255, 257, 268
Zahar, E., 11, 29
Zalta, E., 28
Zanghi, N., 262
Zecha, G., 375
Zellacher, L., 96
Zilsel, E., 41, 52–3, 68
Zörrer, F., 35, 66
Århem, P., 97–8, 100, 102

Index of Subjects

a posteriori, 72–3, 215
a priori, 11, 55, 58, 71–3, 80, 93, 149, 186, 215–17, 304, 334, 361
action, 6, 9–14, 21, 24, 36, 74, 78–9, 81, 84–5, 95, 97, 205, 285, 294, 297, 300, 304–8, 310, 313, 322–3, 332, 334–7, 359, 363, 365, 367
adaptation, 16, 73, 75–7, 79, 81, 89, 137, 149, 160, 163
alternatives to hypotheses, 13, 107, 176–7; see also simplicity
altruism, 364
amoeba, 17, 50, 146, 150, 325
analysis, analytical philosophy, 4, 171, 202, 204, 215, 241, 313, 322, 339–40
animals, ix, 5, 17, 74, 76, 78–9, 81–9, 92, 95–8, 137, 143, 146, 148–9, 151, 154–5, 161, 197, 200, 218
anthropology, 292, 306, 314
anticipation, 71, 73, 80, 87, 150
appearances, 210, 212–14, 216–18
argument, 21–2, 83, 97, 149, 196, 226, 228, 327, 329–32, 335–7, 344, 346, 358
association, 46, 48–9, 52, 73, 90, 93
atomism, atoms, 171, 176, 184, 188, 191–2, 195–8, 218–19, 225, 253, 272–4
Austria, 2–3, 30–3, 35–40, 43, 46–7, 61–2, 352–3, 356, 371
authoritarianism, 24, 44, 298, 308, 336, 339, 343, 366
autonomy, 19, 38, 48–50, 71, 88, 97–8, 226–7, 250, 294, 339, 342, 360–1
auxiliary hypotheses, 26, 130–3, 140
axiom, axiomatization, 52, 70, 123, 175, 179, 230–3, 237, 242, 246–51, 253, 260–1

background knowledge, 78–9, 113, 131–3, 258, 302
basic statements, 4, 6–7, 15–17, 57, 60, 70, 125–42, 211, 329–30, 359, 362
Bayesianism, 112–13, 116–17, 123, 252, 255, 261
behaviour, ix, 22, 49, 70–1, 73–4, 77–9, 81–2, 86–8, 97, 145, 147, 150, 156, 158, 161, 167, 205, 273–4, 288, 298, 303, 307, 322, 327
behaviourism, 197, 273, 277
belief, 4, 12–13, 35, 62, 74, 107, 114–17, 123, 135–9, 196, 198, 200, 224–5, 228, 234, 252–3, 272–4, 276–8, 299, 332, 341
biology, ix, 5–6, 15, 48–51, 69–104, 109, 122, 148–52, 154–5, 158, 161–2, 166, 171–2, 205, 221, 275, 285–6, 305, 325–8, 344, 346
boldness, 1, 3, 8–9, 160, 170, 187, 198, 202
Boolean algebra, 249–52
brain, 20, 49, 70, 85, 87–8, 93, 95–8, 146, 161, 165, 171, 198, 210, 223, 245, 270–2, 275–6, 279–82, 285
bucket theory, 90, 147–8
bureaucracy, public servants, 33, 307, 353, 367–9, 373

