

272

**BOSTON STUDIES IN  
THE PHILOSOPHY OF SCIENCE**

# Rethinking Popper

Edited by  
Zusanna Parusniková  
Robert S. Cohen



**Springer**

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# BOSTON STUDIES IN THE PHILOSOPHY OF SCIENCE

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# RETHINKING POPPER

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# Contents

<b>Introduction .....</b>	<b>1</b>
<b>Part I Reason, Logic, Science</b>	
<b>Experience and Perceptual Belief .....</b>	<b>5</b>
Alan Musgrave	
<b>Critical Rationalism and the Principle of Sufficient Reason .....</b>	<b>21</b>
Gunnar Andersson	
<b><i>Ratio Negativa</i> – The Popperian Challenge.....</b>	<b>31</b>
Zuzana Parusniková	
<b>“Why, and to What Extent, May a False Hypothesis Yield the Truth?” .....</b>	<b>47</b>
Stefano Gattei	
<b>Proof Versus Sound Inference.....</b>	<b>63</b>
Nimrod Bar-Am	
<b>A Problem for Popper’s Fallibilism .....</b>	<b>71</b>
Ladislav Kvasz and Eugen Zeleňák	
<b>Why Advocate Pancritical Rationalism? .....</b>	<b>81</b>
Darrell P. Rowbottom and Otávio Bueno	
<b>Karl Popper and Hans Albert – The Broad Scope of Critical Rationalism .....</b>	<b>91</b>
Vladimír Zeman	

## **Part II Epistemology, Methodology, Evolution**

<b>Problem-Solving and the Problem of Induction .....</b>	<b>103</b>
Donald Gillies	

<b>Popper's Fundamental Misdiagnosis of the Scientific Defects of Freudian Psychoanalysis .....</b>	<b>117</b>
Adolf Grünbaum	

<b>Popper on Refutability: Some Philosophical and Historical Questions .....</b>	<b>135</b>
Diego L. Rosende	

<b>Popper's Thesis of the Unity of Scientific Method: Method Versus Techniques.....</b>	<b>155</b>
Carlos Verdugo	

<b>Popper's Analysis of the Problems of Induction and Demarcation and Mises' Justification of the Theoretical Social Sciences.....</b>	<b>161</b>
Natsuka Tokumaru	

<b>Popper's Theory of the Searchlight: A Historical Assessment of Its Significance .....</b>	<b>175</b>
Michel ter Hark	

<b>From Group Selection to Ecological Niches: Popper's Rethinking of Evolution in the Light of Hayek's Theory of Culture.....</b>	<b>185</b>
Jack Birner	

## **Part III Society, Politics**

<b>Popperian Individualism Today.....</b>	<b>205</b>
Anthony O'Hear	

<b>Popper's Continuing Relevance.....</b>	<b>217</b>
Ian Jarvie	

<b>Open Society and the European Union.....</b>	<b>237</b>
Miloslav Bednář	

<b>Open Rationality: Making Guesses About Nature, Society and Justice .....</b>	<b>245</b>
Alain Boyer	

<b>Logic and <i>The Open Society</i>: Revising the Place of Tarski's Theory of Truth Within Popper's Political Philosophy .....</b>	<b>257</b>
Alexander J. Naraniecki	

<b>Popper and Communitarianism: Justification and Criticism of Moral Standards</b> .....	273
Harald Stelzer	
<b>Popper's Communitarianism</b> .....	287
Jeff Kochan	
<b>Re-examination of Popper's Portrayal of Socrates</b> .....	305
Herzl Baruch	
<b>Part IV Ethics, Economics, Education</b>	
<b>The Moral Underpinnings of Popper's Philosophy</b> .....	323
Noretta Koertge	
<b>Critical Rationalism and Ethics</b> .....	339
Jeremy Shearmur	
<b>Popper's Insights into the State of Economics</b> .....	357
Joseph Agassi	
<b>Popper and Sen on Rationality and Economics: Two (Independent) Wrong Turns Can Be Remedied with the Same Program</b> .....	369
John Wettersten	
<b>Popperian Selectionism and Its Implications for Education, or 'What To Do About the Myth of Learning by Instruction from Without?'</b> .....	379
Joanna Swann	
<b>Applying Popperian Didactics</b> .....	389
Michael Segre	
<b>The Difficulties with Popper's Nontraditional Conception of Metaphysics</b> .....	397
Musa Akrami	
<b>Out of Error: Further Essays on Critical Rationalism</b>	
By David Miller	
A Review by Zuzana Parusniková .....	417
<b>Index</b> .....	425



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# Introduction

Karl Popper was a radically influential philosopher in the past century, ever questioning, stimulating, inventive, rational and humane, always curious, always critical. Almost every aspect of human existence was within his range, in principle and recurrently in practice, and open to his sharp and unrelenting questioning. Epistemology, logic, political economy and social theory, the history of ideas, scientific method, the relation between body and the mind, on and on. His *Logic of Scientific Discovery* challenged the dominating views on method, logic, and science developed by the Vienna Circle. His stunning *Open Society and Its Enemies* in mid-century set a standard for decades of critical admiration, for enthusiasm or rejection. He was a thoughtful gadfly to all.

The significance of Popper's thought, its diversity and its impact on a wide range of intellectual themes, provided the primary stimulus for putting together this book. The principal aim is not to approach Popper's philosophy as a museum piece in a retrospective way but to emphasize its relevance in today's world and to show that it is a source of lively debate within contemporary philosophical discourse.

Popper visited Prague in 1994, and talked with Vaclav Havel. With admiration for the philosopher whom he called "a wise old man", Havel said Popper led the inquiry "why it was so difficult for the idea of an open society to prevail against wave after wave of tribalism". The plain relevance of Popper's inquiries for tribalisms of the twenty-first century, together with the tasks of extending, perhaps revising, the central roles of affirmation and negation in Popperian studies, led to the convening of an international conference in Prague in September 2007.

The response was immense and more than 90 participants from 31 countries attended the meeting. Wide as the international interest shown here is, we can draw attention also to vigorous Popperian studies in other lands: Brazil, China, Greece, India, Korea, Mexico, Pakistan, Russia, Union of South Africa, Taiwan, and Turkey. Several talks return to traditional Popperian issues in the theory of knowledge, such as the critique of induction, the non-justificationist understanding of rationality and scientific method, the demarcation controversy, and the growth of knowledge. Going further afield, other papers reflect Popper's remarkable influence in education, economics, management, engineering, jurisprudence, psychology, physics, biology, and ethics. Ten of the 11 invited speakers have contributed their papers to this volume. (To our regret, copyright restriction prevents our including

the contribution of David Miller, but we have added Zuzana Parusniková's careful review of his recent book.)

The papers in this book are divided into four parts. Part I deals with general questions of rationality, logic, and science. Part II deals with central issues in epistemology and methodology and their bearing on Popper's conception of evolution. Inevitably, the topics in these two parts overlap to some extent since his key concept of the imperative of falsification (criticism) is formative for both reason and scientific method. Part III focuses on his social philosophy and looks at the inspiration that the principles of the open society, methodological individualism, and piecemeal social engineering can offer the world today. Finally, papers in Part IV apply Popper's ideas in other fields of study such as ethics, economics, and education.

We send this book out to the world of scientists and scholars with the hope its contents will lead to more informed criticism of Popper's ideas, and thereby enhance their public recognition.

Zuzana Parusniková  
Robert S. Cohen

# Experience and Perceptual Belief

Alan Musgrave

**Abstract** Are perceptual experiences reasons for perceptual beliefs? The act/content ambiguity of the term ‘belief’ carries over to this question. I argue, following Popper, that experiences are reasons as well as causes for belief-acts, but not for belief-contents. This involves rejecting justificationism, the mistaken view that a reason for a belief-act must be a reason for its content. Popper’s many critics presuppose justificationism, and so miss the main point of his solution to the problem of the empirical basis of science.

## Introduction

My nose itches, and I scratch it. The itch causes the scratching (or helps to cause it). The itch is also a reason for the scratching (or part of the reason). In cases like this, we are happy with the thought that causes of actions are reasons for them, and reasons for actions causes of them. The experience (the itch) is both a cause and a reason for the action (the scratching).

I see a tree, and I form the belief that there is a tree in front of me. The tree-experience *causes* the belief (or helps to cause it). Is the tree-experience also a *reason* for the belief (or part of the reason)? The two cases seem symmetrical. Yet, most philosophers treat them differently. While most philosophers are happy with the thought that the itch is both a cause and a reason for the scratching, they are unhappy with the thought that the tree-experience is both a cause and a reason for the tree-belief. Why the asymmetry?

Part of the reason for the asymmetry is the notorious ambiguity of the term ‘belief’, between the act of believing and the content of the belief. The content of a belief, what is believed, is a statement or proposition. Logic tells us what reasons for statements or propositions are. A conclusive reason for a proposition is another proposition that deductively entails it. An inconclusive reason for a proposition is another proposition that inductively entails it. Only another proposition can be reason, either

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a conclusive or an inconclusive reason, for a proposition. But perceptual experiences are not propositions, any more than itches or tickles are. Perceptual experiences cannot be the premises of arguments, whether deductive or inductive arguments. So, perceptual experiences cannot be reasons for perceptual beliefs. My tree-experience cannot be a reason for my belief that there is a tree in front of me.

But wait! We have forgotten the act/content ambiguity of the term ‘belief’. The foregoing only establishes that perceptual experiences cannot be *logical* reasons for the *contents* of perceptual beliefs. It does not establish that experiences cannot be reasons, as well as causes, for perceptual belief-acts. Why cannot a tree-experience both cause and be a reason for the act of forming the belief or coming to believe, that there is a tree in front of me?

Here, a hidden assumption exerts a malign influence on the epistemology of perception, as it does elsewhere in epistemology. The assumption is that a reason for believing something must also be a reason for what is believed. (Furthermore, it is assumed that a *good* reason for believing something must also be a *good* reason in the logical sense for what is believed.) I call this assumption *justificationism*, for want of a better name. As we have seen, an experience cannot be a logical reason for a belief-content. Given justificationism, it will follow that an experience cannot be a reason for a belief-act either. My tree-experience may cause me to form a tree-belief, just as my itch may cause me to scratch. But the tree-experience, unlike the itch, is not also a reason for what it causes. Epistemic acts, like coming to believe something, are special. What makes them special is that they have propositional content, whereupon justificationism comes into play.

I reject justificationism. I learned to reject justificationism from Karl Popper. As I understand Popper’s epistemology, the rejection of justificationism lies at the heart of it. Popper denies that any of our beliefs (belief-contents) are justified, while affirming that some of our believings (belief-acts) are justified. In other words, there are reasons for believings that are not reasons for the things believed. I have elsewhere argued (Musgrave 2004) that Popper’s solution to Hume’s problem of induction, if he has one, depends upon his rejection of justificationism. Here, I will argue that Popper’s solution to the problem of experience and belief, if he has one, also depends upon rejecting justificationism.

As I said, I learned to reject justificationism from Popper. But most of Popper’s followers, the self-styled ‘Popperians’, think that I learned badly and got it all wrong. They accept justificationism and therefore think that Popper rejects all justifications, whether of believings or of the things believed. Like E. M. Forster, they “do not believe in belief”. They fly instead to Popper’s ‘Third World’ of the objective contents of our thoughts. But, you cannot answer Hume’s inductive scepticism – the thesis that all evidence-transcending beliefs are unreasonable – in that way. And you cannot solve the problem of experience and belief – the problem of whether perceptual beliefs are reasonable – in that way either. Now, I am not really interested in Popper exegesis. I discuss his views only to show that they can be interpreted as resting upon the rejection of justificationism – and also to show how most discussions of these issues, including the criticisms levelled at Popper, presuppose justificationism.

So far, I have written as though I accepted the orthodoxy that as well as conclusive logical reasons, the province of deductive logic, there are also inconclusive logical



reasons, the province of inductive logic. In fact, like Popper, I do not believe in inductive logic. And the reason I do not believe in it is connected with my present topic, as I shall explain in due course. (I should here mention another terminological ambiguity, only to set it aside. The term ‘belief’ is ambiguous, not only between act and content, but also between act and state. It is belief-*acts* that I am interested in, not belief-*states*. I also want to set aside the question of whether belief-acts are *voluntary* acts, whether belief-voluntarism is true.)

## Popper on the ‘Empirical Basis of Science’

Popper discussed the relations between perceptual experience and perceptual belief in his *Logic of Scientific Discovery*, first published in 1934. He called it the ‘problem of the empirical basis’. I think his discussion is a philosophical *tour de force*. Most philosophers think it completely wrongheaded. No part of Popper’s theory of science has come in for greater criticism.

Popper begins by asking whether experiences can justify basic statements. I quote:

Perceptual experiences have often been regarded as providing a kind of justification for basic statements. It was held that these statements are ‘based upon’ these experiences; that their truth becomes ‘manifest by inspection’ through these experiences; or that it is made ‘evident’ by these experiences, etc. All these expressions exhibit *the perfectly sound tendency to emphasize the close connection between the basic statements and our perceptual experiences* [my italics]. Yet it was also rightly felt that *statements can be logically justified only by statements*. [italics in the original] (Popper 1959, p. 43).

Later, in Section 25 entitled ‘Perceptual Experience as Empirical Basis: Psychologism’, Popper defined psychologism as “the doctrine that statements can be justified not only by statements but also by perceptual experiences” (1959, p. 94). Popper, famously, rejected psychologism:

And finally, as to *psychologism*; I admit, again, that the decision to accept a basic statement, and to be satisfied with it, is causally connected with our experiences – especially with our *perceptual experiences*. But we do not attempt to *justify* basic statements by those experiences. Experiences can *motivate a decision*, and hence an acceptance or a rejection of a statement, but a basic statement cannot be *justified* by them – no more than by thumping the table. (1959, p. 105)

Basic statements cannot be logically justified by experience. A statement can only be logically justified by another statement, because logical relations hold only between statements or propositions, and experiences are not statements or propositions. Popper’s basic statements are (the contents of) perceptual beliefs. So, his rejection of psychologism means that perceptual beliefs cannot be justified by perceptual experiences.

What, then, is the role of perceptual experience? What is the “close connection between the basic statements and our perceptual experiences?” The connection is causal. The decision to accept a basic statement is caused by the experience. This decision, this coming to believe a basic statement, is an act or action that we

perform, a believing. So, Popper is here saying that our perceptual believings are caused by our perceptual experiences.

An experience can cause a believing – but does Popper think that it can also be a reason for it? That he did think this is suggested when he says that “the decision to accept a basic statement, and to be satisfied with it” can be not only caused, but also *motivated* by an experience. But ‘motive’ is something of a weasel word, hovering between cause and reason. Popper made it quite explicit that a cause (or motive) is also a reason in his reply to Ayer’s criticisms of his views. Ayer had objected that:

... there seems to be no good reason why we should not regard our experiences as directly justifying ... the sorts of statements Popper treats as basic. We cannot hold that they verify them conclusively; but that is not a bar to our holding that they give us adequate ground for accepting them. (1974, p. 689)

Notice, how Ayer switches from talking of experiences “directly justifying” basic statements to talking of them giving “adequate ground for accepting” basic statements. Ayer is a justificationist, for whom this is no switch at all. He assumes that because Popper denies that experiences justify statements, he must also be denying that they justify (“give us adequate ground for”) accepting statements. Popper replied:

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. (1974, p. 1114)

Needless to say, the reasons mentioned here are reasons for observational believings, not for observational beliefs. Needless to say, in other words, Popper rejects the justificationism that Ayer takes for granted.

Susan Haack discussed the Ayer–Popper exchange. She describes Popper’s reply to Ayer as “completely unconvincing” because it is “flatly inconsistent” with his earlier position. She concludes “Ayer clearly wins this round” (Haack 1993, p. 100). Haack thinks Popper’s idea that experiences are inconclusive reasons for accepting or rejecting observation statements is “flatly inconsistent” with his deductivism, because it means that “there can be reasons which fall short of deductive conclusiveness” (1993, p. 100). But this is Haack’s own muddle. Just as arguments do not have experiences as premises, so also they do not have acts or actions as conclusions. Arguments, whether conclusive or inconclusive, have statements or propositions as their premises and conclusions, not events or actions. Popper here makes no concession at all to non-deductive arguments. He says that a perceptual experience may be not just a cause or motive, but also an inconclusive reason, for an epistemic act.

Gregory Currie echoed Ayer’s criticism. Supposing that *e* is a basic statement inconsistent with some theory *T*, Currie says:

But if Popper’s view of the relation between experience and the acceptance and rejection of basic statements is that the relation is wholly causal (and that is what he says) the following is a consequence of his view. ... The question of whether it is rational to accept *e* and hence

to reject *T* must, on Popper's view, be independent of any questions about what experiences anybody has had. Now the only way in which we can become aware of the outcome of an experiment is by having certain experiences. ... But then we have the conclusion that whether it is reasonable to accept *e* and reject *T* is independent of any experiments that may be conducted. (Currie 1989, p. 425)

This conclusion is, of course, absurd. Fortunately, it does not follow from Popper's view. Currie thinks that it follows because he takes justificationism for granted, and assumes that experience can justify accepting a basic statement (a perceptual believing) only if justifies the statement itself (a perceptual belief). He writes:

Any reasonable methodology must respect the requirement that basic statements are to be accepted only if the evidence of our senses suggests that they are true; only, that is, if experience justifies our acceptance of the basic statement. ... the only justification there can be for accepting a basic statement is, in the end, the evidence of our senses. And if there can be no justification for a basic statement then what reason could there ever be for accepting or rejecting any scientific theory? (Currie 1989, p. 425)

Notice the conflation of "justification for accepting a basic statement" and "justification for a basic statement". Like Ayer, Currie assumes that because Popper rejects the latter, he must also reject the former.

Susan Haack's discussion of Popper's views is also vitiated by her tacit acceptance of justificationism. She quotes Popper's statement that "a basic statement cannot be *justified* by [experiences] – no more than by thumping the table". She turns this immediately into "the startling negative thesis that experience cannot justify *the acceptance of basic statements*" (1993, p. 98; my italics). Notice Haack's transition from "experience cannot justify a basic statement" to "experience cannot justify the acceptance of a basic statement". It is possible to accept the former and reject the latter, provided you also reject justificationism, the idea that you can only justify accepting a statement by justifying that statement itself. Which is precisely what Popper, as I interpret him, does.

Haack argues that Popper cannot solve his 'problem of the empirical basis' because of his deductivism and his anti-psychologism, each of which yields an argument for his 'startling negative thesis'. The 'anti-inductivist argument' combines Popper's deductivist thesis that there are no valid inductive or ampliative arguments from evidence to evidence-transcending statements, with the thesis that basic statements are evidence-transcending or 'theory-impregnated'. The 'anti-psychologistic argument' combines Popper's anti-psychologistic thesis that there are causal but not logical relations between experiences and statements, with the thesis that justification is a logical, not a causal, relation (Haack 1993, pp. 98–9).

As regards the anti-psychologistic argument, Haack is wrong to think that there are causal relations between experiences and statements. Causal relations stand between events, and statements are not events. There are causal relations between experiences and our "acceptance or rejection" of statements, both of which are events. Haack slips from the one to the other, in the same breath. She takes Popper to be saying that "scientists' acceptance of a basic statement like "The needle on the dial points to 7" is in no epistemically relevant way supported or justified by

their seeing the needle on the dial points to 7” (1993, p. 99). Popper never said that. What Popper said is that the statement “The needle on the dial points to 7” is not logically justified by the experience(s) – but that the experience(s) both cause scientists to accept the statement and are an inconclusive reason or justification for that epistemic act. Of course, what Popper wrote is nonsense if you read it wearing justificationist spectacles.

As for the anti-inductivist argument, Haack claims that Popper subscribes to ‘extreme deductivism’, defined as the view that “only deductive derivations are valid, and only valid derivations can constitute reasons for accepting statements” (1993, p. 105). This is an awful mess. First, a valid (deductive) argument can constitute no reason whatever for accepting a statement. Accepting a statement is an act or action that a person performs. A valid argument is no reason for an action. Notice that just as there is an act/content ambiguity in the term ‘belief’, so also there is an act/content ambiguity in terms like ‘argument’ or ‘derivation’. We must distinguish arguments from arguings, and derivations from derivings.

What might be true is that the action of arguing for C or deriving C is both a cause and a reason for accepting C. Running through an argument in your head, which has statement C as its conclusion, might give you a reason for accepting C. There are, of course, countless arguments to any conclusion C, the simplest of which is the rigorously valid “C, therefore C”. If running through a valid argument to C were a good reason for accepting C, then we would have good reason to accept any statement C whatever. Besides, we run through arguments for all kinds of reasons or purposes, and the effects of doing so involve other things than coming to accept their conclusions. We often run through an argument from some premises to figure out what their consequences are – as when we derive predictions from scientific theories. C might be something we independently do not accept. So, when we run through an argument from P to C, the upshot of that might be that we reject P, not that we accept C. But, suppose that we believe premise(s) P, and run through an argument from these premise(s) to conclusion C. That might cause us to believe C as well. But the mental causation here is all between mental events, states or processes. The premise statements are not a reason for the mental act of accepting the conclusion. Nor is the valid argument (assuming that the argument we run through is valid) a reason for that mental act. Statements and arguments are not causes, and reasons for actions are causes.

When is running through an argument from premises you accept to some conclusion a *good* reason for accepting the conclusion? Obvious requirements are that your premises be *true* and that the argument that you run through be *valid*. Deductivists think that the only valid arguments are deductively valid arguments. Given deductivism, there are no valid arguments from premises about perceptual experiences to basic statements. That is, because basic statements transcend the experiences that prompt them – I will discuss this next. Haack rejects deductivism and thinks there are valid or cogent non-deductive arguments from experiential statements to basic statements. Specifically, she thinks we can argue from the former to the latter by abduction or inference to the best explanation. I shall discuss this later.

## Popper's Fallibilist Foundationalism

Popper said that experiences are inconclusive or defeasible reasons for accepting basic statements. Why 'inconclusive'? Because basic statements are fallible – they transcend the experiences that prompt their acceptance, and may always turn out to be mistaken. There is the ever-present sceptical possibility of illusion or hallucination. The basic statements contain universal terms and are 'theory-laden', so that future experience may not fit with them. Popper's critics do not dispute this fallibilist view.

But an earlier response to 'scepticism regarding the senses' was to seek an infallible empirical basis that would not transcend experience. Philosophers attempted to close the logical gap between experience and perceptual belief by formulating statements that just describe the experiences, hoping then to show that these 'experiential reports' might be infallible. Popper opposed this tradition. It led the British empiricists to the disastrous doctrine of sense-data, arguably the greatest mistake in the entire history of epistemology. When Popper wrote his *Logic of Scientific Discovery*, Otto Neurath's doctrine of protocol sentences was doing the rounds of the Vienna Circle. Protocol sentences come from the same stable as sense-data reports, and Popper criticized them.

One can introspect, and come up with reports on experience. And perhaps, experiential reports are in some ways more certain than perceptual reports. Certainly, there are sources of error in perceptual belief that do not apply to introspective belief, and perceptual reports can be criticized in ways that introspective reports cannot. But that cuts both ways: perhaps, there are sources of error in introspective reports that do not apply to perceptual reports. After all, there is still a logical gap between introspective experience and introspective belief, and introspective reports also transcend the experiences that prompt them. More important, perceptual statements transcend introspective experiential statements and cannot be validly derived from them. And it is perceptual statements about the world outside us that we need for the 'empirical basis of science'. We cannot test scientific theories against introspective reports from scientists, because scientific theories (save for theories belonging to introspective psychology) say nothing about what the introspective activities of scientists will or will not reveal about their psyches. If we flesh out non-psychological theories with psychological auxiliary theories about what human observers will experience in this or that circumstance, then we only enhance the security of the empirical basis by making the Duhem problem worse. Which is not to say that basic statements are not criticized, occasionally, by pointing to psychological idiosyncrasies of the observers who make them. (The Astronomer Royal once dismissed his Assistant for issuing incorrect reports about the transit times of stars viewed through a telescope. It turned out that the Assistant just had unusually slow reaction-times.)

What goes for the empirical basis of science also applies to the common affairs of life. Only perceptual beliefs about the world outside us can help us find our way about the world. Before I go swimming, I want to know whether there is a crocodile in the river – not whether it seems to me that there is, or whether I just experienced

a sense-datum as of a crocodile, or whether my protocol sentence at 3.17 is that my speech-thought at 3.16 was that in the river at 3.15 was a crocodile observed by me – or anything like that.

So, the fallibility of the empirical basis is unavoidable. But it is also irrelevant, if all parties to the discussion of some issue are agreed upon the relevant basic statements. Which brings me to what Popper calls the ‘decisional’ or ‘conventional’ element in determining what statements belong in the empirical basis of science. If scientists all agree that the needle on the dial points to 7, then this statement belongs in the empirical basis of science despite the ever-present possibility that it might be false. Popper’s critics have spilled much ink upon this. Some saddle him with the view that the empirical basis is arbitrary, and that any statement that scientists happen to agree upon belongs there. This despite Popper’s analogy between the scientists’ agreement on a basic statement and a jury’s agreement on a verdict, which can hardly be called ‘arbitrary’ if it has been properly arrived at. Popper is clearly right about this. If all the people in a room agree that there is a cat in the corner, because they all see it there, is their agreement *arbitrary*? Another idiotic criticism levelled at Popper is that in his view the agreed-upon basic statement (whether it be that the needle on the dial points to 7 or that there is a cat in the corner) becomes *true by convention*. There is, of course, no truth in that.

By the way, bringing in other parties to the discussion, other members of our epistemic or scientific community, is perfectly commonsensical. The first thing we do, if we seriously suspect that we might be victims of sensory illusion or hallucination, is *ask our fellow-folk*. This can be regarded as a basic way of testing the basic statements that our experience has prompted us to accept. “I see, or seem to see, a cat in the corner of the room. But perhaps I hallucinate. Tell me, do you see it, too?”. Or I might check the deliverances of one sense by those of another. Macbeth said it all:

Is this a dagger which I see before me,  
The handle toward my hand? Come, let me clutch thee:  
I have thee not, and yet I see thee still.  
Art thou not, fatal vision, sensible  
To feeling as to sight? Or art thou but  
A dagger of the mind, a false creation,  
Proceeding from the heat-oppressed brain?  
(William Shakespeare, *Macbeth*, Act II, Scene 1)

Popper’s empirical basis is neither introspective nor infallible. Hence his famous metaphor:

The empirical basis of objective science has thus nothing ‘absolute’ about it. Science does not rest upon rock-bottom. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or ‘given’ base: and when we cease our attempts to drive our piles into a deeper layer, it is not because we have reached firm ground. We simply stop when we are satisfied that they are firm enough to carry the structure, at least for the time being. (Popper 1959, p. 111)

Still, perceptual beliefs are *foundational* beliefs – not in the sense that they are infallible, but just in the sense that they are not obtained by inference or argument from other beliefs.

We need foundational beliefs. Not all beliefs can arise by inference or argument from other beliefs, on pain of infinite regress. And we need some foundational beliefs to be reasonable beliefs, on pain of total irrationalism. Define *logomania* as the view that a belief is reasonable if, and only if it is obtained by logical inference from reasonable beliefs. It will follow that foundational beliefs, unargued premises, are unreasonable, since they are not obtained by logical inference. From which it will in turn follow that all beliefs are unreasonable.

Is anybody a logomaniac? Donald Davidson insists that “nothing can count as a reason for holding a belief other than another belief” (Davidson 2001, p. 141). What is Davidson saying, given the act/content distinction? There are four possibilities:

1. “Nothing can count as a reason for a belief-content other than another belief-content”. This is true – it expresses anti-psychologism.
2. “Nothing can count as a reason for a belief-act other than another belief-content”. This is nonsense – a belief-content may be the premise of an argument, but a belief-act cannot be its conclusion. Reasons for actions are causes, and belief-contents or propositions are not causes.
3. “Nothing can count as a reason for a belief-content other than another belief-act”. This is nonsense – a belief-act cannot be the premise of an argument whose conclusion is a belief-content.
4. “Nothing can count as a reason for a belief-act other than another belief-act”. This makes sense, and is probably what Davidson meant to say. (What he actually wrote was “Nothing can count as a reason for *holding* a belief other than another belief” – which here turns into “Nothing can count as a reason for *holding* a belief other than *holding* another belief”).

Should we accept (4)? It is true that some belief-acts are caused by running through arguments whose premises are (the contents of) other belief-acts. But many (arguably most) belief-acts do not result from running through arguments, but rather from experience, watching TV, reading books and newspapers, listening to what other folk tell you, and so on. Davidson’s slogan tells us that none of these causes of belief count as reasons for them. It follows that none of our ‘foundational beliefs’, beliefs not obtained by arguing from other beliefs, are reasonable, since there is no reason for them. Add that any belief obtained by arguing from unreasonable beliefs is itself unreasonable, and it follows that all beliefs are unreasonable.

To avoid this absurdity, we must reject logomania. But logomania follows from anti-psychologism plus justificationism. Anti-psychologism is correct – so justificationism cannot be. Once you reject justificationism, you can distinguish reasons for belief-contents from reasons for belief-acts, and say that experiences are not logical reasons for belief-contents but are reasons for belief-acts. Or so, Popper and I think.



## Endowing Perceptual Experience with Propositional Content

The only way justificationists can avoid total irrationalism is by resurrecting psychologism. Davidson's slogan ruled out experience as a reason for belief. Or as Wilfred Sellars put it, "experience lies outside the space of reasons" (cited by Currie 2006, p. 14). According to Greg Currie, McDowell tries to bring experience back into "the space of reasons" by endowing it with propositional content, as beliefs are endowed with content. What if we can perceive that *P* as well as believe that *P*? Then, the content of an experience might be a logical reason for the content of a belief – and reason for an act of believing as well (or so justificationists wrongly suppose).

Here, some familiar distinctions are relevant, between seeing, seeing-as, and seeing-that. (We should speak more generally of perceiving, perceiving-as, and perceiving-that – but as is usual, I shall often just speak of vision.) There is, first, a basic sense of 'see' (more generally 'perceive') that is non-propositional and non-conceptual. For example, a cat can see a typewriter without possessing the concept  $\langle \text{typewriter} \rangle$ . My cat sees the typewriter, for she does not bump into it when the mouse she is chasing runs under it. Cats (or people) can see an *X* in that basic sense without possessing the concept  $\langle X \rangle$ , let alone any theory or belief about *Xs*. There are philosophers who, bemused by Kant, deny this. PROOF: I once met a German philosopher who said that cats cannot see typewriters because they lack the concept  $\langle \text{typewriter} \rangle$ . I said that my cat frequently saw my typewriter. She replied that Musgrave's cat could do impossible things – just like Schrodinger's cat, which manages to be both alive and dead until somebody sees it. She even speculated that Musgrave's cat might become as famous as Schrodinger's cat. I should be so lucky!

What the German philosopher should have said, and perhaps meant to say, was that cats cannot see typewriters *as typewriters*, since they lack the concept  $\langle \text{typewriter} \rangle$ . Perhaps, the cat sees the typewriter as something else. Perhaps she sees the mouse as food, and the typewriter as non-food. (After all, she is trying to catch and eat the mouse, not the typewriter.) Perhaps all seeing is conceptual in the sense that whenever *A* sees *B*, *A* sees *B as a C* for some concept *C*. Perhaps that is the truth in the Kantian philosophy of perception.

Then there is seeing-that, which has propositional content, just as believing-that has propositional content. If the cat, lacking the concept  $\langle \text{typewriter} \rangle$ , cannot see the typewriter *as* a typewriter, then neither can she see *that* the mouse has run under the typewriter. Or so it would seem. I am cautious here because ordinary language is not so clear-cut. In describing the cat's behaviour we might say, not just that she saw the typewriter, but also that she saw that the mouse had run under the typewriter. Similarly, we might say of a chess-playing computer that it realized its Queen was threatened. Or we might say of a light-seeking robot that it is afraid of the dark. Or we might say of a wooden grill for sorting apples that by letting some apples fall through it recognizes that they are too small to go to market. We adopt Dennett's 'intentional stance' to all kinds of things. But in some cases at least, it is clear that our formulations are merely 'as if' formulations, that we do not



mean it literally that apple-sorting devices or light-seeking robots or chess-playing computers recognize or fear or realize things. They merely behave as if they did. Similarly, perhaps, with my cat, despite ordinary ways of speaking. She sees the typewriter – after all, she has eyes good for seeing things. But she does not see it *as* a typewriter, let alone see *that* the mouse has run under the typewriter. Seeing-as and seeing-that are inner states that require the possession of concepts – whatever that comes to, psychologically or neurologically speaking. The concept < food > may not be realized in the cat-brain in the same way it is realized in ours. But if the cat sees the mouse as food, the concept < food > must be realized somehow in its brain. That is one view, anyway.

Greg Currie does not share the view. He rejects McDowell's attempt to "bring experience into the space of reasons by seeing it as possessing ... conceptual content" (Currie 2006, p. 14). He agrees that an experience can have exactly the same content as a judgement or belief, and be a (conclusive) logical reason for it. But, he thinks that 'conceptual content' is a misleading term (or that the concept < conceptual content > is a misleading concept?), because concepts are not 'constituents' of that common content. The difference between perception and belief is that the subject needs no concepts to perceive that P, but does need concepts to believe that P. It seems that my cat can *literally* see that the mouse has run under the typewriter after all. It is just that, lacking the requisite concepts, she cannot bring herself to believe it! And lacking the appropriate words, she cannot bring herself to say it, either!

These are deep waters. Fortunately, we need not plunge into them. Let us confine ourselves to perceivers like us, who have whatever it takes to see that P, to believe that P, and to say that P as well. And let us grant that our perceptual experience has content, represents the world as being a certain way. The idea is that the representational content of experience allows it to play a justificatory role in epistemology:

It is the content of experience that matters to epistemology. It is this content which creates the possibility that an experience may provide a rational basis for the assertion of a statement describing some state of affairs. (Currie 2006, p. 4)

Suppose (to use Currie's example) that I have a perception with the content *there is a flying owl*. Obviously, the content of my experience is a conclusive logical reason for (the content of) the belief that there is a flying owl, and an equally conclusive logical reason for (the content of) the assertion that there is a flying owl. After all, the three contents are identical, and C logically implies C. It is equally obvious that the content C of my experience is no reason at all for forming a perceptual belief with content C, let alone for asserting an observation statement with content C. Forming beliefs or asserting statements are actions that we perform. Reasons for actions are causes of them, and contents or propositions are not causes.

Currie will perhaps agree. At least, he says explicitly that "what matters is not content alone":

I am claiming that experience is capable of playing a justificatory role in epistemology because of its content, and hence that some particular experiences – namely those with the right kinds of contents – do justify some assertions. What matters is not content alone, but the content's being the content of an experience. (Currie 2006, p. 9)

An experience with the content *there is a flying owl* presumably has “the right kind of content” for a belief or assertion with exactly the same content. So does the experience justify forming the belief or making the assertion?

It does not conclusively justify or prove the belief or assertion. Suppose that what I see is not an owl, but a pigeon – and suppose it is a stuffed pigeon, that is not flying but has been thrown. In this case, the assertion is false, and the perceptual belief is false, and *the experience is false as well*, for the same reasons. Admittedly, the last is linguistically odd. It seems odd to say that I can see that there is a flying owl without there being a flying owl. ‘Seeing’ is a success-word, like ‘knowing’. As ordinarily used, “A sees that P” entails P, just as “A knows that P” entails P. But once we endow perception with content, we must allow that perception might have false content, and we must rid ‘seeing-that’ of any success connotations that it might carry in ordinary speech. Thus, the fallibility of observation statements or of perceptual beliefs cannot be evaded by endowing experiences with statement-like or belief-like contents. That just makes experiences fallible as well.

Moreover, it is not to be assumed that having an experience with content C invariably issues in a perceptual belief with content C, let alone in an assertion with content C. The latter is obvious – the perceiver may lack spoken language. The former is obvious, too. Seeing is not always believing. I can see that there is a flying owl without believing it – perhaps because I am also possessed of the mistaken belief that owls are flightless birds. Currie says that it is hard to specify “the point at which perceptual systems deliver their outputs and belief takes over” (2006, p. 13). But belief may never ‘take over’ the output of the perceptual system. I may not accept the ‘evidence of my senses’, because of other beliefs that I possess. To take another example, anybody who is not fooled by the Muller-Lyer illusion is rightly correcting the ‘evidence of the senses’ in the light of their other beliefs. Such a person might well say “I see that one line is longer than the other – but I do not believe it”.

Where are we? Seeing that P is not always believing that P, let alone saying that P. And seeing that P, believing that P, and saying that P, might all involve a false P. Can no more be said about the epistemological role of experience? Seeing may not always be believing, but it often is. Having an experience with content C often causes a belief with content C. What is caused is not, of course, the content C – contents or propositions have no causes. What is caused is the formation or adoption of a belief with content C. The epistemological question is whether a perceptual cause of a believing is also some kind of reason or justification for that believing. Popper and I reject justificationism and propose that seeing that P is a (defeasible) reason as well as a cause for believing that P. This holds even when P is false, when both my seeing and my believing are mistaken. Reasonable beliefs may be false beliefs, quite generally. Reasonable perceptual beliefs may be false beliefs, too. Still, sense-experience delivers us evidence, particular beliefs or statements about the world against which we can test other beliefs and statements. It is ‘foundational’ not in the sense that ‘the evidence of the senses’ is infallible, but just in the sense that it is non-inferential. Or so Popper and I maintain. I suspect that Currie’s own view of the justificatory role of experience in epistemology is not much different from this.

## Perceptual Belief as Inference to the Best Explanation

Are perceptual experiences *good* reasons for perceptual believings, in the *epistemic* sense of the term ‘good’? Several authors have suggested that this question can be answered in the affirmative by understanding the relationship between perceptual experience and perceptual belief as a case of inference to the best explanation (IBE). Susan Haack is one example (1993, Chap. 4). Like everybody else, Haack takes it for granted that IBE involves non-deductive or inductive or abductive reasoning, so that deductivists like Popper and I cannot accept it. I disagree. Deductivists can accept IBE, and the deductivist version of it is better than the usual inductivist version.

IBE as it is usually formulated goes like this (Lycan 1985, p. 138):

F is a fact.

Hypothesis H explains F.

No available competing hypothesis explains F as well as H does.

Therefore, H is true.

This is deductively invalid. We can validate it if we view it as a deductive *enthymeme* and supply its missing premise, “The best available explanation of any fact is true”. But this missing premise is obviously false. Nor is any comfort to be derived from weakening the missing premise (and the conclusion) by replacing ‘true’ by ‘probably true’ or ‘approximately true’ – though philosophers have cottage industries devoted to both of these. Nor, finally, is any comfort to be derived from refusing to validate the argument at all, and saying instead that although it is deductively invalid it has some special merit in some special non-deductive or inductive or ampliative logic.

Instead, remember that what we seek here is not a reason for H, but rather a reason for adopting H or preferring H or tentatively believing H. Of course, if justificationism is correct, this is a distinction without a difference – a reason for believing H must be a reason for H. But if we reject justificationism, IBE can be written as follows:

(*It is reasonable to believe that the best available explanation of any fact is true.*)

F is a fact.

Hypothesis H explains F.

No available competing hypothesis explains F as well as H does.

Therefore, *it is reasonable to believe that H is true.*

This argument-scheme is deductively valid, and particular instances of it might well be sound. Arguments of this kind are employed in the common affairs of life, in detective stories, and in the sciences. Of course, to establish that arguments of this kind may be sound, the ‘explanationist’ owes us an account of when a hypothesis explains a fact, and of when one hypothesis explains a fact better than another hypothesis does. These are controversial issues, but they are not the most controversial issue. That concerns the major premise. Most philosophers think IBE, as reconstructed above, is unsound because this major premise is false. So, let me assume that the explanationist can deliver on the promises just mentioned,

and focus on objections to IBE's major premise. All these objections presuppose justificationism.

It is objected that the best available explanation might be false. Quite so – and so what? Any explanation *might* be false, in the sense that it is not *necessarily* true. It is absurd to suppose that the only things we can reasonably believe are necessary truths.

It is objected that the best available explanation might be “the best of a bad lot” and actually be false. Quite so – and again, so what? It can be reasonable to believe a falsehood. Of course, if we subsequently find out that the best available explanation is false, then it ceases to be the best available explanation and it is no longer reasonable for us to believe it. But, what we find out is that what we believed was wrong, not that it was wrong or unreasonable for us to have believed it.

It is objected that being the best available explanation of a fact does not *show* that something is true (or probably true or approximately true). Quite so – and again, so what? It can be reasonable to believe or think true things that have not been shown to be true or proved. To think otherwise is to accept a strong form of justificationism, whereby a reason for believing something must be a conclusive reason for what you believe.

So much for IBE in general. I have argued elsewhere (see, for example, Musgrave 1999 and 2007) that what goes for IBE goes for all so-called ‘inductive’ or ‘ampliative’ arguments. They are all better reconstructed as deductive *enthymemes* whose ‘missing premises’ are epistemic principles of one kind or another. Now let us return to perceptual belief.

Here is a simple example of IBE regarding perceptual belief, as it would usually be formulated: “I see a cat in the corner of the room. The best explanation of this is that there is a cat in the corner of the room. Therefore, there is a cat in the corner of the room”. The claim is not that we *form* perceptual beliefs by running through arguments of this kind. The claim is, rather, that an argument of this kind might be produced to defend the obvious answer you might give to the question “Why do you believe that there is a cat in the corner of the room?” – which is “Because I see it”.

But if that is the question, then the usual formulation just given is not the right answer to it. The question was not “Why is there a cat in the corner of the room?” but rather “Why do you believe that there is a cat in the corner of the room?”. What we are trying to justify or give a reason for is not the statement “There is a cat in the corner of the room”, but rather our mental act of coming to believe or accept this. So the conclusion ought to be “It is reasonable to believe or accept that there is a cat in the corner of the room”. And to validate this argument, what we need to add is the epistemic principle that “It is reasonable to believe or accept the best explanation of any fact”. This missing premise is not obviously false – especially when we remember that any reasonable theory of reasonable belief must make room for reasonable beliefs in falsehoods. If the missing premise is true, then our argument is not just valid, but sound as well.

Notice that if we accept this, then *anyone* may be justified in believing that there is a cat in the corner of the room. A *blind* person, who cannot see the cat for herself,

might form that belief. For if she accepts that *I* seem to see the cat, because I tell her so and she has no reason to think I am fibbing, then she might also come to believe that there is a cat. And if we accept IBE, we will deem her belief reasonable or justified. My experiences may be private to me, but reasonable inferences from them need not be.

The explanationist principle “It is reasonable to believe or accept the best explanation of any fact” is a normative epistemic principle. Applied to facts about perceptual experiences, it means that the having of a perceptual experience provides a *good reason in the epistemic sense of ‘good’*, for accepting a perceptual belief. It is not a conclusive reason. It is defeasible, if we find a better explanation of my seeing a cat than the obvious one. Popper did not discuss IBE, or invoke it in this context. But I invoke it on his behalf, and quite consistently with his overall position.

## References

- Ayer, Alfred Jules. 1974. Truth, Verification and Versimilitude. In *The Philosophy of Karl Popper*, ed. Paul Arthur Schilpp, 684–691. La Salle, IL: Open Court.
- Currie, Gregory Paul 1989. Frege and Popper: Two Critics of Psychologism. In *Imre Lakatos and Theories of Scientific Change*, eds. Kostas Gavroglu, Yorgos Goudaroulis, and Pantelis Nicolacopoulos, 413–430. *Boston Studies* vol III Dordrecht: Kluwer.
- Currie, Gregory Paul 2006. Where Does the Burden of Theory Lie?. In *Rationality and Reality: Conversations with Alan Musgrave*, eds. Colin Cheyne and John Worrall, 7–18. Dordrecht: Springer.
- Davidson, Donald. 2001. *Subjective, Intersubjective, Objective*. Oxford: Clarendon.
- Haack, Susan. 1993. *Evidence and Inquiry*. Oxford: Blackwell.
- Lycan, William. 1985. Epistemic Value. *Synthese* 64: 137–164.
- Musgrave, Alan Edward. 1999. How to Do Without Inductive Logic. *Science and Education* 8: 395–412.
- Musgrave, Alan Edward. 2004. How Popper [Might Have] Solved the Problem of Induction. *Philosophy* 79: 19–31. [Reprinted in Philip Catton and Graham Macdonald (eds). 2004. *Karl Popper: Critical Appraisals*, London: Routledge, 16–27; and in Anthony O’Hear (ed). 2003. *Karl Popper: Critical Assessments of Leading Philosophers*, Routledge, London, Volume II, 140–151.]
- Musgrave, Alan Edward. 2007. The Miracle Argument for Scientific Realism. *The Rutherford Journal: The New Zealand Journal for the History and Philosophy of Science and Technology*, February 2007.
- Popper, Karl Raimund. 1959. *The Logic of Scientific Discovery*. London: Hutchinson.
- Popper, Karl Raimund. 1974. Replies to My Critics. In *The Philosophy of Karl Popper*, ed. Paul Arthur Schilpp, 961–1197. La Salle, IL: Open Court.

# Critical Rationalism and the Principle of Sufficient Reason

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**Abstract** According to the principle of sufficient reason we should always try to give sufficient reasons for the truth (or high probability) of our opinions. All such attempts lead to a trilemma of justification: they force us to choose between infinite regress, logical circle, or dogmatism. According to the principle of critical testing we should always try to test our opinions critically. It is reasonable to claim that opinions that have survived critical tests are true. Such truth-claims are conjectural and do not confront us with any trilemma of justification. Scientific theories can be tested through observations and experiments. Statements about observations and experiments can be tested by experience. Although Critical Rationalism is a philosophical position, it can also be critically discussed. If we follow the principle of critical testing, no trilemma of justification arises when we claim that it is reasonable to accept Critical Rationalism. This opens the way for a critical philosophy stressing the fallibility of human knowledge.

## The Great Transformation and the Quest for Certainty

A great transformation occurred in ancient Greece more than 2,000 years ago. The ancient myths were gradually replaced by philosophy and science. It was a transition from myth (*mythos*) to reason (*logos*), accompanied by a transition from the closed to the open society. During this transformation philosophy, science and democracy were born (cf. Armstrong 2006, pp. 138–146; Popper 1966, Vol. 1, Chap. 10). Plato interpreted it as a transformation from uncertain opinions (*doxa*) to certain knowledge (*episteme*) and tried to show how man could get certain knowledge, get rid of the ancient myths and opinions, and introduce an age of enlightenment and reason.

Certain knowledge could be attained if we were able to prove our opinions, if we were able to give sufficient reasons for them. This ideal of knowledge can be expressed in a principle of sufficient reason according to which we should always try to give sufficient reasons for the truth of our opinions (cf. Albert 1985, Sect. 1).

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Can we get certain knowledge if we follow the principle of sufficient reason? Assume that we have a statement and give a premise as a sufficient reason for the truth of it. If the premise is true, then the statement is true. But is the premise true? If we follow the principle of sufficient reason, we must give a sufficient reason for the premise in the form of another premise. But this only shifts the problem into giving sufficient reasons for the other premise and so on forever. We get an *infinite regress* of sufficient reasons. In order to stop the infinite regress, we can introduce a *logical circle* at some point. From a logical point of view any premise can be derived from itself. But if we are trying to get certain knowledge, neither an infinite regress nor a logical circle is satisfactory. The only remaining possibility is to abstain from giving further reasons at some point where we break the chain of sufficient reasons, declare that some type of reasons are ultimate and accept them without further reasons. If we accept the principle of sufficient reason, it is dogmatic to accept a statement in this way.

The principle of sufficient reason leads to a *trilemma of justification*: it forces us to choose between infinite regress, logical circle, and dogmatism (cf. Münchhausen's trilemma, Albert 1985, Sect. 2). Skeptical Greek philosophers used this trilemma in order to show that we cannot get certain knowledge and have to choose between skepticism and dogmatism. The Greek enlightenment, which began so gloriously, ended in doubt, skepticism, and ultimately in the rebirth of myth and the closed society. Is it possible to avoid this fate, to resolve the trilemma of justification, to avoid having to choose between skepticism and dogmatism?

Logic can be used as an instrument for proving statements. But it can also be used as an instrument for criticism in order to refute statements. Perhaps the important use of reason is not to prove statements, but to criticize them. Perhaps we can resolve the trilemma of justification if we replace the principle of sufficient reason with a principle of critical test, according to which we should always try to criticize our opinions seriously (Albert 1985, Sect. 5). Can critical reason enable us to steer a course between the epistemological monsters of dogmatism and skepticism?

## Scientific Theories

Astronomy was among the first sciences born in Greece. Its success was impressive. Greek astronomers were able to explain not only the movements of the sun, the moon, and the stars, but also the movements of the planets, which appeared to be rather irregular when observed from the earth.

Attempts to prove scientific theories with sufficient reasons lead to the trilemma of justification. However, it is possible to test theories with the help of experience. For example, the position of a planet in the sky at a specific point of time could be predicted with Greek astronomy. Observations showed that such predictions were correct. But the empirical success of Greek astronomy was not a sufficient reason for the truth of it. As is well known in logic, the truth of a consequence does not show that the premises are true: deductive reasoning does not transmit



truth from consequences to the premises. Reasoning from singular test statements to (strictly) universal hypotheses is deductively invalid: no finite number of true singular test statements is sufficient for the truth of a universal hypothesis.

Reasoning from singular statements about observable events (that is test statements or basic statements, cf. Popper 1974, p. 987) to general hypotheses is called *inductive*. In order to bridge the gap between theory and observation, we can try to introduce an *inductive principle* telling us when test statements are sufficient for the truth (or high probability) of the tested hypothesis or theory. Can induction be justified? If we try to give sufficient reasons for an inductive principle, we have to introduce more general principles, which in their turn depend on still more general principles, and so on. We are caught in an infinite regress. If we try to avoid the infinite regress by referring to the empirical success of science, we are arguing in a circle. If we introduce the inductive principle without any reasons, we are dogmatic. Once again, the trilemma of justification appears, this time as a trilemma of justifying an inductive principle. Such arguments led Hume (1967) to skepticism. In spite of the vast successes of modern science, as for example Copernican astronomy and Newtonian physics, Hume argued that science does not give us certain knowledge, only uncertain opinions.

In order to solve the problem of induction, Popper (1959) suggested that we should give up the principle of sufficient reason and instead use the principle of critical testing. Logic can be used not only as an instrument of proof, but also as an instrument of criticism. In proofs truth is transmitted from the premises to the consequence; in criticism falsity is re-transmitted from a consequence to at least one of the premises (Popper 1992, p. 75, Theses 15–17). For the discussion of scientific theories it is important that a theory is false if it has a false consequence. Theories can be falsified by singular statements about observable events, by test statements. It is important to understand that not only isolated hypotheses, like ‘all swans are white,’ but also complicated theoretical systems consisting of many hypotheses and also including auxiliary hypotheses can be falsified in this way (Andersson 1994, Chap. 2C). We do not need any inductive assumptions in order to understand science. They only lead to unnecessary problems (Popper 1959, Sect. 3).

According to Popper (1959, Chap. 10) critical tests are central for science, not proofs. If a theory withstands critical tests, it is *corroborated* (cf. Rowbottom 2008). The ancient aim of certain knowledge is not attained by testing theories. Corroborated theories remain *fallible* and can always be falsified by the next critical test.

Can theories corroborated by experience be accepted for the time being and claimed to be true? In the recent discussion of critical rationalism there are two answers to this question.

A skeptical answer is that we should not claim any theory to be true, not even temporarily, since corroboration is not a sufficient reason for its truth. Nevertheless, according to Miller, we should classify theories as true or false. There are no sufficient or even good reasons for such classifications. Miller’s answer to the question why we think that a theory is true is ‘Why not?’. This is an invitation to show some disadvantage of the theory, for example that it is falsified, not to marshal any reasons



in favor of it, for example that it is corroborated. It is essential for this answer that we should be prepared to classify statements as true or false without having any reasons for so classifying them (Miller 1994, pp. 71–72).

Another answer to the question whether we should claim that corroborated theories are true, is that we should do so because they have withstood serious criticism (Musgrave 1993, pp. 281–283, 1999, pp. 322–325). The corroboration of a theory makes it reasonable to claim the theory is true according to the following principle of critical testing (*CT*):

(*CT*) It is reasonable to claim that a statement is true if and only if it has withstood serious criticism.

Theories claimed to be true remain fallible. The corroboration of a theory is not a sufficient reason for the truth of the theory. Nevertheless, it is reasonable to claim that a well corroborated theory is true.

There are important differences between attempts to find sufficient reasons proving that a theory is true and attempts to test a theory severely in order to be able reasonably to claim that a theory is true. Attempts to find sufficient reasons for the truth of a theory lead to the trilemma of justification. Therefore inductivism fails to justify theories. Attempts to show that it is reasonable to claim that a theory is true do not lead to any trilemma of justification. Such attempts do not presuppose inductivism, only that a theory can be corroborated. Asked why we claim that some theories are true or why we classify them as true, we do not have to answer only ‘Why not?’, but can say that the theory has withstood serious criticism. Falsifiability and the absence of falsifications are not enough. It is important that theories actually have been tested and that they have withstood serious tests. There are no reasons why we should not claim that corroborated theories are true. Such claims do not presuppose any concessions to inductivism or any other kind of justificationism.

Critical tests weed out theories that are refuted by experience, restrict our choice of theories, and allow us to make reasonable selections among them. Such refutations and selections are fallible and open to revision (Andersson 1982, 1994, Chap. 8B). The skeptics are right when they point out that critical tests do not give us certain knowledge, but wrong when they think that we should not claim a corroborated scientific theory is true. We do not have to choose between dogmatism and skepticism, but can choose a third possibility: the critical method of testing our opinions and of claiming that those opinions that have survived critical tests are true.

We need statements about observations and experiments in order to be able to test scientific theories. When we discuss scientific theories, we presuppose the principle of critical testing. In order to show that the trilemma of justification does not reappear, two problems remain to be solved:

1. When we say that a statement has withstood serious criticism, we need test statements. Does the discussion of test statements lead to the trilemma of justification?
2. Why should we accept the principle of critical testing? Does the discussion of this principle lead to the trilemma of justification?

## Test Statements

Like general hypotheses, test statements are fallible, for example the test statement ‘The planet Venus is about 13° above the horizon in the east when observed at 5 o’clock in the morning in Prague on the 14th of September 2007.’ Attempts to find sufficient reasons for test statements, to prove that they are true, lead to the trilemma of justification, in the same way as attempts to find sufficient reasons for general hypotheses did (cf. Fries’ trilemma, Popper 1959, Sect. 25).

What happens if we substitute the principle of critical testing for the principle of sufficient reason in our discussions of test statements? Test statements can be tested by other test statements. For example, we can test if a planet is Venus by observing its brightness. There are no ultimate or last test statements, no foundational basic statements. The chain of test statements is in principle infinite: a test statement can be tested with another test statement, which can be tested with still another test statement, and so on. We get an infinite regress of test statements. From a logical point of view a test statement can be derived from itself. But such a logical circle cannot be called a test in the usual sense of the word: if a test statement is problematic, it should be tested with a less problematic and hence different test statement. A third possibility is that at some point a test statement is dogmatically accepted without any further derivation of test statements. The principle of critical testing seems to lead to a trilemma of testing similar to the trilemma of justification.

However, this is not the case. Test statements can be tested by experience without any derivation of further test statements (cf. Johansson 1975, p. 160). Such derivations end at test statements that are especially easy to compare with experience. “It is fairly easy to see that we arrive in this way at a procedure according to which we stop only at a kind of statements that is especially easy to test” (Popper 1959, Sect. 29). Ultimately, every derivation of test statements has to end at such unproblematic test statements. Consider for example the test statement just mentioned about the position of Venus. In many situations, this statement can be tested directly by observation.

Should it be needed, further test statements can be derived. For example, we can derive test statements about the size, brightness, or phase of Venus and use a telescope in order to test these further test statements.

In order to derive further test statements, auxiliary hypotheses might be needed. From logical and methodological points of view, the derivation of further test statements is unproblematic (cf. Andersson 1994, Chaps. 6B2, 6C2, 8B2).

Assume that Venus is observed in a position predicted by a test statement. Then the test statement has been tested by observation and has withstood criticism. According to the principle of critical testing, it is reasonable to claim that the test statement is true. The infinite regress of test statements is broken by an unproblematic test statement that easily can be *tested* by direct comparison with experience. Such tests do not provide an infallible empirical basis for science, but provide test statements that reasonably can be claimed to be true (Andersson 2007, pp. 180–181).

Impressed by the principle of sufficient reason, Davidson (1986, p. 331) maintains that test statements can be tested by experience only in a metaphorical sense. Davidson probably thinks that in the literal sense a statement can be tested only by another statement. But if we are interested in *testing* statements, not in *proving* them, then there is no reason why it should not be possible to test simple statements directly by experience, without the intervention of any statements. Although a statement can be proved only by other statements, it can be tested not only by other statements, but also by experience.

The claim that statements can be tested by experience does not lead to an infinite regress or to any trilemma of justification. Since we use the principle of critical testing, we do not try to prove that test statements are true. The potentially infinite regress of test statements is broken by test statements that reasonably are claimed to be true after tests by experience. In this way the problem of test statement is solved without any trilemma of justification.

A similar solution to the problem of test statements is to introduce a special principle (E) for the relation between experience and test statements according to which (cf. Musgrave 1999, p. 342):

(E) It is reasonable to perceptually believe that a test statement is true if and only if it has not failed to withstand criticism.

However, since perception can be used in order to test statements, we do not need this principle: perceptual truth-claims have already withstood perceptual tests and can be judged with the principle of critical testing.

The derivation of test statements can be continued and the observation of reproducible effects can be repeated. If somebody doubts the results of a critical test of a theory, we would not try to present sufficient reasons for the truth of test statements for him, but we would ask him to test the theory himself and to repeat the relevant tests (cf. Andersson 1994, pp. 73–77; Popper 1959, Sect. 7). Ultimately, singular test statements have to be tested by experience. This is the critical solution to the problem of test statements that avoids the dogmatism of absolutely certain test statements and the skepticism of arbitrarily accepted test statement.

## Critical Rationalism

Why should we accept critical rationalism and its principle of critical testing? If we try to justify critical rationalism with sufficient reasons, we once again encounter the trilemma of justification, as Popper did when he first introduced critical rationalism and argued that rationalism itself cannot be justified: “Since all arguments must proceed from assumptions, it is plainly impossible to demand that all assumptions should be based on argument” (Popper 1966, Vol. 2, Chap. 24, Sect. ii, p. 230). Such demands would lead to an infinite regress that can only be avoided by a logical circle or a dogmatic break. According to Popper (1966, Vol. 2, Chap. 24, Sect. ii, p. 231): “[W]hoever adopts the rationalist attitude does so because he has

adopted, consciously or unconsciously, some proposal, or decision, or belief, or behavior.” Since the adoption of rationalism cannot be justified by sufficient reasons, Popper calls it unreasonable.

When discussing the introduction of critical rationalism, Popper faces a trilemma of justification and consciously opts for a minimal kind of dogmatism. For Popper, a critical rationalist is a person who is dogmatic only at one point: when he decides to accept the rationalist attitude. Such a person who understands the limits of reason Popper calls a *critical rationalist*. His “fundamental rationalist attitude results from an (at least tentative) act of faith—from faith in reason” (Popper 1966, Vol. 2, Chap. 24, Sect. ii, p. 231).

According to Popper, a critical rationalist makes a minimum concession to irrationalism when he decides to adopt the rationalist attitude. This can be compared to the *academic skepticism* in ancient Greece. In order to avoid the trilemma of justification, these skeptics made a minimum concession to dogmatism in their fundamental decision to accept skepticism. In this way skepticism became a logically tenable position. In a similar way Popper makes a minimum concession to dogmatism in order to avoid the trilemma of justification.

In spite of his minimum concession to dogmatism and irrationalism, in spite of realizing the logical problems of justifying rationalism, Popper analyses the consequences of rationalism and irrationalism and says that there is a vast difference between a blind decision and a decision made with open eyes (Popper 1966, Vol. 2, Chap. 24, Sect. iii, p. 233). The analysis of the consequences of the decisions induces Popper to decide in favor of rationalism (Popper 1966, Vol. 2, Chap. 24, Sect. iv, p. 240).

Why does he not say that a critical discussion of the two positions makes it reasonable to decide in favor of rationalism? The reason why Popper does not say so is that he implicitly assumes the principle of sufficient reason in his discussion of rationalism. But why should we assume this principle as an ultimate principle when discussing rationalism? This question becomes especially urgent when we discuss a type of rationalism stressing the importance of critical discussions and critical tests. This type of rationalism does not aim at proofs and certain knowledge, but at critical tests and reasonable truth-claims. Critical rationalists should reject justificationism and the principle of sufficient reason and should not try to justify critical rationalism by giving sufficient reasons for it. Such attempts are motivated by the old tradition that rationalism is based on sufficient reasons leading to certain knowledge (cf. Bartley 1984, Chaps. 4, 5, especially pp. 104–107).

Why not say that we accept or adopt critical rationalism, because it has withstood serious criticism? An objection is that it is circular to adopt rationalism in this way (cf. Miller 2006, pp. 128–129; Musgrave 1999, pp. 330–331). If we assume the principle of sufficient reason, this is a devastating objection: circular proofs in the sense of ‘*p*, therefore *p*’ are worthless. If we do not assume it, the objection is without force, because no trilemma of justification arises. If we accept the principle of critical testing, it is reasonable to accept critical rationalism because it has withstood serious criticism. Different types of criticism can be directed against principles of rationality. One type of criticism is that they do not fulfill their own

demands. Critical rationalism survives this type of criticism. To show this is not to give a circular proof for critical rationalism, but to show that it is internally consistent (cf. Popper 1966, Vol. 2, Chap. 24, Sect. ii, p. 230).

Another type of criticism maintains that also the principle of critical testing leads to the trilemma of justification. How do we know that a hypothesis has survived critical tests? Are there sufficient reasons for the truth of such claims? If we try to find sufficient reasons for test statements the trilemma of justification arises. It arises also if we try to find sufficient reasons for the truth of the claim that critical rationalism itself has survived critical discussions. However, in both these cases the trilemma arises only if the principle of sufficient reason is assumed, explicitly or implicitly. But a critical rationalist should not assume the principle of sufficient reason, not even implicitly. For a critical rationalist who does not search for certainty, or proofs, or sufficient reasons for truth, no trilemma of justification arises. Such a person does not have to choose between an infinite regress, a logical circle, or dogmatism.

## Conclusions

Our point of departure was the great transformation from the closed to the open society, from myth to reason. Originally, it was interpreted as a transition from arbitrary opinions to certain knowledge. In order to guarantee certain knowledge, the principle of sufficient reason was assumed.

The search for certainty led to unexpected difficulties. The attempt to be reasonable led to a trilemma of justification and often also to dogmatism or skepticism. In our time the situation is similar. Today European enlightenment has ended in postmodernism and a fight between dogmatic fundamentalism and relativistic skepticism. In this fight critical reason is often muted by different kinds of dogmatism.

In order to overcome this situation, it is important to understand that reason is not a road to certainty, but a road to critical discussion and reasonable judgment. If we replace the principle of sufficient reason with the principle of critical testing, many of the difficulties marring modernity and enlightenment can be solved. We can avoid dogmatism and skepticism and find a critical middle way between them. This can contribute to the great transformation to critical reason and enlightenment that today is more needed than ever.

## Comments on a Technical Note

In a technical note Miller (1994, p. 133) has argued that applications of the principle of critical testing (abbreviated to *CT* above) lead to an infinite regress of a special kind. When we use this principle, by Miller abbreviated to *P*, and reports of tests *e* of the hypothesis *H* we can derive that it is reasonable to accept *H* (abbreviated to *AH*):

$$P, e \text{ therefore } AH \quad (1)$$

In a similar way, when critically discussing the principle  $P$  using reports  $e^*$  of such discussions, we can deductively derive that it is reasonable to accept  $P$  (abbreviated to  $AP$ ):

$$P, e^* \text{ therefore } AP \quad (2)$$

Miller admits that (1) is a valid derivation, but remarks that  $P$  is not known to be true, though it is reasonable to accept it according to (2). Since there are no sufficient reasons for  $P$ , Miller suggests that in our derivation (1) we should use  $AP$  instead of  $P$  as a premise. Musgrave (1999, p. 338) has proposed a principle of applied deductive logic:

$$\text{If } AH \text{ and if } H \text{ entails } P, \text{ then } AP \quad (3)$$

Miller derives that it is reasonable to accept that it is reasonable to accept  $P$  ( $AAP$ ) from (1) and (3):

$$AP, e \text{ therefore } AAH \quad (4)$$

According to Miller the looming infinite regress is obvious. In order to avoid it, we could try to argue that if it is reasonable to accept that it is reasonable to accept  $P$ , then it is reasonable to accept  $P$ :

$$\text{If } AAP, \text{ then } AP \quad (5)$$

But the principle (5) is not known to be true either. Although such a principle might be reasonable, it is dogmatic to assume so without further reasons according to the principle of sufficient reason. In this way Miller argues that a trilemma of justification arises, when we use the principle  $P$ . However, this is only the case if we assume the principle of sufficient reason. Why should we assume this justificationist principle when using the principle  $P$ ? Why not overcome the justificationist addiction also in this situation (cf. Miller 2007, Sects. 1 and 2)? If we take our point of departure from the principle of critical testing, no trilemma arises. Then there is no need to use (4) instead of (1). As in most derivations the premises in (1) are not known to be true. We have sufficient reasons neither for  $P$  nor for  $e$ . From premises that can reasonably be claimed to be true we derive  $AH$ . Since we have given up the vain search for absolute certainty, we do not try to find sufficient reasons for the premises  $P$  and  $e$  and avoid the infinite regress.

If we ask for sufficient reasons for the premises in any arbitrary derivation, we get a trilemma of justification. Of course, this is true also for the derivation (1). The solution of this problem is to give up the quest for certainty, the addiction to justificationism, and the associated principle of sufficient reason. Instead we should use the principle of critical testing and try to test the premises severely

and to discuss them critically. This step is crucial in order to avoid the trilemma of justification in our discussion of general hypotheses, of test statement, and of critical rationalism itself.

## References

- Albert, Hans. 1985. *Treatise on critical reason*. Princeton, NJ: Princeton University Press.
- Andersson, Gunnar. 1982. Naive and critical falsificationism. In *In pursuit of truth: Essays on the philosophy of Karl Popper on the occasion of his 80th birthday*, ed. Paul Levinson, pp. 50–63. Atlantic Highlands, NJ/Brighton, Sussex: Humanities/Harvester.
- Andersson, Gunnar. 1994. *Criticism and the history of science: Kuhn's, Lakatos's and Feyerabend's criticisms of critical rationalism*. Leiden, Holland: E.J. Brill.
- Andersson, G. 2007. Test statements and experience. In *Karl Popper: A centenary assessment, selected papers from Karl Popper 2002*, eds. Ian Jarvie, Karl Milford and David Miller, Vol. 2, pp. 177–183. Aldershot: Ashgate.
- Armstrong, Karen. 2006. *The great transformation: The beginning of our religious traditions*. New York: Alfred A. Knopf.
- Bartley, William Warren, III. 1984. *The retreat to commitment*. La Salle, IL: Open Court.
- Davidson, D. 1986. Empirical content. In *Truth and interpretation: Perspectives on the philosophy of Donald Davidson*, ed. Ernest LePore, pp. 320–332. Oxford: Basil Blackwell.
- Hume, David. 1967. (1739–1740) *A treatise of human nature*. Oxford: Clarendon.
- Johansson, Ingvar. 1975. *A critique of Karl Popper's methodology*. Stockholm: Esselte Studium/Akademiförlaget.
- Miller, David. 1994. *Critical rationalism: A restatement and defence*. Chicago, IL: Open Court.
- Miller, David. 2006. *Out of error: Further essays on critical rationalism*. Aldershot: Ashgate.
- Miller, David. 2007. Overcoming the justificationist addiction. <http://www2.warwick.ac.uk/fac/soc/philosophy/staff/miller/wroclaw2a.pdf>. Accessed June 11, 2008.
- Musgrave, Alan. 1993. *Common sense, science, and scepticism: A historical introduction to the theory of knowledge*. Cambridge: Cambridge University Press.
- Musgrave, Alan. 1999. *Essays on realism and rationalism*. Amsterdam: Rodopi.
- Popper, Karl Raimund 1959. *The logic of scientific discovery*. London: Hutchinson.
- Popper, Karl Raimund. 1966. *The open society and its enemies*. London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1974. Replies to my critics. In *The philosophy of Karl Popper*, ed. Paul Arthur Schilpp, Vol. 2, pp. 961–1197. La Salle, IL: Open Court.
- Popper, Karl Raimund. 1992. *In search of a better world: Lectures and essays from thirty years*. London: Routledge.
- Rowbottom, Darrell Patrick. 2008. Intersubjective corroboration. *Studies in History and Philosophy of Science* 39:124–132.

# *Ratio Negativa* – The Popperian Challenge

Zuzana Parusniková

**Abstract** Popper's negative model of reason, in which reason only discards falsehoods, is highly relevant to the current debates on rationalism. Popper rejects foundationism, identifying reason with the duty and the privilege of grounding (proving or justifying) knowledge claims. His arguments against foundationism, as well as his proposed solution, can be tracked back to the dispute with Hume who, as Popper observes, drew the wrong conclusion from the fact that reason cannot justify knowledge, and proceeded to undermine the authority of reason and its normative function in epistemology. Today, Popper's criticism of Hume can be applied to the attempts of postmodernism to devalue reason. His conception of *ratio negativa* has the potential to uphold the value of reason while, at the same time, acknowledging that postmodernists target an important problem: the Western culture has been, for centuries, under the rule of the foundationist, authoritarian rationalism that should be abandoned.

## Introduction

In Chapter II of his *Realism and the Aim of Science* Popper called himself a “laggard of the Enlightenment”, who defends the fortress of rationalism against the “already overwhelming and still increasing demand for an irrational philosophical messianism à la Heidegger from one side, and for a ‘mathematically exact’ philosophical method from the other” (Popper 1988, p. 177). This description depicts precisely the main thrust of Popper's life-long philosophical endeavour.

The first task that Popper set for himself, namely the fight against irrationalism, is by far the most important. Here, Popper endorsed the pure essence of the Enlightenment and assigned reason a grand mission both in knowledge and in history. Reason (for Popper identical with critical thinking) is the ultimate value and the universal authority constituting our culture; and in regard to it Popper talked about “a common language of reason” and “the rational unity of mankind” (Popper 1980, pp. 239, 225). Reason has the power – through science as its highest form – to

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bring us ever closer to the truth and master Nature. In the domain of society, reason can help to establish ever more civilized (democratic) social order.

Echoing Kant, Popper wrote: “Science is the direct result of the most human of all human endeavours – to liberate ourselves. ... to see more clearly, to understand the world and ourselves, and to act as adult, responsible, and enlightened beings” (Popper 1988, p. 259), and continued on the following page: “(Science) represents our wish to know, our hope of emancipating ourselves from ignorance and narrow-mindedness, from fear and superstition” (*ibid.*, p. 260). Again in accord with Kant, Popper ascribed reason an ethical dimension. To choose reason is a “moral decision” (Popper 1980, p. 232), demonstrating the concern for human condition and the commitment to improve it. By developing and internalizing criticism in our thinking and actions we not only expel dogmatism that hinders the dynamic growth of knowledge, but can lessen suffering, political oppression and injustice.

As Hacohen remarks (Hacohen 2000, p. 69), Popper’s position grew out of the progressive intellectual atmosphere in Vienna – termed *Spätaufklärung* – that developed before the First World War in opposition to various conservative, clerical and nationalist, tendencies. The horrors of the War, followed by the disintegration of the Empire when lives were destabilized and all hitherto valid values undermined, made even more acute the task of reaffirming the Enlightenment ideal with its optimistic ethos. However, more was needed than just dusting off the good old ideal. Having experienced the failure of reason, more precisely the failure of societies in the ‘enlightened’ age to act in a rational, civilized, humane way, rationalism needed to be reformulated to regain its credibility in the new historical circumstances. And of course, after the Second World War, the task was even more difficult.

One way forward was to stipulate much stricter and more precise methodological rules of the rational conduct of mind – rules that would perfectly distinguish between the rational and the irrational. Thus, with the help of a universal language based on logic/mathematics, we can protect ourselves from all the unwanted manifestations of irrationalism, such as dangerous ideologies, totalitarian recipes for a perfect society, lures of mystical insights, etc. Then, reason can claim back its authority. This was the route chosen by most philosophers of the Vienna Circle.

Popper went about it differently, more radically and entirely originally. He proposed to redefine reason itself in a way that was strikingly simple but also strikingly outrageous: reason is to be seen purely as an *agent of destruction of all knowledge claims*. *If the intended destruction does not succeed the proposal under scrutiny may keep its ambition to be true; if it does, the proposal is eliminated as false*. This model of reason significantly changes our traditional understanding of rationalism in two ways. *Firstly*, Popper entirely discarded the search for any tedious, rigorous rules that should define a rational method in knowledge acquisition; this, for him, is not where the rationality of science lies. Discovery is an adventurous creative activity, it is ‘free and cool’, and cannot be controlled by any logical or mathematical standards. In other words, there is no scientific ‘method of discovery’.

As Popper made it clear in the *Logic of Scientific Discovery* (sic), discovery contains irrational elements, intuition; we can get inspired even in an alcoholic *Rausch* and the status of the resulting idea need not be any lower than if it was

conceived during an ascetic systematic study – on the assumption, of course, that it is not just a drunken babble but an intellectually coherent and empirically testable statement. Let's not straitjacket the creative human mind, whether in science or in any other intellectual activity – pleaded Popper in opposition to the efforts to introduce 'mathematical exactness' in methodology, as quoted in the introductory paragraph of this paper. All statements are simply conjectures and their fate is decided only later, in the process of testing.

It is then, in the process of testing when reason comes on the stage and it is here that the *second* change of our understanding of reason occurs. The 'late' appearance of reason is more than compensated by a sweeping dramatic effect that stems from its destructive force. The domain within which reason operates is testing the consequences of our conjectures not in order to "... consolidate our knowledge, but to liquidate it" (Miller 1994, p. 111). By defining reason exclusively in negative terms and thus proposing an entirely new model of *ratio negativa* Popper brought about, in my opinion, a revolution in rationalism and changed the rationalist paradigm. To call himself a "laggard of the Enlightenment" is, therefore, needlessly (and surprisingly) modest – the Enlightenment message gains in his philosophy a new momentum, relevant for the twentieth century and beyond. The relevance of Popper's concept of *ratio negativa* is also due to the fact that the irrational tendencies, whose growing influence in Western philosophy in his time worried him so much, have continued to advance and presently culminate in various forms of postmodernism. If we are to rethink Popper's philosophical legacy in today's world with its problems, *ratio negativa* presents the biggest challenge and inspiration.

Popper's philosophical contribution is all the more valuable because its opposition against postmodernism (and irrationalism in general) is not 'cheap', it is not a hysterical assault led by *a priori* hostility to a different type of discourse (as is often the case with attack from the analytical philosophical community). Popper acknowledged that irrationalist tendencies were deeply rooted in human nature; psychologically, we yearn for safety and assurance and believe that this can be delivered if we appeal to positive evidential support. This attitude can take on various dogmatic and authoritarian forms in the social context where it exploits people's dissatisfactions with the current state of affairs and offers 'guaranteed' recipes for an overall remedy and, ultimately, for an ideal society. Popper was concerned about the two main international events of the twentieth century – the two wars and the totalitarian oppression of half the postwar Europe. Today, new global problems arise, such as environmental threats, the deepening rift between the First and the Third worlds, or, say, the rocket-rise of the internet technologies creating a parallel, alienated virtual reality.

All these issues can (and do) provoke doubtful thoughts about whether reason is at all the right guide to achieving freedom, humanity and happiness, to controlling Nature for the benefit of mankind, and to liberating ourselves through knowledge, as claimed by the Enlightenment. The child-like enchantment by reason and science that was present still at the beginning of the twentieth century is certainly gone. In these circumstances, philosophical reflections undermining the status of reason are understandable, and the task for a rationalist, as Popper well knew, is not merely

to attack the ‘wrong’ solutions but to respond to the problem itself in a way that enables reason to maintain its formative role.

This Popper did in his proposal to eliminate all justificationist elements from rationality. For him, the traditional philosophical definition of reason, in which reason is seen as the agent of justification of knowledge claims, thus guaranteeing the rational (scientific) status of knowledge, is flawed; justification is incompatible with reason. The traditional forms of ‘rationalism’ (termed by him ‘comprehensive or uncritical’) are themselves rationally inconsistent since “neither logical arguments nor experience can establish the rationalist attitude.” (Popper 1980, p. 230). Likewise, it is impossible to provide positive (‘good’) reasons in favour of any knowledge claim: “Justificational argument, leading back to positive reasons, eventually reaches reasons which themselves cannot be justified (otherwise the argument would lead to infinite regress).” (Popper 1988, p. 28). This is a dead end street where “...higher and higher level principles are injected into the system in the desolate hope that they can provide some justification for those lower down.” (Miller 1994, p. 49).

Uncritical rationalism is thus an impostor leading us, by its own internal logic, to irrationalism: “irrationalism is logically superior to uncritical rationalism” (Popper 1980, p. 231). Uncritical rationalism must ‘cheat’ to reach its goal and appeal to some *unquestionable authority that acts as the foundation of rational discourse*; history of philosophy offers numerous examples: clear and distinct ideas for Descartes, *a priori* forms of judgement for Kant, the Absolute Spirit for Hegel, or protocol sentences (atomic data) for the positivist.

This endeavour is entirely without merit for Popper for two reasons. Firstly, it is irrational. Secondly, it does not stimulate the growth of knowledge. It is only when we turn from the *static* domain of establishing a claim to the critical *process* that we can employ our invention, we can progress and discover new problems: in Popper’s words, “the problem of giving positive justifying reasons” should be replaced by “the problem of *critically* discussing hypotheses” (Popper 1985, pp. 22–23). Critical rationalism turns the conception of reason upside down by claiming that, in Miller’s words again, rationalism has been “hooked on justification” and it is time that we “kick the habit” (Miller 1994, p. 49). Kicking the habit implies adopting the “critical (or, if you like, the negative) function of reason” and the recognition that “there is no question of proving or justifying or establishing anything” (Popper 1988, pp. 27, 29).

Popper’s negative model of reason can show great vitality in today’s philosophical disputes about rationality. As I remarked earlier, postmodernism has been, for several decades, the leading movement that undermines the privileged status of reason in Western thought. Critical rationalism has a great potential to reject the postmodern conclusions while, at the same time, admitting that the postmodern arguments against the Western *logocentrism* have substance. The crucial question to ask is: *what* reason do postmodernists put on trial? And despite the diversity and the fuzziness of the postmodern arguments we can say that it is the *foundationist model of reason* – in essence, the same model of ‘reason’ that Popper scrutinized and rejected.

Of course, the postmodern jargon is entirely different from Popper’s. Yet, there is a striking resemblance between Popper’s rejection of the rationality of justification

and the postmodern rejection of the rationality of the discourse of legitimation. For both, these respective strategies are self-defeating since we cannot, in a rationally consistent way, establish some indubitable foundation from which justification (legitimation) can proceed. As a result, as Popper and postmodernist could agree, the Grand meta-narrative of the Enlightenment (based on this foundationist model of reason) loses its credibility and power. This parallel will be explained in detail in “The Dispute with Postmodernism”.

It is obvious that Popper and postmodernists fundamentally differ in the response to this problem. For postmodernists, the ‘legitimation crisis’ indicates the demise of the centuries-long rationalist supremacy. Popper, by contrast, cherishes rationalism and proposes a redefinition of reason in *non-foundationist terms*, as *ratio negativa*. I fully agree with Hacohen that “Popper provided the most viable response to ‘poststructuralist’ dilemmas among the disputants: a modified conventionalist non-foundationist philosophy that safeguarded rationalism, but skirted the dangers of absolutism” (Hacohen 2000, p. 263). Let’s, then, take a closer look at his proposal.

## Hume’s Problem

Popper’s dispute with Hume is a good starting point. The impossibility of establishing the foundations of reason led Hume to reject the authority of reason and its normative function in epistemology, and to turn to the inductivist psychology of knowledge. On Hume’s problem, Popper illustrated the ‘wrong’ kind of non-foundationism which throws out the baby along with the bath water.

In this dispute, Popper defined his own non-foundationism. Popper never discussed postmodernism since it gained its strength in the 70th and 80th when he reached old age and his research was already pointed in other directions. Popper’s arguments against Hume will nevertheless be helpful in our attempt to use his non-foundationism as an alternative to the postmodern non-foundationist – but also anti-rationalist – stance. This will enable us to rethink Popper ideas beyond his own explicit words.

Hume’s problem unfolds from what Popper called the logical problem of induction. As is well known, Popper formulated this problem as follows: “Are we justified in reasoning from [repeated] instances of which we have experience to other instances [conclusions] of which we have no experience?” (Popper 1979, p. 4; Hume 1981, p. 96). Popper fully agreed with Hume that the answer is “No”. In other words, “we cannot validly reason from the known to the unknown” (Popper 1988, p. 31). The recognition that induction does not have a rational foundation has far-reaching consequences and Popper concluded that:

- The truth of a universal theory cannot be justified by empirical reasons.
- The validity of inductive inferences (including those that are merely probabilistic) cannot be justified.
- The belief in regularity cannot be justified.

Up to this point, there is an agreement between Hume and Popper. They both could summarize the criticism of induction in a statement that *reason cannot justify knowledge (science)*. From here, there are two possible ways to go; we must either reject the necessity of justification for rationality, or accept that knowledge is not rational. Hume took the second route. To make things worse, he simultaneously claimed that although induction is rationally indefensible, we in fact learn by induction. This clash between logic/reason and psychology led Hume, in Popper's view, to irrationalism; he "destroyed both empiricism and rationalism", and was "believer in an irrationalist epistemology" (Popper 1979, pp. 90, 4).

In sum, Hume left critical rationalists with a two-fold problem. The first one concerns Hume's *identification of reason and justification*. The second is the *reduction of epistemology to psychology*, by which the normative role of reason, and of rationalist philosophers, is made redundant. Popper himself discussed the psychological problem at great length; I shall not go through his arguments here since for our main issue of foundationism, it is the problem of justification that is crucial. But before getting to that, I want to address Popper's accusation of Hume for irrationalism. The harshness of Popper's judgment reveals a deeper level of disagreement than he explicitly admits.

In his words, Hume unmasked our knowledge "as being not only of the nature of belief, but of rationally indefensible belief – of an *irrational faith*" (Popper 1979, p. 5). Hume would certainly not agree with this conclusion. Our beliefs and natural instincts, including cognitive instincts, are *blind* but at the same time, Nature who gave them to us is *wise* (Hume 1777, p. 55, Sect. 45 and p. 151, Sect. 118). True, if we submit these instincts to rational scrutiny we will end in scepticism and "this sceptical doubt, both with respect to reason and the senses, is a malady, which can never be radically cur'd." (Hume 1740, p. 218). But in common life, we set these doubts aside and go along with the powerful current of Nature; she "cures me of this philosophical melancholy and delirium, either by relaxing this bent of mind or by some avocation..." (*ibid.*, p. 269). This is, of course, no solution to sceptical doubts by arguments or reflection (that is not possible) but simply ignoring them and trusting our natural inclinations and common sense; is this an irrational despair or an epistemological paralysis? Certainly not in Hume's philosophical system and calling Hume an irrationalist may be, therefore, too strong.

It is, however, also understandable. Popper, like Kant or Russell, represents the rationalist position, in which it is unacceptable for knowledge and science to be left to natural instincts. The Enlightenment vision, shared by these philosophers, insists that the only philosophically adequate perspective must present reason as the universal value with a normative role in knowledge. Therefore, "...this appeal (to ordinary common sense-Z.P.) is nothing other than a call to the judgement of the multitude, applause at which the philosopher blushes, but at which the popular wag becomes triumphant and defiant." (Kant 1783, p. 10). But since the refuge in Nature is not worthy of a philosopher of the new enlightened age, then the deadly scepticism of reflection becomes overwhelming and the irrationalist charge understandable; using Kant's words again, Hume "deposited his ship on the beach (of scepticism) for safekeeping: where it could lie and rot" (*ibid.*, p. 12).

The clash between the inductivist account of learning and its rational indefensibility results, for the rationalists, in a “schizophrenia” that they perceive as traumatic and expressing a philosophical failure. Hume, however, was a philosopher of a different ‘nature’. He was quite happy about this double perspective: we simply doubt when we must (in a closet) and believe when we must (in company and affairs). He accepted this as the human predicament – it is simply the “whimsical condition of mankind” (Hume 1777, p. 160, Sect. 128) – and its acceptance is also the only way to the achievement of *ataraxia*. Hume, deeply influenced by ancient Pyrrhonism (see Popkin 1979, 1980, and 2003) saw philosophy as a guide to practical goals, to achieving peace of mind. And if “carelessness and in-attention alone can afford us any remedy” (Hume 1740, p. 218), so be it. Popper, who had ‘higher’ philosophical ambitions, couldn’t accept Hume’s acknowledgement of the limitations and the narrow capacity of reason; but even more importantly, he must have felt outraged by Hume’s relaxed, easy-going approach to inconsistencies (‘let them not bother us’).

In the time when most philosophers saw the purpose of philosophy as one of establishing the status of the rational, noetically constructive subject, and formulating, by this procedure, the foundations of modern science, Hume set for himself entirely different goals. Philosophy should help us feel better and live better. In stripping reason of its normative function, he stands aside from the ‘epistemological current’ representing the modern philosophical paradigm. And what is philosophy good for, apart from showing us the road to happiness?

It can serve only two other purposes, neither of them requiring special philosophical insights that other disciplines (psychology, sociology, anthropology, etc.) cannot provide: firstly, it can describe how our mind naturally operates; Hume called this “mental geography” (Hume 1777, p. 13, Sect. 8). Secondly, it can observe and document human behaviour in common life: “I can only observe what is commonly done” (Hume 1740, p. 268), preferably in an entertaining, easily accessible form, suitable for a dinner conversation. The main concern is to keep this activity agreeable and pleasurable. This approach is not only a world apart from Popper’s philosophical goals, but also from his Protestant, almost ascetic attitude to philosophical work. If he registered this ‘hedonist’ flavour in Hume’s personal and philosophical *credo*, that might have further aggravated his lack of sympathy for Hume’s philosophy.

Anyway, it is the difference between the two philosophers in the respective views on justification that can best explain Popper’s unique non-foundationist model of reason. As Popper made it clear, the term foundation, as used by Hume, corresponds to his phrase “justification by positive reasons” (Popper 1988, p. 22). Popper criticized Hume for not having been able to consider the possibility of *separating reason and justification*. Therefore, the logical invalidity of induction was taken by him as the ultimate bad news, as an insoluble misery leading to (theoretically) incurable scepticism. Popper, by contrast, proposed exactly this separation. He admitted that “few will be shocked to hear that their conjecture is ‘entirely without foundation’. To have some ‘foundation’, or justification, may be important for a belief but it is not the kind of thing we should require for a conjecture or a hypothesis...” (*ibid.*).



Foundationism is thus based on several wrong philosophical assumptions that we should get rid off:

The first false idea is that we must justify our knowledge, or our theories, by positive reasons, that is, by reasons capable of establishing them, or at least of making them highly probable; at any rate, by better reasons than that they have so far withstood criticism. This idea implies, I suggest, that we must appeal to some ultimate or authoritative source of true knowledge; which still leaves open the character of that authority – whether it is human, like observation or reason, or super-human. (Popper 1985, p. 29)

When Popper links foundationism to some super-human authority, it is quite easy to agree with him and with his demand to reject it since such an authority invites association with irrationalism and metaphysics. But Popper's definition of foundationism is much more radical; translated into the everyday language, it is "the attitude of a person who says 'I am not prepared to accept anything that cannot be defended by means of argument or experience'" (Popper 1980, p. 230). In this formulation, non-foundationism reveals the uncompromising rejection of any kind of positive defence, any kind of support by 'good reasons'. To a person who is not familiar with critical rationalism, this can seem almost insane: how else can we defend our views, our proposals, than by supporting them by sensible arguments or favourable evidence? Yet, Popper's conception of *ratio negativa* claims that only the total, radical ban on justification can save rationalism. Therefore, critical rationalism cannot be described merely as a promotion of criticism but as a new model of reason constituted by, and only by, the imperative of falsification.

However, one should also avoid simplifying conclusions. The fact that nothing can be rationally defended does not imply that 'anything goes'; in Popper's view, "we can give some reasons for *proposing* a hypothesis, and for submitting it to critical discussion" (Popper 1988, p. 22). These reasons may include, for instance, its potential to solve an urgent problem, to attain more criticizable theories, etc. But, as Popper stressed, "it would not be a reason for supposing it to be true. It may not even be a reason for accepting it tentatively or even for preferring it" (*ibid.*), or for believing it will be successful. To conclude, "*there can be no good reasons* in this sense" (Popper 1979, p. 22). What Popper hoped to achieve is, to use Kant's famous quote again, to unload the superfluous burden of justification from the ship of epistemology; then the ship does not have to be left to rot on the beach of (pessimistic) scepticism but can happily sail away with a new vigour.

Whether Popper succeeded in achieving all these ambitious goals is open to discussion. But his ideas are certainly challenging and I would like to suggest at least one area in which *ratio negativa* could make an important contribution.

## The Dispute with Postmodernism

To show the relevance of critical rationalism to the postmodern disputes about rationality is a tricky task. Postmodernism is an antithesis to all that Popper believed in – it undermines the status of reason, revels in relativism, and indulges

in dark and obscure language. I do not intend to deny any of this. Yet, there is a link between the two philosophical positions that is, in my view, at least as interesting as their differences. Popper and postmodernism provide completely *different responses to a similar, acutely felt problem* – the problem of the authoritarian, foundationist model of reason that has dominated the whole Western thought since ancient Greece (Parusnikova 2006).

Popper argued, as I already demonstrated, that the traditional misconception of reason lies in the unfortunate assumption that epistemological claims must be justified. Postmodernism considers foundationism in a broader metaphysical sense, but there is an analogy with Popper's approach that I want to address.

Postmodernism uses the term *logocentrism* to describe how the Western tradition associated rationality and legitimation claims. Logocentrism is conceived as a position that enables to establish legitimacy of a certain hierarchy of ethical, epistemological and political values and gives those values, grounded in reason, a privileged status and authority. In other words, science, mysticism, intuition, faith, clarity or obscurity, just to mention a few examples, are not just 'stories among stories'; some are superior to others *for good reasons*, and it is *Logos* that is the ultimate, unquestionable source of validation that establishes the hierarchical structure. At the same time, the need for such a foundation is being constantly reaffirmed, since the discourse of legitimation appears as the only rational strategy.

Despite the specific focus of critical rationalism on epistemology, in contrast to the wider focus of postmodernism on metaphysics and its embodiment in language, both agree that (1) our cultural and intellectual 'space' is constituted by *Logos*, and (2) *Logos*, in this tradition, expresses the *need for grounding* that is shared by Western philosophy. Postmodernists term this tradition 'logocentrism'; Popper called it 'uncritical rationalism'. And further, (3) they both expose the foundationist conception of *Logos* as internally inconsistent and, therefore, self-defeating. No need to add, that their respective responses to this trouble are very different; but we shall first look at the similarities.

As for (1), Popper takes this fact for granted and only shows his concern about constant threats to rationalism by various historical forms of irrationalism. Postmodernists describe in more detail how *Logos* presents a center around which our world is structured. *Logos* gives the world order and makes it intelligible to us. As Derrida remarks, "the notion of a structure lacking any center represents the unthinkable itself" (Derrida 1978, p. 279) – the ungraspable amorphousness that could not be at all accessed by our intellect. The order of *Logos* as saving us from the alien, fearful Chaos is well expressed by Hesiod; he describes chaos (chasm) as an unidentified 'space' between the Earth and Tartarus:

This is where the sources and limits of the dark Earth are, and of murky Tartarus, of the barren sea, and of the starry sky, of everything, one after another, dismal and dank, that even the Gods shudder at ... A vast chasm, whose floor a man would not reach in a whole year if once he got inside the gates, but stormwind upon terrible stormwind would carry him hither and thither. It is terrible for the immortal Gods as well, this monstrosity; and there stands the fearful house of gloomy Night, shrouded in clouds of blackness (Hesiod app. 700 BC, p. 63, lines 736–745)



*Logos* imprints order on the world that we inhabit and thus creates familiar, intelligible life space. An example given by Foucault (Foucault 1970, p. xv) could be closer to 'our' problem of epistemological foundationism. By a very complex reference route, he quotes from an alleged Chinese Encyclopaedia named *Celestial Emporium of Benevolent Knowledge*; Foucault takes this example from a story by Borges, which further refers to a German sinologist Franz Kuhn, who apparently translated the Encyclopaedia. There were long disputes about the authenticity of the work, not so surprising in time before the Internet, with the works by Kuhn allegedly displaced during the war. Now it has been proved to be a fake. The 'Encyclopaedia' offers the following taxonomy of animals:

1. Belonging to the Emperor
2. Embalmed
3. Tame
4. Suckling pigs
5. Sirens
6. Fabulous
7. Stray dogs
8. Included in the present classification
9. Those that tremble as if they were frenzied
10. Innumerable ones
11. Drawn with a very fine camelhair brush
12. *Et cetera*
13. Having just broken a water pitcher
14. That from a long way off look like flies

Since this taxonomy lacks the structure of *Logos* it seems crazy. Borges often plays with this theme in his stories and shows that without the rational structure, our 'familiar' world collapses. It becomes a labyrinth of traces that never click together to reveal to us the main 'building plan', to provide us with the access code.

As for (2), critical rationalism and postmodernism could stand side by side in accusing uncritical rationalism/logocentrism of demanding that following 'the path of *Logos*' imposes the duty to justify/legitimate the discourses (knowledge claims) that are to be accepted and to have authority over others.

Let's look at Derrida, a prominent post-structuralist philosopher (and a very unlikely ally of Popper). He argues that the structuring work of *Logos* has been essentially linked to finding some primordial principle as a source of truth – truth, then, can be proved and 'pinned down'; in Derrida's terminology 'made present':

The history of metaphysics, like the history of the West ... must be thought of ... as a linked chain of determinations of the center. Its matrix... is the determination of Being as presence in all senses of this word. It could be shown that all the names related to fundamentals, to principles or to the center have always designated an invariable presence - *eidos*, *arche*, *telos*, *energeia*, *ousia*,... *aletheia*, transcendentality, consciousness, God, Man, etc. ... By orienting and organizing the coherence of the system, the center of a structure permits the play of its elements inside the total form. (Derrida 1978, pp. 279–280).

The ambition of logocentrism is to *make Being present*, to *present* it in front of our eyes, making it transparent to the intelligent (rational, logical) insight. Therefore, Derrida terms Western philosophy the *metaphysics of presence* – another term for what we have so far described as foundationism or *logocentrism*. Since we can, via philosophy, grasp those foundations (quoted by Derrida in various historical forms) we can also legitimate the structures (systems of knowledge, values, beliefs) built upon them.

At first sight, this description of foundationism may seem far away from Popper's 'narrower' focus on the justificationist strategies in inductive reasoning. Yet, the basic drive behind all forms of foundationism is of the same nature, and manifests itself in other foundationist incarnations. The need for stable, indubitable and in the spirit of *Logos* validated foundations may express itself as the need for the essence of Being or, by the same logic, as the need for the principle of induction. In both cases, what we hope to achieve is the possibility of halting the infinite chain of ever higher-order principles of justification, and reaching some firm ground, from which we could 'spring' forth and reverse the direction of the validation process; we could, then, legitimately justify the discourses 'lower down'.

But this goal cannot be reached, as stated in (3). Why not? Popper's arguments concerning the infinite regress do not need to be repeated, but let's look at post-modernism. I shall stay with Derrida who analyzes the foundationist thrust of logocentrism on the structure of our language, namely on the process of textual signification (inspired, no doubt, by Heidegger: by his interpretation of *Logos* as *Rede*, and by his 'onto-theological' analysis of language).

He draws on the structuralist theory of meaning (Saussure) in which meaning is a result of an arbitrary relation between a signifier and a signified. The meaning of a sign is constituted within a structure of differences, against the background of what the sign is not. However, Saussure's conception of structure is too closed and static for Derrida. It assumes that, though arbitrary, the relation between a signifier and a signified can be determined (made present) – the structure within which meaning is generated is organized and can be mastered. Saussure is, for Derrida, just another example of the metaphysics of presence led by the need to "decipher a truth or an origin which escapes the play (of *différance* – Z.P.) ... by the dream of the full presence, the reassuring foundation, the origin and the end of play" (*ibid.*, p. 292).

Saussure, like other representatives of logocentrism, is driven by the hope of finally tracking down all the 'missing bits' of a sign, as in a puzzle that can eventually be completed. The desired center of the structure, or origin, is labelled by Derrida as the 'transcendental signified', '*arché*-sign', or '*arché*-trace'. The transcendental signified – by providing a center that escapes, indeed orders, the play of signification – enables us to get a grip on the world, i.e. on a text, and to decipher (make present) the truth. "Thus it has always been thought", argues Derrida, "that the center constituted that very thing within a structure which while governing the structure, escapes structurality" (*ibid.*, p. 279).

In contrast to Saussure, he argues that a signified forever leads to another signifier, and works with a never-ending delay (deferral) of meaning in time. Presence (that is, truth) is unattainable as there is always something missing from

the sign and it is constituted not only within a network of differences but also by a trace of the absent – and what is necessary to trace further. Derrida declares “... the structural impossibility of limiting this network, of putting an edge on its weave, of tracing a margin that would not be a new mark” (Derrida 1981, p. 40). Presence, truth, always slips away and meaning is deferred *ad infinitum*. The deferral within the network of differences is expressed in Derrida’s famous term ‘*différance*’, the play of ‘hide-and-seek’ in an infinite labyrinth of traces of signs. The essence of *Logos* – presence – is defeated by its own weapons. Against the hopes of logocentrism, we are faced with the predicament of *absence*.

It is perhaps time that we sobered up and turned back to critical rationalism. Are these Derridean speculations about signs and traces at all relevant to it? Obviously, my answer is affirmative. I see an important parallel between the *infinite regress* in searching for some ultimate epistemological foundation, as argued by Popper, and the *infinite deferral* of meaning in the search for the *primum signatum*, as argued by Derrida. Both show that this search, traditionally set as the highest task for philosophy, is futile. Analogically to Derrida, Popper made it clear that it is impossible to halt the infinite regress in our hunt for the proof of truth (or for inconclusive proof or probability); the absence of any epistemological foundation has very much the same effect as the absence or the *arché*-sign. If we use the expression ‘*arché*’ for ‘foundations’, we could call both Popper and Derrida an-archeist.

Likewise, the Derridean impossibility to make meaning present corresponds to Popper’s argument that truth cannot be made manifest. The lack of courage in admitting this has one important consequence that both philosophers are alarmed about. It entails strongly authoritarian implications. “The theory that truth is manifest – that it is there for everyone to see it, if only he wants to see it – this theory is the basis of almost every kind of fanaticism” (Popper 1985, p. 8). If we believe that in a rational discourse, views can, indeed must be, positively justified (legitimated) we then feel vindicated in privileging ‘our truth’ and in suppressing other views. And while in epistemology, the worst manifestations of this authoritarian streak of justificationism is dogmatic thinking, in social life, the dangers are greater. The suppression may target not just different views but also the individuals who hold them; as Popper comments sarcastically, “only the most depraved wickedness can refuse to see the manifest truth” (*ibid.*).

The concern about the oppression exerted by logocentrism is common to all types of postmodernism. In fact, it is the main ‘moral’ accusation against the Western rationality. The belief that we possess some ultimate foundation results in an attitude in which we claim the legitimacy to privilege certain discourses and marginalize others. In other words, we force a singular scheme of explanation on to the multiplicity of life-forms (whether we talk about the world or about texts).

This attitude, sometimes described as Western cultural colonialism, gains in postmodernism many ideological and political connotations under the big umbrella of ‘the rights of the Other’ (aboriginal cultures, gay rights, feminist movements, etc.). Summarized by Lyotard, the last two centuries provided excellent examples of historicism in the Hegelian idea of the Spirit unfolding itself in history, and in the Marxist theory of the liberation of mankind via the elimination of capitalist

exploitation. We experienced “as much terror as we can take. We paid a high price for the nostalgia for the whole and the one” (Lyotard 1984, pp. 81–82).

The shared concerns between critical rationalism and postmodernism show several things. Firstly, critical rationalism is up to the task of addressing the ultimate philosophical problems of today. The issue of Western foundationism is acknowledged as the crucial problem to be faced and solved. Secondly, because of this acknowledgement can critical rationalism offer a strong alternative to the postmodern dethroning of reason? This alternative is philosophically unique: it proposes a new, negative, definition of reason that is free of the foundationist charges. The conception of *ratio negativa* questions the very premise that postmodernists take for granted – namely that reason is by its own nature foundationist.

Since postmodernism sees the link between reason and foundationism as inevitable it has to condemn the traditional status of reason as a universal norm and value; we can see a similar line of philosophizing in the case of Hume. But unlike Hume, who tried to ‘sweep scepticism under the carpet of Nature’, postmodernists emphasize the dead-end of rationalist universalism. Without refuge in the wisdom of Nature, they end up in relativism, in which only local and internal standards apply to different discourses. Or, as in poststructuralism, in the vain search for the *primum signatum*, we are destined both to destruct and construct (*deconstruct*) the logocentric language since we have no other means of expression. Put it more simply, we are aware that we cannot offer legitimate reason but we cannot do without it, including the belief that it is legitimate; therefore, we simultaneously undermine and reaffirm logocentrism.

The paradox of deconstructing the language of logocentrism but not being able to escape it makes the poststructuralist position extremely difficult to understand. The play of *différance* is itself a product of *différance* and operates within the deconstructed system since we cannot step outside a language that has a logocentric drive. This clash creates a permanent tension in which the play of *différance* fulfils a ‘dissident’ role of displaying the instability of language and the indeterminacy of meaning: *différance* “unceasingly dislocates itself in a chain of differing and deferring substitutions” (Derrida 1982, p. 26).

Critical rationalism, however, can reject these postmodern (or post-structuralist) conclusions without sidetracking or skipping the crucial problem itself. The problem, to repeat again, is the foundationist ‘misconception’ of reason. In opposition to that, Popper proposed the methodological (rational) imperative of falsification. According to this imperative, rationality consists merely of the process of criticism and does not attempt to prove its own superiority or to validate other knowledge.

Popper rejects the whole concept of striving for certainty, or probability, or reliability in the assessment of our guesses – this is a sign of foundationism that has to be condemned, as postmodernism would agree. But, as mentioned above, *Popper does not see this principle as threatening reason but as strengthening reason*. The revolutionary negative, destructive definition of reason changed the orientation of rational actions *from proving to seeking, from grounding to discovering*. The wisdom that Socrates teaches us is that “argument rarely settles a question, although it is the only means for learning” (Popper 1980, p. 227).

As a result, we never cross the land of guesses. All we can do is to classify certain guesses as true with the knowledge that this classification is tentative and won't last forever. Since there can be no good reason either for proving or for disproving any conjecture, uncertainty marks the terrain of rational inquiry and forms our ultimate cognitive predicament. In Hacohen's words, Popper's orientation to the negative powers of reason consisting in error-elimination instead of any positive proof marked an epistemological revolution that redefined the methods and goals for science: "The accepted statements remained without conclusive verification, or foundation... Scientists did not ground statements" (Hacohen 2000, p. 230). In Popper's account, rational method allows only expulsion procedures but no confirming proofs of theories, and experience "provides no positive support for them, provides no inductive lift, leaves them floating in the ocean of uncertainty" (Watkins 1984, pp. 353–354).

This non-foundationist conception of reason as criticism operating within the ocean of uncertainties resembles Derrida's conception of *différance* as an open dynamic process of signification with no *primum signatum* as a center. But unlike Derrida, Popper does not view this predicament as shattering for rationality. Quite the reverse: he invites us to welcome and embrace criticism and conjecturality as the true home of reason. Not only are uncertainty and destructive falsification rational (and only *they* are rational) but this new view on rationality is exciting too. It encourages bold guessing, conflicts and disagreements, and enhances the culture of criticism and tolerance.

Thus Popper can be seen as both *less and more radical* in his reflections on reason than postmodernism. He is less radical because he does not undermine the value of rationalism; he is more radical because he redefines reason in non-foundationist terms. In the first sense, he is a successor to the philosophical heritage of Enlightenment and endorses its confidence in the liberating mission of reason. Reason is the right guide to progress, can bring mankind closer to the truth, and can help to establish a better social order.

But, to repeat, rationalism cannot be rationally justified – nothing can. For Popper, reason is the universal foundation of progress and freedom but is accepted as a matter of faith – we must have "faith in reason" (Popper 1980, p. 231); it is not, however, a *blind* faith (Popper's own expression – "irrational faith" – is most unfortunate) but a decision to choose the path that has a historical record of causing less damage than irrationalism. "Rationalism", says Popper "is linked up with the recognition of the necessity of social institutions to protect freedom of criticism, freedom of thought, and thus the freedom of men" (*ibid.*, p. 238). In contrast to that, irrationalism (with its emphasis on emotion and passion), leads to "an appeal to violence and brutal force as the ultimate arbiter in any dispute" (*ibid.*, p. 234).

Popper regards reason as the foundation of our Western culture, foundation that should be cherished. But at the same time, he breaks with foundationism even more radically than postmodernism does. He turns away from the "justified-true-belief-science" tradition that was adopted from the previous "justified-true-belief-religion, Christianity" (Bartley 1984, p. 173) and offers a new philosophical perspective on rationality. Popper can accept and endorse the *authority of reason* because he rejects the *authoritarian status of reason*. By his redefinition of reason, Popper denies the need to sabotage the authority of reason as postmodernism does.

*Ratio negativa* represents an original philosophical vision, relevant to the debates on the modern, authoritarian model of reason. Due to its non-foundationalist, purely destructive nature, it enables us to uphold the value of reason against the postmodern charges and to offer an intriguing alternative. The main message – that we should not ‘cut the branch we are sitting on’, namely the philosophical heritage of *Logos* – deserves to be considered seriously, certainly more than has been considered so far.

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## References

- Bartley, III, William Warren. 1984. *The Retreat to Commitment*. La Salle, IL: Open Court.
- Derrida, Jacques. 1978. Structure, Sign and Play. Transl. A. Bass. In *Writing and Difference*, Jacques Derrida, 278–295. London/Henley: Routledge & Kegan Paul.
- Derrida, Jacques. 1981. Transl. A. Bass. *Positions*. Chicago, IL: University of Chicago Press.
- Derrida, Jacques. 1982. Différance. Transl. A. Bass. In *Margins of Philosophy*, Jacques Derrida, 1–29. Hemel Hempstead: Harvester Wheatsheaf.
- Foucault, Michel. 1970. *The Order of Things, Preface*. New York: Pantheon.
- Hacohen, Malachi Haim. 2000. *Karl Popper. The Formative Years, 1902 - 1945*. Cambridge: Cambridge University Press.
- Hesiod. App. 700 BC (2007). *Theogony*. Volume I. Loeb Classical Library 57. Transl. G.W. Most. Cambridge, MA: Harvard University Press.
- Hume, David. 1740 (1981). *A Treatise of Human Nature*. Ed. by L.A. Selby-Bigge. Oxford: Clarendon.
- Hume, David. 1777 (2005). *Enquires Concerning Human Understanding and Concerning the Principles of Morals*. Ed. by L.A. Selby-Bigge. Oxford: Clarendon.
- Kant, Immanuel. 1783 (1997). *Prolegomena to Any Future Metaphysics: That Will Be Able to Come Forward as Science*. Transl. G. Hatfield. Cambridge: Cambridge University Press.
- Lyotard, Jean-François. 1984. *The Postmodern Condition: A Report on Knowledge*. Transl. G. Bennington and B. Massumi. Minneapolis, MN: University of Minnesota Press.
- Miller, David. 1994. *Critical Rationalism. A Restatement and Defence*. Chicago/La Salle, IL: Open Court.
- Parusnikova, Zuzana. 2006. Popper and Postmodernism. Similar Targets, Different Strategies. *Learning for Democracy*, 1: 7–30.
- Popkin, Richard Henry. 1979. *The History of Scepticism from Erasmus to Spinoza*. Berkeley, CA: University of California Press.
- Popkin, Richard Henry. 1980. *The High Road to Pyrrhonism*. San Diego, CA: Austin Hill Press.
- Popkin, Richard Henry. 2003. *The History of Scepticism: From Savonarola to Bayle*. Oxford: Oxford University Press.
- Popper, Karl Raimund. 1979. (First edition 1972) *Objective Knowledge*. Oxford: Clarendon.
- Popper, Karl Raimund. 1980. (First edition 1945) *Open Society and Its Enemies*, vol. II. London/Henley: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1985. (First edition 1963) *Conjectures and Refutations*. London/Henley: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1988. (First edition 1983) *Realism and the Aim of Science*. Ed. by W.W. Bartley III. London: Hutchinson.
- Watkins, John. 1984. *Science and Scepticism*. Princeton, NJ: Princeton University Press.

# “Why, and to What Extent, May a False Hypothesis Yield the Truth?”<sup>1</sup>

Stefano Gattei

*The whole importance of the refutation of received opinion, or of the best scientific opinion or idea or proposal, is just this: refutation opens the road to innovation. Nothing is more potent heuristic than refutation. Nothing is more conducive to progress than criticism of the current situation; nothing is more likely to herald the new than discontent with the old. Criticism is liberation, the positive power of negative thinking.*

Joseph Agassi

**Abstract** Some of Kepler’s works seem very different in character. His youthful *Mysterium cosmographicum* (1596) argues for heliocentrism on the basis of meta-physical, astronomical, astrological, numerological and architectonic principles. By contrast, *Astronomia nova* (1609) is far more tightly argued on the basis of only a few dynamical principles. In the eyes of many, such a contrast embodies a transition from Renaissance to early modern science. I suggest that Karl Popper’s fallibilist and piecemeal approach, and especially his theory of errors, might prove extremely helpful in resolving such alleged tension. By abandoning the perspective of the inductivist philosophy of science, which is forced by its own standards to portray Kepler as a “sleepwalker”, I focus on the method he followed: he never hesitated to discuss his own intellectual journey, offering a rational reconstruction of the series of false starts, blind alleys and failures he encountered. The critical dialogue he managed to establish in private correspondence with fellow astronomers he later transplanted into his printed works, whose structure closely resembles that of a dialogue, however implicit.

Whereas Galileo is often depicted as the “father of modern science”, his contemporary Johannes Kepler is generally regarded as a transition figure between the Middle Ages and the Renaissance. This is particularly due to Kepler’s overall *a priori* approach to science, which apparently conflicts with Galileo’s – alleged – *a posteriori* procedures (as exemplified, for instance, in the pages of the *Sidereus*

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*nuncius*, 1610): while the former belonged to the Platonic–Pythagorean tradition, the latter managed to free himself from that heavy burden and became (through the clash with Roberto Bellarmino) the chief representative of the new scientific mentality.

Several studies have shown the extent to which this oversimplification is mistaken.<sup>2</sup> However, soaked in the preceding tradition, Kepler was a highly sophisticated and self-critical astronomer: he never got tired of submitting his bold hypotheses to ingenious and critical tests, examining their consequences in the light of the best available evidence and seeking others' help in the search for truth (i.e., in his effort to grasp God's design). It was this critical and self-critical attitude that enabled him to make his great contributions to science. As Karl Popper noticed,

It was not so much the accumulation of observations by Tycho as the critical rejection of many conjectures by Kepler, Descartes, and others, culminating in Newton's mechanics and its subsequent critical examination, which ultimately persuaded everybody that a great step had been made towards the truth. (Popper 1983, p. 59)

Kepler firmly believed in criticism as the very engine of the growth of knowledge. He was persuaded that being rational equals to following the best argument in a critical discussion. He realized how much progress can be achieved by submitting even our best hypotheses to searching critical argument – how important mistakes are, if only we realize that we can learn from them.

## **Interpretations of Kepler's Narrative: From Stream-of-Consciousness to Purely Rhetorical Argumentation**

In the 1937 edition of *Astronomia nova* as volume 3 of Johannes Kepler's *Gesammelte Werke*, Max Caspar, the twentieth century's leading Kepler scholar, declared:

Regardless of all its wrong paths and detours, the internal structure of the work, which unveils itself upon deeper consideration, is dictated by strict logic, and is accomplished clearly in a dramatic step-by-step process. (Kepler 1937–2002, Vol. 3, p. 439)

While editing the book, Caspar knew and described the surviving manuscripts. He compared them with the published text and realized the extent to which they differed. However, Caspar did not regard such variance as a relevant and worth-pursuing issue. On the contrary, it became the very reason for ignoring the manuscripts and portraying Kepler's published text as a sort of rough report of the path he followed:

Kepler himself does not present finished results but rather the story of the discovery of his results, and in fact on the broadest basis, it would not do to draw the work out even further by taking up drafts. (Kepler 1937–2002, Vol. 3, p. 446)

If Caspar failed to recognize the distinction between the *Astronomia nova* as history and as argument, Arthur Koestler did no better. Drawing upon Caspar's



biography of Kepler, in *The Sleepwalkers* he depicted Kepler’s narrative as a valuable example of the irrationality of scientific discovery:

Fortunately, [Kepler] did not cover up his tracks, as Copernicus, Galileo and Newton did, who confront us with the result of their labours, and keep us guessing how they arrived at it. Kepler was incapable of exposing his ideas methodically, text-book fashion; he had to describe them in the order they came to him, including all the errors, detours, and the traps into which he had fallen. The *New Astronomy* is written in an unacademic, bubbling baroque style, personal, intimate, and often exasperating. But it is a unique revelation of the ways in which the creative mind works. (Koestler 1959, p. 314)<sup>3</sup>

Alexandre Koyré, too, one of the leading historians of science of the past century, ultimately understood the narrative of *Astronomia nova* as a reflection of the nature of Kepler’s mind:

[...] Kepler does not restrict himself to setting forth the results, as did Copernicus and Newton: he relates at the time, intentionally as he did in the *Mysterium cosmographicum*, the development of his thought, his efforts, and his setbacks[:]; Kepler’s mind was so constituted that he was unable to find the way to truth without first having explored all the paths leading into error [...]. (Koyré 1973, pp. 165–166)<sup>4</sup>

The structure of *Astronomia nova* has thus come to be treated largely as an innate production of Kepler’s mind rather than a purposeful device. Seminal articles on Kepler’s methodology – such as the pioneering works by Curtis Wilson and Eric Aiton in the late 1960s<sup>5</sup> – although occasionally referring to letters, came to rely heavily on the text of *Astronomia nova* as a true account of Kepler’s work; when correspondence is incorporated at all, it is to elucidate the methodology of *Astronomia nova*.

The breakthrough for contemporary historiography of Kepler’s work came with the publication of Bruce Stephenson’s *Kepler’s Physical Astronomy*, in 1987. As opposed to other scholars – with the exception of Koyré – who focussed on those aspects of Kepler’s work that are considered significant today (namely, the ellipse and the area laws), Stephenson understood physical astronomy to be the central feature of Kepler’s work. He described the development in the *Mysterium cosmographicum* (1596) through the *Epitomes astronomiae copernicanae* (1618–1621) and showed how the ellipse and the area laws are of significance only to the extent that they are supported by Kepler’s physical ideas. In his groundbreaking work, Stephenson also noted that

This profoundly original work has been portrayed as a straightforward account of converging approximations, and has been portrayed as an account of gropings in the dark. Because of the book’s almost confessional style, recounting failures and false trails along with successes, it has in most cases been accepted as a straightforward record of Kepler’s work. It is none of these things. The book was written and [...] rewritten carefully, to persuade a very select audience of trained astronomers that all the planetary theory they knew was wrong, and that Kepler’s new theory was right. The whole of the *Astronomia nova* is one sustained argument [...]. (Stephenson 1987, pp. 2–3)

On the one hand, Stephenson’s claim that *Astronomia nova* “was written and rewritten” was supported by William Donahue, whose analyses revealed that Kepler had worked over at least one chapter of *Astronomia nova* so many times prior to

publication that it scarcely hung together, and that ultimately Kepler resorted to calculating positions with his final finished theory and passing them off as the results of an earlier, observational procedure (Donahue 1988). As the author of the first English translation (1992) of *Astronomia nova* noticed,

the selection of data and arguments, and sometimes the data themselves, were determined by the conclusions. That is, although Kepler often seems to have been chronicling his researches, the *New Astronomy* is actually a carefully constructed argument that skilfully interweaves elements of history and (it should be added) of fiction. Taken as a history, it is often demonstrably false, but Kepler never intended it as history. [...] *Caveat lector!* (Donahue 1992, p. 3)

On the other hand, Stephenson's claim that the published text of *Astronomia nova* is, in Darwin's words, "one sustained argument" was taken up by James Voelkel in his extraordinary study of the composition of Kepler's *magnum opus*. Fully supporting Donahue's view, Voelkel claimed that *Astronomia nova* is "a narrative odyssey through Kepler's development of his astronomical theory" (Voelkel 2001, p. 1).<sup>6</sup> Furthermore, strengthening Donahue's line of interpretation, Voelkel argued that "the account Kepler offers his readers is not a true history of the course of his research – something Kepler never claimed – but is rather a didactic or rhetorical pseudohistory" (*ibid.*). Most interestingly, through a pioneering comparison of the surviving manuscripts and correspondence (especially the letters written to Kepler), Voelkel provided what he takes to be the reason behind Kepler's choice of this form of composition, by relating him to the contemporary astronomical community:

the unique conceptual and stylistic features of the *Astronomia nova* are intimately related: Kepler purposely chose this form of exposition precisely because of the response he knew to expect from the astronomical community to the revolutionary changes in astronomical methodology he was proposing. (Voelkel 2001, p. 2)

In what follows, I wish to slightly qualify Voelkel's analysis by considering Kepler's narrative not simply as a rhetorical tool he effectively used to convince – or rather try to convince – contemporary astronomers, but also as a way to engage them into critical discussion by recognizing the powerful heuristic provided by criticism.

## A Clue into Kepler's Method

By the end of 1610, with the publication of Galileo's *Sidereus nuncius*, the Copernican hypothesis had crossed the threshold of respectability. Even Christopher Clavius, the scholarly and prudent astronomer of the Collegio Romano, went so far as to commend Galileo's discoveries in no uncertain terms. Yet, the battle was by no means won. A mere looking-glass could not dispel a theory about the structure of the world: the Aristotelians felt that Galileo would have to focus the eye of his mind on the real problems before he could persuade them to alter their convictions about the nature of the universe. In his book *Contro il moto della Terra* (*Against the Motion of the Earth*, 1610–1611), for instance, the witty and acute Florentine

Lodovico delle Colombe repeated Tycho Brahe’s objections: though lacking originality, however, those objections were representative of the persistent doubts about the Copernican system among the educated Florentines, and Galileo took pains to refute them at length in the Second Day of his *Dialogue on the Two Great World Systems* (1632).

With the dialogue, Galileo was determined to convince his opponents of the truth of the heliocentric hypothesis by exposing their errors. A few years earlier, I suggest, Kepler implicitly adopted a similar approach: in fact, his works might be seen as having a *dialogical structure*. Just as Galileo, he was very confident in his achievements and aimed at convincing his opponents – but, unlike the Italian scientist, he genuinely wanted to record his own intellectual journey so as to show fellow astronomers the path he followed, the series of false starts, blind alleys and failures he encountered on his road to eventual success. Most importantly, he not only wanted to convince them by exposing their errors, but also to *critically engage* his readers by showing his own errors. This is particularly evident when – as we shall see in the case of *Strena seu De nive sexangula* (1611) – he does not have an answer to his questions, or a positive thesis to offer.

Along with Bruce Stephenson, I claim that Kepler’s works contain a long, sustained argument, carefully constructed and tightly knit. Their purpose is double: on the one hand, Kepler wished to convince his readers of the validity and strength of his conclusions; on the other, he wanted to prevent their criticism by highlighting that he had already contemplated of a particular objection, and followed it until it revealed as a mistake. Furthermore, Kepler believed in the didactic power of errors and wished his contemporary astronomers to learn about and from his own – as he himself had done.

Let us read Kepler’s own declared intentions in the opening of *Astronomia nova*:

The scope of this work is not chiefly to explain the celestial motions, for this is done in the book on Spherics and on the theories of the planets. Nor yet is it to teach the reader, to lead him from self-evident beginning to conclusions, as Ptolemy did as much as he could. There is a third way, which I hold in common with the orators [...]; that is, a historical presentation of my discoveries. Here is a question not only of leading the reader to an understanding of the subject matter in the easiest way, but also, chiefly, of the arguments, meanderings, or even chance occurrences by which I, the author, first came upon that understanding. Thus, in telling of Christopher Columbus, Magellan, and of the Portuguese, we do not simply ignore the errors by which the first opened up America, the second the China Sea, and the last the Coast of Africa; rather, we would not wish them omitted, which would indeed be to deprive ourselves of an enormous pleasure in reading. So, likewise, I would not have it ascribed to me as a fault that, with the same concern for the reader, I have followed this same course in the present work. (Kepler [1609] 1992, pp. 78–79, 1937–2002, Vol. 3, p. 36)

*Astronomia nova* stands in sharp contrast with standard astronomical works, such as Ptolemy’s *Almagest* (second century BC) or Copernicus’ *De revolutionibus orbium coelestium* (1543), in which the exposition of planetary theories proceeds systematically, with very few clues as to their source or development. Although a full account of his work “would be boring and pointless to recount” (Kepler [1609] 1992, p. 187, 1937–2002, Vol. 3, p. 111), Kepler was convinced of the relevance of a historical, or a roughly historical presentation of his ideas.

For sure, his might not be a literally historical account of his enquiry, but a qualified one, tailored according to what he thought was important and to the feedback he received from the people he corresponded with (who acted as peer reviewers *ante litteram*, so to say). Indeed, he writes: “I shall describe only so much of that labour of 4 years (1601–1605) as will pertain to our methodical enquiry” (*ibid.*).

This fact, however, does not make his account fictional: for although he has spared us many thorny byways, he also leads us through much of his erroneous reasoning. Possibly, then, Kepler is mingling some history with the theoretical and didactic content of the book. What is remarkable, however, is his sense that mistakes are important, and that we approach the truth by first being wrong.

## The Polyhedral Hypothesis

Kepler’s idea of a physical astronomy, of a defence of Copernicus based on physical principles, long preceded the publication of *Astronomia nova*. For the ideas expressed in his 1609 masterpiece existed as early as his student disputations in defence of Copernicus while in Tübingen. Kepler subsequently worked out the principles of his planet-moving force in *Mysterium cosmographicum* (1596) and had an eye on these very principles from the start of his research with Tycho Brahe through the discovery of the elliptical orbit of Mars.

Kepler encountered resistance to his ideas all along the way – a resistance that took the form of both theological objections to his student disputations and the material on the reconciliation of heliocentrism and Holy Scripture he had wished to include in *Mysterium cosmographicum*, and astronomers’ objections to his introduction of physical considerations into planetary theory.

As it is clear from their correspondence, he faced resistance also from his former teacher in Tübingen, Michael Mästlin, who had introduced him to Copernicanism. Kepler had conceived his polyhedral hypothesis for the structure of the universe on 19 July 1595, while lecturing on the progression of conjunctions of Jupiter and Saturn. He noticed that the ratio of the circle circumscribing an equilateral triangle to the circle inscribed in it was approximately the same as the ratio of Saturn’s orbit to Jupiter’s. He quickly ran through various arrangements of planar figures, and the following day he hit upon the idea of using the Platonic solids instead. As he wrote in *Mysterium cosmographicum*:

I give you the proposition, conceived in words just as it came to me and at that very moment: “The Earth is the circle which is the measure of all. Construct a dodecahedron round it. The circle surrounding that will be Mars. Round Mars construct a tetrahedron. The circle surrounding that will be Jupiter. Round Jupiter construct a cube. The circle surrounding that will be Saturn. Now construct a icosahedron inside the Earth. The circle inscribed within that will be Venus. Inside Venus inscribe an octahedron. The circle inscribed within that will be Mercury” (Kepler [1596] 1981, p. 69, 1937–2002, Vol. 1, p. 13).

In a series of three letters to Mästlin, Kepler described the material that was to appear in the book. He realized the novelty of his conjecture and was extremely

cautious, assaying the response of his former teacher bit by bit. In his first letter – dated 2 August 1595 – he did not describe his new system, but asked a number of questions directed toward the uncertainty in the distances of the planets as derived from observations.<sup>7</sup> In his next letter to his old master, on 14 September 1595, after he had determined that the polyhedral hypothesis did not disagree significantly with observations, Kepler did give the details of the system. However, he offered his former teacher only a very brief – one single sentence long – description on the order of the solids, and then moved to discussing the cause of the motions.<sup>8</sup> Only in the third and most complete letter, written on 3 October 1595, did Kepler introduce Mästlin to the material that was to appear in *Mysterium cosmographicum*. Whereas Kepler’s first letter to Mästlin had been a cautious query that gave away little, the second contained only the briefest mention of the polyhedra. It is only in his third letter that Kepler began to elaborate his vision toward the complexity that eventually appeared in *Mysterium cosmographicum*.<sup>9</sup>

Physical astronomy was deeply rooted in Kepler’s conviction that Copernicanism was a physically true representation of the world, and this belief was itself a manifestation of Kepler’s faith in the accessibility of the plan of God’s creation to human intellect.<sup>10</sup> *Mysterium cosmographicum* was thus conceived as the first of a series of books<sup>11</sup> devoted to a piecemeal proof of the reality of Copernican heliocentrism (for Kepler had no particular devotion to the details of Copernican planetary theory) by means of arguments based on physical and metaphysical cause – arguments then alien to the traditional scope of astronomy.

Kepler did not want his work to be confused with technical astronomy, which was then classified as a branch of mathematics, whose aim is not to describe reality.<sup>12</sup> By making clear his intention to introduce physical issues into his discussion of what might be otherwise have been seen as a mathematical work, Kepler defined his work as cosmography – and yet, as is clear from references scattered throughout the book, he hoped to convince astronomers, as well as natural philosophers, to accept his arguments.

Over half a century after Copernicus’ *De revolutionibus orbium coelestium*, *Mysterium cosmographicum* was the first theoretical work published in continental Europe that originally developed some of the basic issues of the new Copernican astronomy. Leading Kepler in his solitary intellectual adventure is the firm belief that he could explain *a priori* the rational order of the world. Kepler would pursue this belief throughout his life, devoting to it all his works, from his youthful masterpiece to his mature works, such as *Harmonices mundi libri V* (1619) and *Tabulae Rudolphinae* (1627).

The project for the renovation of astronomy undertaken by the Melanchton circle at the University of Wittenberg – that published the *Tabulae Prutenicae* in 1551 – was completely different from Kepler’s own version. As Erasmus Reinhold, one of the members of this movement,<sup>13</sup> noted: “the heavenly motion is uniform and circular, or it is a compound of uniform and circular motions”.<sup>14</sup> The Wittenberg astronomers merely supplemented the traditional geocentric picture of the universe with new data and new and more accurate measurements: they read Copernicus as providing only new computational devices, completely disregarding the first book

of his work, devoted to the new picture of the world. From this reading emerged a reductive picture, from which any cosmological implication was excluded: no reference to heliocentrism or the motion of Earth; no link with natural philosophy; no solution of the conflict between Copernican cosmology and the Holy Scripture, that remained extremely sharp also in the Protestant world.