cabinet-making, 44, 51
capitalism, 22, 39, 302, 319–20, 339–40, 353, 356

causation, causality, 19, 139, 186, 196–7, 211, 213, 246, 270, 272, 274–6, 279–80, 306–7
causation, downward, 186, 195–6
Celebration (Disney), 370
certainty, 3, 16, 19, 42, 55, 59, 126–7, 139, 151, 189–91, 204, 209, 211–13, 215–17, 219–20, 223–5, 234, 247, 286, 290, 297, 332, 334, 344, 365
chance, 116, 232, 239–40, 242, 245–7, 260
chaos, 80, 243
children, 32–3, 41, 44–5, 74–5, 84, 93, 98, 143, 157, 201, 205, 223, 295, 352, 357
clarity, 41, 93, 172, 272, 292, 302, 323–4
closure of physical world, 195–7, 245, 275–6, 279–80
cold war, ix, 2, 30–1, 356
collectivism, 303, 363–4
common sense, x, 5, 9–10, 18, 133, 208–9, 211, 213–14, 218, 224–5, 228, 291–2, 308, 334
communication, 23, 26, 82–3, 95–7, 308–9, 323, 329–30, 332, 336–7, 340–1, 345
communism; Marxism, 2–3, 23, 30, 37–40, 42–3, 46, 55, 293, 295, 339, 352
community, 32–3, 36, 40, 44, 327–8, 333–4, 338, 370, 374
  scientific, 58, 77, 176, 185–6, 329, 332–4, 345
confirmation, 42, 105–7, 110–16, 120, 124, 176, 255–7, 292, 326, 333; see also corroboration
'conjectures and refutations', 17, 143, 147, 285, 292
consciousness, 6, 16–17, 19–20, 40, 74, 76–9, 81–2, 84–6, 88–90, 97–8, 102, 144–6, 148, 150, 156, 158, 161, 165, 167, 195–6, 200, 205, 270–1, 275, 280–2, 294, 305, 319, 326–7
consensus, 7, 24, 98, 323, 327, 329–30, 332–3, 341, 359–60, 362–3, 367–70
consequences, unintended, 23, 129, 145, 226–7, 248, 286, 288, 293, 304–6, 313–14
conservatism, 2, 31, 33, 35, 39, 45, 78, 81, 117, 119, 289, 300, 353, 356, 373
  epistemological, 117–19
conspiracy, 293–4
constructivism, 216–20
content, 6–7, 10–12, 93, 125, 135–6, 139, 145, 150, 175, 178–84, 186, 196, 198–200, 225–8, 253–6, 258–60, 278–9, 286
convention, 16–18, 57–8, 62, 65, 125, 133–5, 137, 140, 220, 223, 284–6, 310–11, 339
conventionalism, 8, 18, 53, 57, 63, 125, 133–4, 140, 220, 223, 298, 310
corroboration, 9–12, 15–16, 41, 59–60, 106, 113, 143, 176, 178, 186, 247, 254–5, 257, 306, 332
cosmology, 15, 18, 25, 41–2, 170–5, 185–7, 200–2, 205, 244–5
cosmopolitanism, ix, 3, 30, 35–6, 46, 54–5, 65, 338, 345, 347
critical rationalism, i, ix–x, 1, 12, 51, 68, 125–42, 172, 208, 223, 225, 261, 299, 301–2, 307, 319–23, 325, 328, 331, 361–2
'critical theory', see Frankfurt School
criticism, 1, 3, 10, 12, 16, 23, 42, 58, 62, 74, 77–8, 93, 127, 129–31, 135–40, 149–51, 175, 182–3, 185, 187, 211, 224–5, 289, 292–5, 299, 308, 310–11, 319, 323–5, 328–9, 332, 335–40, 342, 344, 346, 360–1

data, 69–70, 90, 93, 111, 115, 117, 119, 121–2, 150, 210–11, 214–17, 219, 274
Darwinism, 17, 85–9, 143–4, 149–67, 205, 326
  active; genetic dualism, spearhead, 81, 85–9, 98, 155–7, 161, 167, 205
decisionism, 318, 340, 358, 361
deliberation, 330, 336–8, 346, 368–9, 373

demands, 359–60
demarcation, i, 1, 4–5, 27, 52, 57–8, 60, 63, 110, 129, 170, 173–7, 186, 285–6
democracy, x, 3, 23–5, 30–1, 39, 47, 55, 61, 173, 290, 300–1, 309, 318, 320, 333, 335–9, 345–7, 354, 361–4, 367, 369
  deliberative, 24, 336–40, 345, 347, 362, 372
determinism, 19, 53, 186, 192–6, 204, 216, 240, 245
dialogue, 23–4, 62, 329, 335, 368
discovery, 4, 16, 59, 70, 77–8, 92–3, 96, 146, 150, 174, 182, 186, 196, 203, 226–7, 329, 361, 367–8
dispositions, 19, 58, 71–4, 79–80, 82, 147–50, 191, 235, 237–40, 243, 345, 365
dogmatism, 3, 16, 25, 39, 45, 56, 74–6, 78, 126–7, 133, 140, 198, 208, 210–12, 214–15, 218, 224–5, 294, 308, 321, 335, 339–40 , 343, 352
domination, 320–1, 323, 337, 340, 344
dualism, 88, 98, 161, 167, 174, 178; see also interaction