Kepler's *Mysterium cosmographicum* marked a sharp divide with this tradition. As Kepler openly declares,

Nor do I hesitate to affirm that everything which Copernicus inferred *a posteriori* and derived from observations, on the basis of geometrical axioms, could be derived to the satisfaction of Aristotle, if he were alive [...], *a priori* without any evasions. (Kepler [1596] 1981, pp. 77, 79 1937–2002, Vol. 1, p. 16, and Vol. 8, p. 33)

The absolute originality of Kepler's project moved from these premises. The number of planets, distance of orbits, motion of individual planets, ratio of distances and revolution periods – everything fitted together, also suiting a kind of *a priori* explanation never attempted before. The exact and real constitution of the universe is not to be sought in the world, in the light of physical or natural proofs, but rather by moving from the very idea of unity and simplicity that was in God's mind and presided over the creation. Only those who undertake the difficult path of going back to the origin of the world can try to grasp God's design in its entirety and discover the trace He followed in designing the book of nature.

Unfortunately, with *Mysterium cosmographicum* Kepler failed in many cases to breach the division in his readers' minds between cosmography and astronomical theory. As it is clear from their correspondence, Johannes Prätorius, Helisaeus Röslin, Georg Limnaeus and Nicholaus Reimers Ursus had nothing at all to say about Kepler's motive power argument, and perhaps did not even perceive it. Prätorius rejected the book outright from an astronomical standpoint. Röslin firmly believed in the truth of a geo-heliocentric system different from either Tycho's or Ursus', and consequently rejected Kepler's Copernican hypothesis, also doubting that the polyhedral hypothesis could in any way prove the truth of the heliocentric system. Those who did recognize that Kepler was attempting in part to make physical inroads into astronomical theory itself with his motive power hypothesis warned him off. Tycho raised technical objections to Kepler's treatment of the equant and questioned the very premises of Kepler's work, highlighting the discrepancies between the values of the eccentricity of the orbits used by Kepler and those he himself recorded over a period of 30 years. Finally, Mästlin, Kepler's former teacher, clearly warned him to abandon this line of argument on philosophical grounds.

Those who read *Mysterium cosmographicum* as a cosmographical work and were intrigued by it by no means, accepted it as a physical proof of heliocentrism. Tycho, Prätorius, Röslin and Ursus did not waver in their geocentrism. The only reader who agreed that Copernicus could be proven on physical grounds and favoured Kepler's work was Galileo. He largely disagreed with the metaphysics underlying Kepler's project, but his initial response was very positive, and his open support for Copernicus was vague enough that Kepler perceived it as a support for himself.<sup>15</sup>



Kepler knew that the struggle was not astronomical but physical, metaphysical and theological. *Mysterium cosmographicum* had been his first effort to open this front. In his subsequent efforts – most notably *Astronomia nova* – he would refine his physical arguments. It is likely that criticism of the metaphysical, *a priori* approach in *Mysterium cosmographicum* eventually played its part in the considerably more refined argument of *Astronomia nova*.

## The New Astronomy: Mathematical Physics

*Astronomia nova* (1609) is as much about astronomical methodology as planetary theory. Kepler’s proof of heliocentrism depended on the validity of physical astronomy. However, establishing the validity of physical astronomy itself entailed a redefinition of astronomy. Although astronomy was subject to physical considerations, astronomical theory was still widely regarded as a purely mathematical pursuit. By describing in detail the considerations that led to his orbit for Mars, Kepler hoped to discredit traditional mathematical astronomy decisively and establish the absolute necessity of physical astronomy.

“The Tychonics” – as Kepler nicknamed Christian Severin Longomontanus (Tycho’s former assistant) and Franz Gansneb Tegnagel van Kamp (Tycho’s son-in-law) – tried their best, for different reasons, to hamper Kepler’s work in his capacity of the new imperial mathematician after Tycho’s death. His response was to seek a mode of exposition that would make his findings public, while at the same time strengthening their chances of persuading the readers of the plausibility and necessity of the revolutionary ideas Kepler was advancing. His solution was to draw a rational reconstruction of the history of his discoveries, one that would depict him as compelled to follow the path that he had, and at the same time reveal the technical challenges he faced.

On the one hand, the intense correspondence with the Tychonics explains Kepler’s decision to present his new astronomy based on physical causes in terms of a commentary on the motion of a single planet, Mars: for Kepler had limited access to the data under Tycho himself and then “under” his heirs, and was then required to present his findings to justify his employment as imperial mathematician. On the other, the book’s narrative was partly determined by the necessity to shield himself against the Tychonics’ accusations that his idiosyncratic approach to astronomical theory was a disrespectful affront to the memory of Tycho Brahe. Kepler’s audience shared many of the Tychonics’ suspicions, and therefore the narrative was also directed at the astronomical community at large.

In this respect, as Voelkel has argued, the correspondence is illuminating. More interesting, though, are the letters he exchanged with other astronomers. Kepler’s first statement that the orbit of Mars is an ellipse is embedded in the midst of a long letter – a full 40 pages in the modern edition of Kepler’s works – to David Fabricius, an East Frisian preacher whom he found a sympathetic and like-minded soul. Both their voluminous exchange and the surviving drafts of the book show

that while composing *Astronomia nova*, Kepler is likely to have gone through their correspondence to isolate troublesome issues. There are numerous marginal notes in the letters that refer to composition of *Astronomia nova*, one of which, for example, reads: “N. B. raise [this matter] in the *Commentaries* [that is, in *Astronomia nova*]”.<sup>16</sup> At the very least, Kepler recognized the usefulness of the correspondence as a stimulus to thought. And, he often worked through problems while writing.

Generally speaking, Kepler took account of where in their correspondence Fabricius expressed his confusion or requested clarification and tailored the presentation of his results in *Astronomia nova* to avoid the same confusion in his readers. In other words, Fabricius acted as a representative member of the astronomical community, expressing what demonstrations would be necessary to convince him of the truth of Kepler’s findings.

Kepler needed him to bounce the ball back, and revised his book accordingly. He found in Fabricius a kind of test case for the acceptability of his new astronomy, one who played a decisive role in determining the content and expository style of *Astronomia nova*. The discovery of how difficult it was to make Fabricius understand his work, together with Fabricius’ success at recasting Kepler’s work into compounds of circular motion, reinforced Kepler’s resolution to intertwine physics and astronomy so that they could not be separated.<sup>17</sup>

## The Six-Cornered Snowflake

Kepler’s dialectical method is evident also at work in some of his “minor” writings, such as a little book on the hexagonal symmetry of snowflakes. At the height of his career, shortly after the publication of *Astronomia nova*, Kepler turned his gaze from the immensity of the heavens to the minutiae of earthly phenomena, from the celestial to the (seemingly) trifling. In his *Strena seu De nive sexangula*, published in 1611 (the very year in which the happiest period of his life, the one he spent in Prague, came to a sad end and he was forced to leave the town), Kepler recognized that the apparently perfect hexagonal form of a few snowflakes that fell on his coat one day while crossing the Charles bridge in Prague presented a challenge to the new mathematical science that was struggling to birth in his own mind: why six? What was the physical cause of the six? What principle selected six from the other possible numbers?<sup>18</sup>

At a superficial glance, Kepler’s little book displays in compact form the antithesis between the medieval outlook and the new mathematical method. Yet, the objective scientific aim is dominant. For beneath its humour and allusive style, and apparently casual repetitiveness, it displays a scientific judgement of the highest calibre. Kepler recognized a genuine problem, discussed several alternative solutions, rejected them all and passed the problem to the chemists for them to solve in the future.



Kepler recognized that snowflakes are composed of several individual units brought together, as he thought, by irregular drifting and that each individual always possesses six corners and not five or seven. In this he differed from Descartes, who held roughly the inverse view, that is, that the hexagonal unit was evolved by a partial melting or an agglomeration of irregular particles. Kepler realized that the six-sidedness could not be inherent either in water vapour or in the coldness of the air, but discussed whether it was imposed by an external factor and arose perhaps because the hexagon was a particularly suitable or efficient form.

This led him to a consideration of ways of filling space with repetitive figures and to the idea that natural structures such as bees' honeycombs and pomegranates achieve their shape from necessity. He discusses, for the first time, cubical and hexagonal close packing of equal spheres, pointing out that, in the cubical arrangement, each sphere is touched by six others and, in the hexagonal arrangement, by twelve others. Hexagonal close-packing was found to give the tightest packing and to produce rhomboidal aggregates. Kepler suggested that a pomegranate needs to store the maximum number of seeds in the smallest possible space, so that hexagonal close-packing is not only the most efficient method, but is also a material necessity.

But Kepler also saw that such arrangements were not necessarily applicable to flat snow crystals and, having argued convincingly that such a form is highly unlikely to result from the reduction of a regular three-dimensional aggregate of snow, even if these were composed of regularly packed rows of globules, he went on to discuss space-filling in two, rather than three, dimensions. He thought that the flatness of the snow crystals might be explained in terms of the cold vapour meeting the warm vapour at a plane interface, but then asked why it should be six-cornered. Regular hexagons can fill two-dimensional space without gaps but so can equilateral triangles and squares. In any case, it is possible to fill space completely with hexagons only if they are of the same size and, as Kepler points out, snowflakes are not uniform in size.

In short, after considering a number of possible explanations for preference of the hexagons, Kepler remains unconvinced by any of them and finally, ponders on the possibility that it may rise from a formative faculty (*facultas formatrix*) in the body of the Earth and be formed as a part of the Creator's design because of its aptness and beauty. But, even so, he admits that Nature produces other shapes in other crystals and so is finally unable to arrive at a satisfactory explanation for the hexagonality of snowflakes. He concludes by “knocking at the door” of chemistry and leaves the problem to future generations of scientists.<sup>19</sup>

Although Kepler was unable to offer a satisfactory explanation of the six-sidedness of snowflakes, his discussion of space-filling and symmetry laid the early foundations of crystallography.<sup>20</sup> For Kepler discusses in print, for the first time, the ordered symmetrical shapes that arise from the packing together of similar bodies, which may themselves lack symmetry. Man has long been aware of the superstructures that can be built from the stacking of repeated geometrical units (think of Egyptian pyramids, or to cannonballs stacks), but the relationship of these models to the minute geometry of matter was not recognized until Kepler.

## Critical Dialogue and the Search for Truth

The scientific revolution was not primarily a matter of new facts, but of new ways of looking at old facts<sup>22</sup>. But looking through the new glasses Kepler asked his fellow astronomers to wear was not an easy task – just as it proved to be extremely hard to see, through the recently invented telescope, what Galileo claimed to see. Not only did Kepler advance new, bold hypotheses to describe and explain the structure and functioning of the universe, but he also wanted to renovate astronomy by merging mathematical with physical issues. Therefore, he needed to supplement strong scientific arguments by appealing to other tools. He chose to tell the story of his own intellectual path. As a scientist, he was an honest seeker after the truth, and demanded his readers to engage in a critical dialogue, thus collaborating with him in the common effort to approach the truth.

This interpretation resolves a broader tension in our view of Kepler's intellectual achievement. Throughout his life, Kepler's astronomical work was devoted to showing that the Copernican heliocentric system of the world was true. Yet, some of his works seem very different in character. The youthful *Mysterium cosmographicum* (1596) argued for heliocentrism on the basis of metaphysical, astronomical, astrological, numerological and architectonic principles. By contrast, *Astronomia nova* (1609) was far more tightly argued on the basis of only a few dynamical principles. In the eyes of many, such a contrast embodies the transition from Renaissance to early modern science.

However, Kepler did not subsequently abandon the broader approach of his early works: similar metaphysical arguments reappeared in *Harmonices mundi libri V* (1619), and he reissued the *Mysterium cosmographicum* in a second edition in 1621, in which he qualified only some of his youthful arguments. Given the persistence of these ideas, it is clear Kepler did not undergo some sort of conversion experience and became a “modern” scientist all of a sudden. Furthermore, and perhaps most importantly, the metaphysics underlying all his works remained the same: he always looked for the reality behind the appearances, trusting that hypothetical reality would explain appearances.

I suggest that Karl Popper's fallibilist and piecemeal approach, and especially his theory of errors might prove extremely helpful in resolving such alleged tension, thus providing a unifying picture that properly puts Kepler within the context of the wider astronomical community of his time and helps us assess his contribution.

The conceptual and stylistic features of the *Astronomia nova* – as well as of other “minor” works of his Prague period, such as *Strena seu De nive sexangula* – are intimately related and were purposely chosen because of the response he knew to expect from the astronomical community to the revolutionary changes in astronomy he was proposing. Far from being a stream-of-consciousness or merely rhetorical kind of narrative, Kepler's rational reconstruction is the reflection of his experience while writing his books – of his own errors as well as of the objections he received from his correspondents. Like all seekers after the truth, Kepler

made many mistakes – but he learned from them, as few others have done.<sup>21</sup> His is a marvellous example of the power and fruitfulness of the method by conjectures and refutations.

**Acknowledgments** I wish to thank Joseph Agassi, Donald Gillies, Ian Jarvie, Carlo Rovelli and Michael Segre for comments on previous versions of this paper.

## Notes

1. “Causa, cur falsa hypothesis verum prodat et quatenus”: it is the title of chapter 21 of Kepler’s *Astronomia nova*.
2. Suffice it to mention the recent Bucciantini (2003).
3. The source of Koestler’s characterization of Kepler as a sleepwalker might well be Brod ([1920] 1928, p. 14), in which he is described as a man moving “past the sharp places of life with the certainty of a sleepwalker”.
4. Interestingly, Koyré displays a thoroughly a *priori* approach to historiography, and yet he falls into the inductivist error: see Segre (1991, Chap. 2) and Segre (1994).
5. See Wilson (1968) and Aiton (1969). The same approach to the correspondence is to be found in Whiteside (1974).
6. In this paper, I follow Voelkel’s analysis but draw a different conclusion from it.
7. Letter to Michael Mästlin, 2 August 1595, in Kepler (1937–2002, Vol. 13, pp. 27–32).
8. Letter to Michael Mästlin, 14 September 1595, in Kepler (1937–2002, Vol. 13, p. 32).
9. Letter to Michael Mästlin, 3 October 1595, in Kepler (1937–2002, Vol. 13, pp. 34–36).
10. Kepler had always wanted to be a theologian. Astronomy as a purely mathematical discipline was not compelling for him. Only as a greater natural philosophical pursuit, in which he could ascertain the traces of the Creator, did it come to captivate him. As he explained to Mästlin shortly before the publication of *Mysterium cosmographicum*: “I am in haste to publish, dearest teacher, but not for my benefit [...] I am devoting my effort so that these things can be published as quickly as possible for the Glory of God, who wants to be recognized from the Book of Nature. [...] Just as I pledged myself to God, so my intention remains. I wanted to be a theologian, and for a while I was anguished. But now, see how God is also glorified in astronomy, through my efforts” (letter to Michael Mästlin, 3 October 1595, in Kepler (1937–2002, Vol. 13, p. 40). For Kepler, the Copernican system was the ultimate manifestation of God’s design of, and presence in, the world. All his astronomical work was thus to a large extent concerned with establishing the truth of heliocentrism. From the very beginning, as a student in Tübingen, Kepler was only marginally interested in the details of Copernican planetary theory, those very details that most contemporary astronomers found particularly compelling. It was heliocentric cosmology that captured his attention, especially the way in which the heliocentric system could be understood as a manifestation of God’s providential design. Because of this fundamental religious stimulus, the heliocentric system could be of interest only if it were the physically true representation of how the universe was put together.
11. Indeed, the whole title reads *The Forerunner of Cosmographical Essays, Containing the Cosmographical Secret: On the Marvellous Proportion of the Celestial Spheres, and on the True and Particular Causes of the Number, Size and Periodic Motions of the Heavens, Demonstrated by Means of the Five Regular Geometric Bodies*.
12. Indeed, Galileo regarded himself as a *philosopher*, not as a mathematician. Just as Galileo, Kepler aimed at providing mathematics with a physical content: he was trying to physicalize mathematics, so to say, not the other way round.
13. Other members included Kaspar Peucer and Johannes Prätorius, one of Kepler’s correspondents.

14. Quoted in Westman (1975, pp. 175–176), my translation. See also Gingerich (2002, pp. 168–278).
15. See Voelkel (2001, Chap. 4, especially pp. 69–77).
16. Letter to David Fabricius, 11 October 1605, in Kepler (1937–2002, Vol. 15, p. 248).
17. As he explained to Fabricius: “I will deeply interweave and entwine Copernicus into the amended astronomy, and so also into physics, such that either each will perish at the same time or both will survive” (letter to David Fabricius, 10 November 1608, in Kepler (1937–2002, Vol. 16, p. 197).
18. Kepler had asked himself the very same questions some 15 years before: considering the heliocentric system as the product of God’s intelligent design, he asked himself some fundamental questions about this design – why were there six and only six planets, and why were they arranged at their particular distances from the Sun? The answer that came to him, that God had constructed the universe using the five Platonic solids as archetypes, was for him a compelling physical proof of the truth of heliocentrism, which he presented to the world in his *Mysterium cosmographicum* (1596).
19. See Kepler ([1611] 1966, p. 45; Kepler 1937–2002, Vol. 4, p. 280).
20. Indeed, the German physicist Max von Laue, who in 1911 succeeded in diffracting X-rays using a crystal grating (and received a Nobel prize for that in 1914), called attention to the earliest observation of the connection between crystallographic regularity and atomism, thus acknowledging our debt across the centuries to Kepler and to his little book on the hexagonal symmetry of snow crystals: see von Laue (1952, p. 1).
21. See Popper (1999).
22. As William Shea remarked, “The world must not only be seen through the telescope, it must be looked at through a new set of intellectual categories” (Shea 1977, p. 121)

## Bibliography

- Aiton, Eric. 1969. Kepler’s Second Law of Planetary Motion. *Isis* 60: 75–90.
- Brod, Max. [1920] 1928. *The Redemption of Tycho Brahe*. London: Alfred A. Knopf.
- Bucciantini, Massimo. 2003. *Galileo e Keplero: Filosofia, cosmologia e teologia nell’Età della Controriforma*. Turin: Einaudi.
- Donahue, William H. 1988. Kepler’s Fabricated Figures: Covering up the Mess in the *Astronomia nova*. *Journal for the History of Astronomy* 19: 217–237.
- Donahue, William H. 1992. Translator’s Introduction. In Johannes Kepler, *New Astronomy*, ed. William H. Donahue, 1–19. Cambridge: Cambridge University Press.
- Gingerich, Owen. 2002. *An Annotated Census of Copernicus’ “De revolutionibus” (Nuremberg, 1543 and Basel, 1566)*. Leiden/Boston/Cologne: Brill.
- Kepler, Johannes. [1611] 1966. *The Six-Cornered Snowflake*, ed. Colin Hardie. Oxford: Clarendon.
- Kepler, Johannes. [1596] 1981. *The Secret of the Universe*. Norwalk, CT: Abaris Books.
- Kepler, Johannes. [1609] 1992. *New Astronomy*, ed. William H. Donahue. Cambridge: Cambridge University Press.
- Kepler, Johannes. 1937–2002. *Gesammelte Werke*, eds. Max Caspar et al. Munich: C. H. Beck.
- Koestler, Arthur. 1959. *The Sleepwalkers*. New York: Macmillan.
- Koyré, Alexandre. 1973. *The Astronomical Revolution: Copernicus, Kepler, Borelli*. London: Methuen.
- Popper, Karl Raimund. 1983. *Realism and the Aim of Science*. London: Hutchinson.
- Popper, Karl Raimund. 1999. Kepler’s Metaphysics of the Solar System and His Empirical Criticism. In *All Life is Problem Solving*, 74–78. London/New York: Routledge.
- Segre, Michael. 1991. *In the Wake of Galileo*. New Brunswick, NJ: Rutgers University Press.

- Segre, Michael. 1994. Peano's Axioms in Their Historical Context. *Archive for History of Exact Sciences* 48: 201–342.
- Shea, William R. [1972] 1977. *Galileo's Intellectual Revolution: Middle Period, 1610–1632*. New York: Science History Publications
- Stephenson, Bruce. 1987. *Kepler's Physical Astronomy*. New York: Springer.
- Voelkel, James R. 2001. *The Composition of Kepler's Astronomia nova*. Princeton/Oxford: Princeton University Press.
- von Laue, Max. 1952. Introduction. In *International Tables for X-Ray Crystallography*, Vol. I, 1–3. Birmingham: Kynoch Press.
- Westman, Robert S. 1975. The Melanchton Circle, Rheticus, and the Wittenberg Interpretation of the Copernican Theory. *Isis* 66: 165–193.
- Whiteside, Derek T. 1974. Keplerian Planetary Eggs, Laid and Unlaid, 1600–1605. *Journal for the History of Astronomy* 5: 1–21.
- Wilson, Curtis. 1968. Kepler's Derivation of the Elliptical Path. *Isis* 59: 5–25.

# Proof Versus Sound Inference

Nimrod Bar-Am

**Abstract** This paper arose out of a study of the notes that Joseph Agassi and Czeslaw Lejewski took at Karl Popper's seminar on Logic and Scientific Method (1954–1955).<sup>1</sup> It ponders on a basic logical distinction Popper had made: between sound inference (valid inference with sound premises) and proof (a collection of inferences that show that a given sentence follows from any premise). The difference between sound inference and proof seems crucial to Popper's epistemology, especially to his emphasis on the distinctness of epistemology and methodology. In this paper, (1) The distinction is explained; (2) The difference is presented as the basis for Popper's view of the history of logic; (3) Some modern hesitations about all this are discussed.

## In Which a Brief Historical Setting Is Presented

Karl Popper published six fascinating papers on logic between 1947 and 1948.<sup>2</sup> As the title of one of them reveals, his ambition was to demonstrate that logic had no assumptions. What does it mean for logic to have no assumptions? I will soon discuss this question. For Popper, as is well known, logic is the rules of refutation, the core of scientific method. His attempt to construct it without assumptions thus shimmers, perhaps inadvertently, with an incredible promise: to secure scientific method, the theory of refutation, as irrefutable by definition: a logic without assumptions, like an inference without premises, cannot possibly be refuted. Let me warn my readers that Popper would attenuate my last description, and as a matter of course, by distinguishing between the claim that logic is a priori valid and the claim that it is vacuously true, in the conventionalist sense. Logic, he would stress, is only vacuously true in the conventionalist sense. But it does not seem to me that the matter is as simple and straightforward as it may, perhaps, seem from such a presentation.

Popper's gush of fascinating publications on logic had stopped with the same suddenness that it started. This may well have been the result of the cold welcome

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that his innovations received.<sup>3</sup> Rather typically, such cold welcomes pushed him to study other fields, where he would demonstrate his unmatched originality time and again. The scholarly debate regarding the merit of his ideas on logic waned quickly until it was rekindled and put out, simultaneously, in 1974 with Lejewski's masterful comparison of Popper's Logic with Tarski's formal semantics, within Lesniewski's system.<sup>4</sup> In a nutshell, Lejewski showed that Popper's logic is both admirable and not entirely without its hidden assumptions. When these assumptions are made explicit, he showed, Popper's 'logic-without-assumptions' and Tarski's 'formal semantics' are equivalent. Lejewski's remarkable paper, although written with utmost admiration for Popper's ingenuity, had made Popper declare his ventures into logic "bad and ill fated" and finally "a failure".<sup>5</sup> My paper is an attempt to ponder his harsh judgment of his output in logic.

In order to complete my epigrammatic historical introduction, I should add one last piece of information that is also the center of focus of this brief essay. Popper's core academic duty at LSE for over 2 decades (1946–1969) was the teaching of a 2-year introductory course on logic and scientific method. In that course Popper had taught his own developed system of logic. I need not tell you that within the bounds of such a basic service course, Popper's proof that logic has no assumptions was not rigorous. But as is often the case with this unusual giant he attempted something even more impressive: he presented his claim (that logic has no assumptions) as part and parcel of the course's basic terminology. We thus come to the distinction between sound inference and proof.

### **In Which the Distinction Between Sound Inference and Proof Is Explained and Some of the Merits of the Distinction Are Exemplified**

I will present now some basic terminology. With a few minor accents it can, and should, be regarded Popper's own.<sup>6</sup> I must ask for your pardon then, as the presentation of terminology is somewhat tiresome. It is also, I hope, worthwhile, for as I will argue, Popper employed terminology to express some very challenging ideas.

I have mentioned that Popper presented logic as the theory of refutation. A refutation is a valid inference with putatively true premises which are discovered to be putatively false due to the discovery that they entail a putatively false conclusion. A *sound inference*, on the other hand, is a valid inference with premises that are all putatively true (hence its conclusion must also be recognized as putatively true). Now, an inference (valid, sound or refuting) is not to be confused with proof, Popper stressed. For a *proof* is a certain collection of inferences that, when taken together, show that a certain sentence is "true, for all times,"<sup>7</sup> regardless of the truth of its premises. A proven sentence is true 'no matter what' since it can be placed as a conclusion to any set of premises (even to an empty set of premises, or to a false premise or premises) and the result will always be the same: a valid inference. Such an inference will be recognized as sound, then, if and only if, its premises are



also putatively true. But, and this is the crucial technical point, the inference does not have to be sound. For example a proof by *reductio ad absurdum*, also known as indirect proof, is never based on the putative truth of its premises. Indeed it is based on showing that their conjunction is a contradiction. (Example: A; if A then B; if A then not B. Therefore: not A.) Thus, stressed Popper, sound inferences and proofs are distinct logical entities and must not be confused.

Popper regularly opened his introduction to logic with a unique and very interesting historical summary of the development of the subject. The distinction between sound inference and proof played crucial role in it, since it enabled him to make very acute historical observations and explain them to students with minimal understanding of the subject and with absolutely no knowledge of its history. Let me give but one example.

The standard (traditional) reading of Aristotle's theory of the syllogism is that it is a theory of sound inference. Indeed the syllogism is defined by Aristotle as (roughly) a valid inference from self evident judgments. Since already Aristotle regarded his logic as complete, Popper asks about its account of indirect proof. Did Aristotle account for indirect proofs at all? That Aristotle was well acquainted with such proofs is unquestionable, since he reports them with great detail but, perhaps surprisingly, with utmost disregard for his theory of the syllogism. (A classic example is the famous proof that 2 has no rational square root, but there are others, of course.) Since the proof is indirect, and since the theory of syllogism only explains sound inference, Aristotle could not account for it. Thus, notes Popper, Aristotle's logic was already outdated at its time of inception: it could not, in principle, account for *reductio ad absurdum*.<sup>8</sup> This fact is especially interesting, since, as both Popper and Szabo have shown, indirect proofs were at least as central to the development of Western thought in general, and logic in particular, as sound inferences.

There is much more to be said regarding the merits of Popper's distinction especially within the study of the history of logic (Bar-Am 2008). However, let us move on to some hesitations about Popper's uses for it in modern context.

## In Which Some Modern Hesitations About the Distinction Are Discussed

Popper often employed his distinction between proof and sound inference as a basic tool to explain his unique view of science. He says,

The idea that science 'proves' is wrong. The word 'proves' is being misunderstood. In the sense of 'prove' discussed above science has 'proved' very little. Look at the changes in science in the last 2,000 years. If on important points science can change its teaching so much in the course of time, then proof, if it occurs at all in science, must be comparatively rare ... it marked a kind of false idea in science, an idea of science in which science cannot change, only grow". (Popper 1954–1955; Oct. 26th, p. 4)

Thus, Popper utilized his distinction also as a tool to distinguish between the logical and the empirical. Within the empirical study of nature presumed sound inferences



may be discovered to be unsound, when their premises are refuted. In a similar manner refutations are never a final matter, in these contexts, since they can also be refuted. And they frequently are refuted, of course, within the history of science; some view this as “progress” nonetheless, and the debate about this point is still open and interesting.<sup>9</sup> In contrast, Popper argued, it is not contested that if a given inference is valid, it will remain so even if we provide different valuations (assigned truth-values) to our sentences. A proven sentence, he stressed, by definition, cannot be refuted. Take a classic syllogism: “All humans are mortal, all Greeks are human; therefore all Greeks are mortal.” Popper insisted that its conclusion can never be regarded as proven, for its truth is conditional upon the truth of its premises. The syllogism can be regarded as proof if, and only if, it is cast in the form of a conditional statement: “If all humans are mortal and all Greeks are human then all Greeks are mortal.” The conditional statement, he said, since it is tautological, is proven ‘once and for all’, it is irrefutable in principle ‘no matter what.’ The distinction between sound inference and proof thus seems to help us distinguish between the empirical and logical: empirical knowledge is always conditional and never proven. Logic, on the other hand, is proven. Needless to say that, this is not to claim that every sentence is either empirical or logical: clearly there are metaphysical and many other statements (e.g. superstitions) which are neither.

This peculiar use that Popper assigned to his distinction invites some tinkering. Modern logic disambiguated the abovementioned definition to sound inference. For, as you may have noticed it is deliberately ambiguous. Sound inference was defined as an inference from ‘putatively true’ premises. Even in highly formal modern deductive systems, the term “true” regularly envelops at least two distinct senses: logical truths on the one hand and contingent truths on the other hand. As I have shown elsewhere, historically the situation is much more elusive and much more interesting: the traditional theory of judgment is often a conflation of the contingent and the tautological.<sup>10</sup>

Within modern formal systems, Popper’s distinction between sound inference and proof is not easy to maintain. A sound inference from axioms is a proof. And, whether we notice it or not, a proven assertion in arithmetic ultimately rests on the axioms of arithmetic being true. Thus, any proof in arithmetic is ultimately a sound inference in disguise. Nothing in arithmetic is ‘independently’ proven ‘no matter what’. This seems to complicate the use of the distinction as a manner of distinguishing between the empirical and the logical.

Incidentally, this explains a fundamental discrepancy between Popper’s terminology and Lakatos’ terminology, which often confuses students: Lakatos, as is well known, presented mathematics as the product of proofs and their refutations. Yet Popper insisted that Proof, by definition, is irrefutable. The discrepancy is resolved when we realize that proof in mathematics is a sound inference in disguise, and as such, of course, can, in principle, be cast into doubt, even when we have no clear idea how to do so. Lakatos did say that formal systems are irrefutable, of course, although their interpretations are. I need not endorse or reject this here.

To make the modern perspective somewhat clearer let us consider Euclidian geometry understood as part of physics (Einstein), that is, take Euclidian geometry

understood as informative. Any sentence that is proven in Euclidian geometry understood as informative is not proven “no matter what”. Rather it is deemed as proven on account of the axioms of Euclidian Geometry (taken as empirical, informative, hypothesis) considered putatively true. A “proven sentence” in such a system is ultimately a sound inference in disguise, whose ultimate premises are the axioms of Euclidian geometry understood as informative. Are things different when we discuss the truths of geometry understood as uninformative? If the axioms themselves are understood as uninformative, every sound inference within such a system seems to be a full-fledged proof. Popper argued that the conclusion of such sound inference is in fact, in a certain harmless sense, true ‘no matter what’ simply because it says nothing about the world. Yet even non-informative axioms can sometimes be replaced, thus yielding new uninformative systems. And the application of different uninformative systems can lead to very interesting empirical disagreements! Since uninformative systems are not about the world, what are they alternatives of?

Are things different when we discuss the truths of logic? Are there alternative logics? What are they alternatives of? Can the use of different uninformative logical systems lead to interesting empirical disagreements? These are the questions that concerned Popper.

In order to understand both Lejewski’s admiration for Popper’s work and Popper’s admission of failure, we have to realize that there are (at least) two very distinct senses by which logic can be considered free of assumptions. There is the strictly formal sense, on the one hand, and a semantic sense, which is a virtual epistemology of sorts, on the other hand. (Accordingly, there are also two very distinct senses by which a sentence can be regarded as true ‘no matter what’: it can be regarded as vacuously true, in the conventionalist sense of saying nothing about the world, or as reflecting meta-logical intuitions about truth, that, if true, are not at all vacuous; no truth is ever completely vacuous.)

In the purely formal sense a logic has no assumptions when it is constructed in such a manner that all its assumptions are pushed, by some mean or other, to the meta-language (which can be a well defined structure, or a mere set of intuitions). All non-axiomatic systems are, in this strict sense, without assumptions. For example Gentzen’s natural deduction is such a system ‘as is’, since it contains only rules of inference. Because his system is complete, all tautologies are provable within it. They can then be regarded as ‘true no matter what’ but only in the formal sense, only in the sense that their proof requires no axioms. This is possible only because the assumptions of the system were presupposed within the meta-language, somehow. For clearly Gentzen’s rules of inference, were not arbitrarily chosen: they were chosen to reflect certain semantical intuitions about valid inferences as truth transmitting. Other, conflicting logical intuitions exist. (Gentzen’s rule for eliminating double negation, to name one obvious example, is rejected by intuitionists). And so competing logical systems express different intuitions about rules for the transmission of truth.

Popper’s system too, is an attempt to construct a logic without assumptions in the formal, abovementioned, sense. It uses definitions of ‘formative signs’ alone,

and, by the mere description of the deductive rules that they abide, by Lejewski has shown that some of these definitions presuppose what, strictly speaking, should be regarded as axioms that reflect the structure of language.<sup>11</sup> This, then, is the first sense by which Popper could have regarded his project as a failure. His logic is merely apparently without assumptions. Unlike Gentzen's system, which successfully pushes all assumptions (and especially those regarding truth transmission) out of its realm, Popper's system has hidden, embedded assumptions. Such a failure, then, is purely technical. It may even be resolvable by purely technical means. And it does not condemn the system that he developed in his service course; for the standards of rigor in a service course are very different from those of an ideal formal logical setting.

Yet Popper also sounds at times as if he felt that the two senses, the formal and the semantic, need not be distinguished sharply. And if the two senses are not kept apart, then the one may inadvertently serve as justification for the other. The most obvious case is his insistence that there is a fundamental difference between sound inference and proof, since only the proven assertion is true 'for all times', 'no matter what'. For, to repeat, even Gentzen's system is not true "no matter what" in the semantic sense. Nothing is true "no matter what" in that sense. His admission of failure then could also be understood as the result of a realization (a-la-Quine) that absolutely nothing is, strictly speaking, proven 'for all times'.

Let me stress that this point is delicate: Popper never recommended that the two senses should be conflated, or that the one should be used as justification for the other. He openly acknowledged and studied competing logics. However, he did suggest that his preference of classical logic over intuitionist logic is based on the fact that the former better fits as a logic of refutation, and moreover that the paradoxes are better solved otherwise. This interesting observation focuses our attention on the fascinating question "what does it mean for one logical system to be better than another?" Clearly, in order for logics to compete as tools of refutation, in order for one logical system to 'better fit' a logic of refutation both systems must have some content. True, this content need not be empirical. It may be heuristic. But if purely nominal systems are infused with such heuristic advantages and disadvantages, then the notion of 'true no matter what' becomes significantly narrower than what Popper seems to have intended, when he admitted failure.<sup>12</sup>

## Notes

1. (Popper 1954–1955) I am greatly indebted to Prof. Joseph Agassi for kindly letting me study these notes, and for discussing them with me.
2. Popper (1947a–c; 1948a–c).
3. "...I had received much discouragement, which had depressed me and had made me give up the attempt at developing my ideas further" says Popper in his reply to Lejewski; (Schilpp 1974, p. 1096). For a list of the discouraging reviews see Lejewski's own paper in that volume (p. 669, Note 2).
4. Schilpp (1974, pp. 630–670).

5. Schilpp (1974) "...Lejewski...has not brought out, or even hinted at, what my intentions were in writing these bad and ill-fated papers...my main intention was to simplify logic by developing what was called by others "natural deduction". I suppose that as an effort to build up a simple system of natural deduction (a commonplace logic, as it were), my papers were just a failure". (pp. 1095–1096)
6. Popper used the term "derivation" in order to designate the logical entity I call "sound inference," and contrasted it with proof (e.g. Popper 1954–1955, Oct. 26th, p. 6). I prefer the term "sound inference" because it is not ambiguous: derivation designates a logical entity as well as an act of reasoning.
7. Popper (1954–1955, Nov. 9th, 1954, p. 1).
8. Some recent readings of Aristotle's theory of the syllogism present it as a system of natural deduction and not as a system of inferences (e.g. Corcoran 1994). These anachronistic readings were devised *ad hoc* to answer the abovementioned criticism of the Aristotelian system made by Popper and others. On this matter see Bar-Am (2004, Note 12; 2008, Notes 4 and 36).
9. Bar-Am (2008, p. 145, Note 5).
10. Bar-Am (2008, pp. 103–108).
11. Popper often stressed that any division of terms within the natural language, into 'formative' on the one hand and 'descriptive' on the other hand, is an idealization of the natural language. He also added, rightly, that a formal account of inferences within any natural language presupposes such an idealization. In other words, logic as a mathematical system is not about inferences that are formulated within a natural language. As a description of such inferences, logic is a mere approximation, and often a rough one.
12. I wish to thank Prof. David Miller for kindly inviting me, during the question-and-answer session following my talk, to compare my paper with the interesting work made by Schroeder-Heister on Popper's logic, a work summed up in Schroeder-Heister's talk at the Popper centenary conference and published later at Jarvie et al. (2006, pp. 17–36).

Schroeder-Heister and I share obvious admiration for Popper's work in Logic. We both take as starting points the validity of Lejewski's criticism of it. Schroeder-Heister directed his attention to Popper's published papers in an attempt to defend what he regards as their gist. He endeavors to provide "a coherent sense" to it (*ibid.* p. 17). He borrows (from A. Koslow) and develops a highly clever technical gear in order to improve Popper's logical theory so as to present it as a description of a mathematical structure in which various deductive systems ("logics and non-logics" *ibid.* p. 32) can be defined. Although this was clearly not Popper's intention in developing his logic, let me say again that it is very interesting.

My own paper has a different focus altogether. It is not by accident that it contains absolutely no technical signs. I find Popper's service course on logic as more interesting (and, in certain respects, which I will now explain, more profound) than his published work on the subject. (For this reason I have studied the notes prepared by Lejewski and Agassi during Popper's service course; I am not aware of any other studies of these notes so far.) Classroom context has a few interesting advantages to that of the published work. Within the classroom it is senseless for a teacher not to be student-oriented (and thus, problem-oriented). In contrast, published works (especially in logic) often suffer from attempts by their authors to achieve ideal levels of rigor, levels that make their reading difficult and that are helpful, at best, to a selected few. By rising Popper's logical system to such an ideal level (by means of Lesniewski's system), Lejewski has shown that it is not the simple system, free of assumptions, that Popper deemed it. This valid criticism does not apply, then, to Popper's classroom work. For attempting to apply the rigor demanded by Lesniewski's system within a service course would be unsuccessful, at best. I thus directed my attention to Popper's means of expressing his ideas on logic to beginners. As Agassi notes (Agassi 2006) Popper had followed here Hilbert in attempting to initiate the study of logic, not by establishing a set of meaningless nominal definitions (a-la Carnap) but by relying on the laymen's high-school intuitions. Logic, then, is not a mathematical deductive system that is parachuted, out of the blue, on the puzzled student. Rather it is a small set of understandings, regarding legitimate modes of

arguing, that is achieved and formulated with the student, in the service of his own intellectual endeavors and aspiration, as well as his contact with his teachers and colleagues. Lejewski's criticism of Popper's published work on logic, then, simply does not apply to his classroom work. Thus, without undermining Lejewski's valid criticism, or correcting Popper's work by means of highly elaborate technical gear, I came to regard Popper's own harsh judgment of his achievements in logic as unjust.

## Bibliography

- Agassi, Joseph. 2006. On Proof Theory. In *Is There Certain Knowledge?* ed. Michael Rahnfeld, pp. 264–282. Leipzig: Leipzigeruniversitätverlag.
- Bar-Am, Nimrod. 2003. A Framework for a Critical History of Logic. *Sudhoffs Archiv* 87, Heft 1: 80–89.
- Bar-Am, Nimrod. 2004. Extensionalism and Induction in Boole. *Physis* XLI: 97–123.
- Bar-Am, Nimrod. 2008. *Extensionalism: The Revolution in Logic*. Berlin: Springer.
- Corcoran, John. 1994. The Founding of Logic. *Ancient Philosophy* 14: 9–24.
- Jarvie, Ian, Milford, Karl, and Miller, David. 2006. *Karl Popper: A Centenary Assessment*, Vol. III. Burlington, VT: Ashgate.
- Lejewski, Czeslaw. 1974. Popper's Theory of Formal or Deductive Inference. In *The Philosophy of Karl Popper*, ed. Paul Arthur Schilpp, pp. 630–670. La Salle, IL: Open Court.
- Popper, Karl Raimund. 1947a. New Foundations for Logic. *Mind* 56: 193–235. (Corrections in *Mind* 1948. 57: 69–70).
- Popper, Karl Raimund. 1947b. Logic Without Assumptions. *Proceedings of the Aristotelian Society* 47: 251–292.
- Popper, Karl Raimund. 1947c. Functional Logic Without Axioms or Primitive Rules of Inference. *Proceedings of the Koninklijke Nederlandsche Akademie van Wetenschappen* 50: 1214–1224.
- Popper, Karl Raimund. 1948a. On the Theory of Deduction, Part I. Derivation and Its Generalizations. *Proceedings of the Koninklijke Nederlandsche Akademie van Wetenschappen* 51: 173–183.
- Popper, Karl Raimund. 1948b. On the Theory of Deduction, Part II. The Definition of Classical and Intuitionist Negation. *Proceedings of the Koninklijke Nederlandsche Akademie van Wetenschappen* 51: 322–331.
- Popper, Karl Raimund. 1948c. The Trivialization of Mathematical Logic. *Proceedings of the 10th International Congress of Philosophy*, I, 722–727.
- Popper, Karl Raimund. 1954–1955. Lecture Notes of Karl Popper's Course on Logic and Scientific Method Taken by Czeslaw Lejewski and Joseph Agassi. Unpublished.
- Rahnfeld, Michael. 2006. *Is There Certain Knowledge?* Leipzig: Leipzigeruniversitätverlag.
- Schilpp, Paul Arthur. 1974. *The Philosophy of Karl Popper*. La Salle, IL: Open Court.
- Schroeder-Heister, Peter. 2006. Popper's Structuralist Theory of Logic. In *Karl Popper: A Centenary Assessment*, eds. Ian Jarvie, Karl Milford and David Miller, Vol. III, pp. 17–36. Burlington, VT: Ashgate.

# A Problem for Popper's Fallibilism

Ladislav Kvasz and Eugen Zelenák

**Abstract** One of the pillars of Sir Karl Popper's philosophy is fallibilism, according to which there is no certain empirical knowledge. When this position is criticized, it is usually claimed that the scope of fallibilism is restricted, and that there are some areas where infallible knowledge is possible. In the paper we develop a different line of argument. We attempt to show that fallibilism is self-contradictory. Let us consider the following proposition: "‘All propositions are fallible’ is fallible" is infallible. We argue that a fallibilist is committed to hold this proposition to be true. In the paper we discuss consequences of this argument as well as some possible strategies of defense.

In his attack a critic of fallibilism claims that fallibilism is self-contradictory:

Fallibilism is a firm commitment to the notion that we should not make firm commitments. Obviously, fallibilism is self-contradictory and therefore it is a mistake (Conklin 1971, p. 35).

For a fallibilist, however, this criticism presents no serious challenge. She can respond to it by saying that her commitment to fallibilism is also fallible. Nevertheless, it turns out that her claim that 'her commitment to fallibilism is fallible' should be infallible; otherwise it would lead to a contradiction. To show this is the aim of the present paper.

## Popper on Fallibilism

While discussing theories of scientific knowledge, Popper makes a distinction between two main groups of philosophers – verificationists or justificationists, and falsificationists or fallibilists. The first group maintains that only justified or supported beliefs may be taken seriously. In other words, scientific knowledge should

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be proved to be true, in the best case with certainty. Popper places himself into the second group of fallibilists, who claim

[R]oughly speaking, that what cannot (at present) in principle be overthrown by criticism is (at present) unworthy of being seriously considered; while what can in principle be so overthrown and yet resists all our critical efforts to do so may quite possibly be false, but is at any rate not unworthy of being seriously considered and perhaps even of being believed – though only tentatively. (Popper 1963, p. 309)

So, according to fallibilists, scientific knowledge is the type of knowledge that can be neither definitively proven nor established with certainty. It ‘may quite possibly be false’; therefore, it is conjectural, or hypothetical. As Popper puts it: ‘The old scientific ideal of *episteme* – of absolutely certain, demonstrable knowledge – has proved to be an idol. The demand for scientific objectivity makes it inevitable that every scientific statement must remain *tentative for ever*.’ (Popper 1959, p. 280).<sup>1</sup>

It seems that mainly features like being conjectural, hypothetical, tentative or fallible have to do something with Popper’s fallibilism. But to what exactly he thinks we should assign these properties? What type of fallibilism does he promote? Maybe a quick look at how he uses expressions ‘fallible’, ‘conjectural’ etc. will shed some light on the issue. Popper occasionally links fallibility with people. He says ‘we are fallible, and prone to error’ (Popper 1972, p. 265) emphasis added), he speaks of ‘our fallibility’, or that ‘our work is fallible’. On the other hand, Popper sometimes speaks of ‘fallible opinions (*doxa*)’, and of knowledge that is ‘mixed with our errors’ (Popper 1963). When he talks about being conjectural, or hypothetical, he usually attributes these features to knowledge or theories. And finally, tentative seems to be mainly ‘adoption of’ or ‘belief in’ the scientific theory, although, in the above quote from his *Logic of Scientific Discovery* he speaks of ‘scientific statements’ being tentative. Does this survey of his formulations help? It appears that exact wording might be from time to time misleading. Thus, we would do better not to follow slavishly the words he uses, but instead we should try to understand what he *means*. This approach is supported also by the fact that some of his formulations could be meaningfully rephrased in such a way that we would end up attributing fallibility to other entities than the original wording implies.

Popper seems to advocate two types of fallibilism. The first one could be summarized in the thesis ‘*Humans are fallible*’. As Popper notices it is a part of human condition that we err, that we make mistakes. And there are several ways how we may fall short of being perfect. We may fail while perceiving but also while communicating, arguing, reasoning, criticizing etc. The second type of fallibilism is captured in the claim ‘*Knowledge is fallible*’. Especially when Popper speaks of conjectural, hypothetical or tentative nature of scientific theories it is this kind of fallibilism he has on his mind. As he puts it, the crucial feature of (scientific) knowledge is that it ‘may quite possibly be false’, which means, in short, that knowledge is fallible.

Now, how should we understand the claim that knowledge is fallible? Should we read it as ‘Mental states of knowing are fallible’, ‘Justifications for our beliefs are fallible’ or ‘Theories and propositions we adopt are fallible’? Should we even reduce the second type of fallibilism to the first one and claim that ‘Knowledge is



fallible' actually means that epistemic agents (humans) are fallible? In his *Logic* Popper adds to his own view that we can be 'absolutely certain' only in holding our subjective experiences and beliefs a comment 'This last remark is of course a psychological remark rather than an epistemological one' (Popper 1959, p. 280, n. 5). So, Popper acknowledges that assertions about the abilities of epistemic agents belong rather to empirical psychology than to (philosophical) theory of knowledge. Epistemology should be, according to Popper, focusing on the objective knowledge. And, as he puts it, 'knowledge in this objective sense is totally independent of anybody's claim to know; it is also independent of anybody's belief, or disposition to assent; or to assert, or to act. Knowledge in the objective sense is *knowledge without a knower*: it is *knowledge without a knowing subject*.' (Popper 1972, p. 109) So, it is clear that if there is any epistemological fallibilism it is certainly not the psychological one attributing fallibility to people. Another implication of the above quote is that if we want to get epistemologically relevant fallibilism, we should base our interpretation of 'Knowledge is fallible' on Popper's claims concerning objective knowledge.

According to Popper objective knowledge is to be located in the third world, the world consisting of logical contents of scientific theories, arguments etc. Or to quote the author himself '*Theories, or propositions, or statements are the most important third-world linguistic entities.*' (Popper 1972, p. 157). Hence, if we are to obtain epistemological fallibilism we should claim that *these entities* are fallible. And this is exactly what Popper seems to mean when he says that 'every scientific statement must remain *tentative for ever*' (Popper 1959, p. 280) or that '*from the point of view of objective knowledge, all theories therefore remain conjectural.*' (Popper 1972, p. 80).

Thus, in what follows we are going to assume that 'Knowledge is fallible' is basically equivalent to 'Propositions are fallible' and that the main thesis of epistemologically relevant fallibilism Popper advocates is provided by 'All propositions are fallible'.<sup>2</sup> Even if it turned out that our reading is not the only possible interpretation of Popper's (epistemic) fallibilism we hope to have shown that our reading is not without any foundation in Popper's work.

## The Informal Argument

So we are going to assume that the main thesis of fallibilism is

(A) All propositions are fallible.

A critic may try to discredit fallibilism by claiming that if (A) holds, it is certain. In this way infallibility of the main thesis would reveal that fallibilism is self-contradictory. Nevertheless, this type of criticism is inconclusive. According to fallibilism, all propositions are fallible; therefore also (A) itself should be viewed as fallible and not certain. By proclaiming that (A) is fallible a proponent of fallibilism can avoid the conclusion implied by the critic. Thus, this argument does not show that fallibilism is self-contradictory but only that (A) itself is fallible:



(B) ‘All propositions are fallible’ is fallible.

Now we are going to demonstrate that proposition (B) cannot be fallible. So our claim is that the proposition

(C) “‘All propositions are fallible’ is fallible” is fallible.

is simply false. By proving this we will show that there are some infallible propositions and thus that the original thesis of fallibilism (A) is false as well. The argument goes as follows: A fallibilist should hold that (C) is true, because it asserts the fallibility of some proposition. If, however, proposition (C) were true, it would mean that there is a possible state of affairs in which (B) is false. What else would one mean by claiming that (B) is fallible? But if (B) were false, that would mean that the original proposition (A) is infallible. And this is a contradiction, because the proposition (A) says that all propositions are fallible.

So it is obvious that from the presupposition that (C) is true a contradiction follows. Therefore (C) is false, which means the proposition (B) itself is not fallible. Unfortunately for the fallibilist this means that despite the fact that one *can* assert the fallibility of the main thesis (A), one can do so *only in an infallible manner*.

## The Semi-Formal Version of the Argument

We are going to assume that in our language there is a predicate  $F$  meaning ‘*is fallible*’. Thus  $F(\varphi)$  stays for ‘ $\varphi$  is fallible’, where  $\varphi$  is any sentence of the language. To assume that the predicate  $F$  is in the language means that the sentence  $F(\varphi)$  belongs to the language together with all sentences of the form  $F(F(\varphi))$ ,  $F(F(F(\varphi)))$ , etc.

By means of the predicate  $F$  we can rewrite (A), (B), and (C) as follows

(A)  $(\forall \varphi) F(\varphi)$

(B)  $F((\forall \varphi) F(\varphi))$

(C)  $F(F((\forall \varphi) F(\varphi)))$

So far we have introduced the syntax. Now let us deal with the semantics. The sentence ‘*The proposition  $\varphi$  is fallible.*’ might be interpreted along the following lines: ‘There exists a state of affairs in which  $\varphi$  is false’, ‘It is not excluded that  $\varphi$  is false’, or ‘It is possible that  $\varphi$  is false’ etc. For the purposes of our argument we will interpret  $F(\varphi)$  as ‘There exists a state of affairs in which  $\varphi$  is false’ or equivalently ‘There exists a state of affairs in which it is not the case that  $\varphi$ ’.

Let us now suppose that (C) were true. If we rewrite the first predicate  $F$  in (C) following our proposed interpretation, we obtain ‘There exists a state of affairs in which it is not the case that  $F((\forall \varphi) F(\varphi))$ .’ From this it is possible to derive that in a certain state of affairs

(D) It is not the case that  $F((\forall \varphi) F(\varphi))$ .

And this seems to mean that in a certain state of affairs (A) is not fallible. Therefore in that state of affairs (A) is infallible. Now the proposition (D) claiming that (A) is infallible is a contradiction, because (A) says that there are no infallible propositions. So we have derived a contradiction from (C) which means (C) is false and so is (A) – the main thesis of the fallibilist.

## A Fully Fledged Formalization of the Argument

We have already stated our argument both in informal and semi-formal way. Nevertheless, a more formal treatment of the problem might be more convincing. So let us interpret  $F(\varphi)$  to mean  $\Diamond\sim\varphi$  and let us suppose that (C) were true. If we rewrite the two predicates  $F$  in the formula (C), we obtain

$$\Diamond\sim\Diamond\sim(\forall\varphi) F(\varphi).$$

In modal logic  $\sim\Diamond\sim$  means necessity, therefore we can rewrite the last formula as

$$(E) \Diamond\Box(\forall\varphi) F(\varphi).$$

On the other hand our proposition (A), claiming that all propositions are fallible, can be written as

$$(\forall\psi) \Diamond\sim\psi.$$

We changed the variable, so that we can now derive from this formula the next one by substituting a particular formula for  $\psi$ . In the following line, let us substitute for  $\psi$  the formula  $(\forall\varphi)F(\varphi)$ . We obtain:

$$\Diamond\sim(\forall\varphi) F(\varphi).$$

Now by double negation we have

$$\sim\sim\Diamond\sim(\forall\varphi) F(\varphi),$$

which means that

$$(F) \sim\Box(\forall\varphi) F(\varphi).$$

The propositions (E) and (F) lead to a contradiction; at least on the assumptions of the system S5 of modal logic. In the system S5 we have (Feys 1965, formula 70.12)

$$\Diamond\Box p \Leftrightarrow \Box p,$$

which allows us to conclude from (E) simply that  $\Box(\forall\varphi) F(\varphi)$  and that is denied by (F).

Of course, this contradiction is derived on the assumption of the axioms of the system S5, which a fallibilist may (or as we have shown is perhaps forced to) deny. So the formal proof smuggled into fallibilism several strong logical assumptions, which the fallibilist is by no means forced to accept. Therefore the semi-formalization of our argument might be viewed as the core of this paper. The systematic formalization requires additional assumptions, and so its force is weaker. But on the other hand the possibility of a formalization of our argument shows that it is coherent and cannot be so easily refuted.

## **Possible Lines of Defense Against the Semiformal Argument**

As it is in philosophical controversies the case, no argument seems to be conclusive and against each line of argumentation there are several possibilities of defence. We can see at least five of them.

### ***To Dismiss the Whole Argument as Irrelevant***

One possible reaction to the above line of argumentation would be to maintain that it is philosophically irrelevant. Surely, fallibilist may claim, such technical quibbles do not seem to be of any use in serious philosophical discussion.

However, we consider such an answer as problematic, at least as long as its proponent does not explain more clearly the boundary between relevant and irrelevant philosophical arguments. Otherwise almost any philosophical thesis could be defended against chosen criticism by denying relevancy of the criticism. Who is the judge to decide which arguments are relevant and which are irrelevant?

Furthermore, this line of defence seems to contradict the Popperian spirit of criticism. And what is perhaps the most important thing; the history of Western thought provides several examples when seemingly irrelevant paradoxes led to important discoveries. It is sufficient to mention the liar paradox, which could have been also conceived as a minor technical quibble, but it proved its fruitfulness when Russell, Gödel and Turing used it in an ingenious way in their work.

### ***To Introduce the Distinction Between Language and Meta-Language***

Another possibility how a fallibilist might answer our argument would be to acknowledge that it is a genuine paradox against which we have, nevertheless, standard logical means of defence. A fallibilist may claim that it is very similar to the standard semantic paradoxes, such as: 'This sentence is false' and so it could be

resolved in the same way. We have simply to distinguish between object language (L), meta-language (ML), meta-meta-language (MML) etc. Then instead of saying that 'All propositions are fallible' we should say 'All propositions in L are fallible', 'All propositions in ML are fallible' etc. This should no longer create paradoxes. (Of course, instead of Tarski's hierarchy of languages one could take Russell's hierarchy of types. But the whole argument would be almost the same.)

This way of dealing with the above argument is more sympathetic to the argument itself. It acknowledges that it is a genuine philosophical argument; nevertheless, it denies its originality. The fallibilist simply subsumes our argument under the heading of semantic paradoxes and considers it resolved. Yet even, if the distinction between object-language and meta-language is fully appropriate in mathematics, physics, or biology, where we can draw a sharp border line between the objects we study and our propositions about these objects, in political science the situation is much more complex. In political science we study the preferences, beliefs, interests and actions of people, considered as agents of political life. Now Popper's fallibilism thesis, which belongs, when considered in the context of physics or biology, to the meta-language, can nevertheless act as an item of the object level in political science. For instance when a committed Marxist reads this thesis of Popper, he could change his political preferences and join the reform movement. This shows that a meta-language thesis can become part of the object level; it can be the cause of some political action. Therefore it seems to us, that the distinction between object-level and meta-level, which for the physical or biological sciences is a sharp one, is much more complicated in the field of political science.

Thus our answer to this second line of defence is that in the context of political science all three propositions of our argument, namely (A), (B), (C), must be considered as propositions of the object level. They can bring about changes in political positions and therefore all three belong to the reality studied by political science.

### ***To Introduce Different Predicates of Fallibility***

The third possible defence against the argument contained in this paper would be to accept that it is a genuine philosophical argument and to accept also that it cannot be resolved using the standard semantic distinction of language and meta-language because of the reasons discussed in the previous point. Then the brunt of the defence against our argument could be concentrated around our analysis of the notion of fallibility.

On the one hand, it is possible to question our interpretation of the sentence '*The proposition  $\phi$  is fallible.*' We interpreted it as '*There exists a state of affairs in which  $\phi$  is false*'. One could question this interpretation, but it is difficult to imagine what else could fallibility of a proposition mean. Another possibility would be to introduce several different notions of fallibility. They all would belong to the object language but would, nevertheless, have different meanings. In this sense, our paradox could be interpreted as an indication that at least in the proposition (C) we have to deal with fallibilism of some (unknown) special type.

While the previous line of defence tried to resolve the problem by standard semantic means, this third line seems to be a rather *ad hoc* one. As long as we have no other motivation for introducing different semantics or even different notions of fallibility; and first of all, as long as we give no clear explanation of their difference, it does not seem to be a plausible way out. However, the burden to deliver such a motivation and semantics lies with the fallibilist. Our aim was simply to point to these problems.

### ***To Apply Fallibility Not to Propositions but to Our Knowledge***

A fourth possible answer to our argument is to say that fallibilism is not about propositions, but about our *knowledge of* or *beliefs in* propositions. Nevertheless, as shown in the quotations at the beginning of our paper, this was not Popper's own view. And we believe Popper had good reasons for not adopting it. The fact that our knowledge and our beliefs are fallible is a simple psychological observation. It is, of course, true, but from an epistemological point of view not very interesting.

If this should be the content of the fallibilism thesis, then fallibilism could not be used against Marxism, one of Popper's main targets. Every reasonable Marxist would, of course, admit that he as a person is fallible. He could even admit that Marx himself was. But still he would claim, that despite all these human shortcomings, Marxism as such expresses the inevitable truth of history. And we believe that the Popperian fallibilism was designed to discredit these types of claims. Therefore, it seems to us, that this fourth option is not a feasible one, at least not for a critical rationalist.

### ***To Apply Fallibility Not to Propositions but to Our Positions of Holding Propositions***

The perhaps most convincing solution of the paradox can be found in David Miller's analysis of a similar problem. It consists in attaching fallibility neither to propositions, nor to our knowledge of propositions, but to our *positions* of holding propositions.

Miller developed his analysis as a defence of the comprehensive critical rationalism against the following attack:

( $\alpha$ ) All positions are open to criticism.

( $\beta$ )  $\alpha$  is open to criticism.

Since ( $\beta$ ) is a consequence of ( $\alpha$ ), any criticism of ( $\beta$ ) will be also a criticism of ( $\alpha$ ). Thus if someone would show that ( $\beta$ ) is false, this would automatically mean that ( $\alpha$ ) also is false. But showing that ( $\alpha$ ) is false means to criticise ( $\alpha$ ), what

means that  $(\beta)$  is true. Thus, if  $(\beta)$  is false, then it is true. Any attempt to criticize  $(\beta)$  demonstrates it; thus  $(\beta)$  is uncriticizable, and  $(\alpha)$  is false.

Miller's idea is to discriminate between a *statement* and the *position of holding this statement*. Miller says:

CCR must not be understood to hold that every statement that a comprehensively critical rationalist counts as true (rationally accepts) is on its own criticizable.... As far as statements ... are concerned, what is important for the rationalist, I suggest, is that each statement that he accepts either is itself criticizable or follows from a statement that he accepts that is criticizable. Any position adopted must be criticizable, but it is no concession to the irrationalist to allow that some logical consequences of the position may not be criticizable. (Miller 1994, p. 89).

This proposal is motivated by the fact that falsifiable statements can entail unfalsifiable ones. Thus Miller views statement  $(\beta)$  in analogy with mathematical propositions or with logical tautologies. According to him they too are unfalsifiable consequences of falsifiable statements.

To follow Miller would be to ascribe fallibility to rationally defensible positions. This option seems to be a viable middle ground between the attempts on the one hand to ascribe it to propositions and on the other hand to ascribe it to our knowledge. In admitting that it is sometimes not meaningful to hold some propositions simultaneously, we are not giving up the possibility to criticise positions like Marxism. Thus, this solution seems to rescue a sufficient amount of the content of the fallibilism thesis. Nevertheless, Miller's distinction between a statement and a position arose some criticism; see for instance (Cintora 2004). Further, in areas like political science, where the discussion involves statements about different positions, it is not clear whether Miller's distinction between a position and a statement can be unequivocally drawn.

## Conclusions

Our paper is, of course, not meant to be a refutation of fallibilism. Its aim is much more moderate. We wanted to point to a possible reoccurrence of some classical problems and arguments in the area of fallibilism.

We modeled our argument on the pattern of Bernard Bolzano's famous proof that there are "*propositions as such*". Bolzano's argument is that if there were none, then the proposition "*There are no propositions as such*" would be a proposition as such. This argument of Bolzano has a peculiar position in the history of mathematics, because in combination with the omniscience of God it is used by Bolzano to provide the only attempt in the entire history of mathematics to prove that there do actually exist infinite collections of objects. The sentence of Bolzano indicates that besides logical tautologies and mathematical theorems there is a third realm of rationally accessible propositions.

What we think is interesting in our argument is that it has the form of a unique sentence, just like in the case of Bolzano, namely "*All propositions are fallible*'

*is fallible*” is *fallible*. It is, of course, possible to decompose this sentence into language and meta-language components or into a position and a sentence and then treat these components in a dialogic way. But we still believe that the sentence itself has a slightly different meaning than its various decompositions.

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## Notes

1. In fact, Popper admits that there is an exception falling outside of the scope of non-demonstrable knowledge, but we are going to ignore this issue here. ‘As soon as we take objective knowledge into account, we must say that at best only a very small part of it can be given anything like sufficient reasons for certain truth: it is that small part (if any) which can be described as *demonstrable knowledge* and which comprises (if anything) the propositions of formal logic and of (finite) arithmetic.

All else – by far the most important part of objective knowledge, and the part that comprises natural sciences, such as physics and physiology – is essentially conjectural or hypothetical in character; there simply are no sufficient reasons for holding these hypotheses to be true, let alone certainly true. (See Popper 1972, p. 75 and Bartley 1984)

2. However, see (Haack 1978) for her preference of agent fallibilism and criticism of proposition fallibilism.

## References

- Bartley, William Warren. 1984. *The Retreat to Commitment*. La Salle, IL: Open Court.
- Cintora, Armando. 2004. Miller’s Defence of Bartley’s Pancritical Rationalism. *Sorites* 15: 50–55.
- Conklin, Kenneth Robert. 1971. ‘Fallibilism: A Terrible Mistake’. *The Educational Forum* 36: 35–42.
- Feys, Robert. 1965. *Modal Logics*. Paris: Gauthier-Villars.
- Haack, Susan. 1978. *Philosophy of Logics*. Cambridge: Cambridge University Press.
- Miller, David. 1994. *Critical Rationalism: A Restatement and Defence*. Chicago/La Salle, IL: Open Court.
- Popper, Karl Raimund. 1959. *The Logic of Scientific Discovery*. London: Routledge.
- Popper, Karl Raimund. 1963. *Conjectures and Refutations: The Growth of Scientific Knowledge*. London: Routledge.
- Popper, Karl Raimund. 1972. *Objective Knowledge: An Evolutionary Approach*. Oxford: Clarendon.

# Why Advocate Pancritical Rationalism?

Darrell P. Rowbottom and Otávio Bueno

**Abstract** This paper provides a rationale for advocating pancritical rationalism. First, it argues that the advocate of critical rationalism may accept (but *not* be internally justified in accepting) that there is ‘justification’ in an externalist sense, specifically that certain procedures can track truth, and suggest that this recognition should inform practice; that one should try to determine which sources and methods are appropriate for various aspects of inquiry, and to what extent they are. Second, it argues that if there is external justification, then a critical rationalist is better off than a dogmatist from an evolutionary perspective.

## Introduction

Consider two individuals. One believes  $h$  dogmatically, and will never give it up. The other believes  $h$  just as strongly (i.e., has the same synchronic degree of belief), but is prepared to reconsider that belief in the light of criticism. Is the latter in a better position than the former? From an ordinary language point of view, it seems as if the advocate of (comprehensively) critical rationalism thinks so; and therefore also believes that the latter is ‘justified’ in believing  $h$  in a manner that the former is not. Why else recommend the critical attitude?

A possible answer is that the former individual is incapable of learning (insofar as  $h$  is concerned), whereas the latter clearly is so capable. But if we accept that one can learn something that is false, as (comprehensively) critical rationalists tend to, then this seems insufficient. Learning could lead one to false beliefs, rather than just true ones. It could also be fatal, rather than merely dangerous.

We endeavour to solve this problem. We argue that the advocate of critical rationalism may accept (but *not* be internally justified in accepting) that there is ‘justification’ in an externalist sense, specifically that certain procedures can track

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truth, and suggest that this recognition should inform practice; that one should try to determine which sources and methods are appropriate for various aspects of inquiry, and to what extent they are. We also point out that Popper seems to accept something similar, in one of his discussions concerning observation statements.

But if there is external justification, then why is a (comprehensively) critical rationalist better off than a dogmatist? We argue that the former enjoys better flexibility and adaptability, and is therefore in a superior position from an evolutionary perspective (*ceteris paribus*).

## Critical Rationalism and Faith

According to Popper's discussion in *The Open Society and Its Enemies*, being a critical rationalist is ultimately a matter of faith:

Whoever adopts the rationalist attitude does so because he has adopted, consciously or unconsciously, some proposal, or decision, or belief, or behaviour; an adoption which may be called 'irrational'. Whether this adoption is tentative or leads to a settled habit, we may describe it as an irrational *faith in reason*. (Popper 2003 [1966], p. 255)

If this is correct, however, then it hardly provides a platform on which to promote critical rationalism – understood, to a first approximation, as the philosophical position that we ought to adopt critical attitudes – by argument. Instead, the way ahead would appear to be to proselytize; to spread the faith by encouraging people to make such an irrational leap. Evangelism and critical rationalism would go hand in hand, in principle if not in practice.

The problem with this, as was forcefully argued by Bartley (1962), is that it provides an excuse for irrationalism. If one admits that being a rationalist requires an irrational move, then one should also admit the primacy of irrationalism. There is hence a great excuse – by the rationalist's own admission – for selecting a different faith:

In sum, the belief that rationality is ultimately limited, by providing an excuse for irrational commitment, enables a Protestant, or any other irrationalist, to make an irrational commitment without losing intellectual integrity. (Bartley 1962, p. 103)

Furthermore, it appears curious to suggest that a genuine rationalist would *want* to encourage anyone, rationalist or not, to perform a totally irrational act: to make a leap of faith and sustain that faith come what may. "Behave irrationally just one more time, because it's the key to behaving rationally!" is clearly no way to advocate rationalism. Nor, indeed, is "Be dogmatic in your non-dogmatism!"<sup>1</sup> Similarly, Christians who are true to the principles of Christianity do not resort to unethical means to win converts. (And if we are not prepared to make irrational leaps of faith, then why should we expect others to do so?)

It comes as some relief, then, that Bartley (1962, 1984) argues that rationalists need not rely on faith at all:

We can assume or be *convinced* of the truth of something without being *committed* to its truth... A rationalist can, while eschewing intellectual commitments, retain both the courage

of his convictions and the courage to go on attacking his convictions—the courage to think and go on thinking. (Bartley 1984, p. 121)

For present purposes, we will assume – although it is by no means uncontroversial – that Bartley is right about this. But simply because there is no excuse for irrational commitment, it does not follow that we *should* be rationalists in Bartley’s sense. In fact, Bartley (1962, pp. 215–216) confesses: ‘[M]y argument does not force anyone to be a rationalist... Anyone who wishes, or who is personally able to do so, may remain an irrationalist.’

Our aim in what follows is to address this outstanding problem, which is one of considerable practical significance for critical rationalism as a movement, as well as one of considerable interest to any self-styled critical rationalist who is intent on assessing the wisdom of continuing to strive to have a critical attitude!

One might object that “Why advocate pancritical rationalism?” is not the sort of question that a pancritical rationalist ought to ask, because it is (or looks like) a request for justification.<sup>2</sup> However, we do not think that the appropriate methodology for answering the question, or indeed the expected form of the answer, is implicit in the question. We contend that it is perfectly reasonable, for instance, to answer it by critically comparing critical rationalism with the alternatives in the relevant domain. The appropriate question is presumably supposed to be “What’s wrong with critical rationalism?” But a dogmatist may answer simply with “It is not endorsed by the authority”, or even “It is conclusively falsified by the authority”. Ultimately, the verbal formulation therefore matters little. What *does* matter is how we tackle the question.

## Two Kinds of Argument for Pancritical Rationalism

In saying that he has no argument against being a dogmatist, Bartley is perhaps a little unkind to himself.<sup>3</sup> In fact, it is possible to reconstruct two separate kinds of argument from *The Retreat to Commitment*. The first of these is ethical in character, and suggests that those with critical attitudes may conduct themselves in a more understanding and generous manner than those without:

[S]ince the rationalist... need be committed neither to his rationalism nor to any other of his beliefs, he need not repudiate people with whom he fundamentally disagrees. In principle, he can act toward them in a remarkable way. (Bartley 1962, p. 216)

The second argument is epistemic in character, and suggests that there are reasons of self-interest for having a critical attitude. The fundamental idea is that we may profit from our interactions with others in a manner that others may not from their interactions with us:

[I]f we treat our opponents in discussion *not as they treat us, but as we would have them treat us*, it is we who profit... We may learn from the criticisms of our opponents even when their own practice prevents them from learning from us. (Bartley 1962, p. 220)

There is a clear sense in which these two arguments are related. By adopting a critical attitude, Bartley thinks, we can behave in a way that enables us to learn from

others when we otherwise might not be able to. Yet doing so will also allow us to behave toward them in a gentler and kinder way than we otherwise might. We need not treat them as enemies of the faith, to be converted or dispensed with. We might even feel rather sorry for some of them (although not, of course, on the basis of a smug assumption of superiority on our own part).

In what follows, we will focus on the second argument; roughly, that there are situations in which we can learn if we're critical, but cannot if we're not. If correct, however, the first argument is still important. For it suggests that increasing the number of people with critical attitudes need not have a detrimental effect at the societal level; that one person's gain in this respect need not result in another's, or indeed the community's, loss.

Before we continue, however, we'd like to cast some doubt on the strength of the second argument, as it stands. Let's accept that having a critical attitude does enable new learning possibilities, as this seems rather uncontroversial; e.g., that a fundamentalist's belief in 'God exists' may not be shaken by any argument, whereas a pancritical rationalist's might be. What, precisely, is the benefit for the latter? 'God exists' may, after all, be true. And if we accept that we can learn things which are false – as one of us, while at secondary school, learned that Europeans in the time of Columbus thought that the Earth was flat – then there may be no advantage whatsoever (or even a serious disadvantage in some scenarios). In short, 'learning' in this sense – simply changing one's beliefs – doesn't seem to have any value.

## Justification Versus Justificationism

The previous point may be made more starkly as follows. For every conceivable person with a large number of false beliefs and a small number of true beliefs who will come to have more true beliefs than false ones through possessing a critical attitude, there is a conceivable person with a large number of true beliefs and a small number of false beliefs who will come to have more false beliefs than true beliefs. Ranging over possible dogmatists, what's more, some lucky souls will be dogmatic just about those things that are true.

In light of this, one option would be to suggest that having a critical attitude provides a kind of internal justification for one's beliefs that having an uncritical attitude does not. But even if this were true, what would the value of such justification be? BonJour (1985, pp. 7–8) suggests that there is none unless justification has a link to truth:

Why should we... *care* whether our beliefs are epistemically justified? ... The goal of our distinctively cognitive endeavors is *truth*... If finding epistemically justified beliefs did not substantially increase the likelihood of finding true ones, then epistemic justification would be irrelevant to our main cognitive goal and of dubious worth... Epistemic justification is therefore in the final analysis only an instrumental value, not an intrinsic one.<sup>4</sup>

Such a link between having a critical attitude and being in a better position to find the truth is, in fact, precisely what is missing. Caring about the truth and doing the

best to find it might even lead you unerringly to believe things that are entirely false. Conversely, not caring a jot about the truth might lead you to commit dogmatically to a whole host of true beliefs.

Similar complaints – that there is a gap between method and aim – have been made about (pan)critical rationalism, and indeed Popper’s philosophy of science, before. Newton-Smith (1981, p. 70), for instance, attacks the putative link between corroboration and verisimilitude. And Watkins (1997, Sect. 13) has more recently suggested, along similar lines, that ‘If one is to aim at X, and pursue one’s aim rationally, one needs to be able to monitor the success or failure of one’s attempts to achieve X’, and therefore proposed a negative answer to the question ‘Are Popperians entitled to claim that one could do so if X were simply *truth*?’<sup>5</sup> Watkins thinks that the answer lies in appeal to possible truth as an aim. But surely this could also be the aim of dogmatic commitment, if fallibilism is accepted. It is, after all, more than merely *logically* possible for theories to be falsified on the basis of false observation statements. It is also more than *logically* possible to be committed to the truth of a proposition which is actually true.

Miller’s (1994, p. 418) reply appears to be that ‘falsificationism is unable to justify (in whole or in part) its role in the search for truth’, and he would presumably add that this goes equally for the critical attitude. But even if we accept this, it is hardly a satisfactory response. We are asking precisely whether there is a link between adopting a critical attitude and improving one’s epistemic lot. And it would appear to be perfectly reasonable to think that there is not such a link, if a suitable possibility cannot even be outlined. (If preferred, as sketched in the second section, we are asking the pancritical rationalist to provide *an argument* against dogmatism. We have seen that “Dogmatists cannot learn” does not appear, taken alone, to do the job.)

As a way into finding an answer, we should first like to note that to advocate pancritical rationalism is to reject authoritarianism, and indeed *authoritarian* accounts of justification, but need not be to reject the notion of justification wholesale. Consider, in this regard, Bartley’s account of ‘justificationism’:

[I]t is the view that the way to criticize an idea is to see whether and how it can be justified... Such justification involves:

- (1) an authority (or authorities), or authoritatively good trait, in terms of which final evaluation is to be made;
- (2) the idea that goodness or badness of any idea or policy is to be determined by reducing it to... the authority (or authorities), or to statements possessing the authoritatively good trait. That which can be so reduced is justified; that which cannot is to be rejected. (Bartley 1984, pp. 186–187)

In fact, it is easy to see that one could fail to be a justificationist, in exactly this sense, while still believing in justification.<sup>6</sup> One need only believe that there are no authorities, or authoritatively good traits, according to which *final* evaluations of hypotheses – or even everyday statements – should be made. In short, one may accept the possibility that one’s means of evaluation are not beyond question, and are in no sense ‘final’, while nevertheless accepting that justification is to be had. And one obvious way to do this, although surely not the only one, would be to

suggest that one can be justified in believing that *p* *without realizing that one is justified in believing that p*.

## Internal Versus External Justification

By emphasizing that justification need not be internal – in the sense that to take something as a justification, we need to be aware that it is a justification – the pancritical rationalist can employ an external notion of justification, rather than an internal one. But which features should such a notion of justification have?

From the discussion above, it is clear that external justification should have a significant link to truth. There may be some mechanism, or process, that links truth and justification in some systematic, regular way. However, for the pancritical rationalist, our ability to recognize such a link is *fallible*. To require that we could infallibly establish, or identify, a means of recognizing the connection between justification and truth would amount to a return to justificationism, with an implicit demand for ultimate justifications. As we saw, a pancritical rationalist wouldn't take that route. However, we emphasize the importance of there being a connection between truth and justification.<sup>7</sup> Otherwise, it's unclear why the critical rationalist would be any better off than the dogmatist.

To articulate an external account of justification meeting the conditions above, we recall the counterfactual account of tracking developed by Nozick (1981). On the counterfactual view, two conditions are needed for our beliefs to track the truth of a statement *p*:

(T1) If *p* weren't true, we wouldn't believe that it is.

(T2) If *p* were true, we would believe that it is.

As an illustration, suppose that you're a biologist investigating the components of the structure of cells using an electron microscope. This instrument offers a much higher resolution than anything provided by an optical microscope. Moreover, the images that are generated by each device overlap. As a result, you can explore the overlap to identify more clearly the new components of cells that are enhanced by the electron microscope. Suppose that, as a result of this process, you identify a new component in the nucleus of cells.<sup>8</sup> In this case, as long as the electron microscope is reliable, you could track the new cellular component, in the sense that: if this component weren't in the cell, you wouldn't believe that it was; and if the component were there, you would believe that it was. A reliable instrument allows you to track novel phenomena.

Of course, you may not know whether the instrument is in the end reliable, and on an externalist view, this lack of knowledge of the instrument's reliability doesn't prevent you from being *justified* in forming the belief about the new component of cells. In fact, in many cases, it's *not* clear that you know whether the instrument you're using is indeed reliable, given that often the only access you have to the sample under study is via the very instrument that you're using to study the sample.<sup>9</sup>

Consider now the tracking conditions (T1) and (T2) as a requirement not on the notion of knowledge (as Nozick takes them to be), but only on the notion of justification. A belief is then justified if it's sensitive to the environment in which it is generated – if it tracks the relevant features of the world, by satisfying the conditions (T1) and (T2). In other words: if  $p$  weren't true, you wouldn't believe that  $p$ ; if  $p$  were true, you would so believe.

On this conception of justification, there is then a close connection between being justified in believing something and having a true belief. However, as noted, the pancritical rationalist doesn't take as a requirement for us to be justified in believing that  $p$  that we are aware (let alone *know*) that we are justified in having such a belief. So the justification the pancritical rationalist invokes is external.

On this picture, our capacity of recognizing that something is a justification is also *fallible*. After all, in many contexts, we may think that our beliefs are tracking certain aspects of the world while in fact they are tracking something else. The fact that we don't know that we are justified in having the beliefs we do makes room for such fallibilism. For example, for a long time Newtonian physicists thought that the notion of gravity invoked in their theorizing tracked a significant aspect of the world. And it did track something, but not what they thought they were tracking. With relativity theory, it became clear that gravity should not be understood as a force, but should rather be conceptualized in terms of space-time. This is the feature of the world that the relevant physical theories should be tracking – at least if relativity theory is true. However, since we don't know whether the latter theory is true, we don't *know* that we are justified in believing that our current theories are indeed tracking what we think they are. But this lack of knowledge, on an externalist view, doesn't prevent us from being justified in believing that gravity is related to the curvature of space-time.

How does the tracking account of justification explain how a pancritical rationalist is better off than the dogmatist? It's not clear that we can guarantee that the former will be better off than the latter in every situation. But we don't think that this is the result that the pancritical rationalist needs. All that needs to be offered is a principled account as to why the pancritical rationalist will be better off than the dogmatist in typical cases. And in fact, a pancritical rationalist – by being open to revising his/her beliefs, experimental procedures, and so forth – can avoid mistakes by tracking the relevant features of the world. A blind commitment to Newtonian physics would have prevented the dogmatist from shifting to relativity theory. Similarly, a blind commitment to employing one instrument (or procedure) in particular may limit one's horizons, even in the event that said instrument (or procedure) is reliable. (Think of refusing to use a telescope to make observations of celestial phenomena, and instead trusting only the naked eye.) And since our ability to recognize that something is a justification is fallible, the account offered here meshes well with the stance adopted by the pancritical rationalist: a critical attitude toward all of his/her beliefs and even 'aims... conjectures, decisions, ideas, policies, [and] programs' (Bartley 1984, p. 183).

A similar argument might be made without mentioning tracking, but instead by suggesting that there may be reliable means by which to form beliefs (or classify statements as true or false) which serve to (externally) justify the beliefs so formed.<sup>10</sup>

Imagine, for instance, that we can employ a procedure which has a high propensity to accurately sort a peculiar class of propositions into ‘true’ and ‘false’ groups. A dogmatist will not be able to accept the results of such a procedure if they conflict with his commitments. Moreover, a dogmatist committed to a procedure that instead has a high propensity to inaccurately sort propositions will not be able to renounce it. (Think of whether we should prefer intuition or observation to generate test statements for our scientific theories.) The dogmatist’s situation will progressively worsen, over time, unless he or she makes lucky commitments. Ranging over possible dogmatists, what’s more, we can see that being lucky has a low *objective* probability. This is clearly the case if we assume what most of us seem to, namely that there are more possible unreliable methods, procedures, and so forth, than there are reliable ones. (We could classify theories as true or false on the basis of coin-flipping, the reading of tea-leaves, the reading of palms, astrological charts of their advocates, etc.) In searching for the reliable, we are looking for a needle in a haystack.

It is worth adding that at one stage, at least, Popper (1974, p. 1114) appears to endorse this sort of 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.

It is unclear whether Popper thinks that the fallibility arises because we may be wrong that our observations are reliable, or only because our experiences can lead us to mistaken beliefs. We think it arises on both counts.

## Conclusion

The considerations above indicate that a pancritical rationalist need not shy away from the notion of justification. As we saw, by adopting an external account of justification, the pancritical rationalist can be better off than the dogmatist while still avoiding justificationism. Although there is much more to be said, we hope we have said enough to indicate why advocating pancritical rationalism is a sensible thing to do.

## Notes

1. Albert (1985, p. 163) similarly intimates, in his discussion of theologians, that critical rationalists should avoid being ‘*critical but nonetheless dogmatic*, critical in the things that are not so important to them, dogmatic in those that seem to be more so.’
2. This suggestion was made to us by David Miller.
3. We will use ‘dogmatist’ and ‘irrationalist’ interchangeably. The point is precisely that the non-rationalist – in Bartley’s sense of ‘rationalist’ – has *commitments* to (rather than mere convictions about) particular propositions, ideas, theories, hypotheses, methods, or the like.



4. See also Rowbottom (2008) for a related discussion and a list of further pertinent references.
5. Watkins does not suggest, of course, that we cannot (or that science should or does not) aspire after truth. His point, rather, is that an aspiration is somewhat different from a rational aim.
6. Note that Musgrave (forthcoming) rejects 'justificationism' in a slightly different sense. Our treatment here does not depend on accepting this either.
7. Those who are sympathetic to an empiricist approach could read this claim as emphasizing the connection between justification and *empirical adequacy* – roughly, truth about the observable phenomena (see van Fraassen 1980; Bueno 1997).
8. In fact, it was exactly through this procedure, and with the crucial use of the electron microscope, that ribosomes were first identified. See Palade (1955).
9. Of course, whenever you can, you will use *additional* instruments to double check and compare the results you obtained with the original instrument. (Palade [1955] also did that.) But the point remains: the reliability of these additional instruments needs to be examined.
10. This may be significant because of the notorious difficulties surrounding the truth-conditions of counterfactual conditionals.

## References

- Albert, Hans. 1985. *Treatise on critical reason*. Princeton, NJ: Princeton University Press.
- Bartley, William Warren. 1962. *The retreat to commitment*. New York: Alfred A. Knopf.
- Bartley, William Warren. 1984. *The retreat to commitment*, 2nd edition. La Salle, IL: Open Court.
- BonJour, Laurence. 1985. *The structure of empirical knowledge*. Cambridge, MA: Harvard University Press.
- Bueno, Otávio. 1997. Empirical adequacy: a partial structures approach. *Studies in History and Philosophy of Science* 28: 585–610.
- Miller, David. 1994. *Critical rationalism: a restatement and defence*. La Salle, IL: Open Court.
- Musgrave Alan. Experience and perceptual belief. [This volume].
- Newton-Smith, William. 1981. *The rationality of science*. London: Routledge.
- Nozick, Robert. 1981. *Philosophical explanations*. Cambridge, MA: Harvard University Press.
- Palade, George. 1955. A small particulate component of the cytoplasm. *Journal of Biophysical and Biochemical Cytology* 1: 59–79.
- Popper, Karl Raimund. 1974. Ayer on empiricism. In *The philosophy of Karl Popper*, ed. Paul Schilpp, pp. 1100–1114. La Salle, IL: Open Court.
- Popper, Karl Raimund. 2003 (1966). *The open society and its enemies, volume two: Hegel and Marx*. London: Routledge.
- Rowbottom, Darrell Patrick. 2008. N-rays and the semantic view of scientific progress. *Studies in History and Philosophy of Science* 39: 277–278.
- van Fraassen, Bas. 1980. *The scientific image*. Oxford: Clarendon.
- Watkins, John. 1997. Popperian ideas on progress and rationality in science. *The Critical Rationalist* 2. <http://www.eeng.dcu.ie/~tkpw/tcr/volume-02/number-02/v02n02.html>. Accessed 07 July, 2008.



# Karl Popper and Hans Albert – The Broad Scope of Critical Rationalism

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**Abstract** It was in the English speaking world that Karl Popper first caught the attention of broader philosophical circles, and it is primarily in this same world that his views still continuously elicit a response, whether positive or negative. However, in Central Europe the slightly delayed Popper's influence has been quite often if not mediated then at least supported by other developments within German philosophy. Not surprisingly, from the very beginning Popper's philosophical return ran up against the prevalent local philosophical tendencies of the times, from a selective but mostly negative Marxist attitude to a patent disinterest and disregard by most existentialists, phenomenologists, and Heideggerians. A rather important role in this complex situation has been played by Hans Albert; from the late 1950s on he has been an outspoken protagonist for Popper's philosophy. The scope of Albert's activities extends from expository endeavors to critical exchanges with the opponents; of the latter the best known being his early involvement in the so-called "Positivismusstreit", covering a substantial part of the 1970s. However, Albert's role was never one of a mere apologist, his own scholarly background allowed him to expand the sphere of problems dealt with as well as the range of thinkers involved in such critical discussions. To discuss those less known aspects of critical rationalism will be the task of this chapter.

It is not our ideas and ideals which shape the world of human communities and civilization but rather what we decide to do with those ideas. An idea in the mind can simply stay where it is and die there, it is what we do with them in the outer world which counts and gets things done.

Jim Dale, Critical Rationalism Forum, posted Sept. 1, 2007.

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## Introduction

In his short intellectual autobiography with a telling heading ‘Vom Kulturpessimismus zum kritischem Rationalismus’ Albert reminisces on his 1958 Alpbach encounter with Karl Popper (Albert 2001, p. 19).

It soon became clear to me that Popper’s Critical Rationalism presented in many ways a more satisfactory conception for the study of science than at that time dominating neo-classical Empiricism or the analytical approach influenced by Wittgenstein’s late philosophy. And that is not to speak at all about hermeneutical and dialectical theories then characteristic for the philosophy of Continental Europe.

*In nuce*, this already presents a useful general setting for our discussion. Negatively, it counters the still widely accepted concept of the rift between continental and analytic philosophy as being too narrow to accommodate critical rationalism. Positively, it points out that the abandonment of such constricting schema opens a possibility not only for a fresh look at a general relevance of Popper’s and Albert’s work but also for a more nuanced picture of the relation between the poles of such presumed rift.

Due to their ongoing polemics, both Popper and Albert are often seen primarily as the critics, sometimes rather harsh, of the positions of their fellow philosophers. However, this view is too narrow to give a proper due to the inner logic of their philosophical development. Yet, there is no doubt that at least on the public side, their own philosophy developed within the process of such critical reckoning with those “others.” In the case of Popper, this applies right from the beginning to most aspects of his rather long professional life, albeit possibly in a milder form during his concluding years. In Albert’s own professional carrier, this feature became clear slightly later, delayed partly due to the external circumstances of his life in the post-World War II Germany; nevertheless since then it has been always present, oriented primarily towards his German contemporaries. However, in our discussion of Albert’s development we will start with what is considered by him and his critics as the point of overcoming certain of his early positions. Since the character of this turn is at least historically central for Albert’s ‘Popperianism,’ we will deal with it first. It is also of crucial importance for identifying and explaining some important differences in their respective foci. Albert himself writes about it in 2003 (Albert 2003a, pp. 37–38).

It thus follows from my presentation of the development of Popper’s views on the methodology of social sciences that the lawfulness discussed in his book *Poverty of Historicism* has not been considered in his later works. It has been replaced by a Principle of Rationality, limited in its content, and the explanation of social relations was reduced to the situational logic, operating without nomological hypotheses. This also appears to have put aside the foundations for social technology, related to his idea of social engineering by small steps. And it also appears that he *de facto* gave up the idea of the unity of scientific method, his own claim notwithstanding. According to his earlier conception the search for lawfulness in all spheres of reality is an essential aspect of such method. **And there is no reason for giving up such search in any given area.** (stress is mine – VZ)

It will only be in the second part of this chapter that we will concentrate on some selected points in Albert’s position which make it complementary to Popper’s own position. For this purpose, we will accept Albert’s main critical claim about the

narrowing in orientation of the late Popper and its overcoming in Albert's own work, setting aside a rather complex problem of its justification.

## Albert Before the 1958 Encounter with Popper

When in 1969 the book presenting the 1961 non-dispute between Popper and Adorno followed by the true though partly apologetic disputes led by others finally appeared, it was read with a great interest by social scientists as well as philosophers. Various other secondary ironies aside, including its considerable disregard for any economy of thought, the work is still of some interest, and this not just for more historically oriented scholars. We shall return to it, considering it together with the *Treatise on Critical Reason*, the first extensive presentation of Albert's position.

In the case of Popper's philosophical development, one almost invariably starts with what he himself presented as an original impetus – his realization that unlike say Einstein's General Theory of Relativity, certain popular psychological and social theories claiming scientific character appeared to be totally immune to any type of testing (Popper 1963). A quarter of a century later, Albert's post-World War II autodidactic period finished after just 1 year after his admission to the Faculty of Business and Social Science of the University of Cologne. His initial goal was to receive a degree enabling him to start a non-academic professional carrier, yet his private reading as well as a selection of topics for his academic assignments led him more and more towards theoretical and methodological problems, to sociology and partly through it to philosophy and philosophy of science as well. The title of his 1952 dissertation, 'Rationalität und Existenz. Politische Arithmetik und politische Anthropologie,' already signals the direction of such development. His ensuing publications and assistantship in sociology, as well as his 1957 'Habilitation' for social politics then confirm it. Even more telling for this development along such continuous path is his own recollection (Albert 2001, p. 18), "Though in 1957 I passed my habilitation for Social Politics, the benevolence of the Faculty allowed me to concentrate in my teaching on logic, philosophy of science and welfare economics." And it is at this very moment that he meets, in 1958, Popper for the first time.

## The 'Überbrückungs Problem'

Already in his first published work from 1954, 'Ökonomische Ideologie und politische Theorie,' Albert concentrated on questions like the relation between facts and values. This focus can in general be seen as a result of Albert's own standing within the German philosophical tradition. However, here we can also already see the difference between his and Popper's background and orientation towards science. Albert as an educated social scientist cannot avoid the problem of balancing between facts and values in social research. That leads him to continuous returns

as well as a careful critical assessment of Max Weber's *Wertfreiheitsprinzip* (Albert 2001, p. 187).

The principle of value freedom, earlier researched by Max Weber in respect of social science, i.e. that value judgments are not free from criticism, has to be reasonably interpreted. Thus one has to go partly beyond Max Weber. The differentiation between facts and value judgments is useful. Knowledge is the kernel of scientific research and in this sense the value judgments are not an immediate element of the body of propositions. On the other hand, and that Max Weber knew as well, valuations continuously participate in the scientific research process. Whatever the theory may deal with, hypotheses and theories are being evaluated, we talk about corroboration.

The derivative problem of the relation between knowledge and decision, "technico-rational analysis" on one hand and "ethico-existential decision" on the other, was to become one of the axes of Albert's philosophical development.

Initially, at least, Albert viewed this latter polarity as a true antinomy, without any opening for a possibility of bridge principles (Albert 2001, p. 12). However, Albert soon realizes a necessity to open the normative sphere to criticism and thus already in 1968 in his first *Traktat* attempts to develop the positive conception of such bridge principles (Albert 1985, p. 98).

One can see now that Max Weber's approach is obliged to make implicit use of a critical principle not found in pure science – the maxim that *ought implies can*. Without this maxim one cannot see how, within the framework of Weber's conception of rationality, any kind of cognitive criticism can be brought to bear upon value convictions. What we have here is a *bridge principle* – a maxim to bridge the gap between ethics and science – which has the function of rendering scientific criticism of normative statements possible.

As soon as the possibility of such bridge principles for critical purposes has been conceded, it is difficult to see why one should restrict oneself to this principle and permit only that criticism of value orientations which has an immediately technological foundation. In fact, a critical moral philosophy will need to set for itself the explicit task of discovering more such bridge principles that will allow knowledge to be utilized for purposes of criticizing normative conceptions.

Calling this principle a Postulate of Realizability, Albert then introduces and discusses the Principle of Congruence – an opening for criticism where normative assertions, more specifically traditional normative assertions, clash with some other views presently held. This allows him to discuss the relation between faith and knowledge, as well as the concept of rational politics. In this sense, most of ensuing disputes with opponents from the fields of philosophy, social science, theology, and politics are based in and made possible by Albert's conception of bridge principles. A similar approach against immunization of philosophical, religious, and all other positions against criticism has been taken by Hans Lenk and others, sometimes under Albert's influence (Grossner 1971, p. 298).

Lenk sees the bridge principles connecting the theoretical knowledge with normative rules for actions as an "urgent task for philosophy," similarly to what Hans Albert derived in his *Treatise on Critical Reason*. The postulate of value freedom should not be misinterpreted as possibly an "ideology of self-defense of ivory tower dwellers" in the sense of a "thesis about a completely independent

research.” (Grossner quotes here from Lenk’s *Philosophie im technologischen Zeitalter*, p. 13 – VZ)

The net result of such openness towards, and search for such bridge principles is embodied in a broader conception of the theory of knowledge as presented by Albert as early as in his paper *Kritizismus und Naturalismus* from 1971 (Albert 2001, pp. 41–42).

Epistemology ... has to start from the *actual* conditions for the possibility of knowledge and from the targets and demands that can be realized in the given domain, as is the case for rational technologies and rationale standardization

The all-important feasibility of action within the political sphere then becomes just another aspect of realizability in general.

## The ‘Possitivismusstreit’ in the 1960s

In a widely read popular book from 1971, *Verfall der Philosophie: Politik deutscher Philosophen*, its author Claus Grossner (a system analyst and freelance publicist) expressed the view (and retroactively we would say hope), generally shared at that time by German intellectuals (Grossner 1971, pp. 32, 33), that “finally there is a dispute not just about different interpretations of the same philosophical movement but about different fundamental conceptions.” This exclamation is then followed few paragraphs later by an even more provocative and thus less supported claim,

We have to thank to Albert’s Don Quixotian ride against the windmill wings omnipresent in scholarly philosophy, to his overstated polemics against the Frankfurt School, that the German philosophy students are today starting to study logic, to read Anglo-Saxon philosophy and to buy books by Popper, Carnap, Nagel and Feyerabend

What was then this dispute, the initial phase of which has been often rightly criticized as a non – dispute?

Almost half a century after the event, it might be useful to offer a short survey of dates and facts. In the German Federal Republic, the conceptual scope of the post-war discipline of sociology was rather broad. It included empirically oriented research on one hand as well as the neo-Marxist and thus more philosophical and ideological approach represented by Horkheimer, Adorno, and their students at Frankfurt University on the other hand. To deal with a real or seeming plurality of methodologies stemming from such variety of conceptions, the German Society for Sociology invited, to its October 1961 conference in Tübingen, Karl Popper and Theodor Adorno to speak on the topic ‘The Logic of Social Sciences.’ Initially, the result was rather disappointing – Popper’s paper appeared to neglect the specific character of sociology as a social science, Adorno’s paper failed to address critically and directly Popper’s methodological proposals, presenting instead his own conception of society and its presumed consequences for sociology. Moreover, neither man seemed to know enough about his counterpart’s work. Ironically, while both speakers criticized positivism, Adorno’s conception of positivism appeared so broad as to include Popper as well. This erroneous point became central to

ensuing discussions, which were led from 1963 on primarily by Habermas and Albert. When the materials finally appeared in print 8 years later under the title *Der Positivismusstreit in der deutschen Soziologie* (Popper et al. 1976), the result was a true white elephant. While the two original papers were together only 44 printed pages long, Adorno's new Introduction and his new paper occupied almost 100 pages. These were followed by circa 160 pages of exchanges between Habermas and Albert. The remaining 40 pages presented a short critical review of the two main presentations by Ralph Dahrendorf and a slightly longer critical text by Harald Pilot.

There has been enough published about this so-called *Positivismusstreit*, primarily though not exclusively in German. It may not be an overstatement to say that this dispute established Habermas as the best known representative of the new generation of the Frankfurt School and Albert as the main representative of German critical rationalism. In general, it was in this very context that in 1968 they both published works that established them professionally – *Erkenntnis und Interesse* by the former, *Traktat über kritische Vernunft* by the latter.

## The Main Features of Critical Rationalism and Albert's Own Position

Significantly, Albert in most of his work focuses on attacking any attempts to develop immunization strategies preventing rational criticism. At the same time, as a social scientist and a philosopher, he cannot reduce his task either to the problematic of falsifiability or even to some sort of intelligent and uplifting conversation, powered by irony (Rorty). Thus already in his first *Traktat* he states (Albert 1985, p. 68)

The method of critical examination will then ...regard as necessary not merely *the search for contrary facts*, but above all the *search for alternative theoretical conceptions*, in order to facilitate the construction and application of competing frames of reference as well as novel solutions to problems.

In his view, critical rationalism has to serve science and philosophy in a constructive way. On a broader level, this also applies to both human thinking and activity in general.

In the Appendix entitled 'Der Kritizismus und seine Kritiker' and added to the 1975 third German edition of the *Traktat* (it does not appear in the 1985 English translation), Albert lists components of Critical Rationalism and thus of his position as well (Albert 1968, here quoted from the 5th ed., pp. 219–220)

There is a whole series of, in detail different but nevertheless related, conceptions that can be counted as belonging in the spectrum of Critical Rationalism. As much as we may try, the exact differentiation is hardly possible and important.

In such a conception there are at least three components, closely connected, that are to be differentiated: *consequent fallibilism*, covering all spheres of human thinking and acting, *methodical rationalism*, separated from the classical rationalism by its task of the general

foundational principle and by the acceptance of the principle of critical testing, and *critical realism*, ready to consider the results of science not only as contributions to the mastering of everyday life but even further as results of our attempts to obtain the true knowledge of reality and thus the foundational stones for our picture of the world.

Thus against either dogmatism or skepticism as well as against either purely “technical” philosophy or non-committal conversations, Albert stresses a broad conception of the interrelating role of philosophy both in respect of various scientific and cultural fields as well as of our general conceptions of the world (as above, p. 220).

One of the essential tasks of philosophical thought consists in producing in a critical and constructive manner a connection among the different areas of human culture. Within the historical process, these areas differentiated themselves and developed into more or less pronounced mutually separated spheres with claims to autonomy and tendencies towards protecting themselves. This is the case of, for example, science, jurisprudence, morality and religion, art and literature, engineering, economy and politics.

Albert’s stress on the role philosophy plays by connecting such separated areas brings us back to the general role of bridge principles; thus he continues,

Philosophy is no special discipline. On the other hand, nor can it limit itself to describing the cultural achievements produced in such areas, leaving aside their validity claims. Rather and first of all, it has to solve the *bridging problems* by critically evaluating the validity claims of such achievements, by bringing them into a meaningful relation which would make such evaluation possible and thus enables them to fertilize each other.

Against many leading philosophers of the day according to Albert, the traditional task of philosophy should be revised but not abandoned. Ten years after his first Traktat, in 1978 in the *Traktat über rationale Praxis*, Albert formulates this same thought in context of a search for a unifying conception of the world and thus for a required knowledge based orientation in it (Albert 1978, pp. 2–3).

It still makes sense to search for a connection between provisional and partial pieces of knowledge by individual sciences. In this way the gap created between them by a division of scientific labor can be bridged by orienting them towards a more comprehensive world orientation. Such *philosophy* oriented towards the old ideal of comprehensive knowledge is possible even today. It would have to be a *bridging effort which is always hypothetical and therefore open to revision*. ... Such occupation with foundational questions has been always oriented towards finding a bridge between the areas of human thought and human acting, thus supporting the creation of a unifying world-conception.

The difference in focus, albeit not in basic conception, between himself and Popper should have become obvious by now. However, the specificity of Albert’s own position comes even more clearly into forefront when we compare his philosophy with hermeneutics on one hand and pragmatism or neo-pragmatism on the other. Against the former, he stresses the context of action rather than just of language or even of communicative action in general, against the latter he assigns the crucial role for social science and social sphere as a whole to economic theories.

Let us return to a point which we already suggested, namely Albert’s incorporation within the broader scope of contemporary German philosophy and its dynamics. In the first half of the twentieth century probably the most interesting project for developing a broad philosophical position congruent with science was



Cassirer's philosophy of symbolic forms. Not in scope but in direction, Albert's search for bridge principles might be favorably viewed as a step beyond the point where Cassirer's plurality of symbolic forms had to be founded on and united through basis phenomena, i.e. with the help of metaphysics of symbolic forms. Nevertheless, to what degree Albert's search can be performed in a systematic and exhaustive way is another question.

## Conclusion

In general, it would be correct to subscribe to the usual identification of Albert as the main representative of Critical Rationalism in Germany (the importance of Hans Lenk at the earlier stage of his work and of late E. Topitsch and others notwithstanding). He has obviously propagated and defended the central ideas of this philosophy for the last half-a-century. In particular, he has been too often identified as the author of Münchhausen's Trilemma. However to broaden this picture, we have also introduced here what might be considered as his own focus – that is, his concentration on the relation between knowledge and action, value and decision. This aspect derives from his reading of some of the central ideas of Max Weber. In this respect, Albert is more important for his practical way of dealing with these themes than for a single theoretical framework. Some of these themes were later picked up by social scientists and politicians. In 1975 there appeared a representative selection of previously published articles under the title *Kritischer Rationalismus und Sozialdemokratie*, with an introduction by the German chancellor at that time, Helmut Schmidt, and two entries by Hans Albert. And even today, Hans Albert well along in his late 80s, is often a featured speaker at various encounters of social scientists, interviewed by media, and he continues to publish as well.

Looking back, Ronald Giere aptly remarked elsewhere that Vienna Circle “operated largely outside the German philosophical establishment.” Unfortunately, that nowadays applies to Hans Albert as well. Professional philosophers mostly take note of his dialogues with Gadamer, Küng, Appel, and Habermas, to name at least a few. Some authoritative surveys of German philosophy do not mention him at all (e.g., the presumably representative *Biographische Enzyklopädie Deutschsprachiger Philosophen* from 2001; though even Popper is given just slightly more space than Lou Andreas-Salomé). Others tend to impose on him the framework of his opponents, which then results in a considerable reduction and even misrepresentation (Bubner); Kröners' *Philosophisches Wörterbuch* which has been re-published in many editions stands out as one of the few notable exceptions. Most non-German sources usually limit themselves to just mentioning his role in the *Possitivismusstreit*.

Where does Albert stand today and where are the limits of his own position? As we have mentioned earlier, to most of his readers Albert has always presented himself as a polemical philosopher. Polemics with living partners have their own dynamics, which are only in part determined by the philosopher's own philosophical



development. In Albert's case, from 1960s to the present, there has been a continuous professional as well as a more public discussion with a slightly younger Jürgen Habermas. To a high degree due to a shift in Habermas' own focus, these polemics have now taken a form of a discussion on the restrictions which the religious faith might place on the use of reason, one of the more recent forms of Kant's famous dilemma. Nevertheless, while Albert aims often directly at theology and theologians like Hans Küng or Joseph Ratzinger, at the end his primary interest remains in philosophy and in the philosophical underpinning of contemporary theology. In 'Der Sinn des Lebens ohne Gott,' his 1984 original contribution to a philosophy reader of mostly classical texts for high school students, we can find an idea that is arguably presupposed in all his polemics around religion (Albert 1984, p. 124).

Anyone who views the meaning of human life as being exclusively dependent on how can this life be made an element within a divine plan should understand that he thus deals with all human purposes arising from this life as irrelevant, unless they can be related to divine planning. In its turn, the whole human life is then instrumentalized, that is conceived as a means to an external purpose only, the purpose which is determined by an external authority.

While Albert's works published in their original language, German, only are within the reach of many European students of Critical Rationalism, the almost total lack of translations into English carries with it a penalty not only for Albert and other German Critical Rationalists but for the English speaking students of this philosophy as well. The fact that the list of Albert's publications now reached almost 300 items notwithstanding. Moreover, it does not help when, though as a rare exception to this general neglect, the 1999 English selection from Albert's works bears a rather neutral and uninformative title *Between Social Science, Religion and Politics*. In contrast, a similar selection published 2 years later under a simple title *Hans Albert Lesebuch* presents a considerably broader and thus more representative picture of Albert's philosophy.

## Bibliography

- Adorno, Theodor. 1976. *The Positivist Dispute in German Sociology*. London: Heinemann. English translation of *Der Positivismusstreit in der Deutschen Soziologie*. Darmstadt: Luchterhand, 1969.
- Albert, Hans. 1978. *Traktat über rationale Praxis*. Tübingen: J.C.B.Mohr.
- Albert, Hans. 1984. Der Sinn des Lebens ohne Gott. In Hoerster, Norbert. ed. *Religionskritik. Arbeitstexte für den Unterricht*. Stuttgart: Reclam.
- Albert, Hans. 1985. *Treatise on Critical Reason*. Princeton, NJ: Princeton University Press. English translation of *Traktat über kritische Vernunft*. Orig. published Tübingen: J.C.B.Mohr, 1968. 5th ed. 1991.
- Albert, Hans. 1999. *Between Social Science, Religion and Politics. Essays in Critical Rationalism*. Amsterdam/Atlanta: Rodopi.
- Albert, Hans. 2001. *Hans-Albert-Lesebuch: ausgewählte Texte*. Tübingen: J.C.B.Mohr.
- Albert, Hans. 2003a. *Erkenntnislehre und Sozialwissenschaft; Karl Popper's Beitrag zur Analyse sozialer Zusammenhänge*. Wien: Picus Verlag.
- Albert, Hans. 2003b. *Kritik des transzendentalen Denkens*. Tübingen: J.C.B.Mohr.

- Albert, Hans. 2006. 'Der religiöse Glaube und die Religionskritik der Aufklärung. Beschränkungen des Vernunftgebrauchs im Lichte kritischer Philosophie,' *Journal for General Philosophy of Science*, Vol. 37, pp.355–371.
- Bohnen Adolph and Alan Musgrave. eds. 1991. *Wege der Vernunft. Festschrift zum siebzigsten Geburtstag von Hans Albert*. Tübingen: J.C.B.Mohr.
- Grossner, Klaus. 1971. *Verfall der Philosophie. Politik deutscher Philosophen*. Reinbek: Werner Verlag.
- Popper, Karl Raimund. 1957. *The Poverty of Historicism*. London: Routledge & Kegan Paul, corrected edition 1961.
- Popper, Karl Raimund. 1972 (first edition 1963). *Conjectures and Refutations: The Growth of Scientific Knowledge*. London: Routledge & Kegan Paul.
- Radnitzky, Gerard and Gunnar Andersson. 1978. *Progress and Rationality in Science. Boston Studies* vol. 58 Dordrecht: D. Reidel.

# Problem-Solving and the Problem of Induction

Donald Gillies

**Abstract** In his 1972 book: *Objective Knowledge*, Popper devotes Chap. 1 to the problem of induction. Elsewhere in the book (in Chaps. 3, 4, 6 and 8), he presents a general schema of problem solving. The aim of this paper is to bring these two strands of thought together. The initial problem ( $P_1$ ) is here the traditional philosophical problem of induction. Popper proposes a tentative solution (TS) to this problem. The paper then proceeds with the problem solving schema by adding error elimination (EE), i.e. criticisms of the tentative solution. These are concerned with computer induction, and with the claim that corroboration is in some sense inductive. This discussion leads in turn to the emergence of a new problem ( $P_2$ ). A suggestion is made about how this new problem might be tackled. The approach involves Neurath's principle, but applied to methods rather than theories.

## Introduction

Popper devotes the first chapter of his 1972 book: *Objective Knowledge* to an extended treatment of the problem of induction. He begins this chapter, and indeed the book as a whole, with his famous claim (1972, p. 1):

I think that I have solved a major philosophical problem: the problem of induction.

Later in the book Popper proposes a general schema of problem-solving.<sup>1</sup> In fact there are discussions of this in Chap.3, p. 119; Chap. 4, pp. 164–165; Chap.6, p. 243; and Chap. 8, p. 297. The formulations of the schema differ slightly in these different discussions. I will use a formulation from Chap. 6, which seems to me the best. The idea of this paper is to apply this general schema of problem-solving to Popper's treatment of the problem of induction, and to see what results.

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## Popper's General Schema of Problem-Solving

Popper writes (1972, p. 243):

Using 'P' for problem, 'TS' for tentative solutions, '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:

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

Earlier Popper describes EE (error-elimination) as (1972, p. 164) "a severe critical examination of our conjecture." He also describes the change from  $P_1$  to  $P_2$  as a 'problem-shift', observing (1972, p. 165):

This leads to the ... relation called '*problem shift*' by I. Lakatos, who distinguishes between progressive and degenerating problem shifts.

Later I will consider whether, in the case of the problem of induction, my suggested change from  $P_1$  to  $P_2$  is a progressive or a degenerating problem-shift.

## The Initial Problem ( $P_1$ ) and Popper's Tentative Solution (TS)

I will now begin applying Popper's general schema of problem-solving to Popper's treatment of the problem of induction. Obviously we have to start by identifying  $P_1$ . This is clearly what Popper calls (1972, p. 2): "*the traditional philosophical problem of induction*". Popper actually gives two formulations of this traditional problem. For simplicity, I will consider only the second, and this gives us:

$$P_1 = \text{What is the justification for inductive inferences?}$$

So what then is Popper's solution of  $P_1$ ? The gist of it is contained in the following passage (1972, p. 2):

The second formulation assumes that there are inductive inferences, and *rules* for drawing inductive inferences, and this, again, is an assumption which should not be made uncritically, and one which I also regard as mistaken.

So Popper thinks that there are no inductive inferences. He goes on to describe the idea that there are such things as (1972, pp. 6–7): "a kind of optical illusion". Now if there really are no such things as inductive inferences, then we do not have to justify them, and this solves  $P_1$ . Popper holds that science progresses through

conjectures and refutations, and this is a process which does not involve any inductive inferences – only deductive ones. Perhaps Popper's most emphatic denial of induction comes in the following passage which I will refer to as 'the 1963 induction is a myth quotation'.<sup>2</sup> It runs as follows (1963, p. 53):

Induction, i.e. inference based on many observations, is a myth. It is neither a psychological fact, nor a fact of ordinary life, nor one of scientific procedure.

It should be noted that Popper here speaks of induction in connection with psychology, ordinary life, and scientific procedure. In the present paper, however, I will confine myself to considering induction in the context of scientific procedure.

Returning to our main theme, we can sum up Popper's tentative solution (TS) to  $P_1$  as follows:

TS = There are no inductive inferences and so there is no need to justify them. Science progresses by conjectures and refutations, and this procedure does not use inductive inferences.

## The EE Phase: (i) Computer Induction

I now pass to the EE phase of the general schema of problem-solving. This consists of a severe critical examination of Popper's tentative solution (TS) of  $P_1$ . Here I will no longer continue quoting from Popper, but rather give the criticisms of other philosophers including myself. The first criticism I wish to present of Popper's TS came from discovering that researchers in artificial intelligence (AI) have developed a form of computer induction. I have given a detailed account of this criticism in my book (1996) *Artificial Intelligence and Scientific Method*, Chaps. 1–3, pp. 1–71. Here I will summarize briefly the results.

There is a branch of AI known as 'machine learning' whose aim is to generate hypotheses automatically from data, in other words to carry out mechanical induction. In my 1996 book, some examples are given of successful machine learning programs (particularly Quinlan's ID3 and Muggleton's GOLEM), and it is argued that these show the existence of inductive rules of inference (or IRIs). It is worth noting the form which these IRIs take. Let  $e$  be the data and  $h$  be the hypothesis inferred from the data. We might think that an IRI would take the form: 'From  $e$ , infer  $h$ '. However it turns out that it always has the form: 'From  $e$  &  $K$  infer  $h$ ', where  $K$  is background knowledge (cf. Gillies 1996, p. 18).

Developments in machine learning since 1996 have only reinforced the claim that inductive rules of inference exist. Hence it can be argued (cf. Gillies 1996, p. 56) that Popper's 1963 induction is a myth quotation can no longer be regarded as correct. In fact programs such as Quinlan's ID3 or Muggleton's GOLEM (and more recently developed machine learning programs) do make inductive inferences based on many observations and have become a part of scientific procedure.

This criticism of Popper's TS to  $P_1$  must be tempered by the following observations (cf. Gillies 1996, p. 66). Popper's 1963 induction is a myth quotation, as applied to scientific procedure, contained a good deal of truth *at the time when it was*

*published*. The first machine learning programs to be used successfully in science were Buchanan and Feigenbaum's Meta-DENDRAL, and Michalski's INDUCE. These appeared in the late 1970s and early 1980s, i.e. more than 15 years after Popper's induction is a myth quotation. Moreover, I can find hardly any genuine and significant uses of Baconian or mechanical induction in science before the machine learning programs just mentioned. Thus my conclusion is that Popper's 1963 induction is a myth quotation has become incorrect because science itself is changing. This change is, of course, brought about by the introduction of computers and is in some ways analogous to the changes brought about at any earlier phase of science's development by the introduction of instruments of observation such as telescopes and microscopes (cf. Gillies 1996, p. 69). In effect, the current development of computers and AI is likely to change science in such a way that Baconian or mechanical induction becomes a standard part of scientific procedure.

The view that computer induction exists is now generally held, but it has been challenged in an interesting paper by Tamburrini (2006).<sup>3</sup> Tamburrini only considers one of the two main examples of computer induction which I give in my 1996 book. This is ID3. He remarks quite correctly (2006, p. 273): "Popper's anti-inductivism was questioned on the basis of ID3 performances (Gillies 1996); ..." ID3 is a system which infers decision trees from data. Like any machine learning system, it assumes some background knowledge (K) which, in this case, is that the domain of objects under consideration is appropriately described by a specified set of attributes. Given K, decision trees are automatically generated from data *e* using built-in algorithms. As the correctness of the decision tree (D say) by no means follows deductively from *e* & K, it seems almost inescapable that we have here an inductive rule of inference (IRI) of the form: From *e* & K, infer D. As I show in my 1996 book (pp. 36–38), this IRI can be considered as generated by the iteration of more basic IRIs. It should also be observed that the decision trees generated, which were not previously known, turn out in many cases to contain substantial knowledge and to be very successful in practice. An example, given in my 1996 book, p. 46, comes from the work of Bratko. He used a developed form of ID3, known as ASSISTANT, to induce decision trees for medical diagnosis. These decision trees performed better than human specialists in all cases in which an objective statistical comparison was possible.

ID3 seems to be such a clear example of mechanical induction that it is difficult to see how Tamburrini can hold the following opinion (2006, p. 268): "... I maintain here that AI investigations on learning systems do not compel one to relinquish Popper's radical scepticism towards induction." The core of Tamburrini's defence of this position, in the case of ID3, seems to lie in the following passage (2006, p. 276):

If the presuppositions of the first kind (ID3 biases) can be suitably stated in declarative form, then a concept learning algorithm such as ID3 can be redescribed as a theorem prover.

The line of thought here seems to be the following. The algorithms in ID3 involve some presuppositions (or, as one might say, heuristics). Suppose we were able to state these explicitly as say P. ID3 involves an inductive rule of inference of the

form: From  $e \& K$ , infer  $D$ . However, if we added  $P$  to  $e \& K$ , we might be able to turn this inductive rule into a deductive rule of the form: From  $e \& K \& P$ , infer  $D$ , where now  $D$  follows from  $e \& K \& P$  by deductive logic. In this way an inductive machine learning system would become like an automated theorem prover which involves only deductive rules of inference.

The first point to note here is that Tamburrini's claim is only hypothetical (2006, p. 276): "If the presuppositions of the first kind (ID3 biases) can be suitably stated in declarative form ...". Of course it would be very difficult indeed, if not downright impossible, to suitably state these presuppositions in declarative form, and Tamburrini doesn't attempt to do so. Thus his suggested reduction of ID3 induction to deduction is purely hypothetical and most unlikely ever to be accomplished.

Let us, however, suppose, as our second point, that this reduction could really be carried out. There is no doubt that ID3, redescribed as a theorem prover, would be much more complicated than the original ID3 presented as an inductive learning system. Why should we introduce all this unnecessary complication which would never be adopted in practice? The question before us is whether to allow the introduction of inductive rules of inference (IRIs), or to allow only deductive rules of inference (DRIs). If we adopt the former position, we get simple computer induction systems which are successful in practice. If we adopt the second position, we are forced to try to transform these systems into equivalent theorem provers which involve only DRIs. This is a difficult, probably hopeless, task which adds complexity with no practical gain. The case, therefore, for allowing the introduction of IRIs is overwhelming.

I will come back briefly to computer induction later on, but, as I have already presented this particular criticism of Popper's tentative solution (TS) to  $P_1$  in some detail in an earlier publication, I will devote most of the rest of this paper to another criticism.

## **The EE Phase: (ii) Is Corroboration in Some Sense Inductive?**

In the course of his discussion of the problem of induction, Popper introduces the notion of degree of corroboration which he characterizes as follows (1972, p. 18):

By the degree of corroboration of a theory I mean a concise report evaluating the state (at a certain time  $t$ ) of the critical discussion of a theory, with respect to the way it solves its problems; its degree of testability; the severity of tests it has undergone; and the way it has stood up to these tests.

The degree of corroboration of a hypothesis  $h$  by the given evidence  $e$  is written  $C(h, e)$ , or perhaps better  $C(h, e \& K)$  where  $K$  stands for the background knowledge. Popper's term 'corroboration' was introduced to contrast his theory of corroboration with Carnap's theory of confirmation (cf. Carnap 1950). Indeed the two theories differ in important ways. For example, Carnap is a Bayesian which means that

he thinks that  $C(h,e)$  satisfies the axioms of probability, whereas Popper is a non-Bayesian and denies that  $C(h,e)$  satisfies the axioms of probability. In symbols, Popper's claim here is that  $C(h,e) \neq P(h|e)$ . Despite these differences I prefer to use the terms 'corroboration' and 'confirmation' as synonyms, and to abbreviate them both by  $C$ . The difference between Popper's theory and Carnap's is expressed by saying that they characterize the  $C$ -function differently.

Returning now to Popper's 1972 discussion of induction, one might ask whether he really needed to introduce corroboration at all. Suppose there are, in a particular area of investigation,  $n$  theories between which scientists have to decide. Could they not devise tests which refute  $n - 1$  of these theories, leaving only one unrefuted which will then become the preferred theory? No notion of corroboration is needed to carry out such a procedure. However, Popper does point out a possible difficulty here (1972, p. 15):

On the other hand, *among the theories actually proposed* there may be more than one which is not refuted at a time  $t$ , so that we may not know which of these we ought to prefer.

Suppose, however, we have a measure of corroboration, we can then prefer the best corroborated theory among those which are unrefuted.

Corroboration is also involved in what Popper calls the *pragmatic problem of induction*. Popper gives two formulations of this problem. I will focus on the second of these which he states as follows (1972, p. 21):

$Pr_2$  Which theory should we prefer for practical action, from a rational point of view?

He goes on to say (1972, p. 22):

My answer to  $Pr_2$  is: ... we should *prefer* as basis for action the best-tested theory.

Given Popper's characterisation of corroboration quoted earlier, we can roughly identify "the best-tested theory" with "the best-corroborated theory". This leads to the following pragmatic principle, which, it should be stressed, is a modification of what Popper writes:

(1) Use, as the basis for action, the best corroborated theory

This principle (1) does not quite correspond to what happens in practice as we can see by considering the following example. Suppose a pharmaceutical firm has developed a new drug  $X$  to treat some illness. Before  $X$  is put on the market, it is important to make sure that it does not have any harmful side effects. Let us therefore formulate the following hypothesis:

$H_x$ :  $X$ , when taken in the appropriate dosage, does not have any harmful side effects

Now before  $X$  can be put on the market  $H_x$  must, by law, be subjected to a series of severe tests – first with animals, and then in the form of clinical tests on humans. Only if  $H_x$  passes all these tests can it be marketed.<sup>4</sup> To put it another way,  $X$  can only be put on the market if  $H_x$  has a sufficiently high degree of corroboration.

This leads to the following pragmatic principle (2):



- (2) Use, as the basis for action, theories which have a sufficiently high degree of corroboration

What is meant by 'sufficiently high degree of corroboration' is specified in the case of drugs by the government regulations on what tests a new drug must pass before it can be put on the market. In general it would be understood contextually as part of the practice of the area in question.

Now we come to the problem. It seems that pragmatic principles such as (2) are indeed accepted as guides to action. But in accepting such a principle, are we not implicitly giving an inductive significance to corroboration? Suppose a theory has a high degree of corroboration. This means that it has explained correctly the results of past observations, and perhaps also given the correct predictions in a number of tests. Let us say in these circumstances that the theory has so far performed well. However, if we adopt the theory as the basis for actions, are we not assuming that it will continue to perform well in the future? In other words accepting a pragmatic principle such as (2) seems to be implicitly adopting an inductive assumption.

The criticism of Popper involving computer induction could not have been formulated before the late 1970s and early 1980s, because it was only then that successful systems for computer induction were created. However, the criticism involving corroboration and the pragmatic problem of induction is much older. In his 1994 book (pp. 20–23), Miller gives a list of no less than 11 philosophers who have made criticisms along these lines, and goes on (1994, pp. 38–45) to try to answer all these objections. One formulation of such a criticism is to be found in Salmon (1968). Salmon considers whether we are acting rationally if we prefer a prediction based on a well-corroborated scientific theory to a prediction based on some theory which has low or even negative corroboration. He writes (1968, p. 97):

Either corroboration has an inductive aspect or there is no logic of prediction. If there is no logic of prediction, it is hard to see how any choice would be 'rational'.

Salmon thinks that we can make rational choices here, so that the conclusion of his argument is that corroboration has an inductive aspect. Salmon further elaborates this criticism of Popper in his 1981.

O'Hear also gives an elegantly formulated criticism of this kind in his 1980, where he writes (pp. 40–41):

it is unclear how Popper is in a position to tell us that it is more rational to act on a well-corroborated theory than to adopt any other policy when it comes to action. ... High corroboration shows only that a theory has done well up to now. Hume's point is that our world might suddenly change to being one where chance might be a good method or where previously falsified theories might be the best to act on or where we might be better off having no method at all. I cannot see how Popper is justified in claiming that these methods are, in the light of his acceptance of Hume's point, worse methods for basing practical actions on.

But what does Popper himself say about the relation between corroboration and induction? There is in fact one passage in which he seems to come close to giving

an inductive significance to his measure of corroboration (1959, New Appendix \*ix, p. 418):

It might well be asked at the end of all this whether I have not, inadvertently, changed my creed. For it may seem that there is nothing to prevent us from calling  $C(h, e)$  'the inductive probability of  $h$ , given  $e$ ' or – if this is felt to be misleading, in view of the fact that  $C$  does not obey the laws of the probability calculus – 'the degree of the rationality of our belief in  $h$ , given  $e$ '. A benevolent inductivist critic might even congratulate me on having solved, with my  $C$  function, the age-old problem of induction *in a positive sense* – on having finally established, with my  $C$  function, the validity of inductive reasoning.

However, it should be noted that the view given here is that of 'a benevolent inductivist critic'. It is not Popper's own as the following passage from *Objective Knowledge* clearly shows (1972, p. 18):

Corroboration (or degree of corroboration) is thus an evaluating *report of past performance*. ... Being a report of past performance only, ... *it says nothing whatever about future performance*

But if degree of corroboration really said nothing whatever about future performance, why should we use it to guide our actions? In using it in this way, we are surely implicitly assuming that degree of corroboration does say something about future performance. In other words we are giving degree of corroboration an inductive significance.

That concludes the EE phase of the general schema of problem-solving, and I will next consider what new problem arises from all this.

## The New Problem ( $P_2$ ): Choosing a C-Function

The new problem which arises is, so I claim, that of choosing a C-function. I will first explain what I mean by this, and then explain why it arises from the EE part of the preceding discussion.

Choosing a C-function sounds like giving the full specification of a mathematical function which for any values of  $h$  and of  $e \& K$  gives a real number  $C(h, e \& K)$ . Perhaps Carnap dreamed of constructing such a fully specified mathematical function, but it is not a very realistic aspiration as far as current practice is concerned. In some AI cases, a C-function is precisely specified in the mathematical sense, and this function is coded into the machine learning program. However, the language used to specify the function, the nature of the background knowledge  $K$ , and the precise details of the function would all depend on the specific application, and would be different in a different application (even if the C-functions used in different applications have some features in common). In ordinary human science, the specification of the function is also highly context-dependent, but here it is qualitative as well. This is clearly shown in the drug case described above, in which what is meant by 'a sufficiently high degree of corroboration' is specified by listing the tests which must be performed and passed to achieve this grade. This listing of the necessary tests is one way of choosing a C-function for a problem.

Then again there are debates concerning general features of the C-function. For example, as already mentioned, the Bayesians hold that  $C(h, e \& K)$  should satisfy the standard axioms of probability while some non-Bayesians such as Popper deny this. Of course this suggests that the C-function might be Bayesian in some contexts and non-Bayesian in others (for a suggestion along these lines, see Gillies 1998, Sect. 4, pp. 155–156).

So, to sum up, what we are here referring to as ‘choosing a C-function’ is actually quite a complex and context-dependent process. It may involve, in a particular AI context, choosing a specific mathematical function. However, in more general contexts, it may be no more than a specification of certain general features of the C-function, and of the circumstances in which the C-function attains some key value.

Let us next analyse how the problem of choosing a C-function arises out of the EE discussion given earlier. I argued that Popper’s own treatment of the problem of induction involves introducing corroboration, and so gives rise to the problem of choosing a C-function. My first criticism of Popper’s approach was that his claim that induction is a myth is wrong because inductive rules of inference are used in successful AI machine learning programs. Now we can connect this criticism with the subsequent discussion of corroboration, because, once a C-function is chosen, we can use it to justify an inductive rule of inference (see Gillies 1996, p. 105). An inductive rule of inference takes the form: ‘From  $e \& K$ , infer  $h$ ’, where  $K$  is the background knowledge,  $e$  is the evidence, and  $h$  is a hypothesis which explains the evidence. The justification of such a rule given a C-function is simple. The rule is justified, if  $C(h, e \& K)$  is sufficiently high.

So the suggestion is to change our original problem ( $P_1$ ): ‘What is the justification of inductive inferences?’ into the new problem ( $P_2$ ) of choosing a C-function. But is this problem-shift progressive or degenerating? Naturally I would like to argue that it is progressive, and will now explain my reasons for thinking that it is.