economics, 3, 34, 39, 109, 285, 291–2, 298, 300, 303–5, 310, 356
education, 3, 33–5, 37, 40, 44–7, 55, 69, 90, 101, 173, 200–1, 205, 292–3, 296, 352–3, 371, 373
elimination, eliminativism, 17, 58, 73–8, 81, 89, 96, 106–7, 122, 146, 148, 150, 155–9, 162–3, 224, 259, 273, 278, 325, 328, 361
elites, 35, 298, 307, 337, 346, 366
emancipation, 34, 320–1, 337, 340
emergence, 17, 19–20, 24, 80, 86, 89, 97, 101–2, 144–6, 150, 157–61, 165–7, 195–6, 226, 231, 244–7, 310, 314, 327
empirical, empiricism, vii, 5, 7, 13–17, 55–6, 60, 72–3, 78, 99, 107, 110, 113–14, 117, 120, 125–42, 149–50, 160, 174, 176–8, 181–5, 187, 203–4, 211, 214–15, 217, 219–20, 222, 231, 233, 235–7, 244, 254–7, 276, 284, 287, 294–5, 299, 319, 322–3, 325–7, 334, 342–5, 359, 361
empirical basis, see basic statements
Enlightenment, 10, 31–3, 36, 290, 300, 309, 319, 339, 341–2, 344
epiphenomenalism, 165, 197, 269–70, 275, 277, 280–2
epistemology, ix, 1, 5–6, 9, 15–18, 23, 25, 48, 50–1, 53, 55, 57–9, 62, 94, 106–7, 110, 114–15, 117–22, 134, 137, 144, 147–51, 158, 165, 171, 174, 200–1, 208–9, 216, 224, 301, 318, 324–8, 333–4, 339, 343–4, 346, 353, 357–63, 372
  evolutionary, ix, 17–18, 50, 59, 86, 137, 144, 147–51, 326, 346
equality, 21, 270, 337, 365
error, x, 16–17, 22, 24–5, 58, 63, 73–8, 81, 89, 92, 96, 149–50, 155–8, 162, 219, 236, 260–1, 270, 292–3, 295–6, 307–8, 325, 327–8, 332, 343–4, 353
essentialism, 290–1, 296, 298, 302, 312, 354, 364–5, 369
ethics, ix, 34, 54–5, 318–19, 323–4, 327, 331–2, 337, 339–40, 345–6, 352, 358, 361–3, 372; see also morality
evidence, 2, 13, 40, 42, 71, 89, 93, 105–8, 110–11, 113–23, 131, 145, 151, 166, 187, 220, 222, 247, 255–7, 259–60, 275–6, 286, 333
  synchronic and diachronic theories of, 114–18, 121–2
evolution, ix, 6, 15, 17–18, 45, 50, 71, 73, 76, 81–2, 84–9, 95, 109, 122, 136–7, 143–69, 195, 197, 205, 226, 234, 275, 280, 323, 325–7, 344–6
evolutionary theory, 15, 17, 76, 86, 109, 136–7, 143–4, 149–67, 197, 234, 275, 280, 325
existential statement, 15, 129, 131
existentialism, 41, 141, 352, 358, 361
expectations, 8, 21, 50, 71, 79, 81, 89–93, 97–8, 148, 287–8, 292, 301, 303, 313, 335, 362, 373

experience, 4–7, 14–17, 40–1, 45, 49–50, 53, 56, 58, 60, 62, 70–3, 84, 91, 98, 110, 114, 125–40, 144, 148, 159, 171, 196, 210–11, 213–23, 274, 280–2, 284, 287–8, 291, 329, 359
experts, 172–3, 333
explanation, 7–8, 17–18, 21–2, 49, 51, 151–2, 159–60, 162–7, 176, 181–2, 187, 194, 196–200, 205, 219, 221–3, 235, 241–3, 260–1, 271, 274, 277, 285, 287–8, 291, 293–5, 297–9, 304–6, 311–14, 320, 326, 333, 343, 357, 364–5

faces, 90–3
fallibilism, fallibility, 10, 16, 18, 23–6, 57, 125–7, 131, 133, 135–7, 139–40, 148–9, 167, 172, 208–10, 270, 305, 308–9, 319, 326–8, 332–3, 335, 339–40, 343–4, 354–5, 357, 362, 365–6
falsifiability, i, 1, 4, 9, 14–16, 20, 27, 42, 52, 57–8, 60, 131, 133, 153, 158, 162–4, 167, 170, 174, 178, 184, 186–7, 202, 233, 253–4, 298, 321
falsification, falsificationism, 16, 19, 56–60, 65, 110, 113, 120, 125, 128–31, 133, 150, 163, 172, 175–8, 186–7, 203, 298–9, 310, 320
fatalism, 303–4
forces, 53, 87–8, 95, 97–8, 159–60, 180–1, 184, 188, 240, 244–5, 253, 291, 296, 303–5
foundationalism, 57–8, 62, 105, 299, 319–20, 322, 337, 341, 345
framework, myth of, 308–9, 330, 360–2
Frankfurt School, 22, 301–2, 312, 318–20, 340, 347, 364
freedom, i, 19–21, 23–6, 36, 43, 45, 82, 88, 195–6, 245, 285, 287, 293, 320–1, 323, 329, 336, 339–41, 342, 344, 353, 356–7, 366, 370, 374
free will, 19, 205, 216
frequency, 19, 159, 204, 230–8, 241–4, 247, 260

game theory, 297–8, 304, 307
geometry, 41–2, 52–4, 56, 63, 70, 214–20, 254
God, 14, 152, 166, 210, 214, 216–17, 222–3
grue, 13, 27

habit, 15, 45, 73–5, 78, 161, 237
hierarchachy, 76, 79, 82, 148, 151, 155
  Maxwell, 181–9, 195–6, 203–4
historicism, i, 20, 151–2, 293, 295–6, 298–9, 302, 310, 312, 328, 353, 355, 357
history, ix–x, 15, 21–2, 25, 32, 47, 55, 58, 60, 86–7, 93–4, 109, 140, 147, 152, 158, 161, 164–5, 171, 184, 188, 197, 205, 210, 245, 287, 292, 296, 298–300, 302–3, 306, 308, 312–13, 319–20, 330, 336–7, 342–4, 347, 353, 355, 366
holism, 21, 25, 289, 291, 304–5, 313, 322, 355, 369
hypotheses, 8, 11–13, 15, 17, 19, 53, 56, 58–9, 69–70, 89–92, 98, 105–6, 110–13, 115–17, 123, 125, 128–32, 137–40, 152–3, 156, 158, 167, 174, 231, 233, 254–9, 288, 300, 307, 309, 326