I argued earlier that there are some exceptions to Popper’s claim that rules of inductive inference do not exist. However, these exceptions are relatively rare. They occur, for example, in the machine learning programs of AI. For the vast bulk of human science both in the past and present, rules of inductive inference do not exist. For such science, Popper’s model of conjectures which is freely invented and then tested out seems to me more accurate than any model based on inductive inferences. Admittedly, there is talk nowadays in the context of science carried out by humans of ‘inference to the best explanation’ or ‘abductive inference’, but such so-called inferences are not at all inferences based on precisely formulated rules like the deductive rules of inference. Those who talk of ‘inference to the best explanation’ or ‘abductive inference’, for example, never formulate any precise rules according to which these so-called inferences take place. In reality the ‘inferences’ which they describe in their examples involve conjectures thought up by human ingenuity and creativity, and by no means inferred in any mechanical fashion, or according to any precisely specified rules. Now the advantage of the new problem ( $P_2$ ) of choosing a C-function is that it solves the original problem ( $P_1$ ), as we have seen, but it also deals with the case of hypotheses generated not by any inductive inference

but by a process of conjecture and testing. Such conjecture-generated hypotheses are justified if, as a result of testing, they become well corroborated. So, if we have agreed on the choice of a C-function, we can provide justification whether a hypothesis is generated by some inductive inference, or whether it is obtained by conjecture and testing. Thus the new problem is more general than the old.

Another advantage of the new problem over the old one is that the traditional problem of induction ( $P_1$ ) suggested a series of approaches which proved to be very unsatisfactory (for some details about these, see Gillies 1993, pp. 8–11 and 34). Formulating the problem of induction in the form: ‘What is the justification for inductive inferences?’ suggested to many thinkers, particularly those of the Cambridge school such as Keynes and Russell, that inductive inferences needed to be justified in terms of general principles such as *the uniformity of nature* or *the principle of induction*. However, two problems emerged with this approach. First of all it proved almost impossible to formulate these alleged principles in any clear fashion. For example, Russell’s formulation of the principle of induction contains an error which vitiates it (see Gillies 1993, p. 10). Secondly it seemed to be impossible to give any convincing justification of such principles. One great merit of the shift from  $P_1$  to  $P_2$  is that it enables us to dispense with such obscure and unsatisfactory principles.

These then are the merits of the shift from  $P_1$  to  $P_2$ , and they seem to me to justify the claim that this shift is a progressive one. However, it should be stressed that Popper’s general schema for problem-solving is a never-ending process. As Popper himself says (1972, p. 164):

*$P_2$  is the problem situation as it emerges from our first critical attempt to solve our problems. It leads up to our second attempt (and so on).*

So we should now look more closely at the new problem  $P_2$ , and see if we can propose a preliminary tentative solution. This I will do in the next section.

## **A Tentative Solution (TS) to the New Problem: An Extension of Neurath’s Principle**

So far I have formulated the new problem  $P_2$  rather loosely, as being that of choosing a C-function. We can, however, split this into two questions. The first question is: ‘how do we set about choosing a particular C-function in a specific situation?’ The best way of approaching this question is to examine the practice of good scientists, and see if we can formulate general principles which underlie this practice. This could be described as *codifying practice* in the choice of C-functions. It is normally studied in philosophy of science under the heading ‘confirmation theory’. But now suppose we have chosen a particular C-function. Then a second question arises, namely:

What is the justification for particular choices of C-function?

Because this question is analogous to the traditional philosophical problem of induction ( $P_1$ ) with which we started, I will from now on take this second question as our  $P_2$ . So the problem shift is from

$P_1$  = What is the justification for inductive inferences?

to

$P_2$  = What is the justification for particular choices of C-function?

Our tentative solution (TS) to  $P_2$  is based on some ideas of Neurath's. These are expressed by Neurath in the following famous passage (1932/1933, p. 201):

We are like sailors who must rebuild their ship on the open sea, never able to dismantle it in dry-dock and to reconstruct it there out of the best materials.

Here Neurath gives his view as an analogy. However, in a previous work (Gillies, 1993, p. 138), I have tried to formulate his position in a more explicit fashion as what could be called *Neurath's principle*. This is a conjunction of two parts, (A) and (B) which may be stated as follows:

- (A) In order to test any scientific statement, we have to assume for the time being some other scientific statements. (This corresponds in the analogy to the fact that we can remove a plank of the ship only if we leave some others in place, since otherwise the ship would sink.)
- (B) There is, however, no scientific statement which cannot be subject to testing, and perhaps abandoned as a result of tests. (This corresponds in the analogy to the fact that any plank of the ship can be removed and checked to see if it is rotten.)

The first application we can make of Neurath's principle is to the body of scientific theories. Here we cannot question, and demand justification for, all our scientific theories at the same time. To test out one scientific theory, we have, for the time being, to assume others – in particular the theories used to interpret the relevant observations and experimental results. Similarly, I now argue, we cannot question, and demand justification for, all our choices of C-function at the same time. What we can do, and what has actually been done in the course of scientific and technological development, is to test out, and perhaps reject or modify, *particular* choices of C-function, while assuming, for the time being, other such choices. There is a circle here, just as there is in the case of testing out scientific theories, but it is no more vicious in the one case than in the other. Although we cannot criticize our choices of C-function all together, there is no particular such choice which cannot be criticized, tested out, and evaluated.

My suggestion then is to extend Neurath's principle from scientific theories to C-functions. We test out our choices of C-functions by experience just as we test out our scientific theories by experience. The only rule in both cases is that we cannot question all our assumptions at the same time. In order to question one thing, other things must, for the time being at least, be assumed. I will conclude by illustrating this with an example.

Let us return to our consideration of testing new drugs to make sure that they have no harmful side effects before they are put on the market. Earlier we formulated this problem by considering the following hypothesis:

$H_X$ :  $X$ , when taken in the appropriate dosage, does not have any harmful side effects

where  $X$  is a new drug developed by a pharmaceutical firm to treat some illness. In this case the  $C$ -function for  $H_X$  is chosen informally as follows. A series of tests  $t_1, \dots, t_n$  is specified. Some of these will be on animals and some will be clinical tests on humans. Suppose our evidence ( $e_n$  say) is that all these tests have been carried out, and  $H_X$  has passed them all. Then the value of  $C(H_X, e_n)$  is judged to be sufficiently high to allow the drug to be put on the market.

Suppose this choice of  $C$ -function is well-established, and has been used successfully for a number of years. Then a new drug  $T$  is devised.<sup>5</sup> The standard tests are performed on  $T$  and it passes them all successfully. So  $T$  is put on the market. However, tragedy ensues.  $T$  is actually very successful at curing the illnesses for which it is prescribed, but a quite unexpected side effect occurs. When it is prescribed to pregnant women, they give birth to babies with very severe defects. This disaster leads to a modification of the choice of  $C$ -function which has been used hitherto by the pharmaceutical industry. A new test ( $t_{n+1}$  say) is introduced which consists in giving the drug to experimental animals which are pregnant, and then checking whether the resultant offspring has any defects. The drug only passes this test successfully if no birth defects are discovered.<sup>6</sup> Let  $e_n$  be as before, and  $e_{n+1}$  be the evidence that the tests  $t_1, \dots, t_n, t_{n+1}$  have been carried out, and  $H_X$  has passed them all. Then the value of  $C(H_X, e_{n+1})$  is judged to be sufficiently high to allow the drug to be put on the market. However, the value of  $C(H_X, e_n)$  is no longer judged to be sufficiently high to allow the drug to be put on the market. The choice of  $C$ -function in this particular context has been changed.

Suppose further that the new choice of  $C$ -function works well, and there are no further disasters occasioned by its use. We can then conclude – of course implicitly assuming other choices of  $C$ -function which have not been changed – that the new choice of  $C$ -function in this particular context is an improvement and was justified. This shows how Neurath's principle allows us to justify changes in our choice of  $C$ -function. It shows indeed that choices of  $C$ -function can be steadily improved along with the rest of science.

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## Notes

1. On the origins of this schema of Popper's, see Ter Hark (2004), particularly pp. 128 and 175.
2. I heard Popper himself utter the fateful words: "Induction is a myth" when I attended his lectures as a graduate student in the academic year 1966–1967. As far as I can remember, Popper

continued: "... and those who use the term 'induction', do not know what they are talking about."

3. I would like to thank David Miller for drawing this paper to my attention.
4. I have simplified somewhat here since, in practice, drugs are allowed to have harmful side effects in some classes of patients, provided these classes can be specified clearly in advance so that it is known that patients in one of these classes should not be prescribed the drug. For example, drugs for some heart conditions may have no harmful side effects for the normal patient, but might have harmful side effects for patients suffering from diabetes. I have ignored these complications since they do not affect the points about corroboration being made here.
5. As the letter T indicates, this hypothetical example is based on the real case of thalidomide. However, my example is a considerable simplification of what actually happened in that case. Some details about the actual thalidomide case are to be found in Timmermans and Leiter (2000). One interesting thing to which they draw attention is that thalidomide has been partially rehabilitated as a drug. It is in fact very effective as a treatment for some very severe conditions such as a tissue inflammatory syndrome which occurs in leprosy and AIDS wasting syndrome. It is now prescribed for these conditions, while taking precautions to prevent it ever being used by pregnant women.
6. Timmermans and Leiter say (2000, p. 45): "After the thalidomide disaster, studies in which pregnant rabbits were given thalidomide produced phocomelia birth defects."

## References

- Carnap, Rudolf. 1950 (1963). *Logical Foundations of Probability*, 2nd edn. Chicago, IL: University of Chicago Press.
- Gillies, Donald. 1993. *Philosophy of Science in the Twentieth Century: Four Central Themes*. Oxford: Blackwell.
- Gillies, Donald. 1996. *Artificial Intelligence and Scientific Method*. Oxford: Oxford University Press.
- Gillies, Donald. 1998. Confirmation Theory. In *Handbook of Defeasible Reasoning and Uncertainty Management Systems*, eds. Dov Gabbay and Philippe Smets, Vol. 1, pp. 135–167. Dordrecht/Boston/London: Kluwer.
- Miller, David. 1994. *Critical Rationalism. A Restatement and Defence*. Chicago, IL/LaSalle, IL: Open Court.
- Neurath, Otto. 1932/1933 (1959). Protocol Sentences. Reprinted in English translation. in *Logical Positivism*, ed. Alfred Jules Ayer, pp. 199–208. New York: Free Press.
- O'Hear, Anthony. 1980. *Karl Popper*. London/New York: Routledge, 2002.
- Popper, Karl Raimund. 1959 (1972). *New Appendices to the Logic of Scientific Discovery*. In 6th revised impression of the 1959 English translation, pp. 307–464. London: Hutchinson.
- Popper, Karl Raimund. 1963. *Conjectures and Refutations*. London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1972. *Objective Knowledge. An Evolutionary Approach*. Oxford: Clarendon.
- Salmon, Wesley. 1968. The justification of inductive rules of inference (+ discussion). In *The Problem of Inductive Logic*, ed. Imre Lakatos, pp. 24–97. Amsterdam: North-Holland.
- Salmon, Wesley. 1981. Rational Prediction. *British Journal for the Philosophy of Science* 32: 115–125.
- Tamburrini, Guglielmo 2006. Artificial Intelligence and Popper's Solution to the Problem of Induction. In *Karl Popper: A Centenary Assessment, Volume II Metaphysics and Epistemology*, eds. Ian Jarvie, Karl Milford and David Miller, pp. 265–284. Aldershot: Ashgate.
- Ter Hark, Michel. 2004. *Popper, Otto Selz and the Rise of Evolutionary Epistemology*. Cambridge: Cambridge University Press, Paperback Edition 2007.
- Timmermans, Stefan and Leiter, Valerie. 2000. The Redemption of Thalidomide: Standardizing the Risk of Birth Defects. *Social Studies of Science* 30: 41–71.



# Popper's Fundamental Misdiagnosis of the Scientific Defects of Freudian Psychoanalysis\*

Adolf Grünbaum

**Abstract** The first impetus for my philosophical appraisal of Freudian psychoanalytic theory came from Popper's report that its edifice had played a pivotal role in his elevation of the empirical falsifiability of a hypothesis by potentially contrary evidence to be the linchpin of his entire philosophy of science, beginning with his anti-inductivist theory of demarcation (Popper 1974, p. 984). Indeed, for Popper, psychoanalysis avowedly served as the centerpiece for the purported superiority of his own falsifiability criterion of demarcation between science and non-(pseudo) science to the received inductivist standard for empirical theory-validation that originated three centuries earlier with Francis Bacon, a Baconian benchmark which Popper erroneously censured as being unacceptably permissive epistemologically.

True, Bacon had erred in supposing that, for any given set of observational data, there is only a *finite set* of alternative hypotheses, each of which might explain the known data. But he emphasized, long before Popper, that – other things being equal – negative instances have greater probative force than positive ones in validating theories, having scoffed at simple enumerative induction from positive instances as “puerile.” Thus, Bacon envisioned the *inductive elimination* of all but one of the supposedly finite number of alternative hypotheses, which would thereby be shown to be true.

But, as we know, there is always a potentially *infinite* set of alternative explanatory hypotheses, *some* of which working scientists may eliminate by means of refuting (disconfirming) instances, while other such hypotheses in the set survive, at least temporarily, as theoretical candidates for inductive acceptance. Therefore, in my 1984 book on *The Foundations of Psychoanalysis*, I advisedly denoted this epistemic vetting process by the locution “neo-Baconian eliminative induction,” which should not have occasioned any carping, although it did. The less so, since Bacon clearly disparaged hypothetico-deductive confirmationism, especially in the case of causal hypotheses, which are ubiquitous in the Freudian corpus.

Early in the twentieth century, Popper fallaciously inferred the non-falsifiability of Freudian and Adlerian psychology from his own declared inability to *imagine logically possible* contrary instances of human behavior. Moreover, he tried to

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buttress his thesis of irrefutability by the incorrect complaint that these two psychological theories *were always inductively confirmed*, come what may. And then, he himself grossly violated his own methodological injunction against immunizing gambits by evasively trying to uphold his claim of unfalsifiability in the face of the demonstrated refutability (disconfirmability) of psychoanalytic hypotheses.

Indeed, as I showed amply:

[I]t is precisely Freud's theory that furnishes poignant evidence that Popper has caricatured the [Baconian] inductivist tradition by his thesis of easy inductive confirmability of nearly every theory (1984, p. 280; italics in original).

Alas, Popper's treatment of psychoanalysis as the avowed anvil of his theory of scientific rationality will turn out to be an intellectual fiasco throughout. But I remain ever indebted and grateful to him, because the critical scrutiny of his theses became a potent stimulus for my much more general systematic examination of the foundations of the Freudian edifice.

## Introduction

In about 1975, I turned to a systematic scrutiny of Popper's very influential philosophy of science. And I published my results a year later, in four papers, three of which appeared in the 1976 volume of *The British Journal for the Philosophy of Science* while the fourth was my contribution to the Imre Lakatos memorial volume (Grünbaum 1976a–d).

Avowedly, Popper's conception of psychoanalysis was perennially inspired by his derogatory belief that "Freud's theory ... simply does not have potential falsifiers" yielding potentially contrary evidence, a belief that he reiterated in 1974 (Popper 1974, p. 1004), but which will soon be seen to be demonstrably untenable.

As Popper emphasized repeatedly, *psychoanalysis* was the prime illustration of his cardinal thesis that the received inductivist criterion of distinguishing science from pseudoscience (and non-science) by reference to available evidential support is *unacceptably permissive epistemologically by countenancing confirmations come what may*. Therefore, he told us, the inductivist criterion needs to be superseded by his allegedly much more stringent scientificity requirement of falsifiability (refutability), or at least disconfirmability, by potential empirical evidence (Popper 1962, Chap. 1). Thus, the susceptibility of a hypothesis to refutation by logically possible adverse evidence became Popper's linchpin for the *scientific entertainability* of a hypothesis.

Clearly, the psychoanalytic enterprise played a central historical and logical role in Popper's falsificationist philosophy of science. Hence, in my critical concern with the warrant for his complaint of pseudoscience against Freud, it behooved me to study the foundations of psychoanalysis, which I began from scratch in the mid-1970s. But, even then, I had a strong hunch that Popper's wholesale, undemonstrated indictment of the Freudian corpus as categorically immune to empirical

refutation or disconfirmation, and as *therefore* pseudoscientific, was a fundamental, and indeed rash, misdiagnosis.

Before turning to the empirical falsifiability of psychoanalysis *vis-à-vis* Popper, let me make a merely *auxiliary* comment on the role of *Marxism* in Popper's theory of demarcation, a role which he likened to that of psychoanalysis.

In its original formulation, he told us, Marxism was falsified by the *failure* of some of its major predictions, such as the dashed expectation of communist revolutions in industrialized capitalist countries, rather than in agrarian Czarist Russia. But, according to Popper, Karl Marx's followers responded to that fiasco regressively by adopting an *unfalsifiable* version of *neo-Marxism*. This modified doctrine, he told us, *no less than psychoanalysis*, necessitated the replacement of the received inductivist criterion of demarcation by his own standard of falsifiability, because inductivism was allegedly much too permissive *epistemologically* by countenancing pseudo-confirmations in this neo-Marxist context as well.

Alas, *my* knowledge of neo-Marxism is far too shallow to enable *me* to assess Popper's complaint of unfalsifiability against it, on the model of my impending meticulous critique of his indictment of Freudian theory as being unfalsifiable. And, to my knowledge, no such rigorous assessment of the empirical testability of *neo-Marxism* exists in the literature. Yet, such a thorough scrutiny is very desirable. For it *might* well be that, *contrary* to Popper, neo-Marxism *is falsifiable*, *no less* than psychoanalysis is demonstrably testable empirically, as we shall soon see below in 'Popper's Thesis of the Empirical Irrefutability of Psychoanalytic Theory'.

I *countenance* this possibility of refutability, because of Popper's rash inclination to claim unfalsifiability *merely because* of his own declared inability to envision relevant testability scenarios, as he did erroneously for psychoanalysis.

So much for my auxiliary comment on his handling of neo-Marxism before I now turn to *Freudian theory*.

## My Odyssey into Psychoanalysis

My own assessment of psychoanalysis became the more imperative, when I turned to the writings of philosophers on it. Soon, I was deeply disappointed to discover that, alas, most of the *philosophical literature* on psychoanalysis, notably also in the English-speaking world, did not come to grips at all with the key issues of its acceptability as an explanatory theory, and was also distressingly unilluminating concerning the logical structure and rationale of Freud's edifice.

Some of the philosophic authors on psychoanalysis merely considered snippets from here and there in the Freudian corpus, morsels which were often selected to implement prior agendas. Regrettably, Stephen Toulmin's writings even tried my philosophical patience beyond endurance, if only because, when dealing with Freud's causal claims, he irrelevantly injected so-called ordinary language philosophy, a style of philosophy which then deservedly died an overdue death on both sides of the Atlantic (Grünbaum 1984, pp. 71, 73, 83).

Nor did I find requisitely cogent philosophical illumination on the stated range of issues in the writings of Jean-Paul Sartre, Ludwig Wittgenstein, Ernest Nagel (a dear personal friend), Sidney Hook, Alastair MacIntyre, and others.

Indeed, I looked in vain for a *systematic philosophical appraisal of Freud's major specific arguments* for his cornerstone theory of psychopathology with its associated hypotheses on the dynamics of therapy, his dream theory, his theory of psychosexual development, and his theory of slips. Nor did I find an epistemological examination of Freud's fundamental method of clinical investigation by allegedly "free association," a method which is purportedly *causally probative* and a pivot of his cornerstone theory of repression. All of these deficits are egregious omissions. Yet, philosophic writers on Freud typically purported to elucidate and appraise the logical architecture of his reasoning.

By the same token, I was dumbfounded that Breuer and Freud's foundational, epoch-making 1893 "Preliminary Communication" for their *Studies on Hysteria* (Breuer and Freud 1893/1955, pp. 3–17), was almost entirely ignored by philosophers writing on psychoanalysis, or was simply lost on them, even when they mentioned it at all. This, despite the fact that in Breuer and Freud's pioneering 1893 paper, the founders of psychoanalysis presented, in a mere *two* of their pages (Breuer and Freud 1893/1955, pp. 6–7), the *evidential crux* for their cornerstone repression-etiology of hysteria (neurosis).

Reading Breuer and Freud's "Preliminary Communication" in James Strachey's English translation (Breuer and Freud 1893/1955, pp. 3–17) from an epistemological point of view, with an occasional glance at its German original, was an electrifying eye-opener for me early on. However, as shown by the writings of legions of *psychoanalysts*, the core reasoning in that historic paper seems to have been lost on them as well, *not just on philosophers*. One deplorable result of this impoverishing neglect was the conceptual murkiness beclouding much of the expository literature on the foundational ideas of psychoanalysis.

The lucid pioneering 1893 paper by the founding fathers convinced me that there was no excuse for this nebulousness. And it inspired me to remedy it within my 1984 book *The Foundations of Psychoanalysis: A Philosophical Critique*, where I presented my initial scrutiny of the Breuer–Freud repression-etiology of the neuroses, a very censorious scrutiny which I have significantly refined thereafter (Grünbaum 2002, p. 125) and especially more recently (Grünbaum 2006, pp. 269–270, 2007, pp. 552–554). The interested reader is also referred to my lengthy "Autobiographical–Philosophical Narrative" (Grünbaum 2008) for a detailed account of the very fruitful evolution of my work on psychoanalysis.

My unfavorable verdict on the quality of the philosophical literature on psychoanalysis which I encountered when I began in the mid 1970s, was corroborated independently by Charles Hanly, a philosophy professor at the University of Toronto, and a veteran practicing psychoanalyst, who is also the current President of the International Psychoanalytic Association. Writing in his review of my aforementioned 1984 book, he averred:

Grünbaum's scholarship on the Freud texts, given his purpose and interests, is *light years ahead* of that of philosophers such as *Popper*, *Habermas*, and *Ricoeur*, not to mention the scholarship of some psychoanalysts [*italics added*] (Hanly 1988, p. 524).

Turning to Popper, let me state more fully his untenable censure of unfalsifiability against psychoanalysis.

## Popper's Thesis of the Empirical Irrefutability of Psychoanalytic Theory

(A) I begin with a retrospect on Popper's treatment of psychoanalysis prior to the appearance of his 1983 Postscript *Realism and the Aim of Science*.

Throughout his career, Popper has repeatedly made two claims: (1) logically, psychoanalytic theory is irrefutable by any observable human behavior; and (2) in the face of seemingly adverse evidence, Freud and his followers always dodged refutation by resorting to immunizing maneuvers to rescue their theory. According to the first of these two claims, *none* of the deductive consequences of Freud's hypotheses are refutable by potentially contrary empirical evidence.

But clearly, this charge of unfalsifiability against psychoanalytic theory *itself* does not follow from the *sociological* objection that Freudians are evasively unresponsive to criticism of their hypotheses. After all, a theory may well be invalidated by known evidence, even as its true believers refuse to acknowledge this refutation. Besides, if Popper were right that "Freud's theory ... simply does not have potential falsifiers" (1974, p. 1004), why would it ever have been necessary *at all* for Freudians to *dodge* refutations by means of immunizing gambits? Popper's two claims are evidently incoherent.

(B) In his 1983 Postscript volume *Realism and the Aim of Science*, he devotes its Chapter 2 (in Part I) to the topic of the demarcation between science and non-science. And therein he tells us that previously, he had "not published any detailed analysis of Freud's method of dealing with falsifying instances and critical suggestions" (p. 164, n. 1). Yet again, if Popper's categorical charge of unfalsifiability is to be believed, no such "falsifying instances" should exist *at all*, so that Freud would never have had occasion to deal with them!

Ironically, it emerges clearly from some of Popper's other doctrines that the alleged recalcitrance of Freudians in the face of falsifying evidence, however scandalous, is not at all tantamount to the irrefutability of their theory. As he tells us, theories, on the one hand, and the intellectual conduct of their protagonists, on the other, "belong to *two entirely different 'worlds'*" (1974, p. 144). Yet, Popper sometimes discusses Freud's theory itself and the behavior of its partisans in the same breath.

(C) Let us turn to Freud's 1915 paper on his homosexual etiology of paranoia, which illustrates that Popper's wholesale complaint of categorical unfalsifiability had, in effect, been shown to be untenable even before he first enunciated it in 1917.

According to psychoanalytic theory, in both genders, strongly repressed homosexual desires are *causally necessary* for paranoia, whatever the variations in the delusional modes of this psychosis. The modes include delusions of persecution, jealousy,

grandeur, and heterosexual erotomania (Freud 1915/1922, 18: 223–232). Thus, every sort of paranoiac is an arena for the psychic conflict between homosexual impulses, and the need to banish them from consciousness as socially objectionable. And his or her delusions (of persecution, etc.) are supposedly the *unconscious defense* against the conscious emergence of the forbidden homosexual cravings.

This defense is held to be mediated by two defense mechanisms: (1) so-called reaction formation, which converts the homosexual love into *hatred* toward the love object, such that the largely repressed dangerous love impulse first *surfaces* in the guise of a more acceptable *contrary* feeling of hate; (2) thereupon, projection occurs by the lover's imputation of the ensuing hatred to the beloved, thus resulting in the paranoid persecutory delusion.

The pertinent case in Freud's 1915 paper is that of a woman suffering from just such delusions. And the title of his paper was "A Case of Paranoia Running Counter to the Psychoanalytic Theory of the Disease." As suggested by its very title, Freud saw her case history as at least potentially supplying a *refuting instance* or disconfirmation of his etiology. In this vein, Freud notes (p. 265) that the patient's conduct might well supply evidence that, though paranoid, she is *not* beset by a conflictual struggle against unconscious homosexual impulses.

More specifically, any individual who is openly and publicly a highly active practicing homosexual surely qualifies as a person who does *not* strongly repress his or her homosexuality. If that same individual also exhibits strong delusions of persecution, then this flamboyant homosexual gives a strong observable indication of being paranoid. Yet, according to the prediction derived from Freud's etiology, just such an openly active homosexual should *not* be paranoid. In short, as Fisher and Greenberg have correctly pointed out, "the appearance of overt homosexual ... acting out in the paranoid would represent a contradiction ... of Freud's theory of paranoia" (1977, p. 259).

It should not be unduly difficult for extra-clinical researchers to sample the population of avowedly practicing homosexuals of either sex to determine whether there are any paranoiacs among them. In fact, presumably it is not a very risky bet to suppose that there are. And, if so, their existence casts strong doubt on the psychoanalytic claim that all paranoiacs are *repressed* homosexuals.

In sum, the psychoanalytic etiology of paranoia is empirically falsifiable or disconfirmable, and Freud explicitly said so in 1915. As he noted (pp. 265–266), empirical indicators can bespeak the *absence* of strongly repressed homosexuality as well as the *presence* of paranoid delusions. Thus, empirical indicators can fallibly discredit the stated etiology.

Our paranoia example clearly demonstrates falsifiability in a further respect.

As Freud also points out, the hypothesized etiologic role of repressed homosexuality leads "to the necessary conclusion that the [fancied] persecutor *must be of the same sex* as the person persecuted" [italics added] (Freud 1915/1924, 14: 265). But, if so, it becomes important to determine empirically to what extent the supposed persecutors are actually of the *same* gender as the persecutees. It is, of course, logically possible that any and every paranoiac feels persecuted *only* by

members of the *opposite* sex. For this reason alone, Freud's etiology is falsifiable by a finite number of such instances in the face of Popper's denial of such falsifiability.

What then are the known findings among paranoiacs as to the identity or difference of the gender of their imagined adversaries? And does Freud conclude that his etiology of paranoia would simply be refuted by fancied persecutors of the *opposite* sex?

Notably, Freud admits unequivocally: "In psychiatric literature there is certainly no lack of cases in which the patient imagines himself persecuted by a person of the opposite sex" (Freud 1915/1924, 14: 265). Indeed, the paranoid young woman of his case imagined herself being persecuted by a young man with whom she had been having an affair (pp. 265–266). How then, if at all, does Freud propose to reconcile such oppositely sexed cases with the requirement of gender identity that he had deduced from his homosexual etiology of paranoia?

He maintains (pp. 266–271), that whenever a supposed persecutor is of the *opposite sex*, then, investigation will presumably reveal *this* antagonist to be only a *secondary* one, whom the paranoiac imagines to be in collusion with an *original primary* persecutor of the *same* sex.

But are there statistics bearing out the first claim that the great majority of paranoiacs believe themselves to be persecuted by members of the *same* sex? Even if there are such statistics, neither Freud nor his followers have provided evidence to support his crucial *further* thesis that, for any presumed persecutor of the *opposite* sex, there demonstrably exists a person of the same sex who was the *original* object of the paranoiac's persecutory delusions. Thus, to the extent that, in the case of oppositely sexed persecutors, psychoanalysts *fail* to find the primary, gender-identical persecutor required by their theory, their etiology of paranoia is *disconfirmed*.

Hence, let us ask: In his 1915 paper on paranoia, did Freud go beyond his avowal of the *falsifiability* of its etiology and admit that the afflicted female was, furthermore, actually a refuting instance of it? The reader was led to expect just that conclusion from the very title of his paper, which – as we recall – is "A Case of Paranoia Running Counter to the Psychoanalytic Theory of the Disease." Regrettably, however, precisely this appellation of his 1915 article is a beguiling piece of ironic and even frivolous false advertising.

After informing us that this case history *might* have undermined his homosexual etiology of paranoia, because the female patient felt persecuted by a male, Freud tells us soothingly that, after all, there was *also* a fancied female persecutor and that "the additional details she [the patient] supplied resolved all doubts and difficulties" (Freud 1915/1924, 14: 266). Thus, in his view, the etiology stands *unrefuted*. Evidently, the title of his paper was a case of false advertising.

But this gamesmanship on Freud's part clearly does not vindicate Popper's charge of unfalsifiability against him!

Hence, this example has an important *general* moral: Whenever empirical indicators can warrant the *absence* of a certain theoretical pathogen *P* as well as a *differential diagnosis* of the *presence* of a certain theoretical disorder *D*, then an etiologic hypothesis of the strong form "*P* is causally necessary for *D*" is *fallibly* refutable. The etiology will be falsified or disconfirmed by any victim of *D* who



has *not* been subjected to the avowedly required pathogen *P*. For the hypothesis *predicts* that anyone *not* so subjected will be spared the miseries of *D*, a prediction also having significant *prophylactic* import for *child rearing*. Equivalently, the pertinent etiologic hypothesis *retrodicts* that any instance of *D* was also a case of *P*. Hence, if there are empirical indicators as well for the *presence* of *P*, then this retrodiction can be empirically instantiated by a person who instantiates both *D* and *P*.

It is a measure of the serious inadequacy of Popper's psychoanalytic scholarship, if not of his sheer incomprehension of psychoanalytic etiologies, that by his own account (Popper 1983, p. 169), he was first made cognizant of the testability of Freud's theory of paranoia by William Bartley in 1980, 65 years after Freud's 1915 article, which had been available to Popper ever since he reached early maturity in 1917.

As Popper relates, Bartley pointed out to him that Freudian theory *rules out* the existence of a homosexually active paranoiac. Bartley gave this example to Popper during the discussion of my own paper at the July 1980 London Symposium on Popper's philosophy. As I argued there and elsewhere (Grünbaum 1984, pp. 38–40, 110–111, 1986, p. 266, 1993, pp. 54–55, 57), a significant decline (or absence) of the social taboo on homosexual activity (as in ancient Greece) should issue in a *decrease* in the incidence of paranoia in homosexually permissive societies, as compared to censorious ones. Hence, if there is no such decrease, Freud's etiology is clearly also disconfirmed *epidemiologically*, in contravention of Popper's claim of empirical non-testability.

It is important to note that Popper tries to practice damage control by qualifying and hedging his admission to Bartley that the case of a homosexually active paranoiac does contradict his thesis of irrefutability. He resorted to *two* plainly disingenuous and unavailing immunizing maneuvers, as we shall now see.

He admonishes us that Freud's repression etiology of paranoia "is not part of the basic theory I [i.e., he] was criticizing." Yet, we are left completely in the dark as to just what does count for him *ad hoc* as the "basic" part of psychoanalytic theory, *and why*. In effect, Popper asks us to overlook that, throughout his career he had leveled the charge of nonfalsifiability *tout court*, rather than only against some so-called "basic" part of the theory, whose identity he evasively fails to specify now.

Vague as it is, even his claim that Freudian etiologic theory is *not* "basic" to psychoanalysis is simply untenable. As Freud told us, both historically and logically, precisely the theory of psychopathology, which features the repression etiologies of the neuroses, is the *most foundational* part of his edifice: As he put it, "The theory of repression is the cornerstone on which the whole structure of psychoanalysis rests. It is the most essential part of it" (Freud 1914/1957, 14: 16).

Not content with being disingenuous, the kettle calls the teapot black, when Popper writes against Freud:

I wish to criticize Freud's way of rejecting criticism....

This self-defensive attitude is of a piece with the attitude of looking for verifications; of finding them everywhere in abundance; of refusing to admit that certain cases do not fit the theory (Popper 1983, p. 163).

In response, I find it hard *not* to exclaim: "Physician heal thyself." Besides, if indeed "certain cases do not fit the theory," as Popper tells us here, are such cases not *disconfirming* or refuting instances of Freud's claims, in contravention of Popper's charge of unfalsifiability?

Very disappointingly, Popper (1986, pp. 254–255) responded to the telling counterexamples to his thesis by waffling evasively in clear violation of his own avowed methodological maxims for acknowledging contrary evidence. Thus, elsewhere (Grünbaum 1986, pp. 266–268, 1993, Chap. 2, pp. 53–62) I demonstrated the untenability of his unavailing counters in thorough detail.

At the July 1980 London Symposium mentioned above, Popper stood before the audience and tried to dramatize the falsifiability of Newtonian physics by contrast to psychoanalysis. Consider, he said, a wheel spinning, say, clockwise. And now suppose, he went on, that the wheel would suddenly start spinning in the opposite direction. In that hypothetical case, he declared astonishingly, any physicist witnessing this reversal of the direction of rotation would instantly give up the Newtonian conservation law for angular momentum. But I objected at once that *no* physicist would do so; instead, she or he would promptly look for the dynamic *source* of a counter-clockwise torque that reversed the wheel's direction of rotation. After all, looking for perturbing influences to explain sudden anomalies is standard operating procedure in science. Alas, Popper had engaged in *myth*-making in the service of special pleading.

(D) In 1958, the philosopher Sidney Hook conducted a widely known published symposium on *Psychoanalysis, Scientific Method, and Philosophy* (Hook 1959). In a valuable contribution to Hook's symposium, the Australian–American philosopher Michael Scriven (1959), who credits the psychologist and psychoanalyst Paul Meehl with the essential ideas, very thoroughly articulated key features of a cogent design for testing the *treatment-outcome* claim that psychoanalysis effects substantial therapeutic improvements in most of its patients.

Hence, Freudian avowals of therapeutic success are potentially disconfirmable. Thus, 4 years before Popper (1962, Chap. 1) categorically reiterated the intrinsic empirical untestability of the whole of the Freudian corpus, Scriven had already implicitly *undermined* that Popperian thesis, though *only* with respect to psychoanalytic *therapy*. During a 1976 visit I paid to Popper in his home in Penn, I asked his judgment of Scriven's counterexample. His unhelpful response was that he, Popper, had not been concerned with therapeutic outcome.

(E) In the 1974 Schilpp volume on Popper's philosophy, Popper (1974, Book II) reiterated that psychoanalysis is an empirically untestable psychological metaphysics, which does "not exclude any physically possible human behaviour" (p. 985). From this allegation of empirical unfalsifiability, he immediately drew the grossly *fallacious inference* (p. 985) that, in principle, psychoanalysis can indeed, *explain* any actual behavior. Thus, Popper tells us that "whatever anybody may do is, in principle, explicable [explainable] in Freudian or Adlerian terms."



But, I object that if a theory, in conjunction with particular initial conditions, *does not exclude* any behavior at all, then it cannot deductively *explain* any *particular* behavior. After all, *to explain deductively is to exclude*: As Spinoza emphasized, *to assert* [entail] *P* is to deny every proposition incompatible with it. Thus, if psychoanalysis were unfalsifiable, how could any actual behavior – let alone *all* physically possible behavior – be explained by it so as to confirm it inductively, as Popper claims (1962, p. 35)? On the contrary, the alleged unfalsifiability would *preclude* such hypothetico-deductive confirmability. Clearly, Popper fallaciously overreached himself polemically.

Moreover, he is impermissibly slipshod in *contriving* what he portrays as psychoanalytic explanations, while studiously refraining from systematically adducing actual examples from Freud's extant case histories to buttress his thesis. Thus, in his (1962) *Conjectures & Refutations* he wrote in a deplorably fuzzy manner:

[E]very conceivable case could be interpreted in the light of Adler's theory, or equally of Freud's. I may illustrate this by two very different examples of human behavior: that of a man who pushes a child into the water with the intention of drowning it; and that of a man who sacrifices his life in an attempt to save the child. Each of these two cases can be explained with equal ease in Freudian and in Adlerian terms.

When proceeding to state the purported Freudian explanations of the actions of the two men, Popper offers a painfully vague and purely concocted exegesis, declaring:

According to Freud the first man suffered from repression (say, of some component of his Oedipus complex), while the second man had achieved sublimation. According to Adler the first man suffered from feelings of inferiority (producing perhaps the need to prove to himself that he dared to commit some crime), and so did the second man (whose need was to prove to himself that he dared to rescue the child). I could not think of any human behaviour which could not be interpreted in terms of either theory (p. 35).

However, Popper does not present a shred of evidence as to how Freud would actually have explained the homicidal behavior of the first man either deductively or probabilistically, and yet, he irresponsibly tells us *pseudo-exegetically* out of the blue that "According to Freud," the first man was oedipally repressed.

Yet worse, if we grant Popper the Freudian explainability he claims, his example of the two men *boomerangs* altogether: After all, why would it be a liability of psychoanalysis at all, if it *actually* could *explain* the divergent behavior of the two men "with equal ease"? Presumably, there actually are such instances of self-sacrificing child-*rescuing* behavior no less than such cases of *infanticidal* conduct. And a *fruitful* psychological theory might well succeed in *actually* explaining each of them, perhaps even *deductively*. How then does the envisioned case of the two men "illustrate" Popper's pejorative averral that "every conceivable case could be interpreted" in the light of Freud's theory? Yet, elsewhere, Popper (1986, p. 254) repeated his unavailing, contrived example of the two men and the drowning child.

(F) In my 1984 *Foundations* book (1984, Chaps. 1B, 11), I had also censured Popper for what I called "Exegetical Myth-Making in Karl Popper's Indictment of the Clinical Validation of Psychoanalysis."

In less than 2 months after that book's appearance, the Science section of the *New York Times* of 15 January 1985 featured a very detailed *front-page* article on it, which reported, under photos of Popper and me, that I had challenged him. Then, it quoted from his response to me via the journalist Daniel Goleman, although Popper avowedly had not read my *Foundations* book.

Indeed, the journal *Behavioral and Brain Sciences* (BBS) devoted its June 1986 issue (Vol. 9, no. 2) to a review-symposium on it. Forty-one authors representing the whole spectrum of viewpoints on Freudian theory supplied "Open Peer Commentary" along with my *Précis* of the book's content, which was followed by my "Author's Response" to all of the commentators under the title "Is Freud's Theory Well-Founded?" (pp. 266–281).

Just pro forma, Popper was one of the commentators but only nominally, since he had not read my book, as shown by the Editor's Note, which introduced his Commentary (pp. 254–255) in part as follows (in italics):

Editorial Note: The following contribution from Professor Karl Popper is not a review of Professor Grünbaum's book but a critique of an argument from the book as conveyed to Popper by Daniel Goleman of the New York Times/Science Times. ... Professor Popper informs us that he has not seen the book (p. 254).

Evidently, Popper saw fit to rebut a *second-hand* summary of one of my arguments, even though he avowedly had "not seen," let alone read, my book.

My very negative reaction to Popper's commentary is included, of course, in my "Author's Response." There, I also exposed (Grünbaum 1986, p. 267, 1993, Chap. 2) the quibbling subterfuge he employed in his (1983) Postscript book *Realism and the Aim of Science*.

Expectedly, some leading psychoanalytic psychologists, such as Robert Holt (1989, p. 328) and Philip Holzman (1993, p. xvii) greeted my exoneration of psychoanalysis from Popper's indictment of irrefutability with much satisfaction amid welcoming my Freud-critique as well-informed. Thus, Robert Holt declared:

Fortunately, a leading philosopher of science, Adolf Grünbaum, became interested in the problem of the scientific status of psychoanalysis in 1976 and began studying its literature. ... Here at last is a philosopher who has done his homework before criticizing Freud (1989, p. 327).

Grünbaum [three references omitted] has earned the gratitude of all of us [psychoanalysts] by taking Popper down a peg, showing that his arguments not only are based on ignorance of what Freud actually said, but have logical flaws as well. ... Grünbaum has unanswerably established the claim of psychoanalysis to a place among the sciences *by Popper's own criteria* [italics added] (p. 328).

And, in his own commentary for the BBS review-symposium on my *Foundations*, Holt had written:

The power and subtlety of the analysis and arguments Adolf Grünbaum presents in this book far surpass those of any previous philosophical evaluation of psychoanalysis. ... The result is a work of such substance that it must be carefully studied by all serious students of psychoanalysis (1986, p. 242).

(G) Although I vigorously defended Freud against Popper, I did take extensive issue with the evidential probity of the pillars of the Freudian edifice. Thus, in my subsequent 1993 book *Validation in the Clinical Theory of Psychoanalysis: A Study in the Philosophy of Psychoanalysis* (Chap. 4), I challenged Freud's ubiquitous inferences of *causal* connections between mental events from *mere* thematic connections (affinities) between them. Hence I should report that, in 1995, the eminent American psychologist, philosopher of psychology, and veteran practicing psychoanalyst Paul E. Meehl hailed my challenge to such psychoanalytic causal inferences as "the biggest single methodological problem that we [psychoanalysts] face" (Meehl 1995, p. 1021). I hope I shall be forgiven for taking great pride and pleasure in *Meehl's watershed acknowledgment of my critique*.

Fittingly, I think, he published it in the official *Journal of the American Psychoanalytic Association* in his 1995 "Commentary: Psychoanalysis as Science." Indeed, Meehl (p. 1021) speaks very soberingly concerning the import of the failure to meet the major epistemological difficulty I posed. As he put it in context:

If that problem cannot be solved, we will have another century in which psychoanalysis can be accepted or rejected, mostly as a matter of personal taste. Should that happen, I predict it will be slowly but surely abandoned, both as a mode of helping and as a theory of the mind [reference omitted].

(H) I had carried out my critique of psychoanalysis within the framework of *eliminative inductivism*, which Francis Bacon had pioneered three centuries before Popper came upon the scene.

As we recall from the *Abstract*, Bacon did err in supposing that, for any given set of observational data, there is only a *finite set* of alternative hypotheses, each of which might explain them. But he emphasized, long before Popper, that – other things being equal – negative instances have greater probative force than positive ones, having scoffed at simple enumerative induction from positive instances as "puerile." Thus, Bacon envisioned the *inductive elimination* of all but one of the supposedly finite number of alternative hypotheses, which would thereby be shown to be true.

But he erred, since, in principle, there is always a potentially *infinite* set of alternative explanatory hypotheses, some of which working scientists may eliminate by means of refuting (disconfirming) instances, while other such hypotheses survive, at least temporarily, as theoretical candidates for inductive acceptance. In view of Bacon's finitist error, I advisedly distanced myself somewhat from him in my 1984 book, and denoted this epistemic process by the locution "*neo-Baconian* eliminative induction." This designation should not have occasioned any carping, although it did at the hands of Joseph Agassi, who chose to label me "the elusive neo-Baconian" as part of his grudging, hollow, and desultory argument that my book contained nothing new (Agassi 1988, Chap. 24).

Popper had very tendentiously and ahistorically glossed over the major probative discrepancy between Bacon's sophisticated eliminative inductivism and its enumerative ancestor. But, having mistakenly saddled psychoanalysis with being

an empirically unfalsifiable pseudoscience, Popper wrongly indicted inductivism *wholesale* as incompetent to detect this alleged fundamental flaw. Thereby inductivism purportedly failed to give a correct diagnosis of the scientific bankruptcy of the psychoanalytic enterprise (Popper 1962, pp. 33–38, 255–258, 1974, pp. 984–985; Grünbaum 1984, pp. 103–107). Whence Popper concluded: “Thus there clearly was a need for a different criterion of demarcation” (1962, p. 256) between science and pseudoscience, *other than* the inductivist one. In this way, psychoanalysis served as the gravamen and benchmark of his erroneous case for the superiority of his own falsifiability criterion of demarcation to the inductivist one.

I contend that my 1984 book thoroughly exploded this Popperian philosophic scenario. Thus, in its concluding Chapter 11, I rejected his treatment of the demarcation problem entirely as follows:

It is ironic that Popper should have pointed to psychoanalytic theory as a prime illustration of his thesis that inductively countenanced confirmations can easily be found for nearly every theory, if we look for them. Being replete with a host of etiological and other causal hypotheses, Freud's theory is challenged by neo-Baconian inductivism to furnish a collation of positive instances from *both* experimental and control groups, if there are to be inductively *supportive* instances ... to this day, [psycho]analysts have not furnished the kinds of instances from controlled inquiries that are *inductively required* [for example] to lend genuine support to Freud's specific etiologies of the neuroses. Hence, *it is precisely Freud's theory that furnishes poignant evidence that Popper has caricatured the inductivist tradition by his thesis of easy inductive confirmability of nearly every theory* [italics in original] (p. 280).

Very recently, I published the first installment of a detailed critical account of “The Reception of my Freud-Critique in the Psychoanalytic Literature.” Its lengthy first installment appeared in the July 2007 issue of *Psychoanalytic Psychology* (Grünbaum 2007), the official journal of the Division of Psychoanalysis of the American Psychological Association.

The overall reception of my 1984 *Foundations*, which includes, of course, the reaction to my strictures on Popper, was comprehensively summarized almost a decade after its publication in a book entitled *Freud and his Critics* by the clearly pro-Freudian Stanford University intellectual historian Paul Robinson (1993, Chap. 3). He wrote his 1993 book before my own *Validation* volume of the same year became available to him.

Surveying the entire first *century* of psychoanalytic history, Robinson wrote:

The *Foundations of Psychoanalysis* has been widely hailed as the most substantial philosophical critique of Freud ever written (p. 180) ...

Although philosophical critiques of psychoanalysis have existed almost since the doctrine first made its appearance at the turn of the century, Grünbaum's analysis is distinguished from these earlier efforts by several features. Most notable are his extraordinary rigor and precision. Grünbaum is manifestly both very smart and very sophisticated, and his critique maintains an unprecedented level of dialectical intensity. At least for the philosophically untutored (to borrow one of Grünbaum's own favorite words), virtually every sentence must be carefully unpacked, so thick and unforgiving (although never obscure) is his habit of thought. At the same time, Grünbaum surpasses all previous philosophical critics of psychoanalysis in the breadth and suppleness of his knowledge of Freud's writings (p. 181).

It behooves me to conclude by dealing with the unfortunate 1983 dénouement of Popper's Freud-Critique, and of his intermingled treatment of the demarcation between science and non-science, in his Postscript of that year. The dénouement focused on Freud's 1900 theory of dreaming.

This wish-fulfillment theory of dreaming asserted at least two theses as follows: The manifest dream "*content was the fulfillment of a wish [first thesis] and its motive was a wish [second thesis]*" [italics in original] (Freud 1900/1958, 4: 119). Then in the opening sentence of Chapter IV of his 1900 *magnum opus* on dreaming, Freud says: "the meaning of *every* dream is the fulfillment of a wish, ... [so that] there cannot be any dreams but wishful dreams" [italics in original] (Freud 1900/1958, 4: 134).

As he tells us, "the idea that *some* dreams are to be regarded as wish-fulfillments" (Freud 1900/1958, 4: 134) had been commonplace in prepsychoanalytic psychology. Hence, Freud propounded a distinctive and exciting thesis about dreaming only when he *universalized* this commonsensical idea, declaring: "the meaning [motive force for the formation] of *every* dream is the fulfillment of a wish (*ibid.*)."<sup>1</sup> And, as he is the first to recognize, *prima facie* this completely general thesis is surely impugned by sundry wish-*contravening* and distressing manifest dream contents (e.g., by anxiety dreams).

Thus, Freud was generally well aware of so-called "counterwish dreams," (Freud 1900/1958, 4: 157) such as was reported by a Viennese trial attorney who dreamt having lost all of his court cases (p. 152) after hearing Freud's lecture on his wish-fulfillment theory in 1917. As Freud explains, "The very frequent dreams, [footnote omitted] which appear to stand in contradiction to my [wish-fulfillment] theory because their subject-matter is the frustration of a wish or the occurrence of something clearly unwished-for, may be brought together under the heading of 'counter-wish' dreams" (Freud 1900/1958, 4: 157).

Clearly, in the face of wish-contravening dreams, Freud's universal wish-fulfillment scenario required that he *distinguish* between the supposedly wish-*distorting* and *disguising manifest* content of a dream, on the one hand, and its *latent*, repressed wish-content, on the other. Hence he wrote: "*a dream is the disguised fulfillment of a [repressed] wish ... dreams are constructed like a neurotic symptom: they are compromises between the demands of a repressed impulse and the resistance of a censoring force in the ego*" [italics in original] (Freud 1924/1925, 20: 45). Drawing on the Greek word "oneiros" for dream, we can speak of Freud's wish-fulfillment theory of dreaming as his "oneirology," and we can also use the adjective "oneiric."<sup>2</sup>

As we learn from Popper's 1983 *Postscript* (Popper 1983, p. 164), the psychoanalytic oneirology had been a major basis for his advocacy of the falsifiability criterion of demarcation *vis-à-vis* inductivism, in the face of allegedly ubiquitous inductive oneiric verifications. Yet, astonishingly, Popper clearly repudiated his erstwhile claim that Freud's dream theory is unfalsifiable, by declaring it to be a "simple fact ... that *anxiety dreams constitute a refutation of the general formula of wish fulfillment*" [italics in original] (Popper 1983, p. 173). But, if Freud's oneirology is falsifiable by anxiety dreams after all, how then could that Freudian

theory be ubiquitously confirmed inductively, and thereby serve to impugn inductivism as a criterion of demarcation in favor of Popper's own criterion?

Unlike Popper, Clark Glymour (1983) and I have supplied a requisitely sophisticated refutation of universal oneiric wish fulfillment, which I summarized in my 1984 book (pp. 233–235). And Glymour did so by showing how Freud contrived, by tendentious misuse of the method of free association, to find a repressed *wish*-motive in *every* latent dream-content.

According to Freud (1900/1958, 4: 151–152), the trial attorney's unwelcome dream about losing in court – precisely by being *wish-contravening* – had cunningly fulfilled his purported unconscious wish to *disprove* Freud's thesis of universal oneiric wish fulfillment, a wish supposedly due to the attorney's lingering resentment of Freud's boyhood primacy when they were both still in school! Mind you, as Freud explained, in counterwish dreams, such as that of the trial attorney, “the *non*-fulfillment of *one* wish meant the fulfillment of *another*?” [*italics added*] (*ibid.*, p. 157).

In thus reconciling such dreams with his oneirology by recourse to the wish to prove Freud wrong, is he rendering it immune to disconfirmation, as in “heads I win, tails you lose”? He is not. At the aforementioned 1958 Hook Symposium, Wesley Salmon gave a lucid account of the sort of empirical evidence that would *disconfirm* Freud's explanation of counterwish dreams (Salmon 1959, pp. 252–265). For instance, Salmon notes that “if one of these [counterwish] dreams which cannot be explained as any other kind of wish fulfillment were to occur to someone who had never heard of Freud's theory [and hence has no motive to prove him wrong], this would be most damaging” to that theory (p. 264).

In my own detailed scrutiny of the counterwish issue in Freud's oneirology, I *argued* for the following unfavorable verdict (1993, pp. 360–370): Freud was unable to uphold his generic explanation of counterwish dreams as being engendered by either the dreamer's wish to discredit his dream theory, or by the dreamer's masochistic craving for mental torture. After all, scores of ardent Freudians the world over, who neither have the wish to disprove Freud's oneiric theory nor are masochists, routinely have counterwish dreams. Hence, in the face of such dreams, Freud was unable after all to sustain his claim that “even dreams with a distressing content are to be construed as wish-fulfillments” (Freud 1900/1958, 4: 159).

As if Popper's forecited *contradictory* verdicts on psychoanalytic oneirology were not enough, the Demarcation chapter in his 1983 *Postscript* (Part I, Chap. 2) confronts us with the spectacle of him flatly contradicting his lifelong conception of demarcation, and of then watering it down beyond recognition to boot! Thus, there, at the end of his section on Freud's theory, Popper tells us amazingly enough that “from the beginning,” his “problem of demarcation ... certainly was *not* a problem of classifying or distinguishing some subject matters called ‘science’ and ‘metaphysics’” [*italics added*] (1983, p. 174). But, this concluding disclaimer *flatly contradicts* the first conjunct of his initially stated motive for dealing with the psychoanalytic dream theory. For he told us that his purpose in treating the dream theory was to show: “that the problem of demarcation is *not merely one of classifying theories into scientific and non-scientific ones*, but that its solution is urgently



needed for a critical appraisal of scientific theories, or allegedly scientific theories” [italics added] (Popper 1983, pp. 163–164).

Even more glaringly, Popper’s concluding disclaimer that he was *not* demarcating science from metaphysics is patently incompatible with two of his earlier landmark reports, as shown by statements he made in 1953, and two decades later, in 1974. Thus in his 1953 Personal Report on the development of his entire philosophy of science, which is reprinted in his 1962 *Conjectures and Refutations*, he informs us that, beginning in 1919, when he was 17 years of age, his problem was “to distinguish between science and pseudoscience ... [or] ‘metaphysics’” (1962, p. 33). And in his 1974 Replies to Critics, he explains emphatically that his criterion of demarcation:

[I]s more than sharp enough to make a distinction between many physical theories on the one hand, and metaphysical theories, such as psychoanalysis, or Marxism (in its present form), on the other. *This is, of course, one of my main theses; and nobody who has not understood it can be said to have understood my theory* [italics added] (Popper 1974, p. 984).

Evidently, in 1983, Popper insouciantly repudiates just this major, central tenet of his whole philosophy without ado. By the same token, he now denies his repeated other historical accounts of the aim that inspired his criterion of demarcation, starting in 1919. Nay, this 1983 retraction occurs on the heels of a qualified, hedged disavowal that plainly serves as a retroactive *immunizing stratagem* for vindicating his prior notion of demarcation apropos of psychoanalysis and Marxism. Thus, in the *Postscript* (1983, p. 174) he tells us astonishingly:

[I]t hardly matters whether or not I am right concerning the irrefutability of any of these three theories [i.e., psychoanalysis, Adlerian psychology, and Marxism]: here they serve merely as examples, as illustrations. For my purpose is to show that my “problem of demarcation” was from the beginning the practical problem of assessing theories, and of judging their claims. It [the problem of demarcation] certainly was not a problem of classifying or distinguishing some subject matters called “science” and “metaphysics.”

Evidently, Popper tells us here that the stated three theories are not essential to the vindication of his falsifiability criterion of demarcation, and are mere illustrations of the *quotidian* need to adjudicate theories. But if so, I must ask: What *other* theories for which scientificity has supposedly been *wrongly* claimed by inductivists can Popper adduce to furnish such a vindication of falsifiability *vis-à-vis* the much older inductivist criterion of evidential support, which he wants to replace as unduly permissive epistemologically? And, as for “the practical problem of assessing theories,” the arch-inductivist Francis Bacon had demanded – three centuries before Popper – that the validation of a given theory *T* requires special so-called “prerogative instances,” that are *contrary* to *T*’s rivals or competitors, while being positive instances of *T*. Indeed, has not *everybody*, scientists and philosophers of science alike, been in favor of “assessing theories” all along? So, how is Popper’s injunction *distinctive* at all?

I conclude that he has fundamentally misdiagnosed the epistemic or scientific defects of psychoanalysis, and *thereby* has radically misportrayed the bearing of these defects on rival criteria of demarcation. Indeed, Popper’s 1983 notion of

demarcation has become a kind of *degenerative* philosophical research program in Imre Lakatos's sense.

Thus, alas, Popper's treatment of psychoanalysis as the avowed anvil of his theory of scientific rationality was an intellectual fiasco throughout.

## References

- Agassi, Joseph. 1988. *The gentle art of philosophical polemics*. La Salle, IL: Open Court.
- Breuer, Josef. and Freud, Sigmund. 1893/1955. On the psychical mechanism of hysterical phenomena; preliminary communication. In James Strachey (Ed.), *The standard edition of the complete psychological works of Sigmund Freud*. (Original work published in 1893.) (Vol. 2, pp. 1–17). London: Hogarth Press.
- Fisher, Seymour and Greenberg, Roger P. 1977. *The scientific credibility of Freud's theory and therapy*. New York: Basic Books.
- Freud, Sigmund. 1900/1958. The interpretation of dreams. In James Strachey (Ed.), *The standard edition of the complete psychological works of Sigmund Freud*. (Original work published in 1900.) (Vol. 4–5, pp. 1–686). London: Hogarth Press.
- Freud, Sigmund. 1914/1957. On the history of the psycho-analytic movement. In James Strachey (Ed.), *The standard edition of the complete psychological works of Sigmund Freud*. (Original work published in 1914.) (Vol. 14, pp. 7–66). London: Hogarth Press.
- Freud, Sigmund. 1915/1922. Some neurotic mechanisms in jealousy, paranoia and homosexuality. In James Strachey (Ed.), *The standard edition of the complete psychological works of Sigmund Freud*. (Original work published in 1915.) (Vol. 18, pp. 223–232). London: Hogarth Press.
- Freud, Sigmund. 1915/1924. A case of paranoia running counter to the psycho-analytic theory of the disease. In James Strachey (Ed.), *The standard edition of the complete psychological works of Sigmund Freud*. (Original work published in 1915.) (Vol. 14, pp. 263–272).
- Freud, Sigmund. 1924/1925. An autobiographical study. In James Strachey (Ed.), *The standard edition of the complete psychological works of Sigmund Freud*. (Original work published in 1924.) (Vol. 20, pp. 7–74). London: Hogarth Press.
- Glymour, Clark. 1983. The theory of your dreams. In Robert Sonn   Cohen and Larry Laudan (Eds.), *Physics, philosophy, and psychoanalysis: Essays in Honor of Adolf Gr  nbaum* (pp. 57–71). *Boston Studies* vol. 70 Boston: D. Reidel.
- Gr  nbaum, Adolf. 1976a. Ad hoc auxiliary hypotheses and falsificationism. *The British Journal for the Philosophy of Science*, 27(4), 329–362.
- Gr  nbaum, Adolf. 1976b. Can a theory answer more questions than one of its rivals? *The British Journal for the Philosophy of Science*, 27(1), 1–23.
- Gr  nbaum, Adolf. 1976c. Is falsifiability the touchstone of scientific rationality? Karl Popper versus inductivism. In Robert Sonn   Cohen, Paul K. Feyerabend and Marx W. Wartofsky (Eds.), *The Boston studies in the philosophy of science: essays in memory of Imre Lakatos* (Vol. XXXIX, pp. 213–252). The Netherlands: D. Reidel.
- Gr  nbaum, Adolf. 1976d. Is the method of bold conjectures and attempted refutations justifiably the method of science? *The British Journal for the Philosophy of Science*, 27(2), 105–136.
- Gr  nbaum, Adolf. 1984. *The foundations of psychoanalysis: a philosophical critique*. Berkeley, CA: University of California Press.
- Gr  nbaum, Adolf. 1986. *Pr  cis of the foundations of psychoanalysis: a philosophical critique, and author's response to 41 reviewers: is Freud's theory well founded?* *Behavioral and Brain Sciences*, 9(2), 217–284.
- Gr  nbaum, Adolf. 1993. *Validation in the clinical theory of psychoanalysis: a study in the philosophy of psychoanalysis*. Madison, CT: International Universities Press.



- Grünbaum, Adolf. 2002. Critique of psychoanalysis. In Edward Erwin (Ed.), *The Freud encyclopedia, theory, therapy, and culture* (pp. 117–136). New York/London: Routledge.
- Grünbaum, Adolf. 2006. Is Sigmund Freud's psychoanalytic edifice relevant to the 21st century? *Psychoanalytic Psychology*, 23(4), 257–284.
- Grünbaum, Adolf. 2007. The reception of my Freud-critique in the psychoanalytic literature, part I. *Psychoanalytic Psychology*, 24(3), 545–576.
- Grünbaum, Adolf. 2008. An autobiographical-philosophical narrative. In Aleksandar Jokic (Ed.), *Philosophy of religion, physics and psychology: essays in honor of Adolf Grünbaum*. Amherst, NY: Prometheus Books.
- Hanly, Charles. 1988. [Book Review of A. Grünbaum's *The foundations of psychoanalysis*]. *Journal of the American Psychoanalytic Association*, 36(2), 521–528.
- Holt, Robert R. 1986. Some reflections on testing psychoanalytic hypotheses. Open peer commentary on Adolf Grünbaum, *Précis of the foundations of psychoanalysis: a philosophical critique*. *Behavioral and Brain Sciences*, 9, 242–244.
- Holt, Robert R. 1989. *Freud reappraised: a fresh look at psychoanalytic theory*. New York: Guilford.
- Holzman, Philip S. 1993. Introduction. In A. Grünbaum's *Validation in the clinical theory of psychoanalysis: a study in the philosophy of psychoanalysis* (pp. xvii–xxii). Madison, CT: International Universities Press.
- Hook, Sidney. (Ed.). 1959. *Psychoanalysis, scientific method, and philosophy*. New York: New York University Press.
- Meehl, Paul Everett. 1995. Commentary: psychoanalysis as science. *Journal of the American Psychoanalytic Association*, 43(4), 1015–1021.
- Popper, Karl Raimund. 1962. *Conjectures and refutations*. New York: Basic Books.
- Popper, Karl Raimund. 1974. Replies to my critics. In Paul Arthur Schilpp (Ed.), *The philosophy of Karl Popper* (Vol. 2, pp. 961–1197). LaSalle, IL: Open Court.
- Popper, Karl Raimund. 1983. In William Warren Bartley (Ed.), *Realism and the aim of science*. Totowa, NJ: Rowman & Littlefield.
- Popper, Karl Raimund. 1986. Predicting overt behavior versus predicting hidden states. *Behavioral and Brain Sciences*, 9(2), 254–255.
- Robinson, Paul. 1993. Adolf Grünbaum: the philosophical critique of Freud. In *Freud and his critics* (Chap., III, pp. 179–266). Berkeley, CA: University of California Press.
- Salmon, Wesley C. 1959. Psychoanalytic theory and evidence. In Sidney Hook (Ed.), *Psychoanalysis, scientific method and philosophy* (pp. 252–267). New York: New York University Press.

# Popper on Refutability: Some Philosophical and Historical Questions

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**Abstract** Popper's falsifiability criterion of demarcation is critically examined, both as a proposal with an independent epistemological rationale and as a condition which modern science is supposed to satisfy, and some famous objections to it are discussed. While possessing an irresistible epistemological appeal, an analysis of the objections arising from the Duhem problem shows that there is an immediate conflict, and not a mutual support, between Popper's methodological approach to falsifiability in *Logik der Forschung* and his formal criterion of demarcation. Moreover, and partly owing to this unnoticed conflict, his classic defence of falsificationism as an alternative to conventionalism is shown to rely on assumptions linking testability with meaning which, contrary to what is sometimes supposed, were shared by Popper with some of his positivist and conventionalist targets, and later superseded by his realist and objectivist conception of scientific statements.

## Introduction

In his classic work *Logik der Forschung*, Karl Popper subscribed to the proposal that only those hypotheses which run the risk of being empirically refuted should be admitted into science. Despite its considerable appeal, this proposal, known as the falsifiability criterion of demarcation, has ever since become a recurrent element of controversy among philosophers of science, and occasionally among scientists themselves. Today, moreover, the problem of demarcation has drawn some public attention owing to notorious disputes over the scientific status of string theory, creationism, intelligent design and alternative medicines, where falsifiability is frequently mentioned as an approximately correct criterion. In what follows, however, I will not be concerned with the application of Popper's ideas to these public controversies, but will assess the rationale behind the falsifiability

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proposal and evaluate some of the objections and defences, classic and recent, it has inspired. Although some classical criticisms of the criterion can be overcome, I will suggest that the Duhem problem poses deeper difficulties, mainly concerning the notion of the *methodological* falsifiability of scientific hypotheses and the way it is related to the epistemological aim of the demarcation criterion. I will also claim that, alongside his usual Kantian, semantical accounts of analytic conventions, Popper's defence of falsificationism in *Logik der Forschung* leads him to adopt a criterion for conventionality which is methodological: a theory's factual or conventional status depends on the way it is handled in practice. Yet this conception, which points in the direction of Quinean indeterminacies, does not seem to match the realism and objectivism about theories Popper also came to embrace.

## Popper's Demarcation Criterion and Its Rationale

"Those among us who are unwilling to expose their ideas to the hazard of refutation do not take part in the game of science" (Popper 1959, p. 280): this is how Popper memorably stated his proposal for demarcation at the end of *Logik der Forschung*. According to this proposal, those hypotheses, no matter how suggestive, which are so vague or indeterminate as not to clash with any conceivable observation should not count as scientific. Popper once exemplified the kind of irrefutable theories he had in mind with Freudian psychoanalysis and Adlerian psychology which, he claimed, were completely out of the reach of serious empirical scrutiny. He illustrated his point with an amusing example, suggesting that if one could claim to have offered an explanation, with the help of these theories, of a person's behaviour, they could also have been used with equal ease to conjure up an explanation for any other behaviour, however dissimilar, he might have happened to display; no conceivable case of human conduct could upset them. He in turn contrasted this situation with Einstein's risky predictions concerning the deflection of light rays, which allowed for a severe test, performed by Eddington during the famous solar eclipse experiment of 1919. Einstein himself, Popper commented, clearly stated the empirical conditions under which his theory would have to be given up, and said he would proceed accordingly if the results did not turn out as predicted. The contrast between these two cases expressed, according to Popper, the crucial difference between science and non-science: "Every 'good' scientific theory is a prohibition: it forbids certain things to happen. The more a theory forbids, the better it is... A theory which is not refutable by any conceivable event is non-scientific. Irrefutability is not a virtue (as people often think) but a vice" (Popper 1963, p. 36).

It is interesting to note that already in the eighteenth century, the scientist and *philosophe* Jean d'Alembert had appealed, in a strikingly similar way, to differences in refutability in his advocacy of Newtonian theory and criticism of Descartes' theory of vortices. Let us quote some of d'Alembert's words:

By means of Descartes' vortex theory one can very well explain the motions of the planets, but in such an incomplete and vague a fashion that if the phenomena were completely

different, they could often be explained in the same way, and sometimes even better. The Newtonian system does not allow any such illusion: a mere article or observation which dismisses the calculations will make the whole edifice crumble and relegate Newtonian theory to the class of so many others created by the imagination and destroyed by analysis. (d'Alembert 1770, p. 231)

In the article on “Experimental sciences” written for the *Encyclopédie*, he had also derided the Cartesian physicists’ *fureur de expliquer tout* in similar terms<sup>1</sup>:

Let [the physicist] refrain above all from trying to account for that which is beyond his reach. Let him resist this furor of explaining everything which Descartes has introduced into physics and which has accustomed the majority of his followers to sustain vague principles and vague reasons, fit equally to sustain an argument for one thing and for the contrary.

It was Popper, however, who explicitly and repeatedly proposed falsifiability as a solution to a fundamental problem. Indeed, the problem of demarcation was one of “die beiden Grundprobleme” (the two fundamental problems) alluded to in the title of his first written book on epistemology; David Miller describes Popper’s solution to it as “the backbone of his philosophy of science” (Miller 2006, p. 89). There are, however, at least two ways in which the problem may be understood. One way is to look at demarcation as a proposal – namely, to limit serious candidates for knowledge to those which satisfy the condition the criterion states. Another way is to think demarcation as providing an answer to the question *What is science?*, and so inviting one to state a “real” definition of it which will have to be correct at least extensionally. There are correspondingly two perspectives from which any criterion of demarcation might be judged. If we favour the first interpretation, one will naturally enquire what the distinguishing mark proposed by the criterion is good for: what makes it so valuable from an epistemological point of view. This seems to be the most important question to ask, and gives the problem immediate philosophical relevance. The second perspective, corresponding to the second interpretation, concerns the correctness of the criterion from an extensional point of view: is the stated characteristic a mark of real scientific theories? Moreover, if a paradigmatically scientific theory should turn out to be disqualified by the criterion, one might think that the proposed characteristic was not really a necessary condition for a theory to be a good theory: after all, the output of our most successful cognitive endeavour, science, does not always satisfy the requirement. This situation suggests that the two perspectives on the demarcation problem are ultimately not independent: in some cases, extensional inadequacy might betray an epistemological overemphasis (indeed, this is what many critics of Popper, from Neurath on, have claimed). However, we must also beware lest the criterion be especially tailored as to deny scientific status to some specific contested discipline without further explanation; if such an explanation is to be given, a discussion of its epistemological advantages will have to be pursued.

What, then, is the epistemological rationale of Popper’s criterion? A very simple explanation, relating to the aim of science, was given by Popper (although not in his *Logik der Forschung*) and more explicitly by Miller (Miller 1994, 2006). After assimilating Tarski’s lessons – and this took place only after *Logik der Forschung* was published – Popper had no qualms in identifying truth as a legitimate and

fundamental aim of science. However, as Popper forcefully argued, true theories are not arrived at by any inference from observation reports, and there is in any case no way in which these could prove our theories to be true, or even probable. Thus, the only way left in our search for truth is to provisionally set forth our conjectures and subsequently try our best to submit them to empirical control. These tests might reveal that our conjecture was mistaken and, *since* we aim at truth, put us in the need of devising a new and hopefully better conjecture. Unfalsifiable theories, however, do not allow any conceivable experience to show their falsity; unless a theory is falsifiable, that is, our theoretical mistakes can never be revealed by any observation, even if they exist. (As David Miller nicely puts it, working with unfalsifiable theories not only makes the perpetration of error possible – this is inevitable – but also its perpetuation.) Thus, we see that the rational procedure of submitting our hypotheses to the test of experience in order to detect possible mistakes requires their falsifiability; and this seems to provide a most compelling epistemological rationale for the proposal. This is why Popper wrote: “[demarcation] was... a practical problem: under which conditions is a *critical appeal to experience* possible – one that could bear some fruit?” (Popper 1983, p. 174, Popper’s emphasis).

This epistemological analysis, although perhaps rather obvious, allows us to articulate our feeling of unease at the two examples presented to us by d’Alembert and Popper. No planetary orbit is ruled out by Cartesian vortex theory, d’Alembert says, but then why bother to make planetary observations in order to assess it? Even if Descartes’ astronomical theory is completely false, we cannot expect any planetary antics to show us this: its falsity will remain, so to speak, forever hidden in the deep. We may demand, it seems, that there be conceivable experimental results capable of awakening us from our theoretical delusions. There is clearly a realist presentiment to this Popperian analysis: if reality was once defined as *that which kicks back*, we can say that “it is through the refutation of our suppositions that we put ourselves in contact with reality” (Popper 1974b, p. 324, 1963, pp. 116 ff.).

Given the undeniable epistemological appeal of Popper’s requirement, it is not surprising that most criticisms of it have not centered directly on its rationale, but on its extensional adequacy. Let us review some of them.

## Existential Quantification and Metaphysics

Some early criticisms raised against Popper’s proposal were Carnap’s (1936–1937) and Hempel’s (1945) well-known references to statements containing existential or mixed quantification. Among these are those which John Watkins has called “all-and-some statements”, which have the logical form ‘ $(x)(\exists y)Pxy$ ’ (“For all  $x$ , there is a  $y$  such that  $x$  has the relation  $P$  to  $y$ ”). “Whether a hypothesis is verifiable or falsifiable (...) depends exclusively on its logical form”, writes Hempel (1965, p. 38), and the logical form of all-and-some statements renders them unfalsifiable. For in order to falsify them, one should have to ascertain that there is some *a* such that, for *no*  $y$ ,  $\text{Pay}$ :  $(y)\sim\text{Pay}$ . But this is a strictly universal, and therefore

not basic, claim; and so is, in general, any piece of evidence which contradicts a hypothesis with an existential component. It is plain, however, that some statements of empirical science must contain mixed quantification. According to Hempel, therefore, Popper's requirement of falsifiability "involves a very severe restriction of the possible forms of scientific hypotheses" (*op. cit.*, p. 45).

Popper's answer to this has been that, contrary to Carnap and Hempel, not every hypothesis involving existential or mixed quantification, even if formulated in an 'empiricist language', is unfalsifiable; and that the falsifiability of an empirical hypothesis, therefore, involves a bit more than its logical form. Popper gave several examples, but ironically we can illustrate his point with a slight modification of an example of Hempel's (1965, p. 197),<sup>2</sup> who gave something like the following instance of a claim which is unverifiable and unfalsifiable owing to its logical form:

$$(E y)(z)(y \neq a \ \& \ (z \neq y \rightarrow \text{Say}z)),$$

where "Sayz" means "a is farther from y than it is from z". That is, this claim asserts that there is an object which is farther from a than any other object (we suppose here that we are speaking of macroscopic, observable objects).<sup>3</sup> The claim is unverifiable since we could never be sure that there is no object which is farther away from a than the y which we might *think* is the farthest from a. The claim is also unfalsifiable, because to falsify it we should need to finitely ascertain that for *every* object y there is some other object z which is farther away, or at an equal distance, from a than y is – which we obviously cannot do. Now if "Sayz" was the observation predicate "a is nearer to y than it is to z", we would obtain another factual statement with the same logical form. But it turns out to be a verifiable and falsifiable claim: it means that there is an object, different from a, which is nearer to a than any other object. If there were only one object to be found in the vicinity of a, the claim would be verified; and the claim could be falsified by observing that there are just two objects, equidistant from a, in the vicinity of a. So it is not generally true that "if R and S are observational predicates, then sentences of the type '(Ey)Ray' are not falsifiable and sentences of the form '(y)(Ez)Sayz' and '(Ez)(y)Sayz' (...) are neither verifiable nor falsifiable" (Hempel, *op.cit.*, p. 198). Of course, a denial of existence will have to be employed in the evidence which describes a falsifying instance of a refutable statement having one of these forms. But, unless we share something like the empiricist strictures of Russell's *Inquiry into Meaning and Truth*,<sup>4</sup> we should not be flustered by this; we will not need to "search the whole world in order to establish that something does not exist, has never existed, and will never exist" (Popper 1959, p. 70).

Popper also discusses some alleged scientific counterexamples to falsifiability which can be dismissed on similar grounds. One of them, adduced by Grover Maxwell (cf. Maxwell 1974), is the neutrino hypothesis: if we say that in every beta emission *there is* a neutrino emitted, this can be naturally rendered into an all-and-some statement. But, as Popper argues, it happens to be a refutable all-and-some statement, if only we have at our disposal (as we do) an experimental method for detecting a neutrino at a given place and consider the circumscribed nature of a nuclear

emission (cf. Popper 1974a, p. 1038). Another alleged counter-example, adduced by Carnap (1963, p. 879), is the class of all statements of physics using differential coefficients, like an ascription of an instantaneous velocity to an object. These are irrefutable, Carnap claimed, since when parsed as assertions on limits in the classical Weierstrassian way, they are seen to contain both existential and universal quantification. But again, Carnap's assertion must be an exaggeration, since the physicist can verify or falsify statements about instantaneous velocities by means of his measuring techniques, which Popper sees as shaping the empirical basis of science.

One might be tempted to complain (again following Maxwell [1974] and Kneale [1974]) that notwithstanding their unfalsifiability, certain *unspecific* existential claims express important and sometimes unexpected truths about the world, and therefore should not be excluded from science. An interesting example to assess and illustrate this point is the thesis of spontaneous generation. If true, the hypothesis states an interesting fact – that there exist certain conditions under which life originates from some non-living matter. But then again, it is reasonable to require that this hypothesis, in order to become scientific, should specify which those conditions are, and which the non-living matter. Stated in this way, the hypothesis allows in principle for a test. But then we are no longer dealing with an unrestricted existential statement, but with a testable hypothesis telling us what to expect given certain specific conditions – a scientific hypothesis which trivially entails (and so asserts) the original, unspecific statement (cf. Miller, *op. cit.*, 96). Popper explained the point in the following way: “Whenever a pure existential statement, by being empirically confirmed, appears to belong to empirical science, it will in fact do so not *on its own account*, but *by virtue of being a consequence of a corroborated falsifiable theory*” (Popper, *ibid.*; Popper's emphasis).

Like Donald Gillies (1993, p. 207), I find Popper's replies concerning existentially quantified statements convincing up to this point. However, in his confutation of Carnap and Hempel's objections, Popper (1983) puzzlingly claims to have provided a deadly criticism of Carnap's *Sinnskriterium* as stated in his *Testability and Meaning* (Carnap), 1936–1937 by having shown that *testability* cannot be equated with *meaningfulness* (this was also Popper's claim in his contribution to the Carnap Schilpp volume; cf. Popper 1963, Chap. 11). The suggestion is that, as we saw in the case of Hempel's example, two statements may be constructed in exactly the same way from empirical predicates and logical signs, being therefore equally meaningful, and yet only one of them may be testable. Moreover, even the metaphysical sentence “God exists”, Popper claimed, can be formulated as a well-formed sentence in Carnap's empiricist language, since it can be construed as meaning “There exists an omnipresent, omniscient, omnipotent personal being”, all of whose predicates are – so Popper tried to show – physicalistically definable (Popper 1963, pp. 274 ff.). He asked further: “Did you [i.e. Carnap] not connect testability and meaning?”; “And do not our examples show that there are non-testable but meaningful statements?” (Popper 1983, p. 214).

The answer, I think, is “No”, and the reason is simply that what Popper calls testable is not what Carnap called testable. According to Popper, testability equals falsifiability; while for Carnap, testability is (practical) *confirmability*, which



is admittedly a much weaker concept: it includes falsifiable, but also verifiable and non-falsifiable, and neither verifiable nor falsifiable sentences. Carnap, in effect, never claimed that the limits of the meaningful coincide with those of the *falsifiable*, but it is just this claim that Popper manages to refute.<sup>5</sup> Thus “There are Fs”, where F is built upon predicates describing a technically realizable property, is by Carnap’s standards an empirical, “testable” claim. Granted, “There exists somewhere, at some time, an F” does not suggest any possible test, nor can any amount of stumbled-upon evidence lead us to find out its falsity. But Carnap defined what *he* meant by “testability” in this weak, non-Popperian way (Carnap 1936–1937, p. 463)<sup>6</sup>; it is this concept, therefore, that we must interpret Carnap as having identified with that of empirical meaningfulness. Nor would Carnap have to consider Popper’s examples, even if testable in his own peculiar sense, metaphysical (cf. Popper 1983, pp. 213 ff.). In his famous essay on the elimination of metaphysics, Carnap distinguished between *mythological* statements which, like statements of magic, are phrased by means of empirical predicates, and *metaphysical* ones, which are not (Carnap 1932, pp. 225 ff., 1963, pp. 875, 881). Popper’s conception of metaphysics – the unfalsifiable – is thus seen to be much broader than that of the positivists, and indeed than that of traditional metaphysicians.<sup>7</sup>

## Lakatos and the Duhem Problem

Imre Lakatos, although for some time a follower of Popper and a “sophisticated falsificationist”, advanced numerous arguments against Popper’s picture of science. Since his main topic was the demarcation problem, an important part of his work on empirical science deals in attacks on the falsifiability criterion. His downright rejection of it was expressed in the following terms: “Exactly the most admired scientific theories simply fail to forbid any observable state of affairs.” (1978a, p. 16).<sup>8</sup>

Lakatos famously went on to instance this claim by means of a “characteristic story” which, if appropriately interpreted, would render the distinction between Cartesian and Newtonian theories much more problematic than what d’Alembert had thought. This well-known example concerns the observed orbit of a planet which differs sensibly from the orbit predicted with the help of Newton’s theory. Scientists, however, do not take this result as a refutation of the theory, but explain it by postulating the existence of a previously unknown planet which is exerting on it its gravitational attraction, to which the perturbation is due. The mass and position of the postulated planet are calculated and new tests are performed. The planet is not detected, but this, again, is not taken as a refutation, either of Newton’s theory or of the hypothesis of the perturbing planet, but another hypothesis is conjured up in order to account for the successive failures. And so on... “This story – Lakatos contended – suggests that even a most respected theory, like Newton’s dynamics and theory of gravitation, may fail to forbid any observable state of affairs” (*op.cit.*, p. 17). It will be convenient to postpone our analysis of the example and pass on to Popper’s reaction to it.



As far as I am aware, Popper published four replies to Lakatos's criticisms: one in the Schilpp volume's "Replies to my Critics"; another in a footnote to his *Objective Knowledge*; a third one in his 1978 Introduction to *Die beiden Grundprobleme*; and another one in his 1982 Introduction to *Realism and the Aim of Science* (the first volume of Popper's *Postscript to Logik der Forschung*). The replies are not identical but they share many arguments. I will concentrate on the reply given in the Introduction to the *Postscript*, since it hasn't received too much attention (its main point has been reiterated in Popper's 1989 entry on "Falsifizierbarkeit, zwei Bedeutungen von" in Radnitzky and Seiffert).

The thrust of Popper's argument consists in distinguishing between two senses of "falsifiability". Falsifiability in the first sense means just the logical possibility of falsification: the existence of at least one potential falsifier. In this fundamental sense, "falsifiability is a purely logical affair" (Popper 1983, p. xx). But falsifiability might also mean the possibility of *conclusively* falsifying a theory. Popper explains the second sense in the following terms:

One can raise the question whether an actual falsification is ever so compelling that one *must* regard the theory in question as falsified (and thus as false). Is there not always a way out for one who wishes to save the theory in question? (*op. cit.*, p. xxi, Popper's emphasis).

Popper's answer to this last question, of course, is "yes". As he goes on to say, he had made it clear in *Die beiden Grundprobleme* and in *Logik der Forschung* that "every theoretical system can in various ways be protected from an empirical falsification", and that "it is always possible to find some way of evading falsification, for example by introducing *ad hoc* auxiliary hypotheses" (*ibid.*). Lakatos's criticisms, according to Popper, confuse the two senses of falsifiability just explained. By pointing to the difficulties and uncertainties of falsification, Lakatos has merely shown that falsifiability in the *second* sense cannot be had. But Popper never said that the process of falsification should be certain or easy; he only required falsifiability in the first, logical sense: possible test results which could *in principle* refute the theory. Yet, according to Popper, "an entire literature rests on the failure to observe this distinction" (*op. cit.*, p. xxii).

As is well known, Popper's historical claim is right: he had indeed insisted that even formally falsifiable theories can be made unfalsifiable due to systematic use of "conventionalist stratagems", that is to say, by means of an *unscientific* methodological policy of protecting the theory from any impending falsification.<sup>9</sup> This possibility, however, was overcome by a "methodological supplement" (Popper 1959, p. 54) to the purely formal falsifiability criterion, proposing that "all rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification" (*ibid.*). That is to say, although these evasions are "logically possible" (*op. cit.*, p. 42) the situation is remedied by an appeal to a critical procedure which limits the use of such stratagems.

This raises the further question whether these rules are adequate for doing empirical science. But there is a more elementary problem for Popper's explanation. Let **e** be any potential falsifier of **h** in Popper's sense. Then, by definition, **e** is incompatible with **h**. Can any *ad hoc* hypothesis save **h** from the falsifier **e**?

Certainly, no *additional* statement (in any monotonic logic) can eliminate the inconsistency between **h** and **e**; **e** being accepted, **h** must necessarily be rejected. The situation, rather, is that *ad hoc* hypotheses replace certain *auxiliary* hypotheses **A** which were essential for obtaining, together with **h**, a failed observable prediction:

$$\mathbf{h}, \mathbf{A} \vdash \neg \mathbf{e}$$

Replacing **A** is how **h** is “saved from falsification”. But notice that the acceptance of **e** is not a falsification, in Popper’s logical sense, of the hypothesis **h**, since **e** is incompatible only with the *conjunction* of **h** and **A**, and not with **h** itself. This is, of course, the main premise of the Duhem problem.

But from this it follows that Popper’s logical criterion of demarcation actually conflicts with the presupposition of his methodological supplement to it, since to “save a theory from falsification” by means of *ad hoc* hypotheses is in fact logically impossible, a falsification being, for Popper, the acceptance of a potential falsifier which is by definition inconsistent with the theory at hand.<sup>10</sup> An immunization from empirical criticism via *ad hoc* hypotheses is always possible only if the theory in question is not in logical conflict with this kind of criticism.

We therefore see that Lakatos’s point is at heart, *pace* Popper, a logical one. Some important scientific theories, he claims, are not falsifiable in Popper’s *first* sense; and this, in turn, implies crucial logical difficulties in the process of falsification, thus enhancing their unfalsifiability in the second sense.<sup>11</sup> Lakatos’s claim that this point generalizes to all important scientific theories may be overblown, especially since much background knowledge can simply go into the acceptance of a basic statement; but he certainly had a point in relation to Newtonian theory. Moreover, all of Popper’s examples (1974a, pp. 1006 ff.) of alleged potential falsifiers of Newton’s theory (square orbits, sudden changes in the motion of the Mars etc.) can conflict only with the conjunction of the theory with certain auxiliary hypotheses, a fact quite rightly pointed out by Putnam (1974, 1979). As Lakatos would dramatically state it, the examples show anomalies, and not Popperian refutations (Lakatos 1973).

Joseph Agassi, however, has expressed strong disagreement with Lakatos’s criticisms. In an essay on “Popper’s Popular Critics”, he writes that “those who say theories should be rescued from refutations thereby admit that they are refuted, so that there are refuted theories, so that there are refutable theories, and these surely are scientific – by Popper’s demarcation of science as refutability” (Agassi 1999, p. 9). But as our foregoing discussion suggests, if the statement which describes the refutation is a Popperian potential falsifier, the only “rescue” logically possible in this case is one which rejects the potential falsifier.<sup>12</sup> This excludes any procedure that retains and explains the problematic evidence while also retaining the tested theory, and in fact leads Agassi, I think, to misdiagnose these latter cases. In his interpretation of Popper, the reply to Duhem is to note that there *is* a logical conflict between theory and test statement, but that the decision concerning the latter involves the use of many other hypotheses which of course may later have to be revised (Agassi 1983, p. 22). His account of

the kind of Newtonian anomalies which inspired Lakatos's story is correspondingly as follows:

Some observations were made that were found to conflict with the theory; it was then declared that the facts are not as observed. This was explained, once as the result of optical effects that had been ignored (aberration), and once as a result of the oversight of a planet that interfered with the deviant planet: it was caused by an unknown planet. In both cases it turned out that once the damning evidence was corrected it was then no longer damning, no longer in conflict with the theory. (Agassi 1999, p. 7)

But here two quite different cases are given, misleadingly, an equal treatment. In one of them, the way out does involve an allegation that "the facts are not as observed", and the evidence is indeed corrected. But in the second case – of an interfering planet – the facts were still held to be as *observed*, but a new explanation compatible with the truth of Newton's theory – actually, on the basis of it – was given for them. This explanation involved the change of an assumption which was not employed in the acceptance of the observation report – namely, that the forces acting on the anomalous planet were just the gravitational forces resulting from the known celestial bodies of the solar system. In these Lakatosian examples, therefore, the hypotheses employed to decide on test statements are not the only problem, but the further dynamical assumptions necessary to obtain, alongside Newton's theory and the test statement reporting the anomaly, an inconsistent set of propositions.<sup>13</sup>

Thus, if Duhem and Lakatos are right, as they appear to be, some important scientific hypotheses cannot be said to have an empirical content of their own, in the sense of logically prohibiting some possible observational results: these cannot *entail* that the hypothesis is false. Therefore, if these results exhaust the ammunition of empirical criticism, we should conclude that the hypothesis in question does not live up to the possibility of being refuted by experience – precisely the requirement that we found so enticing in the beginning of our discussion. If refutability is to be retained as a condition which science can satisfy, the process of refutation by experience might have to be characterized in a more complex but perhaps epistemologically less enticing way.

## Methodological Falsifiability

As we suggested before, a possible answer to the Duhem problem (and, in general, to the logical unfalsifiability of a scientific theory) is to adopt a methodological policy of treating certain sets of experimental results *as* refutations, even if, logically speaking, there is still a chance that the theory under test is true. After all, this is what happens with statistical statements which, as Popper recognized in *Logik der Forschung*, are "impervious to strict falsification". How are these methodological falsifying instances to be conceived?

It is well known that Popper's main aim in devising an anti-conventionalist set of methodological rules was to avoid dogmatism. Popper explains this both in 1932 *Die beiden Grundprobleme* (Book II, Chap. iii) and in *Logik der Forschung*

(Chap. 2 and Sect. 20). It is not forbidden by *logic*, Popper says, that scientists decide to stick to a theory and neglect any kind of criticism that might be put against it. Therefore, we are in need of “methodological conventions or stipulations which ensure the possibility of refutation” (1998, Book II, Chap. iii, Sect. 8, p. 470), “forbidding certain procedures by means of which it is always possible to make a theory coincide with experience” (1998, Book II, Chap. iii, Sect. 3, p. 452). Perhaps the clearest statement of this point occurs in *Die beiden Grundprobleme*:

A theoretical system cannot be characterized as refutable in an absolute manner, for we can always find procedures which make any refutation impossible. But there are also procedures for achieving the opposite, that is, procedures which make a theoretical system refutable...[T]he demarcation criterion cannot, therefore, be a purely logical one, but a methodological one, which refers to the procedures to which the theory is submitted in science ... Which methodological conventions make of a theory a refutable theory? (1998, Book II, Chap. iii, Sect. 1, p. 447)

This is sometimes explained as the suggestion that we must not only have *in principle* refutable theories, but also “in practice”; it would seem as if the role of methodological falsifiability were to prevent spoiling through dogmatism the benefits inherent in logical falsifiability. Popper himself writes that “only in the light of such a critical attitude and the corresponding critical methodological approach do ‘refutable’ theories retain their refutability” (Popper 1968, p. 94).<sup>15</sup> But, as we have already seen, once we are dealing with a theory consistently defensible from empirical evidence, we must suppose that the theory is not logically falsifiable by such evidence; which puts us in the need – if falsifiability by experimental results is to be retained – of stipulating empirical conditions under which the theory will have to be rejected, perhaps by setting some kind of limit to the admissible modification of auxiliary hypotheses in the face of such results, or by banning outrageously regressive problemshifts. Although it may sound strange to speak of a “dogmatic defence” of a theory from that which it does not strictly exclude, *some* empirical limits to its acceptance – so this line of thought suggests – must be put.

But then we must very clearly stress the difference between the purely logical notion of falsifiability and this new, purely methodological notion. Given its peculiar characteristics, the logical falsifiability of a hypothesis is the possibility of its being proved *wrong* by experience; working with falsifiable hypotheses, in this sense, implies that certain results, if properly obtained, will reveal a theoretical mistake (so long as we use logic correctly). The methodological falsifiability of a hypothesis, on the other hand, is the feasibility of its being *given up* by researchers under certain empirical conditions (by application of methodological rules). This notion by itself does nothing to indicate what epistemological goal will be achieved, or what epistemological liability will be avoided, by the correct application of the rules to true experimental data. Of course, the rules might help to avoid the possible situation in which the hypothesis is false and forever retained, which is why the role of methodological falsifiability is easily confused with that of logical falsifiability. But the correct application of falsification rules to veridical results tells us something only about the rejection of the hypothesis; this might be a lucky event, but it also might not be. In fact, once we allow for a dose

of *Menschenwerk* in the specification of potential falsifiers, the epistemological point of the demarcation criterion begins to be blurred while the issue of combating dogmatism gains in importance. Unless we take the rules of rejection as simply specifying part of the meaning of the tested hypothesis, we must beware that these rules do not lead us too easily into classifying a true hypothesis as false.

In this respect, Popper's formulations are not always very happy ones, as they give the impression that there need be no constraints on the specification of potential refuting instances. For example, in a famous footnote to *Conjectures and Refutations*, he writes:

Criteria of refutation have to be laid down beforehand; and for this purpose it must be agreed which observable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refute to the satisfaction of the analyst not merely a particular analytic diagnosis, but psychoanalysis itself? (Popper 1963, p. 38, footnote 3)<sup>16</sup>

Lakatos concluded from such statements that "all that [Popper] wants is that the scientist should always specify for his theory, in an *ad hoc* way, at least one potential falsifier of his own choice" (1978b, pp. 213–214, footnote 4).<sup>17</sup> More charitable readings of these passages, however, can be attempted. One might suppose that the message Popper is trying to get across is that we must be clear as to *what theory we are really proposing*, and that a good way to become aware of this is by performing critical thought experiments. Decisions in this case might affect for good the empirical definiteness of the theory and its concepts: indeed, it is often a good procedure, in order to make clear both to ourselves and others what we are discussing, to think out under what conditions, if any, our theory could be recognized as false (consider, for example, the difficulties in specifying the exact content of Marx and Engels' historical materialism in *The German Ideology*, or Pasteur's theory of fermentation in relation to Berthelot's discoveries). This suggests a link between agreement on potential falsifying evidence for a hypothesis and its content, between testability and meaning. In this case, such agreement is a way of specifying the content of the hypothesis, and so a refuting instance is, epistemologically speaking, a perfect piece of negative evidence. On the other hand, uncritical defences will take the form of meaning shifts or content-reducing stratagems and should be *eo ipso* discouraged. As a way of dealing with the Duhem problem, however, this line of thought should not carry too much weight if it is to conform to another aspect of critical rationalism: its realistic bent. In the following section we will explore this conflict within Popper's philosophy.

## Testability and Meaning

Popper attempts some interesting defences of methodological falsifiability along the lines just mentioned in *Die beiden Grundprobleme* (1998, Book II, Chap. iii, Sect. 6 and Appendix vi; cf. also 1959, pp. 74–75), which linger in *Logik der Forschung*, Sect. 20 and also in later writings. One of the main arguments he there provides for the indispensability of a methodology and the insufficiency of purely

logical analyses of scientific systems, although it is admittedly very sketchy, is that the meanings of the terms of a theory are not completely determined by given definitions but depend on how the statements in which they appear are dealt with in scientific experience; a dogmatic methodology involves, according to this, a semantic choice of a weaker or vacuous hypothesis. We may call this Popper's *semantical argument* for falsificationism. Popper's clearest explanation of it is given in the following passage<sup>18</sup>:

[If] auxiliary hypotheses are introduced ad hoc, every theory can be immunized from a possible refutation, but in this way it would be transformed into a theory that does not assert anything; from an empirical point of view, it would become a tautological or metaphysical system...The correspondence with reality [of the terms of a scientific theory] is not given through each one of the fundamental terms separately, but only for the theory as a whole with all its concepts (precisely when it is determined under which circumstances the theory would be regarded as refuted). (1998, II, Appendix VI, p. 531, Popper's emphasis)

This conception suggests that it is through the rules and practice of refutation that theories are guaranteed their empirical cash values and thereby their falsifiability. Rather than on any independently given content, a theory's falsifiable and empirical status, as opposed to its conventional or analytical character, crucially depends on the way it is handled, on its being *used as* an empirical statement. As Popper puts it in *Logik der Forschung*: "Only with reference to the *methods* applied to a theoretical system is it at all possible to ask whether we are dealing with a conventionalist or an empirical *theory*" (1959, p. 82, second emphasis added). A theory which is inoculated against empirical criticism is therefore turned into a convention and loses its empirical character. We thus see that Popper in 1934 seems to be subscribing to a Quinean conception: the conventional or empirical character of a statement, he says, is given by our methodological behaviour in relation to it, in particular – to use Quine's locution – by our readiness to give it up before recalcitrant evidence. Popper himself, in a footnote to *Conjectures and Refutations*, claimed to have anticipated Quine's theses against the analytic/synthetic distinction pointing out that, according to his *Logik der Forschung*, the vacuous or factual status of a theory "exhibits itself" in the scientists' attitude in relation to negative evidence (Popper 1963, p. 74, footnote 14).

However, there are several problems for this apparently Quinean defence of falsificationism. First, we must recall that Quine intended to behaviouristically *reduce* the conventional or analytic character of a statement to its being protected from empirical falsification (analogously, he also reduced the possibility of its being *undermined* by the facts – its factual character – to that of its being *rejected* by scientists; since virtually anything, according to him, could be in principle retained or rejected given any amount of evidence, the dichotomy collapsed). Popper, on the other hand, seems to posit a real, semantic contrast between a hollow convention and a physical hypothesis, suggesting that a dogmatic methodology will result in our endorsing the first, while only a critical methodology will allow us to deal with the second. However, his argument does contain a disconcerting Quinean element, since scientific theories are there conceived as *formulae* whose semantical content can be decided *a posteriori* through scientists' propensities to endorse or



reject them in different evidential situations. This is a disconcerting feature if only because there does not seem to be much sense in a scientist's being dogmatic or critical except in relation to an already understood theory, as opposed to an uninterpreted sentence. Popper's proposal of a critical scientific *ethos* makes little sense in a setting of radical interpretation.

Moreover, even if this setting were appropriate for a semantical theory, we may still ask if a persistent immunization from empirical evidence (in the context of the Duhem problem) involves endorsing a tautological convention as opposed to an empirical hypothesis. And Popper, curiously enough, could have been the first one to realize that this need not be so, since he instructively replaced the analytic/empirical dichotomy by the analytic/*indeterminate*/empirical trichotomy: some synthetic statements, he said, may be too unspecific to conflict with observable states of affairs – to be empirical in Popper's sense, that is (cf. Sect. 3).<sup>19</sup> Given this, the fact that a sentence is not rejected despite challenging evidence does not even suggest that it is tautological or vacuous, but proves at most that it is not strictly empirical (i.e. not in direct logical conflict with observations); the further semantic choice between its being an analytic or a synthetic but firmly believed factual theory is, as Quine showed, unconstrained.<sup>20</sup> This is what Russell pointed out to Poincaré when discussing the Duhem problem in his review of *La Science et l'hypothèse*:

Thus a principle, it would seem, becomes conventional the moment we are less willing to abandon it than to seek a supplementary hypothesis to preserve it. It is possible, however – though Mr. Poincaré takes no account of the possibility – to believe that such a principle is strictly true, and any supplementary hypothesis which may be necessary to preserve it is thereby proved to be also true. (Russell 1966, p. 74)

Once we admit that there is no longer a direct logical contact of theory with observation (as Quine did), immunization from empirical evidence need not imply a semantic change in the assertive strength of our theory, since the evidence in question was not part of its information content to begin with (to think otherwise would be to absorb into it the empirical content it shares with its context). Popper may have missed this due precisely to his insistence on the logical falsifiability of immunized theories (cf. Section “Lakatos” and the Duhem Problem).

It was Popper himself, however, who through his objectivist theory of science challenged the presuppositions of the semantical argument we have been discussing. Just as Popper objected to the positivist criterion of meaning that it is “necessary to *understand* a theory in order to judge whether or not it could be verified” (Schilpp 1974, p. 63), essentially the same complaint could be put in this way: it is only after we have become acquainted with the factual content and empirical import of a scientific theory that we can know if it is falsifiable; any further methodological spur (or tardiness) will affect its handling and not the content by means of which the theory itself is identified. Adhering dogmatically to the theory, therefore, need not amount to exploit “the possibility of making laws such as Newton's true by convention, by means of adopting a methodological policy involving the use of immunizing stratagems” (Popper 1974a, p. 1187, Note 79a); methodological irrefragability need not be a criterion for semantic conventionality. Understandably,

Popper repudiated his early semantic assumptions in other places (cf. Popper 1983, p. 210), and it certainly seems in line with his philosophy to conceive scientific theories as systems which have a fixed content prior to any methodological practice or rule for their empirical evaluation.<sup>22</sup> It is precisely the possible falsity of such previously understood ideas, according to Popper, that we investigate by means of critical empirical tests.

If we allow at all for this realist interpretation of the meaning of theories, however, it follows that the famous Popperian paradox pointed out by Musgrave, namely that “Marxism was once refutable (and refuted) but has now become (through the use of conventionalist stratagems) irrefutable” (Musgrave 1968, p. 82) cannot be resolved, as Musgrave once attempted to do, by pointing to the methodological rider of the falsifiability criterion. The paradox can mean that some people use Marxian theory defensively, in such a way that it has become irrecusable *for them*. But this hardly bears on the theory’s objective vulnerability to disproof, nor need it impinge, as we have suggested, on its factual character (even as conceived by its adherents). Or else, the paradox might mean that some people have simply turned Marxian theory, through successive *semantical* stratagems – that is, sheer changes of meanings – into a weaker, perhaps tautological or definitional theory. But then Popper would seem to mistake Marx’s theory for its syntactic formulation, since what alleged adherents of Marxian theory will have done is to actually change it for a tautology with the same wording; true Marxism remains as refutable and refuted as it was before, and has not been “rescued” (cf. Popper 1963, p. 37).

The source of these methodological oddities, I think, is Popper’s not having fully assimilated the Duhemian insight, referred to above, that a theory like Newton’s, and what Poincaré called *principles* as opposed to hypotheses, cannot be said to have an *empirical* content of their own, even after they are agreed to be factual, synthetic statements; if Quine’s suggestion that “it is misleading to speak of the empirical content of an individual statement” (Quine 1953, p. 43) applies at all, it surely applies to them. We cannot therefore say of any such principle that it satisfies Popper’s requirement of empiricalness or falsifiability, i.e. that it “divides the class of all possible basic statements into (...) those with which it is inconsistent (or which it rules out, or prohibits) (...); and second, those basic statements which it does not contradict (or which it ‘permits’)” (Popper 1959, p. 86). It is always a context which includes it that may do so. When we systematically “defend” a physical principle from seemingly adverse evidence, therefore, we need not be draining its content – or deciding it – in any way. A realist should say that what the principle asserts is always the same, and that any stage of science in which it is preserved will simply describe a possible world in which certain specific physical conditions (described by our background assumptions) change, but not the fundamental facts expressed by the principle. Popper’s discussions of conventionalism, however, stated the demarcation problem as a choice between two exhaustive possibilities: either we decide for a system which experience can methodologically *refute*, or else we are dealing with a conventional system of implicit definitions: *tertium non datur* (cf. Popper 1959, pp. 72 ff., 1998, I, Chap. viii).<sup>23</sup>



Like Poincaré, Popper did not consider the possibility of conceiving the *isolated* system as an independent, synthetic but non-falsifiable assertion; and he did not appreciate that its immunizability via *ad hoc* hypotheses was itself a sign of its strict unfalsifiability. This is strange, since it was at about the same time that he hit upon the idea of there being confirmable but non-falsifiable hypotheses among synthetic statements.<sup>24</sup>

## Conclusion

If we set aside the conventionalist and positivist remnants of Popper's philosophy of science in *Logik der Forschung* in favour of his later realist and objectivist interpretation of scientific theories, we must sharply distinguish between the content of a theory and the methodological conditions for its falsification. A methodologically falsifiable theory, that is, will usually have a content which is not strong enough to rule out the experimental conditions which will lead to its rejection. It seems to follow that the falsifiability of a theory will reveal not its ability to clash with experience, but our ability to behave as if it clashed with it; susceptibility to removal might not be identical with susceptibility to be shown wrong. If so, the neat explanation of the criterion's rationale we attempted in the second section appears to be no longer available for those advocating a falsificationist philosophy. At any rate, the channel of *retransmission of falsity* (as Popper put it) from test statements to theories will not be an *a priori* logical process, and so falsificationist methodology, like inductivism, will have to rely on contingent assumptions.

I know of some available ways to answer this epistemological quandary. One is to forget about a principled justification of the tools of criticism and vindicate them by appeal to actual scientific practice. Popper called this a *transcendental method*; Donald Gillies appeals to it in his own proposal for falsifying probability statements (cf. Gillies 1993, pp. 208 ff.). Another is to offer such synthetic assumptions as ingredients of an explanation of the criterion's merit, yet without assuming that such an explanation provides any justification for it. This sceptical answer, espoused by David Miller, will invite us rather to say what we think is wrong with such assumptions. After all, justification is pointless even when tried on the *a priori* rules of deduction – and then even more blatantly so. A great deal has been written in the past decades on falsificationist rules of scientific practice, and this is not the place to begin a new analysis (but I do believe that the works of Duhem, Polanyi, Kuhn and others have shown that the Duhem problem cannot be solved by appeal to any specifiable *rules* enhancing empirical falsification). I may also say that I think Miller's epistemological approach to our quandary, to borrow Hume's phrase, "admits of no reply and produces no conviction". But as Miller would be quick to answer, "so much the worse for conviction".

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## Notes

1. However, it is well known that in his *Principia* Newton claimed to have refuted a version of Cartesian vortex theory by showing it to contradict Kepler's third law.
2. This particular example is also adduced in Stegmüller 1978, p. 403, yet as a criticism of Popper's criterion, which Stegmüller incidentally takes to be a *meaningfulness* criterion. Popper also exemplifies his point by means of a comparison between Goldbach's conjecture and the twin prime conjecture (1983, pp. 197 ff.). The former is falsifiable by bounded counterinstances while the latter isn't, yet both can be expressed as all-and-some statements and, as Popper shows, almost identically:

$$(x)(Ey)(\text{Prime}(x + y) \ \& \ \text{Prime}((2 + x) - y)) \text{ (Goldbach)}$$

$$(x)(Ey)(\text{Prime}(x + y) \ \& \ \text{Prime}((2 + x) + y)) \text{ (twin prime).}$$

Cf. also Popper's Note \*2 to Sect. 66 of Popper 1959.

3. That the object is other than *a* is stated in the formula but can go here without saying.
4. Cf. Russell 1940, Chap. 22. Russell there criticizes Carnap's 1936–1937s notion of empirical basis for not being as basic as it can be. Exactly the same kind of defence of Carnap against Russell's criticisms can be made in defence of Popper against Carnap.
5. Not that this is unimportant. As Popper explained, it shows why his falsifiability criterion should not be regarded as a criterion for meaningfulness.
6. Popper himself notices this difference in Popper 1963, pp. 278–279 (footnote 60).
7. If St. Augustine, in his *Confessions*, complained against those who would have God's essence captured by Aristotle's ten categories, he would be scandalized by the attempt of conjuring it by means of a Carnapian formula from "the language of science". The metaphysical predicates Carnap had in mind could not be defined using empirical terms: for example, God's omnipresence is usually understood not empirically but analogically by theologians.
8. We must note that this remark is directed against what Lakatos calls *dogmatic* falsificationism (a position he does not ascribe to Popper), and that the observable state of affairs referred to are (allegedly) "purely" observable. However, Lakatos also thinks that his remark is a problem which challenges "naïve" falsificationism, which allows for a much broader notion of observability. This brand of falsificationism solves the problem by means of methodological decisions which interpret certain *logically* permitted state of affairs as "forbidden" by the tested theory.
9. We should add, however, that one of Lakatos' points is that the described process does not amount to a conventionalist stratagem in *Logik der Forschung's* sense. In fact, if none of the successive predictions generated by the amendments were corroborated, we'd still have a "theoretically progressive problemshift", which is not *ad hoc* by *Logik der Forschung's* standards.
10. "A theory is refuted or falsified if...a "basic statement" incompatible with the theory happens to be true; which means that something happens which is excluded or forbidden by the theory". (Popper 1998, Introduction, p. 34); Cf. Popper 1959, p. 22, and Popper 1974, p. 987.
11. In fact, the moral Lakatos draws from his own story is stated in the following way: Such theories never alone contradict a basic statement: they contradict at most a conjunction of a basic statement describing a spatio-temporally singular event and a universal non existence

- statement saying that no other relevant cause is at work anywhere in the universe (Lakatos 1978, p. 17). The reason for the unfalsifiability of classical mechanics is not really disconnected from our discussion of existential hypotheses in the previous section. The first axiom of Newton's theory (law of inertia) has a prenex form  $(x)(E)y$ , where there is no limit upon confirming instances of the posited  $y$  (which is a force). The second axiom can also be interpreted as involving an unrestricted existential quantification.
12. "Now it is logic that says, contrary evidence is refuting, so that admitting it is also admitting the falsity of the theory it contradicts", Agassi, *op. cit.*, p. 16. Of course, as Agassi points out, there also exists the possibility of changing meanings, either of the theory's terms or of the evidence. But in either case, the *assertions* are not preserved, but surreptitiously replaced by different ones. Cf. Popper 1959, p. 81.
  13. Grelling wrote: "Even if we are not conventionalists, and even when we can eliminate the influence of mistakes in observations, we must acknowledge that the relation between experiment and theory is substantially more complex than in Popper representation of it." (1937, p. 139). Other scientific theories often mentioned as irrefutable by experimental findings are the principle of conservation of energy and Darwin's theory of natural selection.
  14. (1998, [Book] II, [Chap.] iii, [Sect.] 4, p. 455): "The logical kind of considerations constitute in a sense the *presuppositions* of the methodological demarcation; for even if it is true that it is impossible to determine whether statements are empirical without appeal to methodological conventions, it is possible to characterize as unempirical all those statements which do not entail observation statements". Cf. Popper 1959, p. 22. Similarly, Musgrave (1968, p. 79) claims that Popper's demarcation criterion contains *both* a logical and a methodological requirement, although he later admits that sometimes the logical requirement need not really be satisfied (p. 82).
  15. When explaining *Logik der Forschung's* response to conventionalist objections, Miller writes: "If we want science to progress...we must adopt methodological rules whose overriding objective is to encourage falsification of our theories and the elimination of the mistaken ones among them" (2006, p. 6). I suppose 'and' does here an exegetical job.
  16. Similarly: "we can demand that anyone who advocates the empirical-scientific character of a theory must be able to specify under which conditions he would *be prepared to regard it as falsified*" (1983, p. xxi, emphases added); "[Newton] made it clear what kind of 'observable states of affairs' would refute his theory to *his own satisfaction*. (...) Einstein repeatedly pointed out what kind of results *he would regard as crucial refutations* of his theories" Popper 1974, p. 1008, emphases added. Popper also writes that after Geiger and Marden's experiments with alpha particles were accepted, Rutherford and other physicists "were satisfied" that another theory was needed and thus proposed "that Thomson's theory *be regarded as falsified*"; yet in other cases "it took some time before refutation was accepted *as such*" (1983, p. xxx, emphases added).
  17. Lakatos refers to Popper's reply to him in footnote 5 to Chap. 2 of *Objective Knowledge*. This footnote is indeed quite confusing, since it seems to imply (i) that potential falsifiers of a theory are logically incompatible with it, (ii) that theories can be immunized against such falsifiers and (iii) that their status as falsifiers depend on the scientist's choice.
  18. Cf. Also 1998 II, iii, 6, p. 462: "If the conception we have just defended is correct, then it is impossible to determine the kind of use or application of statements by means of definitions of the terms which figure in them, for precisely what is understood by the meaning of a concept is determined by the application of the statements in which it appears. To analyse this application is the task of methodology".
  19. In *Conjectures and Refutations* (1963, *ibid.*) he actually corrects his 1934 proto-quinean argument against the objectivity of the "conventional or empirical" dichotomy, replacing "empirical" by "factual", and emphasizing the importance of there being factual statements between the analytic and the empirical. This amendment, however, spoils his case.
  20. Of course, Quine argued that, since the decision between the two semantical interpretations is not forced by the evidential facts, there is no fact of the matter as to which is correct. Russell surely would not have endorsed this view.

21. Popper also writes that “the capacity (...) almost every theory has of being immunized does not affect their falsifiability in the sense of the demarcation criterion: the existence of potential falsifiers” (1998, Introduction, p. 36).
22. Popper has even been criticized for having been immune to the Wittgensteinian insight that “speaking of propositions as true or false tells us nothing in abstraction from the ways in which we set about verifying or falsifying them” (O’Hear 1980, p. 207). Donald C. Williams had proposed such a realist interpretation of theories and criticized the positivist theory of meaning in a 1938 *Erkenntnis* paper: “Meaning is equally independent of verification and falsification, direct and indirect, strong and weak, complete and incomplete, actual and possible, by me or by other persons” (p. 175).
23. In (1998, II, ii, 3, p. 452), Popper also speaks of the assignment of an *empirical sense* to scientific systems (through methodological rules for falsification) as the only alternative to *a priori* true conventions. As we pointed out earlier, throughout *Die beiden Grundprobleme* (with the exception of II, i, 2) and also in his 1933 Note to *Erkenntnis*, Popper formulates demarcation as the question which statements *say something about reality*. Cf. 1998, I, xi, 46 and viii, 30. It is not improbable that he wrote this under the influence of Philipp Frank’s *Das Kausalgesetz und seine Grenzen* (referred to several times in *Logik der Forschung*) where Frank distinguishes between definitional or conventional laws and those which “say something about reality” (*Wirklichkeitssätze, über die wirkliche Welt*). Frank’s discussion of conventionalism (which also appears in his 1941) is also important in connection with the whole subject matter of this section.
24. In his analysis of geometrical conventionalism (1998, I, viii), Popper explains that the conventionalist can always explain away a problematic measurement by means of an “auxiliary hypothesis” stating that the proposed measuring rod has been deformed. By forbidding or limiting this kind of manoeuvre, the empiricist “stipulates certain methodological conditions that enable the geometry to conflict with experience” (*ibid.*). Two interpretations could be given of what is going on according to Popper: either that we are considering two factually definite sets of hypotheses (geometrical plus physical) describing different states of affairs which only jointly have empirical manifestations; or that we deal with a geometrical theory whose factual content can only be fixed once we decide on the kind of physical processes that can be postulated. It would seem that according to Popper the second is the correct interpretation (in these and many other passages he even equates assertions “referring to reality” with falsifiable assertions). By making the methodological difference a difference in the content of the geometrical theory, he seems to preclude a realist interpretation for it.

## References

- Agassi, Joseph. 1983. Theoretical Bias in Evidence: A Historical Sketch. *Philosophica*, 31(1), 7–24. Page numbers correspond to the slightly modified online version: <http://www.tau.ac.il/~agassi/joseph-papers/bias.pdf>
- Agassi, Joseph. 1999. Let a Thousand Flowers Bloom: Popper’s Popular Critics. *Anuar*, 7, 5–25. Page numbers correspond to the online version: <http://www.tau.ac.il/~agassi/joseph-papers/flowers.pdf>
- Carnap, Rudolf. 1932. Überwindung der Metaphysik durch logische Analyse der Sprache. *Erkenntnis*, 2(4), 219–241.
- Carnap, Rudolf. 1936–1937. Testability and Meaning. *Philosophy of Science*, 3, 4.
- Carnap, Rudolf. 1963. Replies and Systematic Expositions. In (Schilpp 1963, pp. 859–1013).
- d’Alembert, Jean Le Rond. 1770. *Mélanges de littérature, d’histoire, et de philosophie*, Amsterdam: Zacharie Chatelain & Fils.
- Frank, Philipp. 1941. *Between Physics and Philosophy*. Cambridge, MA: Harvard University Press.
- Frank, Philipp. 1988. *Das Kausalgesetz und seine Grenzen*, Frankfurt am Main: Suhrkamp (First edition 1932).

- Gillies, Donald. 1993. *Philosophy of Science in the Twentieth Century. Four Central Themes*. Oxford: Blackwell.
- Grelling, Kurt. 1937. Karl Popper. *Logik der Forschung. Theoria*, 3, 134–139.
- Hempel, Carl Gustav. 1945. Studies in the Logic of Confirmation. *Mind*, 54, 1–26, 97–121.
- Hempel, Carl Gustav. 1965. *Aspects of Scientific Explanation*. New York: Free Press.
- Kneale, William. 1974. The Demarcation of Science. In (Schilpp 1974, pp. 205–217).
- Lakatos, Imre. 1973. *Science and Pseudoscience*. Radio Lecture, Open University. Reprinted with changes as Introduction to Lakatos 1978a.
- Lakatos, Imre. 1978a. *Philosophical Papers. Volume 1, The Methodology of Scientific Research Programmes*, eds. John Worrall and Gregory Currie. Cambridge: Cambridge University Press.
- Lakatos, Imre. 1978b. *Philosophical Papers*. In *Mathematics, Science and Epistemology*, eds. John Worrall and Gregory Currie. Cambridge: Cambridge University Press.
- Lakatos, Imre and Musgrave, Alan. (eds.). 1968. *Problems in the Philosophy of Science*. Amsterdam: North Holland.
- Maxwell, Grover. 1974. Corroboration Without Demarcation. In (Schilpp 1974, pp. 292–321).
- Miller, David. 1994. *Critical Rationalism. A Restatement and Defence*. La Salle, IL: Open Court.
- Miller, David. 2006. *Out of Error. Further Essays on Critical Rationalism*. Aldershot: Ashgate.
- Musgrave, Alan. 1968. On a Demarcation Dispute. In (Lakatos and Musgrave 1968, pp. 78–88).
- O'Hear, Anthony. 1980. *Karl Popper*. London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1959. *The Logic of Scientific Discovery*. London: Hutchinson.
- Popper, Karl Raimund. 1963. *Conjectures and Refutations*. London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1968. Remarks on the Problem of Demarcation and of Rationality. In (Lakatos and Musgrave 1968, pp. 88–102).
- Popper, Karl Raimund. 1974a. Replies to My Critics. In Schilpp (1974, pp. 961–1197).
- Popper, Karl Raimund. 1974b. *Conocimiento Objetivo*. Madrid: Tecnos.
- Popper, Karl Raimund. 1983. *Realism and the Aim of Science*. London: Routledge.
- Popper, Karl. Raimund. 1998. *Los dos problemas fundamentales de la teoría del conocimiento*. Madrid: Tecnos. (Spanish translation of *Die beiden Grundprobleme der Erkenntnistheorie*. Tübingen: J.C.B. Mohr.)
- Putnam, Hilary. 1974. The Corroboration of Theories. In (Schilpp 1974).
- Putnam, Hilary. 1979. Retrospective Note (1978): A Critic Replies to His Philosopher. In *Philosophy as It Is*, Ted Honderich and Myles Burnyeat (eds.), pp. 377–380. Harmondsworth, England: Penguin.
- Quine, Willard. 1953. *From a Logical Point of View*. Cambridge, MA: Harvard University Press.
- Radnitzky, Gerhard and Seiffert, Helmut. 1989. *Handlexikon zur Wissenschaftstheorie*. München: Ehrenwirth Verlag.
- Russell, Bertrand Arthur William. 1940. *An Inquiry into Meaning and Truth*. London: Allen & Unwin.
- Russell, Bertrand Arthur William. 1966. *Philosophical Essays*, London, Allen & Unwin.
- Schilpp, Paul Arthur (ed.). 1963. *The Philosophy of Rudolf Carnap*. La Salle, IL: Open Court.
- (ed.). 1974. *The Philosophy of Karl Popper*. La Salle, IL: Open Court.
- Stegmüller, Wolfgang. 1978. *Hauptströmungen der Gegenwartsphilosophie*. Stuttgart: Alfred Kröner Verlag (sixth edition).
- Williams, Donald. 1938. The Realistic Interpretation of Scientific Sentences. *Erkenntnis*, 7(3), 169–178, University of Buenos Aires

# Popper's Thesis of the Unity of Scientific Method: Method Versus Techniques

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**Abstract** Recently some philosophers have claimed that Popper's conception of the unity of method in science presents some serious problems. According to these philosophers, there is a clear divergence in his defense for the unity of scientific methodology and his thesis that the method of situational analysis or the logic of the situation constitutes "the proper method of the social sciences". I will show that these criticisms can be met by using certain distinctions that have been fashioned in contemporary philosophy of science, among others, the distinction between scientific method and scientific techniques and also between the context of discovery and the context of validation.

The structure of this work is the result of following Popper's advice that in science and philosophy we have to formulate a problem and then propose a solution which should be subject to critical discussion. I have also had in mind a remark made by F. Eidlin in his article entitled "Matching Popperian theory to practice" in which he declares: "Debates and discussions in a genuine Popperian spirit are very rare. So is research conducted in a genuinely Popperian spirit" (Eidlin 2003, p. 206). Thus, after a brief introduction, I shall present the problem that I want to deal with, then I shall formulate a tentative solution and, finally, I shall ask you to criticize it.

In the Preface to the first English edition (1959) of Popper's seminal work *Logik der Forschung* (1934) we find his thesis of the unity of method of all rational discussion, i.e. of the natural sciences as well as philosophy. According to Popper it consists of "stating one's problem clearly and of examining its various proposed solutions critically" (Popper 1959, p. 16). He also stressed that by the terms "rational discussion" and "critically" he meant the following attitude: that whenever we propose a solution to a given problem, instead of defending it "we ought to try as hard as we can to overthrow our solution" (Popper 1959, p. 16).

In one of his later works Popper formulated another methodological unity thesis: the unity of method of all empirical sciences. In this new proposal, he emphasized once again the paramount importance of criticism.

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This new thesis is contained in Section 29 of *The Poverty of Historicism* “In this section I am going to propose a doctrine of the unity of method; that is to say, the view that all theoretical or generalizing sciences make use of the same method, whether they are natural or social sciences” (Popper 1957, p. 130). This doctrine is found again in *The Open Society and Its Enemies*. Here, Popper remarked that the social sciences have to face their problems “with the help of the theoretical methods which are fundamentally the same in all science” (Popper 1966, p. 222).

For the purpose of our analysis, we need to remember Popper’s view about scientific method. Actually, we can find different formulations of it. This is one of them:

My view of the method of science is, very simply, that it systematizes the pre-scientific method of learning from our mistakes. It does so by the device called *critical discussion*.

My whole view of scientific method may be summed up by saying that it consists of these four steps:

We select *some problem* perhaps by stumbling over it.

We try to *solve* it by proposing a theory as a tentative solution.

Through the *critical discussion of our theories* our knowledge grows by the elimination of some of our errors, and in this way we learn to understand our problems, and our theories, and the need for new solutions.

The critical discussion of even our best theories always reveals new problems.

Or to put these four steps into four words: *problems – theories – criticisms – new problems*.  
(Popper 1994, p. 158)

Shorter versions of his conception about scientific method assert that it consists of bold conjectures and ingenious and severe attempts to refute them or that this method is simply a criticism, i.e. attempted falsifications.

## The Problem

In two recent books, *Karl Popper and the Social Sciences* (2006) and *Karl Popper. Critical Appraisals* (2004), it has been claimed that Popper’s thesis about the unity of method of all empirical sciences faces serious problems.

Thus, William Gorton writes:

Especially in his earlier writings, Popper argued that hypotheses testing and the search for general laws should also be the goal of the social sciences (PH, 61 – 62). However, despite Popper’s strong support for the unity of scientific method, he also recommended a unique approach for studying the social world – a method that, he admitted, has almost no direct parallel in the natural sciences and that represented “perhaps the most important difference” between the natural and social sciences (PH, 141; see also UQ, 117). That method is, of course, situational analysis.

(Gorton 2006, p. 5)

On the other hand, Wesclesao Gonzalez makes a similar criticism:

To accept the existence of that dualistic wider framework in Popper’s methodological approach – falsificationism versus the logic of situation – is very damaging for his conception



of the unity of method in science. In effect, accepting such a divergence between the falsificationism and the logic of situation, it seems rather obvious that *de facto* natural sciences and social sciences will adopt different methodologies.

(Gonzalez 2004, pp. 79–80)

As we can see both authors are formulating the following claim:

Popper's acceptance of the method of situational analysis is incompatible with, or at least very damaging to, his thesis of the unity of scientific method.

In order to understand and evaluate this claim, we need to describe briefly the main elements of the so-called "situational analysis of a social situation", the "logic of a social situation" or, more briefly, "situational logic". As an example of this type of analysis, Popper invites us to consider an individual Richard who wants to catch a train and has to cross a road crowded with some moving and parked vehicles. The problem is to explain Richard's behaviour in crossing the road.

In order to explain Richard's behaviour, Popper says that we have to take the following elements into consideration:

Thus the situational analysis will comprise some physical things and some of their properties and states, some social institutions and some of their properties, some aims, and some elements of knowledge. Given this analysis of the social situation, we must be able to explain or to predict, Richard's movements as he crosses the street.

(Popper 1994, p. 168)

Actually, any situational analysis is a model representing a typical case:

[...] our method of situational analysis always turns Richard into "anybody" who may share the relevant situation, and reduces his personal living aims and his personal knowledge to elements of a *typical situational model*, capable of "explaining in principle" (to use Hayek's term) a vast class of structurally similar events.

(Popper 1994, p. 168)

Popper states that our situational models, like models in the natural sciences have to be animated; this animation is achieved by the use of the so-called "rationality principle", the assumption "that the various persons or agents involved act *adequately*, or *appropriately* – that is to say, in accordance with the situation" (Popper 1994, p. 169).

According to Popper, in the case of the social sciences, the different models and situational analyses are our empirical explanatory theories or hypotheses and they can be criticized and tested even though these tests are rather difficult to administer because, as also happens in natural sciences, any model is a rough and schematic over-simplification. Anyway, in spite of the comparatively low degree of testability, we can decide sometimes which of the competing models is better.

Although, Popper accepted that situational models present very deep problems in relation to his doctrine that science searches for truth, he declared that he "[...] wish[es] nevertheless to uphold the method of situational analysis as *the* proper method of the social sciences" (Popper 1994, p. 173).

This quotation seems to show that to the extent that the natural sciences, especially the physical sciences do not utilize the method of situational analysis for explaining or predicting, Popper cannot defend, at the same time, his doctrine of the unity of method of all empirical sciences. But, then, how should we understand



what Popper called “the main thesis” of his essay “The Logic of the Social Sciences”, according to which “The method of the social sciences, like that of the natural sciences, consists in trying out tentative solutions to those problems from which our investigations start”? (Popper 1992, p. 66).

Is Popper guilty of contradicting himself?

## The Tentative Solution

I think that a solution to our problem can be found in a distinction made in contemporary philosophy of science which has proved to be crucial in any discussion whether the same scientific method is applicable in the study or investigation of social as well as of non-social phenomena. Following Richard Rudner, I refer here to the distinction between scientific method and scientific techniques. About this distinction Rudner says:

The claim has often been made in the past that the social sciences are radically different from other sciences because their pursuit requires a “methodology” radically different from that required in the pursuit of other sciences. Too frequently, makers of this claim have done so out of a confusion between methodology and technique – a confusion that has vitiated either the significance or the tenability of their claim.

(Rudner 1966, pp. 4–5)

According to Rudner, the method of science is the rationale on which a scientific discipline bases its acceptance or rejection of hypotheses or theories. Thus, problems of methodology are those related to the logic of scientific inquiry. On the other hand, scientific techniques are any procedures, including instruments or devices, that are used to make observations, experiments, or experimental controls. Examples of scientific techniques are methods of measurement, statistical methods, instruments of observation such as the telescopes and the microscopes. These techniques of investigation are very important in the process of testing and formulation of new theories. As we can see, scientific techniques of investigation are procedures or, ways of doing things, and in this sense we can talk about them as “methods”. In other words, the term “method” is used to refer both to general and to more specific (particular) procedures or ways of doing things.

I want to claim that Gorton and Gonzalez’s criticism of Popper’s thesis of the unity of scientific method is based mainly on a failure to recognize the basic and necessary distinction made by Rudner. Or, in other words, they do not realize that it is absolutely necessary to be aware that the term “method” can be applied both to a very general procedure, to a general way of undertaking or conducting a scientific inquiry i.e. a general logic of scientific inquiry the so-called scientific method and more particular procedures of investigation, sometimes also called “scientific methods” instead of scientific techniques.

I do believe that Rudner’s distinction provides a solution to the apparent divergence, tension, or worse, contradiction, between Popper’s doctrine of the unity

of method of all empirical science and his tenet that the method of situational analysis is the proper method of the social sciences. Let us see how this is possible.

According to Popper, models are attempts to solve problems of explanation. In the case of social sciences, they are products of the method of constructing typical situations that is, the method of constructing models. The different models and situational analyses are explanatory theories or hypotheses. In this sense, the so-called method of situational analysis can be considered as a method, a particular procedure or a scientific technique for framing and formulating hypothesis or theories.