idea-ism, 210–14, 216
idealism, 18, 146, 208, 210, 212–18, 220, 223, 225, 270
ideology, 23, 318–20, 322
identity, ix, 35–6, 85, 217, 364
  mind-body, 165, 197, 269–70, 279–82
indeterminism, 19–20, 25, 188, 193, 240, 244–5
individual, individuals, 3, 20–2, 24, 40–2, 44–5, 73, 76, 81–4, 92, 97, 145, 151, 154, 156, 160, 204, 235–6, 241, 243, 288, 290–1, 299, 301, 307, 311, 313–14, 320–2, 328–30, 335–6, 342, 345, 358–61, 367, 369, 374
individualism, 21, 23, 82, 291, 293, 301, 310, 312–14, 320–1, 353, 363–4

methodological, 21, 288–9, 291, 293, 301, 303–5, 310, 312–14, 322
individuality, 44–5, 82–3, 85, 97, 240
induction, inductivism, i, 1, 3–15, 27, 51–3, 55–7, 60, 63, 69–70, 73–4, 79, 90, 93–5, 105, 116, 162, 174, 209, 215, 218, 221, 224–5, 231, 247, 253, 256–61, 310–11, 334
pragmatic problem, 4, 6, 10–11, 14
inequality, 320, 365
initial conditions, 21, 26, 128–30, 150, 159, 192, 287, 298, 311, 372
institutions, social, 21, 26, 46, 99, 286–9, 291, 294, 299–302, 305, 308, 310, 313, 335–6, 338–9, 345–6, 354, 364–5, 369
instrumentalism, 18, 146, 208, 214, 219–21, 223, 298, 343
interactionismism, 20, 70, 85, 88, 95, 98, 145–6, 165, 186, 196, 198, 228, 269–76
intelligent design, 166–7
intersubjectivity, 16, 22, 24, 49, 58, 60, 127–8, 131–2, 151, 217, 329, 331, 335, 341, 345, 359, 361
invariance, invariants, 82, 171, 181, 188, 194–5, 202
irrationalism, 13, 22, 135–6, 140, 224, 299, 302, 306–8, 312, 330, 346, 361–2

Jews, ix, 3, 32–6, 41, 48, 54
jury, 125, 329
justification, 1, 4, 9–12, 17, 28, 56–7, 106, 124–7, 133–7, 139–40, 146, 162, 224–5, 322–3, 325, 327, 331–3, 335, 344, 346, 359, 372

knowledge, x, 5–10, 12, 15–18, 22–3, 25–7, 34, 42–3, 49–51, 55–9, 62, 69–70, 72, 75, 77–9, 84, 86, 90, 93, 99, 105, 110, 113, 126–7, 131–4, 138, 143–51, 153, 160–3, 165–7, 170–5, 177, 182–7, 196–7, 200–1, 203, 208, 210–12, 215, 227, 254, 277–8, 281, 284–6, 288–90, 292, 297–8, 306, 308–9, 318–21, 325–8, 332–5, 338–45, 352–4, 360, 362–3, 364–6, 373
growth of, x, 15, 17, 49, 75, 143, 146, 148, 150–1, 162, 166, 182–3, 185, 187, 202–3, 227, 256, 270, 290, 297, 308, 324–8, 343–4, 346

language, 37, 49, 56, 59, 83–5, 90, 95–7, 113, 126, 143–4, 145–50, 155, 161, 199, 205, 225–7, 253–5, 285–6, 288, 290, 296, 299, 323, 339–40
law, jurisprudence, 32, 34, 55, 285, 335, 337, 347, 357, 365
laws, scientific, 4, 14, 21, 45, 53, 56–7, 74, 98, 123, 151–4, 159–60, 174–6, 178–81, 184, 192–5, 197, 210, 215–16, 219–21, 232–3, 238, 241–2, 244–5, 247, 250, 254, 285, 287–8, 296, 299, 310, 313, 372
learning, 5–6, 15–16, 24, 45–8, 72–4, 76–9, 95, 116, 125, 128, 132, 147, 150, 160, 172–3, 175, 202, 210, 292–3, 302, 309, 327–8, 332, 341, 344, 353, 355, 358, 360–2, 367, 369–70
liberalism, ix, 2–3, 22–5, 28, 30–3, 35, 39, 43, 54, 300, 302, 320–1, 323–4, 336–40, 347, 356, 360, 369–70
liberty, see freedom
life, 17, 19, 45, 50, 70, 75, 78–81, 84–8, 98, 122, 143–5, 150, 152–4, 158–9, 163–5, 195–6, 200, 205, 231, 244–5
logic, 1, 4–5, 16, 22, 47–52, 54, 69–73, 75, 86, 93–6, 104, 111, 113–14, 128–31, 146, 149, 162, 247–8, 250–3, 255, 257–8, 301, 304–5, 319, 321–2, 324–5, 329–30, 346
logical positivism, 1, 30, 54, 63–5, 313, 321, 324
London School of Economics, i, x, 10, 30, 287, 295, 300, 303, 311