From this point of view the method of situational analysis is a part of the second of the four steps that Popper claims characterize the scientific method, i.e. that step in which social or natural scientists can use different procedures, techniques or particular methods for proposing hypotheses or theories as tentative solutions to their problems, including explanations of natural or social phenomena.

As we can see, Popper's assertion that the method of situational analysis is the proper method of the social sciences amounts only to the thesis that it is the best procedure or particular method for explaining and predicting certain kind or type of social events. This is because, according to him, we never have sufficient laws and initial conditions at our disposal to explain or understand a social event with their help. But, after we come up with an explanation, we have to go through the third step of scientific method, i.e. the critical discussion and test of this tentative solution.

Nothing in this analysis shows any contradiction in asserting as Popper does that there is only one method, the scientific method, which is a general procedure that is pervasive through all the sciences, and is applicable to the investigation of social as well as non-social phenomena and, on the other hand, that there are particular procedures or less general methods for framing explanatory theories that are testable and subject to criticism, again by scientific techniques, procedures, and methods.

Finally, I think that this distinction between a general method, scientific method, and more particular methods (or scientific techniques) helps us to understand the following two passages from Section 29 of *The Poverty of Historicism*, which really sound controversial and contradictory due to the ambiguous use by Popper of the term "method". He starts saying:

In this section I am going to propose a doctrine of the unity of method; that is to say, the view that all theoretical or generalizing sciences make use of the same method, whether they are natural sciences or social sciences ... I do not intend to assert that there are no differences whatever between the methods of the theoretical sciences of nature and of society; such differences clearly exist, even between the various natural sciences themselves, as well as between the various social sciences. (Compare, for example, the analysis of competitive markets and of Romance languages.) (Popper 1957, p. 130)

And finish with:

In concluding this section, I have to mention what I consider to be the other main difference between the methods of some of the theoretical sciences of nature and of society. I mean the specific difficulties connected with the application of quantitative methods, and especially methods of measurement. Some of these difficulties can be, and have been, overcome by the application of statistical methods, for example in demand analysis. (Popper 1957, p. 142)

A careful attention to the use of the term “method” shows that at the beginning of this quotation, Popper is talking about scientific method as a general procedure applicable to the study or investigation of social as well as natural phenomena, consisting in the four steps itemized above as *problems – theories – criticism – new problems*. On the other hand, when he admits that there are differences between the methods of the theoretical sciences of nature and of society, or even the natural sciences themselves, he is using the term “method” to refer to particular or specific procedures, best described, following Rudner, as scientific techniques.

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## References

- Eidlin, Fred. 2003. Matching Popperian theory to practice. In *Popper's Open Society After Fifty Years*, eds. Ian Jarvie and Sandra Pralong, 203–207. London: Routledge.
- Gonzalez, Wenceslao. 2004. The many faces of Popper's methodological approach to prediction. In *Karl Popper. Critical Appraisals*, eds. Philip Catton and Graham Macdonald, 78–98. London: Routledge.
- Gorton, William. 2006. *Karl Popper and the Social Sciences*. Albany, NY: State University of New York Press.
- Popper, Karl Raimund. 1957. *The Poverty of Historicism*. London: Routledge.
- Popper, Karl Raimund. 1959. *The Logic of Scientific Discovery*. London: Hutchinson.
- Popper, Karl Raimund. 1966. *The Open Society and Its Enemies*. London: Routledge.
- Popper, Karl Raimund. 1992. *In Search of a Better World*. London: Routledge.
- Popper, Karl Raimund. 1994. *The Myth of the Framework*, ed. Mark Notturmo. London: Routledge.
- Rudner, Richard. 1966. *Philosophy of Social Science*. Englewood Cliffs, NJ: Prentice-Hall.

# Popper's Analysis of the Problems of Induction and Demarcation and Mises' Justification of the Theoretical Social Sciences<sup>1</sup>

Natsuka Tokumaru

*The human search for knowledge must always encounter something that it cannot trace back to something else of which it would appear as the necessary effect. There is always in science some ultimate given.*

L. v. Mises (Mises 1962, p. 23)

*There are no ultimate sources of knowledge. Every source, every suggestion, is welcome; but every source, every suggestion, is also open to critical examination. ... we usually examine the asserted facts themselves, rather than investigate the sources of our information.*

K. Popper (Popper 1996, p. 49)

**Abstract** In his *Die beiden Grundprobleme der Erkenntnistheorie* Popper explains that different epistemological positions can be regarded as attempts to solve the problems of induction and demarcation. Inspired by Popper's approach, I consider Ludwig von Mises' epistemological position as an endeavor to resolve those problems with respect to the special situation of the social sciences. Mises states that the theoretical social sciences can be justified only as an a priori discipline, which he calls 'Praxeology'. In his view, all statements of praxeology presuppose an a priori valid 'category of human action'. Given his argument, I suggest that the 'category of action' may be interpreted in four different ways; as (1) an observational statement or a statement describing experiences from introspection; (2) a proposition about the basic ontological form of the social universe, describing its essential characteristics; (3) a definition, adopted as a convention; (4) a methodological principle, of the kind required by methodological individualism. However, under no interpretation can Mises justify the social sciences as a priori disciplines.

According to Popper, the problems of induction and demarcation are the two fundamental problems of epistemology. It is well known that he analyzes these

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two problems in his famous *Logik der Forschung* (*The Logic of Scientific Discovery*) (Popper 1959), which appeared in 1934/1935. However, as Popper explains in his autobiography (Popper 1974, 1976), *The Logic of Scientific Discovery* originated from a much longer book, *Die beiden Grundprobleme der Erkenntnistheorie* (*The Two Fundamental Problems of the Theory of Knowledge*) (Popper 1979), which he started to write around 1929 and completed in 1932. This book was not published until 1979 after having been edited by Troels Eggers Hansen, and originally seems to have consisted of two volumes; the first one dealing with the problem of induction and the second one dealing with the problem of demarcation.<sup>2</sup>

In *Die beiden Grundprobleme der Erkenntnistheorie* (GE), Popper derives different epistemological positions from premises that describe the problem situation triggered by Hume's logical objection against inductive inference. According to Popper, different epistemological positions such as naïve inductivism, strict positivism and apriorism, can be regarded as attempts to solve the problems of induction and demarcation (cf. Popper 1979). Inspired by Popper's approach, I shall try to interpret Mises' epistemological and methodological positions as an endeavor to solve the problems of induction and demarcation with respect to the special epistemological situation of the social sciences.<sup>3</sup>

In the preface of the German edition of his *Grundprobleme der Nationalökonomie* (*Epistemological Problems of Economics*) (Mises 1933), Mises remarked that the main task of the book is to provide an epistemological justification of economics that is not premised on the traditional link between the epistemological position of empiricism and its technical counterpart, the method of induction.

What is denied is the possibility of deriving *a posteriori* from historical experience empirical laws of history in general, or of economic history in particular, or 'laws' of 'economic action' within a definite historical period. (Mises 1933 preface to the German edition of 1933, p. xiv)

In *Human Action* (Mises 1949) as well as *The Ultimate Foundations of Economic Science* (Mises 1962), Mises also tried to establish his view of the fundamental epistemological principles of the comprehensive theoretical social sciences, in particular of economics. According to Mises, the theoretical social sciences are not physical, psychological or mathematical, but a *sui generis* discipline, which he collectively calls 'praxeology'. According to him, all propositions of praxeology can be a priori derived from an 'ultimate category of action', which asserts that man acts rationally to achieve ends. He states:

Praxeology is a priori. All its theorems are products of deductive reasoning that starts from the category of action. ... What praxeology asserts with regard to human action in general is strictly valid without any exception for every action. (Mises 1962, p. 44)

In order to justify praxeology as an a priori valid discipline, Mises develops an epistemological position that some authors interpreted as apriorism, whereas other authors interpreted it as a 'dogmatic' rationalism which cannot be defended as it stands (Blaug 1992, p. 81). Even Hayek, one of his most famous early colleagues, remarked that Mises's endeavor to derive the propositions of the social sciences from one ultimate category only was too radical (Hayek 1994, p. 72).<sup>4</sup> In the meanwhile, Mises' idea of praxeology has been defended by some authors, especially by

the representatives of the modern Austrian school of economics (Rothbard 1976; Hoppe 1988; Leeson and Boettke 2006), and his apriorism seems to have exerted an influence on them. However, it seems to me that it is quite unclear what kind of apriorism Mises defends, and on what grounds. Although at times he seemingly applies Kantian terms, his epistemological concepts and arguments nevertheless lack clarity and as a consequence, his aprioristic position is also defective.

Mises claims that all statements of praxeology start with the 'category of action', which states 'all humans act to achieve aims', that is, it asserts something about the world and may be regarded as a synthetic statement. But, if the statements of praxeology are synthetic and a priori valid, the question arises, how does he justify this claim? Are his arguments consistent or do they have to be rejected on methodological, logical and epistemological grounds? These are the questions I am going to clarify in this chapter. In order to answer these questions, I shall firstly analyze Mises' position of the epistemological status of the natural sciences; that is, his treatment of the problems of induction and demarcation. After having discussed Mises' position on the natural sciences, I shall review his epistemological arguments with respect to the social sciences.

According to Popper, empirical science is characterized by two requirements: strict universality and empiricism. The first requirement asserts that the statements and theories of science make claims that are strictly universal, that is, independent of time and location. The requirement of empiricism means that all empirical statements are decided by experience. But the two requirements seem to conflict. How is it possible that strictly universal statements that transcend experience are decided by experience? The method of induction, according to which content-enlarging and truth-preserving inferences are valid seems to be able to solve that problem: strictly universal statements are verified by singular statements. However, Hume argued that inductive inferences are invalid; therefore, the two requirements defining empirical science conflict, and empirical science cannot be demarcated in this manner from non-empirical science (Popper 1979, pp. 3–5).

## Mises' Epistemological and Methodological Views of the Natural Sciences

With respect to the natural sciences, Mises shares the view that empirical science is defined by strict universality and empiricism. Mises also believes that the method of induction characterizes natural science. According to Mises, inductive inferences are applicable to the natural sciences because of the ontological form or structure of nature, that is, *regularity*. He states:

All human knowledge concerning the universe presupposes and rests upon the cognition of the regularity in the succession and concatenation of observable events. ... Inductive inference is conclusion from premises that invariably include the fundamental proposition of regularity. (Mises 1962, pp. 21–22)

However, Mises is aware of Hume's argument (Mises 1957, p. 9) and introduces a kind of *induction principle* in order to justify inductive inferences. He states,

No scientist doubts that what is *correctly* observed in one case must also be observed in all other cases offering the same conditions. ... The conclusions derived from experimentation are not based upon the repetition of the same arrangement, but upon the assumption that what happened in one case must necessarily also happen in all other cases of the same type. (Mises 1962, p. 22)

According to Mises, this induction principle is a precondition for all inductive inferences and entitles one to draw valid content-enlarging and truth-preserving inferences. According to him, experiments provide not only singular observational statements but more importantly, repeatable observations (Mises 1957, p. 210). In his view, the possibility of making repeatable observations is essential in order to uncover regularities, that is, strictly universal statements.<sup>5</sup> Thus, Mises believes that the method of induction is applicable in the natural sciences only because the natural universe shows regularity or uniformity as its basic ontological form or structure.

However, even if Mises inserts a 'principle of induction', that is, "what is *correctly* observed in one case must also be observed in all other cases offering the same conditions" (Mises *ibidem*), the question of its justification remains. In order to justify an inductive inference, Mises needs to justify this fundamental proposition of regularity in an epistemologically proper way. And since the conclusion of an inductive inference is a strictly universal and empirical statement, the 'principle of induction' itself needs to have similar characteristics; that is, it needs to be synthetic (if not empirical), strictly universal, and proven true. One may try to add a meta-principle that establishes the principle of induction. Yet if one introduces a second-order principle of this kind, the question of its justification arises again and the whole procedure triggers an infinite regress of induction principles of higher orders.

It is interesting to note that Mises makes no further attempt to justify his induction principle. "There is no deductive demonstration possible of the principle of causality and of the ampliative inference of imperfect induction; there is only recourse to the no less indemonstrable statement that there is a strict regularity in the conjunction of all natural phenomena" (Mises 1957, p. 9). Obviously he takes its validity for granted. But the simple introduction of an induction principle does not solve the problem of induction. The main point here is that one cannot solve the logical and epistemological problems of inductive inference by referring to a structural characteristic of the universe. The conclusion of my analysis here is that, with respect to the natural sciences, Mises defends a position Popper called *naïve inductivism* (Popper 1979, pp. 42–43).

According to Mises, inductive methods are applicable in the natural sciences because of the characteristic structure of the natural universe, that is, regularity. In contrast he emphasizes that social regularities, although strictly universal, are totally different from those that exist in the natural sciences. These ontological considerations form the basis for his methodological reasoning; whereas he defends a position of naïve inductivism in the natural sciences, which is based on the traditional link of the epistemological position of empiricism and the logical technique of inductivism, he claims that the theoretical social sciences can be justified only as an *a priori* discipline because of the special characteristic of the social universe.



## What Is a Priori?

With respect to the term 'a priori', Popper distinguishes 'the psychological a priori' from 'a priori validity'. The 'psychological a priori' refers to our psychological or genetic endowment, which enables us to perceive the world we live in. This may be interpreted as a set of expectations that of course may be disappointed and are therefore not a priori valid. For instance, babies are born with expectations to be loved, yet, a baby may be wrong and when wrong commits a deadly error. Despite the phenomenon of imprinting in some species, however, the human genetic and 'psychological a priori' shows plasticity and may change. In contrast to the genetic or 'psychological a priori', the concept of 'a priori validity' refers to the foundation of the truth-value of statements (Popper 1979, pp. 81–136).

In order to establish the social sciences as a priori disciplines, Mises sometimes seems to use psychological (or genetic) discussions that refer to some kind of human endowment that makes experience possible. He says that humans are endowed with a certain mental, or genetic a priori, which he calls 'categories', that makes experience possible, and only those species have survived, whose categorical endowment – or psychological a priori – has produced a correct model of the world. He states: "Only those groups could survive whose members acted in accordance with the right categories, that is, with those that were in conformity with reality and therefore – to use the concept of pragmatism – worked". (Mises 1962, p. 15) However, Mises states that theories of praxeology are a priori valid, that is, "...perfectly certain and incontestable, like the correct mathematical theorems" (Mises 1949, p. 39) and should be distinguished from psychology (Mises 1957, pp. 264–282). When he states that praxeology is a priori discipline, he meant 'a priori valid', not 'psychologically a priori'.

## Mises' Epistemological and Methodological Position in the Theoretical Social Sciences

In the light of the absolute distinction between 'a priori validity' and 'psychological a priori', I shall examine whether Mises could have justified praxeology as 'a priori valid' discipline. As already mentioned, Mises claims that all propositions in praxeology can be derived a priori from an 'ultimate category', that is, human action. Given his arguments, I would like to suggest that the category of action can be interpreted in at least four different ways, which are all supported to some extent by his writings:

1. The 'category of action' is an observational statement or a statement describing experiences from introspection.
2. The 'category of action' is a proposition about the basic ontological form of the social universe, describing its essential characteristics.
3. The 'category of action' is a definition, adopted as a convention.
4. The 'category of action' is a methodological principle, of the kind required by methodological individualism.

With respect to the first and second interpretations, Mises would have to show that the ‘category of action’ is a synthetic statement a priori, since he characterizes the theoretical social sciences as a priori valid disciplines. But on the basis of Mises’ arguments the ‘category of human action’ cannot be justified as synthetic statement a priori. However, since analyticity implies aprioricity, Mises’ category of action can be interpreted also as a definition of human social behavior. Consequently, one may regard Mises’ position as a kind of conventionalism. Although logically correct, this position not only conflicts with Mises’ original intentions; it has to be rejected on epistemological grounds as well. But Mises’ ‘category of action’ can also be regarded as a methodological principle required by methodological individualism. Yet, methodological principles have pragmatic values only and cannot establish the social sciences as a priori disciplines. The situation will become clearer, if I discuss these four possible interpretations in more detail.

### **Mises’ ‘Category of Action’ Is an Observational Statement or Is a Statement Describing Experiences from Introspection**

According to Mises, observation and introspection suggest that individuals consciously evaluate situations and choose between different aims. From this observation, Mises might have drawn the conclusion that singular observational statements constitute an empirical basis from which social regularities are induced by generalization; and, from this consideration, he might have formed the view that the social sciences start with singular statements that describe the results of observation and introspection. Mises actually states:

The starting point of praxeology is a self-evident truth, the cognition of action, that is, the cognition of the fact that there is such a thing as consciously aiming at ends (Mises 1962, pp. 5–6).

Such an inductive inference may read as follows: from singular and proven true statements describing introspection or personal experiences such as ‘I act’, the strictly universal statement ‘all humans act’ is inferred. In order to establish such an inductive inference as valid, Mises might have introduced a principle of induction like the one he relied on in his discussion of the natural sciences. By adding to the inference the premise “what is *correctly* observed in one case must also be observed in all other cases offering the same conditions” (Mises 1962, p. 22), he could have validly inferred the strictly universal statement or social regularity that ‘all humans act’. This inference may be reconstructed as follows:

#### **Premise (singular statements)**

At time  $t_1$  and location  $k_1$  there is a human who acts rationally to achieve an aim.  
 At time  $t_2$  and location  $k_2$  there is a human who acts rationally to achieve an aim.

**Fundamental proposition of regularity (principle of induction)**

What is *correctly* observed in one case must also be observed in all other cases offering the same conditions (Mises, *ibidem*).

**Conclusion (a strictly universal statement)**

All humans act rationally to achieve aims.

In this sense, the 'category of action' is the result of an inductive inference. The observational and introspective statements constitute the empirical basis from which social regularities can be induced by generalization.<sup>6</sup>

However, just as in the case of natural sciences, it is obvious that the validity of the category of action 'all humans act' depends on the validity of Mises' presupposition of regularity, his induction principle. But the simple introduction of such a presupposition is insufficient for establishing the theoretical social sciences as a priori disciplines. In order to justify the theoretical social sciences as a priori disciplines, Mises would have to show that the principle of induction itself can be justified as a synthetic statement that is a priori valid. As Popper shows in *GE* the impossibility of synthetic statements a priori, this way of justifying the social sciences as a priori valid disciplines has to be rejected on logical grounds (Popper 1979, Sect. 3).

## **Mises' 'Category of Action' Is a Proposition About the Basic Ontological form of the Social Universe, Describing Its Essential Characteristics**

According to Mises, the natural world is characterized by regularity or uniformity. With respect to the social sciences, he argues that the structure and the regularities that characterize the social universe result from human action (Mises 1933, pp. 197–198). In his view, theoretical social knowledge can be acquired only if the regularity 'humans act' is presupposed as a 'fundamental category' from which social science must start. Accordingly, Mises' category of action may be interpreted as fundamental ontological form that characterizes the social universe. In this sense, human action is 'ultimate', that is, "... a datum that man's reason cannot trace back to other data" (Mises 1957, p. 182). As an ontological form the category of action asserts that 'human action exists'. "Praxeological reality is not the physical universe, but man's conscious reaction to the given state of this universe" (Mises 1949, p. 92).

However, even if Mises claims that the social world is so constituted, the problem of justification still remains. How can he show that praxeology is an *a priori valid* discipline? Such an existential statement 'human action exists' or 'there exists human action' is a synthetic statement at best valid a posteriori, and not a priori. If Mises wants to show that the social sciences are a priori disciplines, he has to show that the statement 'human action exists' is a synthetic statement a priori. Since Mises thinks that the existence of the synthetic a priori in the social sciences is a self-evident premise, he concludes that the task of epistemology is to clarify or justify already 'given' methods. He says "We do not want to discover a new

method, but only to characterize correctly the method that is actually used in economics.” (Mises 1933, p. 18) Thus his deduction may be read as follows:

**Premise 1:** Social knowledge exists (social knowledge is possible).

**Premise 2:** If social knowledge exists, humans exist who act rationally.

**Conclusion:** Humans exist who act rationally (there exists human action).

In order to show the conclusion ‘humans exist who act rationally’ to be a synthetic a priori statement, the premise itself should be changed to a synthetic a priori statements, that is, a statement that is valid under all circumstances. Again, an infinite regress of statements that claim to be a priori valid seems to be the result. As in the previous case, Mises’ arguments cannot justify that ontological existential statement as a synthetic judgment a priori. One cannot derive the existence of a synthetic statement that is valid a priori from a statement of the ontological form of the world or of the existence of knowledge. Again, his attempt to show that the theoretical social sciences are a priori disciplines has to be rejected on logical and epistemological grounds.

## Mises’ ‘Category of Action’ Is a Definition, Adopted as a Convention

Since analyticity implies aprioricity, the ‘category of action’ can also be interpreted as a definition. Indeed, Mises states that “Action is, *by definition*, always rational.” (Mises 1933, p. 3). Accordingly, the theories of the theoretical social sciences are a priori valid, but definitions only. Given this interpretation, one may regard Mises’ position as a kind of conventionalism. Yet, Mises also claims that the theoretical social sciences explain the social universe that is, “praxeology conveys exact and precise knowledge of real things” (Mises 1949, p. 39). If the theories of praxeology pertain to reality, one would expect their truth-values to be determined by experience, yet according to Mises, they are not. Thus, the question regarding the relation between experience and theory remains open.

How can praxeology convey absolutely certain knowledge about reality without recourse to experience? – One possibility for showing that the theories are absolutely true and pertain to reality is to show the possibility of synthetic statements a priori. However, this possibility has already been excluded. The other possibility is methodological. Basically, Mises tried to justify theories of praxeology as a priori valid statements that pertain to reality, by dint of introducing several methodological rules that reject experience as a critical standard of the theories.<sup>7</sup> In order to consider these methodological issues, I briefly explain the idea that Popper’s concept of falsifiability presents experience as the critical standard of which theories may fall short. I shall then interpret theories of praxeology in terms of what one may call ‘definitive causality’, and proceed to examine in which cases these theories might have to be judged to be false. After having identified those cases, I ask how Mises could have evaded these falsifications by introducing special methodological rules, that is, by introducing conventions.

According to Popper, falsifiability is a logical relation between singular statements (statements reporting experiences) and strictly universal statements (theories) (cf. Popper 1959, Chap. 3, Sect. 15; Chap. 4, Sect. 21). Strictly universal statements that are empirical can be expressed using the universal quantifier 'all', as in 'all swans are white'. The logical equivalent of such a statement is that of a negative existential statement, namely 'there are no non-white swans'. Obviously, the singular statement 'at time  $t$  and location  $k$ , there is a black swan' logically contradicts the statement 'all swans are white' or its logical equivalent 'there are no non-white swans'. The statement 'all swans are white' is falsifiable since it *prohibits* the singular statement 'at time  $t$  and location  $k$ , there is a black swan'. Therefore, if a theory (a strictly universal statement) asserts something about the world, it means that the theory prohibits some experiences (singular observational statements), which contradict it. If the singular statements are true, the theory is falsified. The criterion of falsifiability, Popper's criterion of demarcation, needs to be strengthened by additional methodological rules that prevent any attempt to escape the falsification of a theory. However, if special rules are needed in order to secure falsifiability, other rules may be introduced with precisely the contrary intention; that is, to prevent experience from deciding the truth of our theories.

In order to show in which cases theories of praxeology might have to be judged to be false, I shall first try to find a logical formulation. Mises defends a very deterministic view of causality. He writes:

Whatever happens in the universe is the necessary evolution of forces, powers, and qualities which were already present in the initial stage of the X out of which all things stem. (Mises 1957, p. 74)

As we have seen, Mises claims that all statements of praxeology (that is to say, social explanations) should be derived from the 'category of action', and without presupposing this category, social regularities are not possible at all. In consequence, he seems to exclude any causes other than human rational behavior from causal explanations in praxeology, and in this sense the category of action is a 'final cause' (Mises 1949, p. 23) for all social causation. Hence, we may formulate Mises' idea of causal explanation in praxeology as: "The cause of any effect in the social sciences is the rational behavior of humans in certain situations." Logically, one may express this causal explanation as the following strictly universal statement:

T1: whenever humans act rationally  $B$  in pursuing aims, and the condition  $Cx$  exists as a cause, the social effect  $Px$  is always observed; and if there is a social effect  $Px$ , it is always the outcome of human rational behavior under the conditions  $Cx$  (the cause).

$$T1': \forall x [(B \wedge Cx) \longleftrightarrow Px]$$

T1 expresses a necessary connection of causality between a cause  $Cx$ , and an effect  $Px$ , mediated by the postulate of human rational behavior  $B$ ; that is,  $(B \wedge Cx) \rightarrow Px$ ; and T1 also says that, if there is a social effect  $Px$ , there is always human rational

behavior  $B$  and a cause  $Cx$ , that is,  $Px \rightarrow (B \wedge Cx)$ .<sup>8</sup> Hence, T1 is falsified if the cause is given but not the expected effect related to that cause, or vice versa. We can reformulate this as T2.

T2: T1 is falsified if:

- (1)  $Px$  is given, but not  $(B \wedge Cx)$ .
- (2)  $(B \wedge Cx)$  is given, but not  $Px$ .

In order to show how the theories of praxeology could nonetheless be declared to be a priori true, we may now consider what kind of rules are needed in order to evade falsifications of T1, which might formally arise in three different ways.

- $Px$  and  $Cx$  hold, but  $B$  fails: This kind of falsification is impossible if  $B$  is held to be analytic and a priori true.
- $B$  and  $Cx$  hold, but  $Px$  fails: The truth of T1 may be preserved by introducing methodological rules that permit verifying instances only. Such a rule could say: "Pay attention only to observations that support a theory; disregard falsifying observations".
- $Px$  and  $B$  hold, but  $Cx$  fails: This kind of falsification does not contradict the basic tenets of praxeology. Mises states that experience may show that we have provided a false description of the conditions, and that  $Cx$  is false (Mises 1933, p. 30). In other words, in order to preserve the truth of  $B$ , a rule is introduced that states that falsifications are principally to be directed against the conditions  $Cx$ .

However, the second and third cases can be interpreted differently. We can avoid the result that T1 is false, by saying that  $B$  has no truth-value at all because according to praxeology, its falsity is out of the question. If  $B$  has no truth-value, T1 cannot be asserted. Then how is  $B$  to be interpreted? One interpretation is that  $B$  can be regarded as a methodological principle that is neither true nor false but suitable or unsuitable only (see next interpretation).

In order to support his rejection of experience as a standard of truth-value in the social sciences, Mises seems to defend a conventionalistic position, according to which theories are definitions that can never be falsified and are a priori true. Yet, he claims that these theories not only are a priori true, but pertain to reality as well; "...praxeology conveys exact and precise knowledge of real things" (Mises, *ibidem*). In order to explain the possibility of theories which although definitions nevertheless pertain to experience, he uses pragmatic arguments.

Mises frequently emphasizes that observations are impregnated by theories, that is, they are *theory-laden*. If we make observations, we usually make use of instruments to measure or observe something. In order to observe planets or stars, we use telescopes that are built according to our optical theories. From this, Mises concludes that data that would falsify a theory rather falsify the model that is constructed according to that theory. The theory is always correct, it is 'a priori' true, that is, true by definition, and if falsifications occur, the model has to be abandoned. Mises states:

The a priori categories are the mental equipment by dint of which man is able to think and to experience and thus to acquire knowledge. Their truth or validity cannot be proved or refuted as can those of a posteriori propositions, because they are precisely the instrument that enables us to distinguish what is true or valid from what is not. (Mises 1962, p. 18)

As mentioned in the third case above, Mises admits that the condition  $Cx$  is in principle revisable. "The disagreement between the theory and the facts of experience consequently forces us to think through the problems of the theory again." (Mises 1933, p. 30) However, he states also that a system of true definitions will be useful when models that are constructed according to the definitional system provide verifying instances if they are not falsified. Since Mises dogmatically excludes social effects  $Px$  that are not caused by the category of action, if one observes any such effect  $Px$  that does not contradict a theory of praxeology  $T1$ ,  $T1$  has to be regarded being verified. "... so long as a re-examination of the theory uncovers no errors in our thinking, we are not entitled to doubt its truth. The science of action deals only with those problems whose solution directly or indirectly affects practical interests." (Mises 1933, pp. 30–31) Thus, Mises in a way replaces the idea of truth by that of success. The theories of the social sciences are then successful or unsuccessful like instruments, and Mises consequently would have to defend an instrumentalist position, which contradicts his intention of showing that the social sciences are a priori true.

There are the other problems that remain if one interprets the category of action as a definition. Mises defines the category of action in an unidentifiable way as "The most general prerequisite of action is a state of dissatisfaction, on the one hand, and, on the other, the possibility of removing or alleviating it by taking action. ... Only this most general condition is necessarily implied in the concept of action. The other categorical conditions of action are independent of the basic concept; they are not necessary prerequisites of concrete action." (Mises 1933, p. 24) Yet saying 'to act rationally means that we remove or improve discomfort' amounts to little more than a tautology because any action, rational or irrational, can be explained in such a way. Rational and irrational behavior cannot be distinguished on this basis and thus is neither testable nor falsifiable.

To conclude, if Mises had taken the third option for showing that the social sciences are a priori, he would have had to substitute the idea of usefulness for the idea of truth and would have had to defend an instrumentalist position that conflicts with his intention of showing that the social sciences are a priori true. The falsity of  $T1$  can be avoided through the introduction of adequate rules that exclude experience as a critical standard, of which the theories of praxeology may fall short. Many such rules can be introduced in principle since it is always possible to find verifications for a theory. One can always restrict the domain for which a theory claims validity by reducing that domain by sufficiently strong ad hoc assumptions.

### **Mises' 'Category of Action' Is a Methodological Principle of the Kind Required by Methodological Individualism**

Since Mises defends methodological individualism, the 'category of action' can be interpreted as a methodological principle as well. The position of methodological individualism asserts that social facts, processes or phenomena are satisfactorily explained only if they are explained as the unintended result of the interplay of the



intended actions of individuals. In order to develop models that explain intentional individual actions, methodological individualism requires a so-called principle of rationality according to which individuals try to achieve their situational aim as well as possible. Accordingly Mises' category of action 'all humans act to achieve aims' may be interpreted as a special variant of the rationality principle. In this sense, Mises states, "we explain a phenomenon when we trace it back to general principles" (Mises 1933, p. 130). As a methodological principle, the category of action asserts that individuals act according to the logic of their situation. Its introduction is necessary in order to animate social models.

Interpreting Mises' category of action as a methodological principle seems to me to be very well in agreement with his general intentions. However, methodological principles cannot be true or false, but suitable or unsuitable only according to the aims one wants to achieve. If Mises requires that the social sciences state theories that are *a priori* valid, the question arises as to whether it is possible that the interpretation of the 'category of action' as a methodological principle provides a way of showing this. In that case, it is perfectly acceptable to say that the empirical content of a social theory resides in the typical initial conditions  $Cx$  describing the social situation that is animated by the rationality principle. Experience may thus still be regarded as the critical standard, of which our theories may fall short. Experience then does not determine the truth-value of the rationality principle because, as a methodological principle, it cannot have one. But experience determines the truth-values of the typical initial conditions  $Cx$ . As we have seen, Mises admits that initial conditions  $Cx$  can be false or true. But it remains unresolved whether the decision to assume that the category of action is always true under all circumstances is in agreement with Mises' aim to show that the social sciences are *a priori* disciplines.

In the history of economic thought, one may often find epistemological discussions that can be seen as endeavors to solve the problems of induction and demarcation, such as those of Mill (cf. Mill 1843) and Menger (cf. Menger 1883).<sup>9</sup> Mises' epistemological discussions were one of the efforts to solve these problems with respect to the special situation of the social sciences. His epistemology was directed to achieve that certain and definitive knowledge that Popper rejected from all scientific inquiries. Such knowledge is not only not possible but its pursuit may restrict those of our scientific inquiries which we learn from experience and from falsification.

## Notes

1. I cordially thank Karl Milford and David Miller for providing comments and critiques. This research was partially supported by the Japanese Ministry of Education, Culture, Sports, Science and Technology (MEXT), Grant-in-Aid for twenty-first century COE Program "Interfaces for Advanced Economic Analysis" and the Urrutia Elajalde Foundation. Naturally, the responsibility for errors is mine.
2. There exists a discussion between Hansen and Hacothen whether that second volume really existed, but I am not going into this discussion in this chapter. See (Jarvie et al. 2006, pp. 67–82, pp. 99–110).

3. This paper is different from those approaches that interpret Mises' position as an application of special philosophical positions such as Kant's (cf. Parsons 1990) or Aristotle's (cf. Smith 1990).
4. In his paper "Economics and Knowledge" Hayek implicitly intended to criticize Mises' aprioristic position and discussed the way in which explanations of economic orders or markets require empirical content (Hayek 1980, pp. 33–56). However, according to Hayek, Mises does not seem to have accepted Hayek's paper as a critique.
5. A typical induction may be reconstructed as follows:  
 Premises (singular observational statements):  
 At the location  $k_1$  and time  $t_1$  and given the conditions  $a, b, c, d, \dots$  it was observed that the mixture of liquids  $A$  and  $B$  resulted in liquid  $C$ .  
 At the location  $k_2$  and time  $t_2$  and given the conditions  $a, b, c, d, \dots$  it was observed that the mixture of liquids  $A$  and  $B$  resulted in liquid  $C$ .  
 Fundamental proposition of regularity (principle of induction):  
 "What is *correctly* observed in one case must also be observed in all other cases offering the same conditions" (Mises *ibid.*).  
 Conclusion (a strictly universal statement):  
 If conditions  $a, b, c, d, \dots$  prevail, the mixture of liquids  $A$  and  $B$  results in liquid  $C$ .  
 This corrected way of induction Mises calls 'ampliative induction' in order to distinguish from simple inductions (Mises 1957, p. 9). Note that the expression 'ampliative induction' these days has a much wider connotation.
6. Kirzner refers to an episode when he asked Mises how one can know that other people act rationally. According to Kirzner, Mises' answer to the question was "by observation" (Kirzner 2001, pp. 88–89).
7. Although Mises uncompromisingly claims that the possibility of synthetic apriori is an apodictic and indisputable presupposition (Mises 1962, p. 5), it is interesting to note he also refers to a kind of nescience argument. He states: "Science does not give us absolute and final certainty. It only gives us assurance within the limits of our mental abilities and the prevailing state of scientific thought. A scientific system is but one station in an endlessly progressing search for knowledge" (Mises 1949, p. 7). In my opinion, Mises strategically tries to solve the problem of demarcation, which cannot be adequately solved by metaphysical justification, by dint of transforming it into a methodological problem.
8. This formulation is not in conformity with causality as it is generally understood. For Mises the category of action is a prerequisite for any social knowledge, and he therefore dogmatically excludes all possibilities of causal explanations in the social sciences other than those triggered by human action. He states, "For the purposes of science we must start from the action of the individual because this is the only thing of which we can have direct cognition. ... *Everything social must in some way be recognizable in the action of the individual*" (Mises 1933, pp. 44–45 emphasis mine).
9. Milford analyses special epistemological positions in the social sciences such as Mill and Menger as endeavors to solve problems of induction and demarcation. (Cf. Milford 1992, 2002).

## References

- Blaug, Mark. 1992. *The Methodology of Economics: or How Economists Explain*. Cambridge: Cambridge University Press.
- Hayek, Friedrich August von. 1980. *Individualism and Economic Order*. Chicago, IL: The University of Chicago Press.
- Hayek, Friedrich August von. 1994. *Hayek on Hayek: an Autobiographical Dialogue*, eds. Stephen Kresge and Leif Wenar. London: Routledge.
- Hoppe, Hans-Hermann. 1988. In *Defense of Extreme Rationalism: Thoughts on Donald McCloskey's The Rhetoric of Economics. The Review of Austrian Economics*, 3: 179–214.

- Kirzner, Israel. 2001. *Ludwig von Mises: The Man and His Economics*. Wilmington, DE: ISI.
- Leeson, Peter and Boettke, Peter. 2006. Was Mises Right? *Review of Social Economy*, LXIV(2): 247–265.
- Menger, Carl. 1883 (1963). *Problems of Economics and Sociology (Untersuchungen über die Methode der Socialwissenschaften, und der Politischen Oekonomie insbesondere)*, trans. Francis J. Nock. Urbana, IL: University of Illinois Press.
- Milford, Karl. 1990. Menger's methodology. *History of Political Economy*, annual supplement to vol.22, Duke University Press, Durham, London 215–239.
- Millford, Karl. 2002. Auf der Such nach sicherem Wissen: Zur Wissenschaftstheorie von J.S.Mill, ed. Erich W. Streissler, *Studien zur Entwicklung der oekonomischen Theorie XIX*. Berlin: Duncker & Humblot.
- Mill, John Stuart. 1843 (1974). A System of Logic Ratiocinative and Inductive: Being a Connected View of the Principles of Evidence and the Methods of Scientific Investigation. In *Collected Works of John Stuart Mill*, Vols. VII & VIII, ed. John Mercel Robson, London/Toronto: Routledge & Kegan Paul/University of Toronto Press.
- Mises, Ludwig von 1933. *Epistemological Problems of Economics (Grundprobleme der Nationalökonomie: Untersuchungen über Verfahren, Aufgaben und Inhalt der Wirtschafts und Gesellschaftslehre)*, trans. George Reisman, Princeton, NJ: Van Nostrand (1960).
- Mises, Ludwig von 1949. *Human Action: A Treatise on Economics*. New Haven, CT: Yale University Press.
- Mises, Ludwig von 1957. *Theory and History: an Interpretation of Social and Economic Evolution*. New Haven: Yale University Press.
- Mises, Ludwig von 1962. *The Ultimate Foundation of Economic Science: An Essay on Method*. Princeton, NJ: Van Nostrand.
- Parsons, Stephen D. 1990. The philosophical roots of modern Austrian economics: Past problems and future prospects. *History of Political Economy*, 22: 295–319.
- Popper, Karl Raimund. 1959. *The Logic of Scientific Discovery*. London: Hutchinson.
- Popper, Karl Raimund. 1974/1976. *Unended Quest: An Intellectual Autobiography*. La Salle, IL: Open Court (1985).
- Popper, Karl Raimund. 1979. *Die beiden Grundprobleme der Erkenntnistheorie (The Two Fundamental Problems of the Theory of Knowledge)*, ed. Troels Eggers Hansen, Tübingen: J.C.B. Mohr.
- Popper, Karl Raimund. 1996. *In Search of a Better World: Lectures and Essays from Thirty Years*. London: Routledge.
- Rothbard, Murray Newton. 1976 (1939). Praxeology: The Methodology of Austrian Economics. In *The Foundations of Modern Austrian Economics*, ed. Edwin Dolan, Kansas City: Sheed & Ward.
- Smith, Barry. 1990. Aristotle, Menger, Mises: An Essay in the Metaphysics of Economics. *History of Political Economy: Annual Supplement to vol. 22*, 263–288.

# Popper's Theory of the Searchlight: A Historical Assessment of Its Significance

Michel ter Hark

**Abstract** On the basis of a correspondence between Karl Popper and the Dutch psychologist Adriaan de Groot, it is argued that the former's epistemology of the searchlight is historically rooted in early cognitive psychology of Otto Selz. It is furthermore argued that Popper's later critique of information processing psychology is the fruit of his assimilation of Selz's evolutionarily inspired program. In light of the current interest in evolutionary approaches to the mind, it is argued that this Popper–Selz program is as actual as ever.

In my recent book *Popper, Otto Selz and the Rise of Evolutionary Epistemology* (ter Hark 2004), I have attempted both to trace the origins of Popper's epistemology of the searchlight back to early German psychology, in particular the work of Otto Selz, and to point out the way the work of the latter diverges from the mainstream psychology at the time including the programme of the closely related Würzburger School of psychology. As I also pointed out in my book, and as has been mentioned by others as well, Otto Selz played a formative role in the rise of cognitive science in the early 1950s of the last century in the USA, notably the work of later Nobel laureates Allen Newell and Herbert A. Simon. A mediating role here has been played by the Dutch psychologist and methodologist Adriaan de Groot, who was the first to apply Selz's ideas to the thinking processes of chess masters, and whose book *Het Denken van den Schaker* (de Groot 1946) was studied by Newell and Simon, in the original language, in 1954, a year before the revolution started. In 1965, an English translation of de Groot's book, *On Thought and Chess* appeared. As some of the new footnotes make clear, de Groot in his turn has been influenced by the American reception of Selz's ideas. For now, in the computational era, he claims that thought processes, as analyzed by Selz in the pre-computational era, might be simulated by a machine-program. As the further development of cognitive science has shown, the idea of so-called strong artificial intelligence, vigorously defended by Simon by means of his notion of a physical symbol system having the necessary and sufficient means for general intelligent action, has come increasingly under attack (Simon 1996). More and more, the idea of physical symbol systems has made place for an evolutionary or biological approach to the study of intelligence.

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As was pointed out in the final chapter of my book, the evolutionary approach to intelligence has been a pervasive feature of Selz's work, even neglected by de Groot. But not by young Popper. In his work on epistemology and the mind-body problem from the 1960s onwards, the evolutionary approach is again a dominating feature. From this perspective Popper's correspondence with de Groot is particularly interesting. My point of departure in this chapter is a particular letter in which they discuss the significance of Selz both for methodology and psychology, as well as the approach taken by Newell and Simon. What is illustrated by this letter is not only that there has been a typical European approach to the study of cognition, different from the American approach, but also that young Popper showed precocious awareness of the lasting significance of one of the most important contributors to this European tradition.

## The Popper–de Groot Correspondence

The main correspondence between Popper and de Groot dates from 1977 till 1990 and covers 19 letters. However, there are also letters kept from an earlier period, which unfortunately I have not been able to study yet. Popper and de Groot met twice, in 1946 at the International Significance Conference in the Netherlands, and in 1955 in London. The immediate occasion for the letter I will quote is the publication by the Dutch psychologists Nico Frijda and A. D. de Groot (Frijda and de Groot 1980) from the University of Amsterdam of a book on Otto Selz celebrating his 100th birthday. A copy of the book was sent by de Groot to Popper. The letter is dated 1 June 1990.

I am deeply in your debt for the copy of the book on Otto Selz, edited by Nico H. Frijda and yourself: every admirer of Otto Selz must be in your debt. It is an excellent and highly informative book - even Herbert Simon's article is very valuable (although I greatly disagree with his "Information-Processing" Interpretation.)

It must have been in 1929 - after I had taken my PhD in 1928 - that I realized all thinking process are problem-oriented, and that this has been discovered by Otto Selz years before. And that all attempts to solve a problem are trial-and-error elimination processes, as has also been 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 was to discover something unknown. The logical situation made it impossible for him who does not know to use any other method than the method of hypothesis (trial) and error-elimination.

I illustrated the logic of the situation of trying to discover something by the story of the black man who searches in a black cellar for a black hat (that may not even be there). What can he possibly do? He can move his hands in certain directions (each movement in a certain direction corresponds to a hypothesis that the hat may be just there): he must make the same trial-and-error movements which H.S. Jennings described in *The Behaviour of the Lower Organisms*.

So in the objective Situational Logic is the explanation of the universal validity of Otto Selz's (and my own) discoveries.

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.

However, this was the reason why I gave up my psychological studies and became a methodologist. And I am happy to be able to say that several great scientists - physicists, chemists, biochemists and evolutionary theorists - said that they could not have got their results without having been told by me that data (=information) processing leads nowhere, but having bold hypotheses (and critically testing them) is the key to success. (Popper 1990)<sup>1</sup>

This letter contains some features familiar from Popper's published writings including his autobiography (Popper 1974), such as the priority of logic over psychology, i.e. the principle of transference, the importance of trial-and-error, yet the tribute paid to Selz in the formation of his own thought is much more explicit. Moreover, never before Popper has spoken of the 'universal validity' of Selz's psychology.

It is possible that Popper felt further supported in this appreciation of Selz by the chapter of Herbert Simon in the book on him (Simon 1980). Even though Popper disagrees with Simon's inductive and computational approach to cognition, it is clear to him that some of the main concepts of Selz, in particular his idea of schematic anticipation, has been adopted successfully by the computational paradigm. Another support for the 'universal validity' of Selz's work comes from a rather different direction: the genetic epistemology of Piaget whose important contribution was delayed at the international level until the 1960s, when his innovative use of the concept of schema became important in cognitive psychology. However, the mutual isolation of French and American psychology prevented the significant influence that the French psychologists would have deserved. Popper was familiar with Piaget's work, and what must have struck him upon reading his work was the similarity with his own epistemology. Indeed, he even sent a copy of his *Objective Knowledge* (Popper 1972) to Piaget noting the similarity between their views, especially at the level of world 2. Assuming that Popper read Piaget's most philosophical book, i.e. *La Psychologie de l'Intelligence*, he must have noticed Piaget's great appreciation of and indebtedness to Selzian psychology in particular his notion of the schema. Piaget's book was published in 1947, hence 20 years before two of the most influential but rather divergent psychologists turned to the forgotten and marginalized Otto Selz, young Popper showed precocious awareness of the importance of Selzian psychology when writing, in his 1928 dissertation, that there are similarities between ordinary cognition and scientific cognition and that the scientific method corresponds completely to the Selzian scheme of problem solving. His was not an immature or exaggerated assimilation of an isolated German professor in Mannheim, but a deep and prophetic insight into the nature of cognition. The later work of such giants as Piaget and Newell and Simon proved it. Selz's work was universally valid.

## Otto Selz

In a recent book by George Mandler, *A History of Modern Experimental Psychology* (Mandler 2007), Selz, if only in three pages, finally gets the tribute he deserves:

Probably the major turning point in the history of the study of thinking came with the work of Otto Selz. Although Selz studied with some of the Würzburg psychologists, his *magna opera* were written elsewhere and published in 1913 and 1922. Not only does he deal in these two volumes with the problem of directed thinking, but he is the first psychologist who is both willing and able to deal with the problem of productive thinking under the same rubric as reproductive thinking. Neither in his original two volumes of work nor the summary of his theory of productive and reproductive thinking received adequate attention during the ensuing years. Selz himself restricted his work in subsequent years, spent in minor academic position in Mannheim, to a restatement of his position, and much of the psychology of thinking between 1920 and 1950 might have advanced faster had his work been used more extensively. He was killed during the 1940s in a German concentration camp. (Mandler 2007, p. 111).

One of the reasons of the delayed reception of Selz not mentioned by Mandler is the association of his work with the study of 'imageless thought' as practiced by the Würzburgers. With this notion, based on experimental introspective findings, the Würzburger psychologists and philosophers (e.g. Oswald Külpe) believed to have demonstrated the inadequacy of the traditional definition of the mind in terms of sensations, images and feelings. The experiments of Karl Marbe, Heinrich Watt, Narziss Ach and also Bühler, produced evidence for the existence of thought-elements, that were not reducible to these classical ingredients. But soon controversy arose over the admission of these thought-elements, both in Germany where Wundt attacked the doctrine on methodological grounds, and in the USA, where Titchener and his collaborators found no evidence for them. According to Richard Ogden (1951), pupil of both Külpe and Titchener, the controversy ultimately created a favourable moment for the rise of the opposite of the psychology of thinking, behaviourism. 'Imageless thought' passed into the limbo of inert conceptions, and Selz shared its fate.

But Selz's account of imageless thought was different from the mainstream of the Würzburg school. If these differences had been noticed at an earlier phase, the short-sighted reaction of Titchener and his allies might have been prevented. Now Selz had to wait for a wider (posthumous) recognition of his achievements on Allen Newell and Herbert Simon who, aided by A. D. de Groot, reminded us that what Selz had discovered was that the study of memory and thought processes had to "make provision for two-termed relations as well as simple predicate links" (Simon 1980, p. 151). Simon was undeniably right in pointing out that Selz's theory of thinking was in fact a theory of relational structures, but by not taking into account the historical context his portrayal was hopelessly anachronistic. Selz's theory was as far removed from information-processing psychology, with its emphasis on unconscious factors, as it was from association psychology with its emphasis upon mechanical factors. His theory of relations was not based



on a hypothesis concerning unobservable internal processes but was drawn from *Gegenstandstheorie*, in particular Meinong's (1904).

What Selz (1913) did was to describe psychological phenomena by means of concepts introduced by and analyzed in a more general *Gegenstandstheorie*, a theory encompassing not only psychology and the other empirical sciences, but also logic and mathematics. The two most important concepts were "relational fact" (*Sachverhältnis*) and "relation" (*Beziehung*). It was precisely his use of the concepts of relational fact and relation which marked the departure of his *Denkpsychologie* from the mainstream of empiricism and associationism in Germany and earlier British empiricism. The most significant use Selz made of Meinong's psychology was the latter's theory of indirect representation (*indirektes Vorstellen*). Meinong's discovery of indirect representation was based on his broader insight that the meaning of many concepts is not fixed until the meaning of other concepts to which they are related is represented. The importance of relational concepts was especially shown, according to him, by the frequent occurrence of problem situations in which the only clue about an unknown element (*b*) of the problem situation was provided by the information one had about another element (*a*) and its relation (*R*) to (*b*). For instance, someone wants to form an idea of the physical height (*b*) of a person unknown to him based on the information that the person is as big as (*R*) an acquaintance (*a*). The knowledge situation is called indirect because the information about an element is determined by means of the information about its relation to an element one is acquainted with. Significantly, the idea of indirect cognition was suggested to Meinong by a mathematical analogy. In mathematics, he noted, one often operated with equations or abstract functions with one or more unknown elements.

Selz fitted this theory of indirect representation into his theory of complexes and schematic anticipations. Knowing objects, he claimed, is often mediated by one's being conscious of the relational fact to which they belong. Diagrams of schematic anticipations making clearly visible that the awareness of a problem was related to the structured whole to be realized as the scheme of a whole to the completed whole appeared in Selz's *Zur Psychologie des produktiven Denkens und des Irrtums* (Selz 1922) and his *Die Gesetze der produktiven und reproduktiven Geistestätigkeit* (Selz 1924). In Selz, then, Meinong's thoughts about indirect representation were transformed into a theory of abstract thinking that departed radically from the empiricist view that thinking was a process of cementing relations between mental representations given in advance. In fact, their view was the opposite: the network of relations the unknown item of knowledge maintained with other items was known from the start and in fact expedited the problem solving.

Young Popper's claim that Selz, in his study of reproductive thinking, had clearly demonstrated that forgotten information had not simply been overlooked but had left "an unoccupied space in memory, analogous to the unknown *x* in a mathematical equation, which prompts the urge to fill it in (complex completion)" (Popper 1931, p. 616), showed awareness of the fact that (abstract) knowledge of relational structures precedes empirical knowledge, and contributed enormously to his deductive psychology and theory of knowledge. But what about Popper's

biological approach to the study of (scientific) cognition? How is this important dimension of his work related to Selz?

## **Trial and Error Behaviour**

In the letter to de Groot, Popper also mentions the theory of trial and error put forward by Jennings (1905). Similar references occur in his published work. Jennings's theory of trial and error is part of a purely biological theory, but he has also attempted to give a more general application of biology which was congenial to young Popper. In an article in *Science* (Jennings 1927), a copy of which is in Popper's papers, Jennings eloquently and passionately argued for the doctrine of emergent evolution both as a program in biology and as a philosophy of science and life. He opposed the then prevailing mechanistic view of evolution as "the working of a great machine that never alters its mode of action nor the nature of its product." (Jennings 1927, p. 20) The method of science based on this mechanistic and deterministic view of evolution, Jennings explained, is mainly rationalistic and to but a minimal extent empirical. Against this view, the doctrine of emergent evolution holds that new things, not thus computable, emerge as evolution progresses: "It holds that with these emerge new methods of action, following new laws; methods not before exemplified; methods that falsify the results of computations based on former methods of action." (Jennings 1927, p. 21) It is this emergence of new things which distinguishes the living from the non-living, Jennings maintained. From this standpoint of emergent evolution Jennings pleaded for a radical experimentalism in science. Since there is always the possibility that new things or properties have emerged, he averred, they cannot be discovered by ratiocination but only by observations and experiment. Thus radical experimentalism as a philosophy of science and emergent evolutionism as a philosophy of biology are mutually supportive. The method of trial and error is not only the method of science but also of evolution itself. Indeed, the only possible method for progress in emergent evolution is by trial and error, Jennings went on. "In such progress by trial and error will indeed be found free play for the utmost sharpness of vision as to what it is best to try ... but in the end a trial it must be, with no antecedent certainty as to results." (Jennings 1927, p. 22). Yet Jennings's method of trial-and-error is inductive.

A deductive or quasi-deductive form was developed by Selz. Automatic learning, as Selz also calls the fruit of 'trying-out behaviour', is a process equally initiated and guided by schematic anticipations (cf. Selz 1924, pp. 48–50). His main argument for the selecting role of schematic anticipations upon automatic learning, however, comes not from animal biology but from his study of the acquisition of skills in sport and aircraft. A beginner at ninepins, Selz explains, seeking to hit a particular pin, will initially try out deliveries of varying force, trajectory and spin within a range circumscribed by previous bowling experiences. Among these "trying-out movements" a small group will lead to a positive result (R), the others

will produce negative results (N1, N2, N3...). In later attempts, the subject cannot anticipate the exact movement which has led to R as a solving method to achieve his goal, yet, Selz emphasizes, anticipating R prompts only those memory traces of earlier attempts which actually have led to R. Indeed, he recalls, it turns out to be more efficient to concentrate on the goal of the movement than on the movement itself, awareness of the goal heightening the selective effect of the anticipation of the correct movement. Finally successful movements will be made with great precision owing to the process of routine application of solving methods. The most important difference, then, between blind trial and error and trying-out behaviour is that in the latter case attempts are based on a partial insight into the situation. Always showing a clear sense of direction, the organism tries out within a pre-set, goal-determined and limited domain of solution possibilities; in Selzian terms, schematic anticipations co-determine the where and what of search and trying.

As early as 1928, Popper refers to Selz's theory of 'trying-out behaviour' as the key to the (psychological) study of the growth of knowledge:

Perhaps there are important parallels in the methods and operations of the scientific and the "pre-scientific" induction?

To give just one example: The Selzian concept of trying-out behaviour seems to me to have important parallels in objective scientific research. Science tries out its methods, its "models" (as Bühler puts it), and in such a way as to correspond completely with the Selzian scheme. As is well known the actual ways of scientific research in no way correspond with the logical principles of the representation; as little as the operations described by Selz correspond with the objective logical operations. Despite this science is in the end clearly driven by tasks, the determining tendencies come clearly to the fore. (Popper 1928, p. 69–70)

The earliest sign of his concern with the nature of scientific research, this passage, with Selz calling the tune, unmistakably shows Popper's ideas on the logic of scientific discovery emerging in the context of the psychology of scientific discovery. Although rejecting the relevance of the psychology of discovery for the philosophy of science in *The Logic of Scientific Discovery* (Popper 1935/1959), in the post war years he unwaveringly adhered to his proposal to compare individual and scientific cognition, and use the (Selzian) method of trial and error as a measure. Indeed, he gave the (Selzian) method of trial and error the highest general sense possible, incorporating not only individual psychology but all the sciences, including the *Geisteswissenschaften*, and evolution. At this juncture, however, Popper seemed not that far. Another indication of the still rudimentary grasp of the depth and implications of Selz's work at this juncture is his repeated use of the term "induction." For Selz's detailed and frontal assault on association psychology, and his defence of a theory of schematic anticipations, in fact boiled down to a view of the animal or human organism as an active cognitive subject constantly putting forward tentative proposals or hypotheses rather than as a passive recipient, patiently waiting for the accumulation of information to be inductively safe. As I have shown elsewhere (ter Hark 2004), in *Die beiden Grundprobleme der Erkenntnistheorie* (Popper 1979), Popper finally integrates his Selzian stance in psychology in his deductive theory of knowledge.

## Information-Processing Psychology and the Searchlight Theory

Already in his dissertation of 1928, Popper was critical of physicalism as regards the mind–body problem and the science of psychology. In it, he subjects the physicalistic program common to Schlick (1918) and Wolfgang Köhler to a critical and detailed analysis. His suggestion is that the best prospects for a solution of the mind–body problem, or the relation between psychology and physiology is a biological rather than a physicalistic stance. This proposal is in conformity with Selz who had predicted that the twentieth century would see the ‘biology of the inner man’ (Selz 1922, p. xii), a phrase quoted approvingly by Popper (1928, p. 77).

In Popper’s and Eccles’s *The Self and its Brain* (1977), this criticism of physicalism is continued and broadened so as to include also behaviourism and functionalism. Functionalism, especially in the form defended by David Armstrong – who is discussed by Popper – is merely a transitional stage to physicalism. On the other hand, functionalism’s key notion of “realization,” although a denial of the idea that mental properties and neural properties are identical, is the “exact complement of the concept of emergence.” Yet Popper’s most ponderous argument against functionalism, especially the variant exploiting the analogy between computer programs and the human mind, is not ontological but rooted in his evolutionary outlook on psychology. Commenting on Alan Turing’s belief that one day machines can be built that can think, he retorts: “But computers are totally different from brains, whose function is not primarily to compute but to guide and balance an organism and help it to stay alive.” Unlike contemporary criticisms of functionalism focusing on the specific physical make-up of computers and its supposed incapacity to produce what is deemed characteristic of the mind, intentionality and consciousness, Popper does not touch such ontological questions at all, and instead points to the misleading analogy between the input to computers and the stimuli biological organisms encounter on their way. In a series of notes, after first making his by now familiar point that for organisms problems arise mainly because of disappointed expectations, he argues that “the computer does not search its environment for new programs,” although he concedes that this feature could be built into the computer. Yet fundamental differences remain. As he argues in an unpublished fragment:

Problems mean something different to organisms than to computers. A computer can be built to any specification. Present computers only work with specified problems, that is to say, the programs must not be too unexpected. To a certain extent this also holds for organisms. But although it will be possible to produce or specify a program, which the computer “considers” as a disappointed expectation, the program with the entailed problems will always have to be set to the computer. Only organisms have their own problems and are able to develop some initiatives in answer to problems. Life may be said to be the simultaneous emerging of problems and structures (August 1969, Karl Popper Papers, box 104, file 105, Hoover Institution Archives).

It is not its capability of being stimulated which makes the biological organism, Popper wants to say here, but it is the other way round: the built-in expectations, the anticipatory trial movements are what makes the stimulus. His main point of

criticism therefore is again based on the contrast between the Bucket theory and the Searchlight theory, and especially the many shapes in which the former keeps on surfacing in psychology and philosophy (Popper 1949). The organism is not a bucket, passively waiting for whatever stimulus impinges upon its receptors, but a searchlight actively exploring its surroundings in need of a solution for some vitally important problems. In order to be perceived or classified as a stimulus at all, it has to be a function of the organism's (immediate) concerns. Animals have their own problems in the sense that the ecological niche they live in is pre-structured in terms of their aims and needs, and what counts as a stimulus is already a function of those aims and needs. As the animal psychologist of his youth, Hans Volkelt (1914) explained, outside its web, flies are not even recognized by the spider as edible objects. With biological organisms relevance is built in. Popper's earlier criticism of Pavlov's theory of the conditioned reflex to the effect that there are no naturally repeatable stimuli which the organism cannot fail to recognize as being the same, therefore can be generalized to computational systems that, not living in an ecological niche, have to treat all facts as possibly relevant for solving their problems.

Despite their common emphasis on problem solving then, the Searchlight theory and the computational theory of mind espoused by Newell and Simon differ fundamentally in their view of the nature of problems and their meaning for the organism. It is unsurprising therefore to see Popper rejecting the inductive information-processing model in his correspondence with de Groot. Seeing and discussing these differences, Popper shows awareness of the gulf between the tradition of German *Denkpsychologie* and the cognitive revolution of the 1950s in a way some of its leading revolutionaries have not. Given the current interest in evolutionary approaches to the study of the mind, Popper's Selzian program is as relevant and as alive as ever before.

## Note

1. This letter is kept at the *Archief en Documentatiecentrum Nederlandse Psychologie* (The Netherlands, Groningen) which disposes of the correspondence and manuscripts of Adriaan de Groot. I thank Dr. Jacques Dane for sending me this letter.

## References

- Frijda, Nico and de Groot, Adriaan. 1980. *Otto Selz: His Contribution to Psychology*. The Hague: Mouton.
- Groot de, Adriaan. 1946. *Het Denken van den Schaker*. Amsterdam: N.V. Noord-Hollandse Uitgevers Maatschappij.
- Groot de, Adriaan. 1965. *Thought and Choice in Chess*. The Hague: Mouton.
- Hark ter, Michel. 2004. *Popper, Otto Selz and the Rise of Evolutionary Epistemology*. Cambridge: Cambridge University Press.
- Jennings, Herbert. 1905. *Behavior of the Lower Organisms*. London: Indiana University Press.

- Jennings, Herbert. 1927. Diverse Doctrines of Evolution, Their Relation to the Practice of Science and of Life. *Science* LXV: 19–26.
- Mandler, George. 2007. *A History of Modern Experimental Psychology*. Harvard: MIT Press.
- Meinong, Alexius. 1904. *Untersuchungen zur Gegenstandstheorie und Psychologie*. Leipzig: Barth.
- Odgen, Richard. 1951. Oswald Külpe and the Würzburger School. *American Journal of Psychology* 61: 4–19.
- Piaget, Jean. 1947. *La Psychologie de l'Intelligence*. Paris: Librairie Armand Colin.
- Popper, Karl Raimund. 1928. *Zur Methodenfrage der Denkpsychologie*. Ph.D. diss., University of Vienna.
- Popper, Karl Raimund. 1931. Die Gedächtnispflege unter dem Gesichtspunkt der Selbsttätigkeit. *Die Quelle* 81: 607–619.
- Popper, Karl Raimund. 1949. Naturgesetze und Theoretische Systeme. In *Gesetz und Wirklichkeit*, ed. Simon Moser, 43–61. Wien: Europäisches Forum. (Translated as The Bucket and the Searchlight: Two Theories of Knowledge. In Karl Raimund Popper. 1972. *Objective Knowledge*. Oxford: Clarendon.
- Popper, Karl Raimund. 1972. *Objective Knowledge*. Oxford: Clarendon.
- Popper, Karl Raimund. 1974. *Unended Quest*. London: Routledge.
- Popper, Karl Raimund. 1979. *Die beiden Grundprobleme der Erkenntnistheorie* [1930–33]. Edited by Troels Eggers Hansen. Tübingen: J.C.B. Mohr.
- Popper, Karl Raimund and Eccles, John. 1977. *The Self and Its Brain*. New York: Springer.
- Schlick, Moritz. 1918/1985. *Allgemeine Erkenntnislehre*. 2nd ed. Berlin: Naturwissenschaftliche Monographien und Lehrbücher. (*General Theory of Knowledge*. La Salle, IL: Open Court).
- Selz, Otto. 1913. *Über die Gesetze des geordneten Denkverlaufs*. Stuttgart: Verlag von W. Spemann.
- Selz, Otto. 1922. *Zur Psychologie des produktiven Denkens und des Irrtums*. Bonn: Verlag von Friedrich Cohen.
- Selz, Otto. 1924/1980. *Die Gesetze der produktiven und reproduktiven Geistestätigkeit. Kurzgefasste Darstellung*. Bonn: Bouvier Verlag. (“The Laws of Cognitive Activity, Productive and Reproductive: A Condensed Version.” In Otto Selz: *His Contribution to Psychology*, eds. Nico Frijda and Adriaan de Groot. The Hague: Mouton).
- Simon, Herbert. 1980. Otto Selz and Information-Processing Psychology. In *Otto Selz: His Contribution to Psychology*, eds. Nico Frijda and Adriaan de Groot. The Hague: Mouton.
- Simon, Herbert. 1996. Computational Theories of Cognition. In *The Philosophy of Psychology*, eds. William O'Donohue and Richard Kitchene, 160–173. London: Sage.
- Volkelt, Hans. 1914. *Über die Vorstellungen der Tiere*. Leipzig: Verlag von Wilhelm Engelmann.