magic, 300, 306–7
making and matching, 92, 99, 275
markets, 2, 25, 43, 297, 305, 357, 365–6, 369
materialism, 20, 165, 269–71, 273–9, 281–2, 319–20

mathematics, 10, 37, 41, 43, 52–4, 61, 70, 105, 109, 120, 172, 201, 214–16, 218, 220, 226–8, 231–2, 241, 248, 261, 272, 277
mechanics, 41–2, 80, 97, 154, 159–60, 214, 240, 244, 285
memorization, memory, 44–6, 73, 78, 90, 92–3, 99, 223, 275
metaphysics, 1, 4, 18–20, 25, 42, 56–9, 108, 143–4, 152–5, 159–62, 164–6, 170–1, 173–7, 181–9, 195–6, 201–4, 208–30, 235, 240, 245, 269, 271–2, 286–7, 291–3, 298, 311, 318–19, 324, 333, 343–4, 357–9, 362; see also research programmes
method, unity of, 20, 50, 285, 297–8, 323–4, 344
methodological rule, 8, 16, 18–19, 185–6, 203, 233, 291–2, 304, 329, 358
methodology, 3–4, 8–9, 16–24, 27, 33, 39, 48–53, 55, 58, 60, 62–3, 69–77, 81–2, 93–4, 99, 127, 137, 143, 147–51, 158–61, 167, 171–2, 174–6, 178, 184–8, 201–3, 209, 230, 232–3, 240, 284–325, 328–9, 331–3, 335–9, 342–4, 355, 357–8, 361
mind, ix, 1, 17–18, 20, 46, 48–9, 55, 63, 70, 84–8, 90, 94–5, 97–8, 102, 113–14, 147, 161, 165, 171, 186, 188, 196, 210, 212–14, 228, 269–83, 285
models, 17, 21–2, 53, 56, 88–9, 96, 107, 109, 117, 119–20, 123, 143, 147–9, 158, 162, 166, 288, 291, 297–300, 303–4, 306, 308, 313, 322, 325, 338, 368, 370
morality, 19, 21, 23, 25, 38, 55, 84–5, 166, 289–91, 301, 303–4, 308–9, 311, 325–6, 330–2, 337–8, 345, 353, 358, 360–1, 363–4, 372; see also ethics
music, 32, 43, 79, 93, 288
myth, 188–9, 271, 273, 297, 313

norm, normative, 15, 19, 23–6, 147, 149, 161, 165, 285, 288, 307, 320, 322, 327–9, 331–2, 334, 336–8, 340, 343–5, 374
national socialism, 1, 3, 331
nationality, nationalism, 34–7, 46, 55, 296, 338–9
nature and convention, 284–6, 310–11
natural philosophy, 18, 170–207; see also cosmology
natural selection, ix, 16, 73, 76–7, 80–1, 89, 143–4, 146–7, 150, 153–6, 159–64, 166, 325, 346
naturalism, 17, 137, 161, 166–7, 284–5, 296–7, 310, 312, 326–7, 344, 346, 354
neo-Darwinism, 76, 144, 154–9, 165
New Zealand, x, 2, 30, 44, 62, 75, 290, 307, 331, 373
nominalism, 21, 208, 290–1, 303, 312
noumena, 215–17
numbers, 18, 83, 96, 145, 208, 226–7, 232, 236, 241–2, 248, 277–8

objectivity, 16–19, 22, 49, 58, 60, 62, 71, 84, 86, 95, 116, 125, 127–8, 134, 144–9, 156, 158, 167, 200, 217, 225–8, 234, 236, 241, 243, 247, 252, 285–6, 289, 299, 322, 327, 329, 331, 333–6, 339, 341, 372
observation, observability, 3, 5, 7, 16–18, 41, 55–6, 58, 69, 73, 90, 93, 98, 105–8, 110–15, 118–21, 123, 125–6, 129–36, 138–40, 149, 152, 171, 188–91, 194–5, 204, 208–9, 211, 218–22, 233, 237, 244, 257, 273–4, 288, 299, 313, 323, 325, 359
Oedipus effect, 293, 297–8, 304, 312
ontology, 21, 274, 278, 287–8, 290–1, 304, 313–14; see also 'world 1', 'world 2', 'world 3'
open society, i, x, 23–6, 31, 36–7, 321, 336, 338, 345, 366–7; see also Popper, *Open Society*
optimism, 3, 24, 34, 45, 108–9, 116, 123, 304, 341, 344, 362
organisms, 71, 73–6, 78–82, 85, 87–90, 92, 95, 97, 137, 144–50, 153–61, 163, 165, 325

panpsychism, 197, 269, 280–2
paradigm, 8, 286, 330
particularism, 16, 108
perception, 49, 70, 72, 76, 79, 86, 90–2, 95–6, 127, 134–9, 174, 209–12, 214, 216–17, 271, 275, 281
personality, 81–2, 85, 97
phenomena, 8, 16, 18, 21, 69–71, 77, 81–2, 93, 95, 166–7, 171, 174–6, 179–84, 193–5, 202–3, 216–17, 219, 221–3, 227, 239, 271, 275, 280–1, 285, 303, 305, 310, 365
philosophy, i, vii, ix–x, 1–4, 16–18, 20, 22–3, 25, 30–1, 34–6, 39, 41–2, 44–6, 48–62, 69–70, 75, 79, 104–5, 107–9, 111, 115–19, 121, 126–7, 138, 140, 153, 160, 162, 165–6, 170–7, 184, 186–7, 195, 198, 200–2, 205, 210–14, 219–22, 224–5, 235, 241, 247–8, 261, 269–70, 274, 279, 281–2, 284–6, 290, 293, 295, 298–9, 303, 309–11, 313, 318–326, 327–9, 334, 336, 339–44, 352, 359–63
  natural, see cosmology
physicalism, 49, 182–4, 186, 195–8, 208
physics, 3, 7, 21, 41–2, 49, 51–3, 59–60, 70, 90, 109, 122, 170–95, 201, 203–4, 211, 219–21, 230, 233–6, 240, 244, 284–5, 287, 295, 326
piecemeal reform, 21, 23, 323, 340, 353, 355, 365–6, 374
plan, planning, 33–5, 39, 84–5, 97, 285, 305, 311, 354–6, 363, 370, 373
politics, i, vii–x, 1–3, 19, 20–26, 30–46, 50–51, 54–5, 58, 62, 172, 269, 287, 289, 296–8, 306–7, 310, 318–76
politicians, 24, 46, 297, 306–7, 314, 366–9
Popper,
  Archives, 2, 15, 31, 52, 291, 296, 331, 372
  Autobiography; *Unended Quest*, 2, 4, 31, 39, 42, 51, 63, 73, 78, 80, 158, 162, 312, 352–3, 357
  Conjectures and Refutations, 63, 149, 178–9, 230
  Die beiden Grundprobleme; *The Two Fundamental Problems*, 5, 7, 14, 51, 54, 56–7, 63, 70, 74, 85, 95
  'Facts, Standards and Truth', 308, 346, 372
  'Logic of the Social Sciences', 300–1, 319
  Logik der Forschung; *Logic of Scientific Discovery*, 1–2, 4, 7–9, 15–17, 28, 30–1, 50, 59–63, 65, 70, 75, 125, 128, 130, 134, 143, 147, 162, 174, 178, 189, 230, 293–5, 299, 310–11, 329, 346, 353, 357–9, 361
  Myth of the Framework, 308, 361–2
  'Natural Selection and the Emergence of Mind', 86, 164
  Objective Knowledge, 93, 144, 147, 358
  Open Society, 1–2, 23, 30, 37, 39, 43, 47, 50, 58, 62, 90, 287, 289–90, 293–6, 298–9, 301–3, 308–10, 320, 330, 346, 352–61, 371–3
  Postscript, 2, 63, 230, 234, 245, 358
  Poverty of Historicism, 39, 43, 50, 152, 162, 289–90, 294–6, 298, 303, 309–11, 320, 355–6
  'Prediction and Prophecy', 300, 356
  Philosophy of Karl Popper, 4, 10
  The Self and Its Brain, 7, 78, 85, 161, 165
  theses, 26, 41–2, 45, 48, 50, 52–4, 56, 63, 70, 74
populism, 24, 39, 365–7
positivism, 1, 18, 22, 30–1, 49, 54, 56, 58–9, 70, 76, 94, 124, 208, 313, 321–2, 324
Positivismusstreit, 22–3, 302, 318–21, 318–24, 344
postmodernism, 22, 309, 341–4
poststructuralism, 22, 341–4
power, 20, 145, 195, 287, 290, 332–4, 338, 342–3
pragmatism, ix, 327, 334–5, 340, 346

prediction, 4, 13, 15, 18, 20–2, 39, 47, 56, 93, 109–10, 116–17, 128, 130, 163–4, 171, 176, 191–2, 221–3, 232, 287, 296–8, 300–1, 356, 372
probability, 11, 13–14, 19, 59–61, 86, 105, 107, 116–17, 120–1, 123, 143, 186, 191–6, 204, 221, 230–68; see also axiom; axiomatization
  frequency interpretation, 19, 159, 204, 230–8, 241–4, 247, 260
  logical interpretation, 230–1, 247, 252–60
  propensity interpretation, 19–20, 28, 189, 191–4, 204, 230–1, 234–47, 260–1; see also propensity
  single case, 19, 235, 238–44
problems, 4, 8, 15–18, 23–4, 50–3, 56–7, 60, 70, 74–82, 86, 89, 96–7, 144, 146, 151, 155–8, 166–7, 170–5, 200–2, 208, 226–7, 260–1, 295, 299, 310, 318, 321, 324–33, 338–9, 345–6, 361, 363, 366–8
  practical, 13, 20–1, 292–3, 295–6, 347
problem-solving, 15–17, 23, 50, 70, 74–82, 86, 89, 95, 146, 155–6, 256, 295, 318, 321, 325–9, 333, 335, 338, 344–5
  schema, 76–7, 95, 156–8, 328
progress, 9, 22, 26, 34, 42, 50–1, 55, 172, 174, 182–5, 187, 202, 256, 309, 322–4, 328–9, 336, 339, 341, 343–6, 358
progressivism, 3, 31–6, 39, 44, 48, 58, 62, 296
propensity, 19, 28, 189, 191–6, 204, 234–5, 238–47, 327 see also probability
prophecy, 20, 22, 30, 41, 297–8, 300, 312, 356
propositions, 3, 18, 41–2, 55, 136, 198–9, 200, 208, 215, 219, 227, 273, 278–9, 332
'protectionism' (Popper), 353–4, 361
Protestantism, 3, 32
psychoanalysis, 3, 40–2, 94, 293, 299, 303; see also Freud
psychologism, 50, 60, 94, 125–7, 134, 139–40, 294, 299, 301, 303–4, 313
psychology, x, 3–7, 9, 15–16, 26, 40–2, 45, 47–52, 55, 69–103, 113–14, 134, 211, 221, 271, 274, 285, 294, 296, 299–300, 312–13
public policy, 24, 359–60, 362, 366
pubic service, see bureaucracy
public sphere, 23–4, 31, 331, 336–7, 341, 345, 347, 366, 368–9

quantum theory, 59–61, 171, 184, 187–95, 204, 230, 234, 241

randomness, 149, 151, 163–4, 232–3, 237, 242
rationality, x, 19–20, 22, 25, 31, 53, 62, 94–5, 136, 139, 166, 173, 177, 185, 202, 224, 271, 284, 293, 297–301, 305–9, 318–19, 322, 328–9, 331, 337, 339, 345, 352, 357–60, 362
  principle, 94–5, 297–8, 300, 306
ravens problem, 111–13
realism, x, 8, 16, 18, 25–6, 107–9, 120–3, 145–6, 148, 171, 179, 187, 191–5, 204, 208–29, 279, 291, 308, 312, 327, 333–5, 346, 357–8, 365
  common-sense, 206–9, 213, 218, 224–5
  critical, 18, 210–11, 216, 223, 225
  micro-realism, 193–5
  Platonic, 206–9, 225–8, 312
reason, reasons I, 4, 12–14, 17, 19, 31, 51, 55, 59, 84, 93–4, 134–7, 139–40, 146, 154, 160, 174, 196–7, 224–5, 318–9, 322, 330, 332, 335, 338–9, 340–2, 345–6, 357–8, 371–2
reduction, 49, 196–8, 204, 221–2, 241, 278, 285, 314
reform, 3, 23, 30–1, 33–5, 39–41, 43–7, 54–5, 59, 247, 290, 296, 305, 309, 321, 323, 340, 353–6, 366

refutation, 6, 42, 50, 80, 92, 149, 187, 202, 216, 220, 242, 287, 292, 295, 298
relativism, 18, 22, 62, 127, 140, 217–18, 308–9, 324, 333, 339, 343, 372
relativity, 3, 41, 53, 171, 184, 243, 270, 276–8, 280
religion, ix, 21, 25–6, 32–3, 35–6, 55, 172, 216, 222–3, 289, 339, 354, 372, 374
republicanism, 23–5, 337, 353, 361
research programmes, 8, 10, 143–4, 152–4, 159–67, 185–9, 193, 202–5
revolution, 89
  intellectual, ix, 3, 41–2, 51–9, 62–3, 78, 81, 166, 215–16, 221, 292–3, 323
  political, 24, 31, 34, 36–9, 42–3, 299–300, 319–20, 323, 367
rights, 338, 347
Romanticism, 27, 55

scepticism, x, 10, 16, 18–19, 105–9, 120–2, 133, 136, 140, 208, 210–14, 218–20, 222–5, 252–3, 289–90, 308, 324, 333, 339
science (natural), ix–x, 1, 3–11, 13, 15–23, 26, 31, 34–7, 41–3, 49–53, 56–8, 62, 69–70, 77, 82, 90, 93–4, 104–10, 117–19, 122–3, 125–7, 129, 131–2, 140, 144–5, 149–50, 166, 170–211, 213–16, 218–25, 230, 233–4, 244–5, 254–5, 261, 271, 277–8, 280, 284–7, 289–91, 293–6, 298–9, 308, 310–11, 321, 324–30, 332–3, 336, 340, 345–6, 352, 362, 367
  aim of, 8, 11, 13, 123, 209, 219, 254, 292, 296–7, 357–8
  philosophy of, i, vii, ix–x, 1, 10–11, 16–18, 27, 30–1, 39, 45, 69, 74, 104–24, 140, 170–5, 177–8, 219–20, 261, 269, 274, 284, 301, 319–20
searchlight, 90, 95, 98, 147–8, 311
self, 70, 82–5, 97, 100, 161, 271
self-interest, 363–4
semantics, probabilistic, 248, 251–2
semasiology (Gomperz), 48–51, 94–5
sense-data, 90, 210–11, 215–17
senses, 90, 92, 98–9, 209–11, 214, 216, 333
simplicity, 9, 21, 41, 53, 60, 63, 119–20, 131, 172, 175–88, 192, 202–3, 219–20, 289
situational analysis, logic, 22, 86, 162, 234, 299–301, 304–6, 312–14, 324
social sciences, i, vii, 1, 20–3, 35, 37, 39, 50, 171, 234, 284–324, 343, 355, 357
social democracy, 2, 23–4, 47, 309, 321, 339–40, 346, 352–3
social engineering, 21, 23, 39, 289–90, 355, 374
socialism, socialists, 3, 24, 30, 33–5, 37–41, 43–4, 46–7, 55, 61, 290, 319, 321, 339–40, 352–3, 355, 357, 360
sociology, 21, 23, 26, 41, 51, 93–4, 287, 294, 298, 300, 303, 311, 319–20, 331, 336, 342–4
solipsism, 212, 214, 217
specialization, 171–3, 343
state (political), 2, 21, 23–5, 33, 35, 290, 303, 321, 336–8, 353, 356–7, 363–4, 367
statistics, 240–3, 252
subjectivism, 55, 61, 69, 94, 125, 139–40, 204, 230, 234, 236, 247, 252, 256, 261, 339
suffering, 24, 85, 173, 347, 352, 357, 360
support (evidence), 16, 40, 105–6, 111–15, 118, 257–60 see also confirmation
survival, 1, 16–17, 80, 87, 106, 115, 136, 138, 143–4, 148–50, 153–4, 157–9, 163, 165, 186, 326, 338, 345–6
swamp, 17, 105, 332
systems of theories, 14, 128–31

technology, 21, 34, 39, 77, 199, 228, 270, 292, 304, 368
television, 357
tests, testability, testing, 5–9, 13, 15–18, 21, 25, 42, 53, 56–60, 69,

71–2, 78, 85, 89, 90–2, 95–6, 98, 105–6, 108, 110–17, 119, 125–43, 146, 148, 150, 152–5, 158, 162–4, 167, 170, 176, 178, 185–8, 191, 203–4, 235, 238, 240–2, 244, 252, 256, 258, 270, 287, 293, 295, 300, 303, 305, 309, 313–14, 324–6, 329, 333, 341, 359
time, 27, 73, 80, 83–4, 86–7, 115–16, 120–1, 129, 131, 136, 138, 144, 154, 164, 171, 179, 180–2, 184, 194, 208, 210, 217, 225, 227, 238, 243–4, 270, 278, 311
toleration, 336, 344, 361–2
totalitarianism, 46, 295–6, 298, 302
tradition, 18, 21, 23, 25, 36, 45–6, 54, 59, 62, 77–8, 81, 109, 173, 288, 290–1, 294, 300–1, 305, 308–9, 319, 324, 326, 329, 334, 336, 339–41, 343–4, 362
transcendental argument, 55, 302, 308, 322–3, 325–6, 331
transference, principle of, 5, 49–51, 93–5
trends, 20, 151–3, 165, 296, 298
trial and error, 16–17, 25, 63, 73–7, 78, 81, 89, 96, 149–50, 155–6, 158, 162, 293, 325, 328, 344, 353, 370
truth, 8, 18, 21, 26, 31, 62, 72, 83, 93, 109, 115, 117, 120, 128, 134, 136, 138–9, 173, 185, 208–9, 215, 219–24, 226–8, 247–8, 257–9, 289, 292, 301, 307–8, 311, 325, 327, 329–30, 332–5, 342–5, 357, 361, 372

unconscious, 40, 78–9, 97–8, 275, 281, 327
underdetermination, 117, 120–2
universals, 51, 126
universalism, post-metaphysical, viii, 22, 318–51
utilitarianism, 360
  negative, 24, 347, 359
utopia, utopian, 23, 31–5, 302, 320, 355–6, 360, 363, 367, 369, 372, 374

values, 3, 21, 23–5, 35, 83, 146, 166, 197, 235, 288–9, 298, 301, 318, 325–6, 332, 335–6, 338–9, 344–5, 360, 363
verification, 11, 52, 56–7, 125–6, 129, 140, 176–7, 220–1, 233, 257, 261
verisimilitude, 10, 26–7, 109
Vienna, vii, ix, 1, 3, 5, 30–68, 70, 101, 129, 321
Vienna Circle, 1, 3, 5, 30–1, 41, 49, 53–4, 56, 59–60, 62, 125–7, 129, 138, 140, 220–2, 321
Vienna Pedagogical Institute, 44–5, 47–8, 69
violence, 3, 20, 38, 47, 288, 295, 321, 331, 335, 337, 339, 345, 357

welfare, 33, 39, 290, 337, 369
'world 1', 17, 20, 84, 90, 143–6, 159–60, 165, 196, 227–8, 269, 271–6, 278–80, 286
'world 2', 20, 82, 144–6, 159, 165–6, 196, 227–8, 269, 271–6, 280, 286; see also consciousness, psychology
'world 3', 17, 19, 49, 84–5, 144–6, 159–60, 166, 196, 198–9, 209, 225–8, 269, 271–80, 286
World War I, 3, 33, 36, 38–9, 42–3, 54, 64, 173, 352
World War II, 25, 30, 235, 320, 355–6
Würzburg School, 7, 48, 52, 75

zombie argument, 281–2