# From Group Selection to Ecological Niches

## Popper's Rethinking of Evolution in the Light of Hayek's Theory of Culture<sup>1</sup>

Jack Birner

**Abstract** Hayek's *The Sensory Order* contains a physicalistic identity theory of the mind. Popper criticized it, saying that it could not explain the higher functions of language. Hayek took up that challenge in a manuscript but failed to refute Popper's arguments. Drawing upon the same manuscript, Hayek developed a theory of behavioural rules and cultural evolution. Despite his criticism of the theory of mind on which this evolutionary theory was based, Popper adopted Hayek's idea of group selection. He transformed it into a theory of the selective power of ecological niches. This became a central element of Popper's theory of evolution. The chapter traces the influence Popper and Hayek had on each other in the fields of the philosophy of mind and evolutionary theory. This is documented, *inter alia*, by their correspondence. Popper's theory of evolution, which is based on his dualistic theory of mind, is presented in its various stages of development. The chapter concludes with a possible application of that evolutionary theory, some thoughts about David Hume as the source of the differences between Popper and Hayek, and on the possible impact Popper's criticism had on Hayek's role in artificial intelligence.

## Introduction

Karl Popper and Friedrich Hayek were close personal friends and intellectual comrades-in-arms. For example, both were opposed to reducing explanations in social science to psychological regularities; both criticized relativism and collectivism; they shared the conviction that our knowledge, particularly of society, is limited, from which both drew the conclusion that we should proceed with extreme caution when trying to change the world. Given these shared ideas, it is surprising that no systematic comparison between these two great intellectuals has been made so far. William Bartley might have done so, but he did not live to complete the planned biographies of both men. Malachi Hacoheh only hints at what united – and what

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divided – Popper and Hayek.<sup>2</sup> Bruce Caldwell asked whether Popper and Hayek influenced each other's thought and concluded that they didn't.<sup>3</sup>

I come to a very different conclusion: Popper and Hayek had a profound influence on one another. That is not a thesis that can be proved. Instead, I will give arguments that show its plausibility.

Popper himself observes that there are different forms of intellectual influence:

[I]f I say "Brouwer's thought was influenced by Kant" or even "Brouwer rejected Kant's theory of space" then I speak at least partly about acts of thought in the subjective sense: the word "influence" indicates context of thought processes or acts of thinking. If I say, however, "Brouwer's thought differs vastly from Kant's", then it is pretty clear that I speak mainly about contents. And, ultimately, if I say "Brouwer's thoughts are incompatible with Russell's", then, by using a *logical term* such as "*incompatible*", I make it unambiguously clear that I am using the word "thought" only in Frege's objective sense. (Popper 1967, p. 110)

I will argue here that Popper and Hayek influenced each other both "subjectively" and "objectively" with regard to the mind–body problem and evolutionary theory.<sup>4</sup> In order to find out who suggested what to whom, dates matter. With correspondence that is not much of a problem. It is more difficult to date unpublished papers and first and later versions of manuscripts. In the case of Popper even the dating of ideas in published work poses a problem since he kept rewriting, and adding to, subsequent editions of his publications. I will refer to Popper's and Hayek's texts and ideas with the year in which, to the best of my knowledge, they were first presented orally or in writing. As a complement to this little piece of intellectual history I will go into some of the similarities and differences in the contents of Popper's and Hayek's ideas. But let me first say something about the background or historical context of their thought.

## Mind and Evolution

From early on, Popper and Hayek were interested in psychology and the philosophy of mind. Later, in the 1960s, both started to elaborate their ideas on evolution. I will argue that a particular idea of Hayek's, on group selection, gave an important impulse to Popper's ideas on evolution. More specifically, Popper took over from Hayek the idea of group selection and transformed it into a theory of ecological niches. Popper's and Hayek's ideas on evolution are interesting in their own right. No less interesting is the fact that they originated out of Popper's criticism of Hayek's theory of mind. That criticism has also far-reaching consequences for the rather widely diffused idea that they agreed on almost everything, but this is not the place to go into this.<sup>5</sup>

Popper's and Hayek's interest in the theory of mind and psychology dates from long before they first met, which was, apparently, in 1936.<sup>6</sup> In their early work, they both investigated the relationships between psychology and the philosophy of mind on the one hand, and epistemology on the other. Popper submitted a doctoral dissertation on the methodological problems of cognitive psychology in 1928.<sup>7</sup>

Both men were influenced by Moritz Schlick's *Allgemeine Erkenntnislehre*, but they find themselves on opposite sides of Schlick's thought: Popper rejects Schlick's idea that mental processes can be reduced to physical processes<sup>8</sup>; Hayek makes it the starting point of his theory of mind. He does so in a manuscript that dates from 1920, "Beiträge zur Theorie der Entwicklung des Bewusstseins".<sup>9</sup> That text was elaborated, without fundamental changes, into *The Sensory Order*, published in 1952.<sup>10</sup> I will argue that this book played an important role in the exchange of ideas between both authors.

Finally, both combined an interest in cognitive processes with biology, but as I mentioned above, neither actively elaborated a theory of evolution until much later in their careers. Hayek, however, mentions the need for an evolutionary approach in *The Sensory Order*.

The book describes the working of the human mind in terms of the development of the connections of neural networks. Although Hayek does not give an evolutionary explanation of the mind, he puts it on the research agenda:

[I]t should be pointed out ... that in one respect in which the task we are undertaking is most in need of a solid foundation, theoretical biology is only just beginning to provide the needed theoretical tools and concepts. An adequate account of the highly purposive character of the action of the central nervous system would require as its foundation a more generally accepted biological theory of the nature of adaptive and purposive processes than is yet available. (Hayek 1952, 4.5)

In later years, Hayek dedicated himself to evolution, though not evolution in general. What he did instead was to elaborate the idea that the evolution of the mind and of human behaviour has an important social component. He first did so in "Notes on the Evolution of Systems of Rules of Conduct; The Interplay between Rules of Individual Conduct and the Social Order of Action" of 1967. Behavioural rules are presented as originating in the mental processes that Hayek had described in *The Sensory Order*. The article elaborates the ideas of that book in the direction of theory of cultural evolution.

Four years earlier, in "Rules, Perception and Intelligibility", he had investigated the function of rules. There, Hayek gives a description of how rules guide human perception and behaviour, and to what extent the communication and learning of rules involve a shared mental framework. He argues that the "ground rules" that enable individuals to communicate and interact are part of a complex and complementary set of rules. These rules are mostly implicit, which implies that they cannot be consciously taught.

In a letter of July 18, 1962, Popper expresses his admiration for the manuscript of the *Rules* article ("the first pages, which greatly impressed me", "your very beautiful paper", "I am greatly impressed by this paper"). He is particularly full of praise of the last section, "Supra-conscious Rules and the Explanation of Mind": "Section 9 is the best, in my opinion. It is really exciting..." And after writing that the ideas expressed there would need to be elaborated, he corrects himself by saying that this might "spoil the impression of freshness", adding: "As it is, it is most excellent."<sup>11</sup> Why is Popper so enthusiastic? Before addressing this question, I would first like to discuss another matter.

## The Mind–Body Problem

In the same letter Popper gives Hayek the advice to make the text of the paper independent from *The Sensory Order* and to put references to the book in the footnotes. That suggestion is not as innocent as it looks. In order to see this, let us consider Popper's reaction to *The Sensory Order* just after it had been published. In a letter in which he thanks Hayek for sending him a copy of the book, Popper writes:

I am not sure whether one could describe your theory as a causal theory of the sensory order. I think, indeed, that one can. But then, it would also be the sketch of a causal theory of the mind.

But I think I can show that a causal theory of the mind cannot be true (although I cannot show this of the sensory order); more precisely, I think I can show the impossibility of a causal theory of the human language (although I cannot show the impossibility of a causal theory of perception).

I am writing a paper on the impossibility of a causal theory of the human language, and its bearing upon the body–mind problem, which must be finished in ten days. (letter of 2 December, 1952, Hayek Archives, box 44/1, Hoover Institution on War, Revolution and Peace, emphasis deleted, missing parenthesis supplied)

It is not immediately clear what Popper means by a causal theory of the mind. His discussion in the paper that he refers to suggests that it includes determinism and a behaviouristic stimulus-response or inductivist model of the mind. And certainly, even though he does not say so in his letter, Hayek's neural-network model of the mind must have had too inductivist a flavour for Popper's taste.<sup>12</sup>

The article was published in 1953 as "Language and the Body–Mind Problem."<sup>13</sup> Popper criticizes all theories of the mind–body problem that combine a denial of the existence of the mental and the physical as distinct realms with the thesis that we need two different languages to talk about them. Hayek's theory belongs to this category. His theory of mind is a physicalistic identity theory, and his "language dualism" is based on the idea that we, with our human minds, can only talk about mental phenomena in a psychologistic language that cannot be fully translated into physical terms.

Popper criticizes this position by arguing that if the two languages are not intertranslatable, they are about different kinds of facts or events. His main criticism, however, is that a causal, physicalistic theory of linguistic behaviour cannot explain the higher functions of language, *viz.*, description and argumentation. This goes back to Karl Bühler, who distinguishes the expressive, the signalling and the descriptive functions of language. To these three Popper has added the argumentative function. In addition, Popper argues that the impossibility of behaviourism, epiphenomenalism, psycho-physical parallelism, the two-languages theory, physicalism and materialism to explain argument or criticism makes these theories self-defeating. This is because the arguments of these theories imply the non-existence of arguments.

Then Popper goes on to show that even very complex machines that seem to simulate human linguistic behaviour are limited to expressing their inner

states and to signalling (for instance the temperature in the case of a thermometer); any possible semblance of description or argument would be due to the intentions of the human designer of the machine. According to Popper, this also solves the “problem of other minds”: “In arguing with other people ... we cannot but attribute to them intentions, and this means mental states.” (Popper 1953, p. 297)<sup>14</sup>

In the light of what happened next, Popper’s introduction of the machine argument is particularly interesting. Hayek started working on a thought experiment with two communicating machines or automata (Hayek refers to von Neumann) that function according to the principles of his theory of mind.<sup>15</sup> He shows that these systems<sup>16</sup> are capable of performing the first two functions of language in Bühler’s hierarchy,<sup>17</sup> expressing their internal states and signalling them. His purpose is to show that these machines are also capable of description. So it looks as if Hayek took Popper’s criticism very seriously indeed.

But he failed to counter it. He reminisces:

[I]n the first few years after I had finished the text of [*The Sensory Order*], I made an effort to complete its formulations of the theory in one respect. I had then endeavoured to elaborate the crucial concept of “systems within systems” but found it so excruciatingly difficult that in the end, I abandoned the longish but unfinished paper that apparently nobody I tried it upon could understand. (Hayek 1982, p. 290)

Hayek is referring to a manuscript with the title “Within System and about Systems; A Statement of Some Problems of a Theory of Communication”<sup>18</sup>. That Hayek was wrestling with the subject matter may be gathered from the often convoluted (even for Hayek) formulations – and from the fact that the manuscript is indeed unfinished. The text breaks off in the middle of an argument that serves to demonstrate that two causal systems are capable of communicating descriptions to each other. The systems stand for minds, and description is modelled as the capability of one system to generate “signs” (which unlike symptoms and signals stand for the same classes of events for the emitting and the receiving systems) capable of changing the other system’s dispositions to act. Not only is the part on description unfinished, a discussion of the argumentative function of language is completely lacking. The abrupt end of the manuscript is consistent with Hayek’s comment that he abandoned work on the paper; he had failed to explain one or more higher functions of language within his own theory – *as Popper had predicted!*<sup>19</sup>

In case any doubt remains as to whether the manuscript is a reaction to Popper’s criticism, here are some further arguments. The first sentence states the purpose of the paper as “deriving from the study of certain kinds of causal systems conclusions concerning the character of our possible knowledge of our mental processes.” Not only is this the problem Popper had argued could not be solved, it also mentions causal systems, an expression of Popper’s which Hayek had never used before. On the next page Hayek writes that he will concentrate on communication and particularly description “because these raise in the clearest form the problems involved when we talk about mental phenomena...”. Neither communication nor description were discussed in *The Sensory Order*, and now Hayek mentions them as the main problems of the paper (p. 19). On p. 3 he states that his endeavour will be successful

if he can reproduce an instance of what “such mental functions as “thinking” or “having an intention”, or “naming”, or “describing”, or “communicating meaning”, or “drawing an inference” and the like” have in common. Hayek does not mention arguing, but all the other functions are mentioned in Popper’s article. Hayek had never before written about describing or naming, to which Popper dedicates an entire section of his article.<sup>20</sup>

## The Tale of the Framework

Let me now turn to the question why Popper, despite his criticism of *The Sensory Order*, had such a high opinion of Hayek’s *Rules* article, that was based on the same book. In the last section, which Popper specially liked, Hayek elaborates some of the ideas of the *Systems* manuscript. The main message is that “[i]f everything we can express (state, communicate) is intelligible to others only because their mental structure is governed by the same rules as ours, it would seem that these rules themselves can never be communicated” (*Rules*, pp. 60–61). It is not clear what Hayek exactly means; he seems to vacillate between two different positions. In some passages he suggests that we cannot explicitly state the rules upon which the working of the human mind and communication are based *as a matter of principle*, or absolutely (“never”). In others, however, he seems to intend that we cannot make *all* such rules explicit *at the same time*; in order to discuss, or analyze, these “constitutional rules”, we must always take some others as given.<sup>21</sup>

Is the reason for Popper’s praise the bare fact that Hayek had responded to his criticism of Hayek’s theory of mind (in public, moreover, which was highly unusual between the two friends<sup>22</sup>)? That seems too futile a motive to me. I find it more plausible that Popper saw in Hayek’s discussion an elaboration of something that he himself had suggested in “Language and the Body–Mind Problem” and that I have already referred to above, *viz.*, the idea that we cannot communicate (Popper says “argue”) with others unless we attribute mental states to them. Hayek’s argument in *Rules* is an elaboration in the sense that it emphasizes the need for *similar* mental states. He formulates this in terms of the existence of general rules that communicating individuals must share, and he raises the problem whether this implies that such rules cannot be communicated themselves.

An alternative explanation of Popper’s praise is that Hayek’s emphasis on the “absolute” impossibility to communicate the common framework that we need for communication set Popper thinking about the ideas that he later published as “The Myth of the Framework”.<sup>23</sup> In fact, it is on this point that Popper in his letter shows himself to be critical, saying that the unconscious frame of assumptions “can become conscious, especially if it is challenged and criticized; it is criticizable in principle, otherwise we end in relativism (and in Mannheim’s Total Ideology!).”

This last conjecture takes us back to Hayek’s theory of mind, on which the *Rules* article is based. One of the conclusions of *The Sensory Order*, namely, is that the human mind can never fully understand or explain its own working. Hayek draws

this conclusion from the following premises: (1) The mind is a (dynamic) system of hierarchical classifications. (2) Explanations are classifications. (3) In order to classify something, that which does the classifying must have a higher degree of complexity than that which is classified. Therefore, the most that we can achieve are “explanations of the principle” according to which the mind works. From this argument<sup>24</sup> Hayek arrives at a stronger conclusion than Popper:<sup>25</sup> for explanations in the social sciences, we have to assume the similarity of minds: “In the study of human action, in particular, our starting point will always have to be our direct knowledge of the different kinds of mental events, which to us remain irreducible entities.” (Hayek 1952, 8.88).

## Cultural Evolution

If, as Hayek had argued in *Rules*, the ground rules on which communication is based are implicit, they cannot be taught explicitly. So, how are they transferred? He only gives a very partial answer in the next article on rules, *Evolution of Rules* (Hayek 1967). He mentions – in passing – imitation (“a process of which we know very little”, p. 78) as a learning process.<sup>26</sup>

His main interest in the article is the question how rules of individual conduct may give rise to a stable social order. In his discussion Hayek invokes an evolutionary framework. Rules of behaviour are *transferred* from one individual to another; but which rules survive, or are *selected*, depends on how efficiently the group that is sustained by a particular pattern of behavioural rules reacts to changes in its internal and external environment. What Hayek discusses here is cultural evolution and group selection.

Individuals may not even be aware of what rules are conducive to the survival of the group to which they belong. The mechanism of selection is impersonal and we are usually not even aware of it; more efficient groups replace less efficient ones. The survival of the group depends partly on the capability of a particular complex of rules to correct internal or external disturbances of the stability of the group. Earlier, in “Individualism: True and False” (1945), Hayek had argued that not only are individuals often not aware of the behavioural rules on which the survival of the social order depend, they may even endanger the survival of the spontaneously emerged order if they deliberately try to change the rules on which it is based.

Rules are interdependent or complementary (which is why Hayek speaks of systems of rules); a change in one particular rule may increase or diminish the contribution other rules make to the stability of the order:

[C]hanges in one rule may make beneficial other changes, *both of a behavioural or somatic character*, which before were harmful. It is thus likely that even culturally transmitted patterns of individual behaviour (or the resulting patterns of action of the group) may contribute to determine the selection among *genetic changes of a behavioural or somatic kind*. (Hayek 1967, p. 71, my italics)

The reference to genetic and somatic aspects looks out of place in Hayek's discussion of social or cultural evolution. I suggest that they indicate Popper's influence: the emphasized passages show a striking resemblance with the distinction that Popper had introduced in "The Aim of Science" of 1957 and "Evolution and the Tree of Knowledge" of 1961 (both published in *Objective Knowledge*) under the name of genetic dualism. Popper had introduced this idea in order to answer the question why, despite the randomness of variations, evolutionary processes often look as if they were guided by a goal or a purpose (an idea known as orthogenesis). Genetic dualism refers to the idea that each organism has two distinct parts: one part controls behaviour, the other executes the behaviour. Mutations in the one take place independently from mutations in the other. Popper's hypothesis is that variations in the control system have a lead over those in the executive system. This means that changes in the executive system without changes in the control system may get out of hand (literally "run out of control") and hence are likely to be lethal. Changes that first occur in the control system, on the other hand, do not have to be lethal; they may provide an adequate governance of parts of the executive system that are already active, or they may activate dormant parts of the executive system. Popper says that this hypothesis may be tested. That would imply that the theory of evolution is a scientific theory and not a historical prophecy, as he had argued in *The Poverty of Historicism*.

It seems clear that Hayek took over the distinction between (possibly genetic) changes in behavioural and somatic features from Popper. What he did not adopt, however, was the idea that the former need to precede the latter to make a positive contribution to survival value. Instead, he concentrates on the role of the group in the selection of rules of behaviour, an example of what Campbell later has called downward causation.<sup>27</sup>

## More on Popper on Evolution

In *The Poverty of Historicism* Popper discusses evolution in the context of his criticism of the idea that in the social realm there are laws of evolution (a position which he calls historicism). What he says there about the theory of evolution in general is that evolution is a unique process and therefore no law of evolution exists. In 1961, in "Evolution and the Tree of Knowledge", Popper starts to investigate the implications of an evolutionary approach to knowledge. What he writes about the evolution of knowledge is inspired by his criticism of inductivism. His aim is to develop a "largely [*sic*] Darwinian theory of the growth of knowledge" (Popper 1961, p. 261) which should be capable of "simulating" Lamarckian explanations of the growth of knowledge, i.e., explanations that appeal only to instruction. What Popper means is that the Darwinian approach to knowledge should be capable of explaining everything that a Lamarckian account can explain. In addition, it should explain some phenomena that Lamarckism cannot explain. (Popper had discussed this "principle of correspondence" between two theories in general in "The Aim of



Science,” 1957, Chap. 5 of *Objective Knowledge*.) He repeats the idea that he had already expressed in *The Poverty*, viz., that the theory of natural selection describes a unique historical process and that hence there is no universal law of evolution.

“Of Clouds and Clocks” (1965) addresses the problem how abstract entities (such as rules) can influence physical reality. Popper calls this Compton’s problem. He considers it to be more important than Descartes’ problem, by which he means the question how the mind can influence bodily processes. According to Popper, Compton’s problem presupposes physical indeterminism. It has been solved by the evolution of the higher functions of language, which allow for better control of the world. Abstract meanings and contents are products of this process of linguistic evolution.

In the development of his ideas on evolution, Popper keeps moving back and forth between the evolution of knowledge and evolution in general. *Epistemology Without a Knowing Subject* (1967) starts with a discussion of the evolution of knowledge and then generalizes this to biological evolution. For instance, Popper describes animals and plants as problem solvers. It is in this context that he introduces ecological niches, which he describes as potentialities that may be activated by the discovery of new solutions. What he means is that a new theory, a new organ or a new behavioural pattern may be adapted to an existing environment that the organism had not discovered so far.<sup>28</sup> Popper hints at the selective influence of the environment: “selection is a two-edged sword: it is not only the environment that selects and changes us – it is also we who select and change the environment, mainly by discovering a new ecological niche.” (p. 149). As far as I know, this is Popper’s first attempt to integrate ecological niches, and what amounts to group selection (he does not use the expression), in his own, neo-Darwinian, theory of evolution.

Or should we call it Popper’s neo-Darwinian research programme? Popper had already shown a (mainly methodological) interest in the theory of evolution in *The Poverty*. The discussion above provides arguments for thinking that Popper’s interest in the content of evolutionary theory was revived as a consequence of discussions with Hayek, especially between 1962 and 1967. In the latter year both Popper and Hayek introduced the idea of group selection in their publications.

In 1969 Popper gave a series of lectures on the mind–body problem at Emory University (published in 1994 as *Knowledge and the Body–Mind Problem*). The third lecture, “World 3 and Emergent Evolution”, contains the most complete statement of Popper’s theory of evolution. He introduces it by drawing attention to the similarities between his theory of World 3 – more in particular the idea that it is both man-made and autonomous – on the one hand, and ecological niches on the other. Popper says that these are the two “decisive insights” that led him to publish his ideas about World 3 and evolution (Popper 1969, p. 52). The theme of the autonomy of World 3, in the form of the descriptive and argumentative functions of language, had played an important role in his criticism of Hayek’s theory of mind. As I have argued above, Hayek’s theory of cultural evolution in *Evolution of Rules* arose out of Hayek’s reaction to this criticism. My conjecture is that Popper adopted group selection from this article and transformed it into a theory of the

emergence and selective influence of ecological niches.<sup>29</sup> In doing so, Popper wanted to demonstrate that evolutionary theory, which now includes group selection, has no need for Lamarckian instruction, even in the cultural domain (which is what Hayek discusses in *Evolution of Rules*<sup>30</sup>).

But Popper does not limit himself to a criticism of Lamarckism. In the same lecture he tries to repair what he thinks are defects of neo-Darwinism. As such he considers its failure to explain specific mixes between hereditary stability and variability. Popper also criticizes the idea, known as evolutionary ascent, that the organisms that survive in the course of evolution are the higher forms of life. His argument is that higher life forms are not always more adapted than more primitive organisms, and he mentions overspecialized organisms in a changing environment as a counterexample. He also repeats his earlier criticism that if adaptation is defined as the ability to survive, evolutionary theory is a tautology. The theory of evolution that Popper presents answers these criticisms, and integrates ideas that he had proposed earlier. Its main features are as follows. All organisms are problem solvers whose instruments, or solutions, consist of schemes of behaviour which they try to adapt. Behavioural schemes (or complexes of rules, as Hayek would have called them), the most important of which is language, are part of the control system. The control system and variations in it are more important than anatomical variations (or variations in the executive system). Mutations in the latter can only be successful if they are compatible with a behavioural scheme that already exists (genetic dualism).

There is a hierarchy in evolution in which parts act as possible solutions to problems of the wholes to which they belong: individual organisms are the “instruments” of the species just like types of behaviour are the instruments of individuals.

Behavioural novelty is caused by changes in the aims or preferences of individual organisms, and these “behavioural spearheads” precede anatomical changes (or changes in the executive system). New behavioural patterns have the advantage that they may be tried out “virtually” in the mind before being applied. So they do not immediately endanger the survival of the individual. In addition, not all problems are problems of survival.

Behavioural novelty may create new ecological niches. These in their turn will act as units of selection. New ecological niches are examples of emergent evolution.

Finally, evolution does not proceed from primitive to superior or higher organisms, but from less to more complex ones. This is the major prediction of evolutionary theory. Since it is falsifiable, evolutionary theory is a scientific theory.

The spearhead idea was developed further in a paragraph that Popper added to “Evolution and the Tree of Knowledge” when it was published in *Objective Knowledge* in 1972. Its title is “The Hopeful Behavioural Monster.” Here Popper seeks to complete the integration of his earlier ideas on evolution. First of all he says that in earlier publications he should have talked about genetic pluralism rather than dualism. What he refers to is the idea that evolution may take place at more than two different levels. Thus, while genes are the sources of variation for individual organisms, organisms may be the sources of variation of species, species the

sources of variation of clades, etc. Popper then combines genetic pluralism with a theory that is due to Richard Goldschmidt. It says that whereas most big mutations are lethal, some may survive. Goldschmidt calls these mutations “hopeful monsters”. New and “monstrous” behaviour may be due to genetic mutations, to changes in the ecology of the organism, or may occur “without any observable anatomical novelty.” (Popper 1972, pp. 282–283). But whatever its origin, behavioural monstrosity is less likely to be lethal than anatomical monstrosity.

I thus submit a variation of Darwinism in which behavioural monsters play a decisive part. Behavioural novelty leads, if successful, to the selection of those ecological niches which in their turn operate selectively – operate, that is, to make use of these behavioural novelties and thus exert a selection pressure in a partly predetermined direction: in the direction determined by some genetically possible undetermined *aim*, for example a taste for a new kind of food, or an enjoyment of utilizing light-sensitive regions in the skin. Thus we may get orthogenesis. (Popper 1972, p. 284)

Popper repeats the idea of the primacy of changes in behaviour over changes in the anatomy of the organism:

The anatomical structure can change only slowly, in the main. But its changes will for this reason remain insignificant if they are not guided by those in aim-structure and skill-structure. Thus the evolution of a genetic apparatus establishing a primacy of aim-structure and skill-structure over anatomical structure might, in principle, be explained on Darwinian lines.

It will be seen that this Darwinian theory of hopeful behavioural monsters “simulates” not only Lamarckism, but Bergsonian vitalism also. (Popper 1972, p. 284)

Popper’s emphasis on the slowness of anatomical changes suggests a contrast with the speed with which changes in the control system may succeed each other. As to the evolution of knowledge, he often writes about the importance of inventing our conjectures and correcting them as fast as possible. In general, changes in people’s ideas might explain why cultures evolve more rapidly than biological entities.<sup>31</sup>

## Tradition and Entrenchment

Popper argues that if new forms of behaviour are successful, i.e., if they contribute to the survival of the organism, they may give rise to a *tradition*. Popper defines a tradition as a set of behavioural patterns that remain stable even if and when alternative (and presumably viable) patterns of rules are available. A tradition may degenerate into a *genetically entrenched* pattern of behaviour when viable alternative rules are no longer available or accessible. If a tradition in a particular ecological niche “becomes the spearhead of genetic entrenchment”, the organisms in that niche risk extinction as a consequence of a change in the environment. Popper predicts that every genetic entrenchment becomes lethal in time (Popper 1994, p. 61).

In the context of his lecture Popper is thinking of the extinction of living organisms. But his idea can be generalized using Popper’s own multi-level idea of evolution: preferences may be the spearheads of behavioural patterns, behavioural patterns the spearheads of individual organisms, organisms the spearheads of the

species, etc.<sup>32</sup> If, due to a change in preferences or aims, all behavioural patterns but one are selected against, the set of rules of behaviour (to use a Hayekian term) that was a tradition becomes *behaviourally entrenched*. If the environment of the ecological niche changes, the behavioural pattern may become extinct. I will give a possible example of this in my last Afterthought.

In Popper's later work on evolution the spearhead function of behavioural monsters is not mentioned as such.<sup>33</sup> The theory of the selective power of ecological niches is presented as a form of downward causation.

We may speak of downward causation whenever a higher structure operates causally upon its substructure...

I suggest that downward causation can sometimes at least be explained as *selection* operating on the randomly fluctuating elementary particles. The randomness of the movement of elementary particles – often called “molecular chaos” – provides, as it were, the opening for the higher-level structure to interfere. A random movement is accepted when it fits into the higher-level structure; otherwise it is rejected.

I think that these considerations tell us a lot about natural selection. While Darwin still worried that he could not explain variation, and while he felt uneasy about being forced to look at it as chancelike, we can now see that the chancelike character of mutations, which may go back to quantum indeterminacy, explains how the abstract invariances of the environment, the somewhat abstract selection pressures, can, by selection, have a downward effect on the concrete living organism – an effect that may be amplified by a long sequence of generations linked by heredity. (Popper 1977, p. 147)

Next, Popper passes to human behaviour:

The selection of a kind of behaviour out of a randomly offered repertoire may be an act of choice, even an act of free will. ... A choice process may be a selection process, and the *selection* may be *from* some repertoire of random events, *without being random in its turn*. This seems to me to offer a promising solution to one of our most vexing problems, and one by downward causation. (*ibid.*, italics in the original)

The problem that Popper refers to is the compatibility between the goal-directedness of rationality and the randomness of variations.

## Conclusion

I have argued that Popper transformed Hayek's concept of group selection into a theory about the emergence and the selective power of ecological niches in a Darwinian framework. The niches may emerge from changes in behavioural patterns, and these new rules of behaviour are responses to random<sup>34</sup> variations in the aims or preferences of individual organisms. In order for a theory of cultural evolution to account for the role of human knowledge and creativity it must be indeterministic and non-materialistic.

Popper's theory of behavioural monsters that create ecological niches, which subsequently act as units of selection, may be adapted to the domain of social and cultural evolution. Rational responses of individuals to random changes in their

goals and preferences may give rise to new institutions that influence the survival of a social order. New institutions, and new social orders, if they continue to exist side by side with other institutions or orders, may develop into traditions. If, for some reason, for instance because of the competitive success of a particular tradition, other traditions disappear, the only surviving one becomes entrenched. This, according to Popper, will lead to its extinction when the environment changes in ways to which the entrenched tradition itself has no adequate response, because the individuals that are part of that tradition can no longer resort to others.

## First Afterthought: Back to Hume

Popper's theory of evolution as applied to the social realm has a place for creative individuals who act rationally and critically. Hayek on the contrary stresses that individual man is irrational and that "Reason" is a characteristic of the social system or the culture or the group,<sup>35</sup> i.e., of tradition. In his later work,<sup>36</sup> he develops this into a conservative social philosophy that warns against the dangers of irrational individuals' tinkering with traditions whose function they cannot understand (approvingly referring to David Hume<sup>37</sup>). This stands in stark contrast with what Popper writes about tradition:

The assertion that we have an irrational inclination to be impressed by habit and repetition is something quite different from the assertion that we have a drive to try out bold hypotheses which we may have to correct if we are not to perish. The first describes a typically Lamarckian procedure of instruction; the second a Darwinian procedure of selection. The first is, as Hume observed, irrational, while the second seems to have nothing irrational in it. (Popper 1972, pp. 96–97)

Popper argues that Hume was right in thinking that there is no solution to the logical problem of induction. But he was wrong in thinking that induction is psychologically necessary. From this paradox, as Popper calls it (Popper 1972, p. 95), Hume draws the wrong conclusion that man is irrational and a product of "blind habit" (*ibid.*):

The solution of the paradox is that not only do we reason rationally, and therefore contrary to the principle of induction. We do not act upon repetition or "habit", but upon the best tested of our theories which, we have seen, are the ones for which we have good rational reasons; not of course good reasons for believing them to be true, but for believing them to be the *best available* from the point of view of a search for truth or verisimilitude (Popper 1972, p. 95)

He adds that "[t]his solution of Hume's paradox does not, of course, say that we are thoroughly rational creatures. It only says that there is no conflict between rationality and practical action ..." (*ibid.*).

Popper had raised the same criticism in correspondence with Hayek:

And as to Hume, his irrationalism was based, exclusively, upon his correct finding that induction is impossible – exactly as Polanyi's anti-rationalism. But this perfectly correct finding has no anti-rationalist consequences – except if you are (as Hume and Polanyi) a disappointed inductivist. (letter of 11 November, 1958)<sup>38</sup>

Popper's wording may suggest that he is accusing Hayek of being a disappointed inductivist, too. (I have already suggested in the above that Popper thought that Hayek's theory of mind was inductivist.) Whether or not that was his intention, I think that all differences between Popper and Hayek go back to Hume. Hume's work indicates two directions. Popper took the road of skepticism and anti-inductivism. Hayek took the empiricist and conservative direction.<sup>39</sup>

## **Second Afterthought: How Beneficial Was Popper's Influence on Hayek?**

This article is about Hayek's influence on Popper, which can only be understood by considering Popper's influence on Hayek. I do not want to sign off without adding something about the latter question. In the light of later developments in artificial intelligence, we may ask whether Hayek was not put on the wrong track by Popper's criticism of his theory of mind. Hayek was the first to elaborate a neural-network model of the mind.<sup>40</sup> But Popper gave Hayek a very difficult problem to solve: to explain the descriptive and argumentative functions of language with that same neural-network theory. Given the state of the art in psychology and computer science in the 1950s, that problem would have been too difficult even for the (very few) specialists in these fields. It has not even been solved today. Had Hayek limited himself to developing his ideas on learning by neural networks, he might have made important contributions to AI.<sup>41</sup>

## **Third Afterthought: Critical Rationalism: Tradition or Entrenchment?**

In "Popper today: far from forgotten or irrelevant", Ian Jarvie argues that the survival of critical rationalism is endangered by the fact that its practitioners have failed to create the social conditions for a school of thought. That may be explained by Popper's "generalized" idea of entrenchment (see above).

Here is the explanation. By its very nature, critical rationalism attracts philosophers who believe in the force of critical argument. Let us assume (realistically, I think) that the type of personality with a firm belief in the power of criticism is less prone than the average academic to devote resources to the social networking and academic politicking that are needed to create an environment in which his or her intellectual offspring can survive. Perhaps it is even contrary to the critically-minded character to sacrifice resources for breeding a sufficiently numerous and robust intellectual offspring at all.

Until recently not only philosophers but some (outstanding) practising scientists (who are more likely to cultivate the social conditions that are necessary to carry on with their work), too, supported critical rationalism. This allowed critical rationalism

to become a tradition for at least a couple of generations. In the mean time, however, the number of scientific disciplines, journals and scientists has increased. The professionalization of science has gone hand in hand with a drop in interest in philosophy on the part of scientists. The academic environment has changed and selection pressures have increased greatly. In this new environment, the support of scientists is lacking while the contents of critical rationalism continue to select against the type of personal characteristics that make it possible for individuals to adopt or have access to at least elements of different traditions. The situation of carriers of the tradition of critical rationalism being incapable of adopting or having access to alternative traditions is tantamount to the non-existence of alternative traditions. If critical rationalists continue to fail to adopt alternative traditions, or at least elements of them, critical rationalism risks becoming entrenched – and extinct.

## Notes

1. I thank Gunnar Andersson for critical comments and Rob de Vries for a suggestion that led to my second afterthought.
2. Hacoen (2000), see the entry “Hayek” in the index.
3. Caldwell (2006).
4. A fuller comparison between Popper and Hayek can be found in Birner (2001), on which the current article is based. Parts of it have been published in Birner (1999).
5. I will briefly discuss this below, in the First afterthought. For a more complete analysis cp. Birner (2001).
6. Cp. Hacoen (2000, p. 316).
7. “Zur Methodenfrage der Denkpsychologie”; cp. Hacoen (2000, Chap. 4, and particularly pp. 156–163).
8. Cp., for instance, Hacoen (2000, p. 158) and further below.
9. “Contributions towards a theory of the development of consciousness.”
10. For a discussion of Hayek’s manuscript and its history, including the role of Schlick, see Birner (2004).
11. Popper repeated his praise after he had received an offprint of the published article (letter to Hayek of November 11, 1962, Hayek Archives, Hoover Institution on War, Revolution and Peace, box 44/2).
12. Cp. for instance Popper’s criticism of “[c]lassical epistemology which takes our sense perceptions as “given,” as the “data” from which our theories have to be constructed by some process of instruction...” (Popper 1967, p. 145). This has always been his criticism, but in this particular article he adds that this “can only be described as pre-Darwinian. It fails to take account of the fact that the alleged data are in fact adaptive reactions, and therefore interpretations which incorporate theories and prejudices and which, like theories, are impregnated with conjectural expectations...” (*ibid.*).
13. In *Proceedings of the XIth Congress of Philosophy*, 7, North-Holland, pp. 101–107. It was later included in *Conjectures and Refutations*. The main criticism contained in it is repeated, in stronger terms, in *The Self and Its Brain* (Popper and Eccles 1977, p. 58).
14. Attributing mental states to individuals is also a necessary part of explanations in social science and historiography, as Popper argues in *The Poverty of Historicism* and *The Open Society and Its Enemies*.
15. This, together with the neural-network theory of *The Sensory Order*, entitles Hayek to be recognized as one of the pioneers of artificial intelligence. For a possible reason why he wasn’t, cp. my “Second afterthought” below.



16. Which he defines, with a reference to von Bertalanffy, as “coherent structure[s] of causally connected parts” (p. 4).
17. On p. 20 Hayek refers to Popper for the English translations of Bühler’s terminology.
18. Hayek Archives, box 94/51, Hoover Institution on War, Revolution and Peace.
19. The first page of the typescript bears the comment in Hayek’s handwriting: “seems incomplete.” The ms. breaks off in the middle of a sentence. That may suggest that part of it has been lost (perhaps it has been filed in a different folder). But even though some part may have gone physically missing, Hayek’s own comments seem convincing.
20. That Hayek did not take Popper’s rejection of his theory of mind and psychology of perception lightly may also be concluded from the fact that years later he writes to Popper that he feels “that some day you ought to come to like even my psychology” (letter of 30 May, 1960, Hayek Archives, Hoover Institution on War, Revolution and Peace, box 44/2).
21. Cp. for instance: “there will always be some rules governing a mind which that mind *in its then prevailing state* cannot communicate...” (p. 62, my italics).
22. And admitting that he had failed to answer it. Cp. the last sentence of the last paragraph of *Rules*, where Hayek says that the task of bringing his endeavour to a satisfactory end “would exceed ... the powers of its author.”
23. After Kuhn’s philosophy of science had made a criticism of the argument more urgent?
24. One should have expected Popper not to share Hayek’s premises because he considers explanation to be more than classification. But he found Hayek’s “impossibility theorem” worthy of mention in *The Self and Its Brain*. It should be noted, however, that Popper replaces Hayek’s reference to classification by explanation: “It has been suggested by F.A. von Hayek (1952, p. 185) that it must be impossible for us ever to explain the functioning of the human brain in any detail since “any apparatus ... must possess a structure of a higher degree of complexity that is possessed by the objects” which it is trying to explain.” (Popper and Eccles 1977, p. 30).
25. See the text to Note 14.
26. Much later, in *The Fatal Conceit*, the mechanisms mentioned are tradition, learning and imitation (Hayek 1988, p. 12).
27. Cp. Campbell (1974).
28. “A new tentative solution ... may discover a new virtual ecological niche and thus may turn a virtual niche into an actual one.” (Popper 1967, p. 145).
29. Without attracting much attention. Commentators are divided on the question whether group selection gained a foothold in biology. Thus, Hertel says it hardly did (Hertel 1997), while Gould dedicates many pages of his monumental *The Structure of Evolutionary Theory* to “the revival” of group selection (p. 624), “not[ing] with delight that [it] has risen from the ashes to receive a vigorous rehearing ...” (Gould 2002, p. 623) from the 1970s and 1980s.
30. Hayek attached great importance to group selection. In an unpublished ms. with the title “The origins and effects of our morals: a problem for science” (box 96/126 of the Hayek Archives at the Hoover Institution of War, Revolution and Peace) he writes: “A chapter in the draft of a book on cultural evolution [*The Fatal Conceit*] threatened to explode in the process of revision ... till I found the key to mastering the unmanageable complexity of the process by making the starting point a fact I had already established in an earlier chapter. ... Namely that cultural evolution, *unlike Darwinian evolution*, rests almost entirely on group selection instead of the selection of individuals. What has sometimes been called a “collective mind” group [*sic*] is nothing but the common morals of its members, something very different from and autonomous of the mind proper though of course in constant interaction with it.” (I have added italics to draw attention to an important difference between Hayek and Popper, who thinks he can fit group selection into a Darwinian framework.)
31. Those who are familiar with the work of Stephen Jay Gould will have noticed many similarities between the latter’s theory of evolution and Popper’s. This is not the place to go into this; suffice it to say that neither refers to the other.
32. Gould (2002) dedicates much space to multi-level evolution. Cp. for instance Chap. 9, “Species as Individuals in the Hierarchical Theory of Selection.”

33. According to Watkins, Popper said that he had forgotten about it (Watkins 1995, p. 192). That should perhaps not be taken too literally. In Popper (1977) the control function of the mind and language play an important role. That Popper does not mention the complementary executive parts of functions of organisms may be explained by the fact that the lecture is an evolutionary defence of an interactionist theory the mind–body problem. It is not about evolution in general.
34. I think much confusion in the discussion of cultural evolution and the evolution of knowledge could be avoided by using the term “isotropic” instead of random, as Gould proposes. Isotropic variation is “variation [that is] unrelated to the direction of evolutionary change; or, more strongly, ... nothing about the process of creating raw material biases the pathway of subsequent change in adaptive directions.” (Gould 2002, p. 144, italics deleted)
35. “[H]uman Reason, with a capital R, does not exist in the singular, as given or available to any particular person, as the rationalist approach seems to assume, but must be conceived as an interpersonal process in which anyone’s contribution is tested and corrected by others.” (Hayek 1945, p. 15)
36. Cp. for instance *Law, Legislation and Liberty*.
37. Hume is one of Hayek’s intellectual heroes. Cp. for instance Hayek 1945, 1963a.
38. Hayek Archive, Hoover Institution on War, Revolution and Peace, box 44/1.
39. This is elaborated in Birner 2001. As is argued there, Popper is, of course, an empiricist, too. But his empiricism goes together with the “principle of theoretism”; (cp. Milford 1994, p. 336). Hayek prides himself on his radical empiricism, saying that all previous empiricist efforts to explain knowledge had not been empiricist enough; (cp. Hayek 1952, Chap. 8, p. 27).
40. I remind the reader that he did so in 1920.
41. Hayek may also have been discouraged from developing his ideas in this direction because Donald Hebb’s *The Organization of Behavior*, which contains a neural network model of the mind that is very similar to Hayek’s, was published 3 years before *The Sensory Order*. Cp. Hayek’s comment in the Preface to *The Sensory Order*.

## References

- Bartley, III, William Warren. 1976. Critical Study. The Philosophy of Karl Popper. Part I: Biology & Evolutionary Epistemology. *Philosophia*, 6(3–4): 463–494.
- Bartley, III, William Warren. 1978. Critical Study. The Philosophy of Karl Popper. Part II. Consciousness and Physics. Quantum Mechanics, Probability, Indeterminism, The Body–Mind Problem. *Philosophia*, 7(3–4): 675–716.
- Birner, Jack. 1999. Making Markets. In *Economic Organisation and Economic Knowledge: Essays in Honour of Brian Loasby*, eds. Sheila C. Dow and Peter E. Earl, pp. 36–56. Cheltenham: Edward Elgar.
- Birner, Jack. 2001. The Mind–Body Problem and Social Evolution. Unpublished paper, presented at the workshop on The Nature and Evolution of Institutions, Max Planck Institute of Economics, Jena, January.
- Birner, Jack. 2004. *F.A. Hayek’s The Sensory Order. An Evolutionary Perspective*. Unpublished paper.
- Caldwell, Bruce. 2006. Popper and Hayek: Who Influenced Whom? In *Karl Popper: A Centenary Assessment*, eds. Ian Jarvie, Karl Milford, and David Miller, Vol. I, pp. 111–124. Aldershot: Ashgate.
- Campbell, Donald. 1974. “Downward Causation” in Hierarchically Organized Biological Systems. In *Studies in the Philosophy of Biology*, eds. Francis Ayala and Theodore Dobzhansky, pp. 179–186. London: Macmillan.
- Gould, Stephen Jay. 2002. *The Structure of Evolutionary Theory*. Cambridge, MA: The Belknap Press of Harvard University Press.

- Hacohen, Malachi Haim. 2000. *Karl Popper – The Formative Years, 1902–1945. Politics and Philosophy in Interwar Vienna*. Cambridge: Cambridge University Press.
- Hayek, Friedrich August. 1920. Beiträge zur Theorie der Entwicklung des Bewusstseins. Hayek Archives, Hoover Institution on War, Revolution and Peace, Box 92/1.
- Hayek, Friedrich August. 1937. Economics and Knowledge. In *Individualism and Economic Order*, ed. Friedrich August Hayek, pp. 33–56. London: Routledge (1949).
- Hayek, Friedrich August. 1945. Individualism: True and False. In *Individualism and Economic Order*, ed. Friedrich August Hayek, pp. 1–32. London: Routledge (1949).
- Hayek, Friedrich August. 1952. *The Sensory Order. An Inquiry into the Foundations of Theoretical Psychology*. Chicago University Press.
- Hayek, Friedrich August. 1963. Rules, Perception and Intelligibility. In *Studies in Philosophy, Politics and Economics*, ed. Friedrich August Hayek, pp. 43–65. Chicago, IL: University of Chicago Press (1967).
- Hayek, Friedrich August. 1963a. The Legal and Political Philosophy of David Hume. In *Studies in Philosophy, Politics and Economics*, ed. Friedrich August Hayek, pp. 106–121. Chicago, IL: University of Chicago Press (1967).
- Hayek, Friedrich August. 1967. Notes on the Evolution of Systems of Rules of Conduct. In *Studies in Philosophy, Politics and Economics*, ed. Friedrich August Hayek, pp. 66–81. Chicago, IL: University of Chicago Press (1967).
- Hayek, Friedrich August. 1979. Law, Legislation and Liberty. In *The Political Order of a Free People*, Vol. III. London: Routledge & Kegan Paul.
- Hayek, Friedrich August. 1982. The Sensory Order After 25 Years. In *Cognition and the Symbolic Process*, eds. Walter Weimer and David Palermo, Vol. 2, pp. 287–293. Mahwah, NJ: Lawrence Erlbaum.
- Hayek, Friedrich August. 1988. *The Fatal Conceit. The Errors of Socialism*. London: Routledge.
- Hebb, Donald. 1949. *The Organization of Behavior*. New York: Wiley.
- Hertel, Rainer. 1997. Was kann die Evolutionsbiologie zur Diskussion der Ethik beitragen? In *Anthropologie und Ethik. Biologische, sozialwissenschaftliche und philosophische Überlegungen*, Hrsg. Jean-Pierre Wils, pp. 148–175. Marburg: Francke Verlag.
- Milford, Karl. 1994. In Pursuit of Rationality. A Note on Hayek's *The Counter-Revolution of Science*. In *Hayek, Co-ordination and Evolution*, eds. Jack Birner and Rudy van Zijp, pp. 323–340. London: Routledge.
- Popper, Karl Raimund. 1953. Language and the Body–Mind Problem. In *Conjectures and Refutations. The Growth of Scientific Knowledge*, ed. Karl Raimund Popper, pp. 293–298. London: Routledge & Kegan Paul (1963).
- Popper, Karl Raimund. 1957. *The Poverty of Historicism*. London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1961. Evolution and the Tree of Knowledge. In *Objective Knowledge. An Evolutionary Approach*, ed. Karl Raimund Popper, pp. 256–284. Oxford: Clarendon (1972).
- Popper, Karl Raimund. 1965. Of Clouds and Clocks. An Approach to the Problem of Rationality and the Freedom of Man. In *Objective Knowledge. An Evolutionary Approach*, ed. Karl Raimund Popper, pp. 206–255. Oxford: Clarendon (1972).
- Popper, Karl Raimund. 1967. Epistemology Without a Knowing Subject. In *Objective Knowledge. An Evolutionary Approach*, ed. Karl Raimund Popper, pp. 106–152. Oxford: Clarendon (1972).
- Popper, Karl Raimund. 1972. Two Faces of Common Sense. In *Objective Knowledge. An Evolutionary Approach*, ed. Karl Raimund Popper, pp. 32–105. Oxford: Clarendon (1972).
- Popper, Karl Raimund. 1977. Natural Selection and the Emergence of Mind. First Darwin Lecture, Darwin College, Cambridge. In *Evolutionary Epistemology, Rationality, and the Sociology of Knowledge*, eds. Gerhard Radnitzky and William Warren Bartley, III, pp. 139–156. Lasalle, IL: Open Court (1987).
- Popper, Karl Raimund and Eccles, John Carew. 1977. *The Self and Its Brain*. Berlin: Springer International.
- Popper, Karl Raimund. 1994. Knowledge and the Body–Mind Problem. In *Defence of Interactionism*, ed. Michael Notturmo. London: Routledge.
- Watkins, John. 1995. Popper and Darwinism. In *Karl Popper: Philosophy and Problems*, ed. Anthony O'Hear, pp. 191–206. Cambridge: Cambridge University Press.

# Popperian Individualism Today

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**Abstract** Popper's original vision of the open society is criticized as being, in a certain sense, utopian. Discussion alone will not resolve fundamental political differences, particularly where those differences bear on the premises of liberalism itself. It is noted that Popper later presented a more nuanced view of openness and liberalism, one which sees these concepts as embedded in a tradition of political thought and practice, and in a substantive and not merely a procedural political world view. New problems for the application of this world view in Western democracies are raised by the growth in recent decades of significant groups within Western societies who do not share its assumptions. Possible responses on the part of defenders of the open society to this new situation are considered.

In this article, I will consider some of the themes of Karl Popper's *The Open Society and Its Enemies* (Popper 1945), attempting in the first instance to criticize some of them. I shall indeed argue that there is a sense in which Popper's own ideas might be found to be dangerously utopian, in that the unrestricted disposition to criticize which he sees as central to the open society will not be enough to hold a society together without some principles or traditions which are regarded as immune to criticism, and hence not subject to Popperian openness. Then, in keeping with the spirit of this book, I will examine how Popper's underlying intuitions about desirable political arrangements might be developed in today's world, a world rather different from that in which Popper was himself writing.

Here there is no space to describe Popper's vision in detail. However it is worth saying that what Popper is doing in contrasting closed societies with open ones is to distinguish between societies which are run by closed groups and those in which all involved have both the right and the ability to criticize and improve what is going on. The closed groups Popper is opposed to might be the enforcers of ancient taboos, which prevent scrutiny of customs and traditions, or they might be rulers guided by an intellectualist ideology, which they think gives them the right to dictate to the rest. In his own discussions of these matters, the two great intellectualist

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enemies of the open society are Plato and Marx, both of whom advocated rule by elites, philosophers or vanguardist as the case may be.

Even if advocates of the closed society are initially well-meaning, their projects are fundamentally flawed, because they are pretending to possess knowledge they cannot have, and by dictating to the rest and repressing their voices, they are silencing the main source of genuine knowledge of the operation of a society, namely the effects of those policies and institutions on those on whom they are imposed. It is a key element of an open society, that those affected by policies and structures should have a voice. In practice most open societies will be democracies, not because all democracies are actually open (Popper was well aware that a majority could tyrannise), but because democracies, in theory at least, have the key instrument of openness, the ability of the ruled regularly and peacefully to get rid of the rulers.

Popper is, of course, interested in the key values of liberalism, such as tolerance, respect for life and property, free speech, the rule of law and so on – which he thinks are most likely to flourish in open societies. But because of his fallibilism and an insistence that all policies will have unknown and unintended consequences, he also thinks that those interested in openness should avoid large-scale blueprints for societies. They should focus on what he calls piecemeal social engineering: the careful identification of manifest ills in society, the formulations of policies to remove these ills, and the scrupulous monitoring of the effects of the policies, so as to counter the unintended and unwelcome consequences which will inevitably occur with any social action or project. So in an open society, what Popper calls negative utilitarianism will be the guiding principle, rather than ambition to produce happiness for all, and certainly rather than what Popper calls utopianism, large-scale planning and social canvas cleaning in the name of some grand or revolutionary project. There will also be a constant attempt on the part of all in an open society to maintain a spirit of openness, in Popper's sense, because he is acutely conscious that in an open society over and above good institutions one needs a spirit of openness in those manning them.

Underlying Popper's conception of the open society are five basic ideas, the first four of which are firmly within the tradition of Enlightenment rationalism and optimism. The first idea is that of the unity of mankind. That is to say, in the open society anyone may criticize and contribute, regardless of origin, race, religion, class or gender. In this context, it is noteworthy that Popper himself was resolutely anti-nationalistic, even to the extent of criticizing the very existence of Israel. He also made himself unpopular in the 1960s by saying that Jews in the Germanic countries should have sought to suppress their differences from Germans and Austrians, so as not to have aroused antagonism (not that Popper had any truck with Pan-Germanism). In this dislike of national differences and distinctions, Popper parallels Kant's views on the topic, though whether, as some have suggested, he was yearning for the palmy days of the structurally multi-cultural post-1848 Austro-Hungarian Empire is perhaps more doubtful.

The second support to openness is the notion of individualism. Popper had an almost pathological hatred of any form of group rule or collectivism (even to the extent of refusing in his 80s to go round a Portuguese palace in a group or 'collective'

as he put it). But in his early political writing in *The Open Society and Its Enemies* (later modifications in some respects notwithstanding), his insistence on individualism, ontological, political and methodological, was not just pathological. It was based on a Kantian sense of the rational autonomy of each individual, for in Kant's scheme of things each individual *qua* individual is endowed with reason and the consequent freedom and duty to decide for him or herself what is morally and politically demanded at any time.

Thirdly, and connected to the first two notions, is that of impartiality. Any view may be worth hearing, whatever its provenance. This is not just an Enlightenment view. For Popper it is also an essential similarity between the open society and the ideal scientific community, in which valid criticism of a scientific theory can come from any quarter. The extent to which a human society, open or not, can actually resemble the community of science is something which will concern us later, but for now it is worth remarking that impartiality in this sense will also generate a degree of tolerance in the sense that one must always be prepared to listen to what Popper characteristically refers to 'the other fellow's point of view', particularly when we realise that the other fellow may be more acutely aware of the failures of my policies or actions than I am. So impartiality and fallibilism have a close connexion here.

Then, fourthly, there is humanitarianism. Popper was horrified from the time of his encounters with revolutionary socialists after the First World War by the readiness of idealists to sacrifice individual lives today in order to hasten the birth of some ideal society in the future. Even if there were a greater chance of the ideal society coming about through revolutionary terrorism than in fact is the case, it could never be justified to treat the lives of others as means to the end, again a striking parallel with Kant for whom we must always treat others as ends in themselves.

The fifth strand to Popper's thought is where, to a degree, he departs from the classical Enlightenment position. For Popper does not believe that the unfettered use of reason is bound to produce truth or absence from error or good policies. He is, in fact, a fallibilist, one who believes that we can never rule out the possibility of error or failure, even in the best regulated and conducted of enquiries or institutions. While Popper does think that progress can be made both in science and in society, for him neither truth nor desirable social outcomes are the inevitable fruit of the use of reason. It is partly because of this that Popper makes criticism and openness so central to the open society, because criticism and openness are the best ways we know of uncovering error and failure in the social sphere. And, as part of his fallibilism about social outcomes, Popper will stress the reality of human freedom, and the way the free decisions of individual human beings affect what happens in both big and small affairs. Not the least of his objections to Marxism is the arrogant assumption of the Marxists that they know how history is going, and that the rest of us simply have to submit both to history and to their superior knowledge of it. It is this idea that history has a definite direction which Popper characterizes as historicism, and he thinks it is nonsense. We are free, and within limits we can act, individually and collectively, sometimes even to change what all the experts confidently assert to be the course of history (as, in their different ways President Reagan and



Mrs. Thatcher and their followers did both in their own countries and in the Evil Empire, and as the Ayatollah Khomeini and his followers did in Iran).

The open society is presented by Popper as the antidote to the nightmares which arise when totalitarian rulers impose on whole populations their utopian visions based on claims to a degree of knowledge and insight they could not possibly have. Popper's open society, by contrast, is not founded on any plan to be imposed centrally and forcibly on the whole of society, nor on unattainable knowledge, supposedly vested in a few visionaries or some party committee furnished with insight into the course of history or the destiny of mankind. It is, by contrast, pluralist; and its underlying philosophy is negative utilitarianism, rather than some positive vision of heaven on earth.

Without denying these criticisms Popper makes of the closed societies he attacks, there are certain key respects in which the open society, as envisaged by Popper, is itself utopian; that is, it has at its heart an unrealised and unrealisable vision of human society, and one which will actually tend to loosen the bonds which in fact tie societies together.

Before turning to this point, however, have we any reason to think that critical rationalism is actually likely to promote a better form of politics than its competitors? One of its supposed advantages, and the one much emphasised by Popper himself, is that it enables disputes in society to be resolved peacefully, and that by discussions and continual monitoring of policies we may get nearer to the truth. All, of course, hangs on what is meant by 'resolved'.

The methods of science, on the whole, tend, for some time at least, to produce a consensus among scientific enquirers. But this is because there is a fairly general agreement within the scientific community about the aims and methods appropriate to science. As Popper himself acknowledged in the original edition of *Logik der Forschung* (*The Logic of Scientific Discovery*), though not in subsequent editions from which the sentence was excised, 'only those sharing the same goal can rationally argue over differences of opinion' (Popper 1935, p. 10). But Popper's open society will precisely not be a community in that sense. In his doctrine of negative utilitarianism he explicitly repudiates the requirement that in an open society there must be any general consensus about the aims of human life, or about ethics more generally. He hopes to secure enough consensus on policies through insisting that any acceptable policy be guided by the principles of negative utilitarianism, that is that it should be aimed at what otherwise unconnected individuals can agree is the removal of manifest suffering and injustice.

But this, too, is dubious. It is hard, if not impossible, to separate the notion of manifest suffering or injustice from some more general conception of what a good life is. What might be the serious oppression of women to one society might be according women their proper role and status in another (or so many Islamists claim about their rules and customs in this area). Is the mere existence of chains of abortion clinics a manifest injustice (to the unborn), as those who see abortion as murder would claim? Or would their absence, and the prevalence of back-street abortions, bring about manifest suffering? Further, permissible means of removing ills are not always ethically neutral either. We can all agree that the existence of



spina bifida is a manifest ill, and that getting rid of it is good. But by any means? By aborting babies with the condition? Many will think not, and others, perhaps against the majority in the West, will think that even if it could produce a cure for some terrible disease or affliction, research on embryos is not a permissible means either.

In such circumstances, decisions on policy will still have to be made. But given such entrenched disagreement at the level of principle as exists in the cases we have considered, and in other cases too in many areas, policy is not likely to be based on agreement. It is more likely to be based on pragmatism, not to put too fine a point on it, on what the majority will put up with. Of course, such pragmatism may help to avoid violence or bloodshed, which is not to be dismissed, but this is a different thing from agreement, let alone a morally good outcome.

On how policies on ethical matters in Western democracies are actually made, see the candid and revealing remarks of Mary Warnock, who has been a legislator as well as a philosopher, in her *An Intelligent Person's Guide to Ethics*. According to Baroness Warnock the morality which rules in legislating in morally contested areas is 'a morality, which though not agreed, is nevertheless broadly *acceptable*', acceptable being just that, what people will in the main accept, something to be distinguished, as she points out, from deciding on the basis of a policy being morally right (or wrong). (Warnock 1998, pp. 49–50) She says that she came to understand that this notion of 'acceptable' was not just 'a civil service cop-out', but was actually crucial to making an important distinction between what she calls private and public (i.e. legislative) morality. We are quite a long way there from any high-minded getting nearer to the truth or goodness, and, although Warnock does not say this herself, far closer to the stealthy if not actually cynical implementation of a morally progressivist agenda which we have seen in legislation over the past half century or so.

In these circumstances the decision-making process is by no means necessarily going to lead to truth or genuine welfare. It is certainly not aimed at truth or welfare. It is aimed rather at consensus and at a sort of tolerance. But just because a majority concurs or pragmatics dictates it, a decision is not thereby right. If abortion, or its criminalisation, or slavery or torture or imprisonment without trial is wrong, then they are wrong, whatever a majority might think, and even if the majority view had arisen from as much openness and discussion as one could imagine or wish.

This problem aside, there is in *The Open Society and Its Enemies* the demand that institutions, policies and principles should be subjected to a permanently critical evaluation, in order to monitor how well they are solving the problems which, according to the Popperian account, they were brought into existence in order to solve. This indeed seems to be the key feature of the open society in its initial formulation. But, and this is my crucial point of disagreement, to think that a disposition to criticize might on its own be enough to hold a community together is itself utopian, and it could be as destructively utopian as some of the other utopias Popper correctly criticizes. For Popper encourages a relentlessly critical cast of mind concerning settled values and institutions – what he calls a social technology whose results can be tested by piecemeal social engineering, in which what we

do and think is treated as hypotheses we invent and then submit to practical tests. He also inveighs against teachers attempting to impose 'higher' values on their pupils, as opposed to stimulating their interest in values. (Popper 1945, Vol. II, pp. 222, 276) But he neither explains how in his regime inexperienced pupils are to be led to acquire habits of virtue, without which moral education will not take place, nor does he suggest any limits on which policies and institutions are to be subjected to trial and error, as if they were scientific hypotheses. Should we, for example, treat our basic laws in favour of free speech or property or against torture as hypotheses to be tested?

The point is that openness, if unrestricted and outside a framework of unquestioned values may not be neutral regarding social bonds. It may, as we see in certain political projects today, such as the European Union, actually loosen the bonds which tie a people together, by eroding the sentiment and prejudice on which a form of life depends. And once these ties are eroded, the impeccably liberal and critical community which emerges may not have within itself enough self-belief to defend itself effectively against an enemy which actually believes in something positive, and does not feel constrained by the niceties of rational discussion and openness.

In 1941, in an article entitled 'Wells, Hitler and the World State' George Orwell asked what had kept England on its feet in the past year: 'In part, no doubt, some vague idea about a better future, but chiefly the atavistic emotion of patriotism, the ingrained feeling of the English-speaking peoples that they are superior to foreigners. For the last twenty years the main object of English left-wing intellectuals has been to break this feeling down, and if they had succeeded, we might be watching SS men patrolling the London streets at the moment.' By contrast, 'for the common-sense, essentially hedonistic world-view which Mr Wells puts forward, hardly a human creature is willing to shed a pint of blood' (Orwell 1984, pp. 195–196).

What about Sir Karl's open society? It is true that much blood was in the past, has been quite recently and maybe will be shed in defence of Britain and the USA, and Britain and the USA are, as far as any societies are, open societies. Our values are those Popper admired: tolerance, liberalism, democracy, free speech and the rule of law. No doubt part of what we fight for, when we fight, and for all the complications, failures, compromises and muddying of waters in conflict, is for the defence of these values. But in all probability values which are not part of their nature as open societies have also been crucial to the willingness of our fellow-countrymen to fight and shed their blood on many occasions in the past 100 years. With many of these non-liberal aspects, Popper was clearly uncomfortable, so he reconfigures his ideal society as Britain or the USA with the patriotic, atavistic bits airbrushed out.

To that extent, to the extent that it refers to no actual society and not even to the reality of those societies he most admired, Popper's open society is a utopian vision. And neither he, nor anyone else so far, knows whether a society which was completely open and relentlessly self-critical in his sense would be possible. Whether, even if it were possible, it would be desirable, or whether it would be a managerial nightmare (of continuous self-scrutiny and endless target setting) is another question altogether.

Having made these criticisms of Popper's open society, it is only right to put them in the context of the development of Popper's own thought. The vision of society we have been considering is that to be found in *The Open Society and Its Enemies*. That book was written in New Zealand, in the early 1940s, but very much in response to events occurring in Europe, particularly in the lands dominated by Hitler and Stalin. As a counter-blast to those societies and ideologies, *The Open Society* is *sans pareil*. And, given its oft-repeated message – that any large-scale attempt to re-mould society centrally is bound to lead to tyranny – it is puzzling that in that book there are many statements which can be seen as, up to a point, friendly to Marxism. I would argue that from the perspective of *The Open Society*, communism cannot be a good idea which went wrong. Involving as it does radical and continual intervention by the state in all areas of life, it was always a terrible idea, and Popper shows us exactly why. Nevertheless, its great merits notwithstanding, *The Open Society* remains over-rationalistic and utopian about reason and about the sort of society it is advocating.

However, it is arguable that by the time he wrote 'Towards a Rational Theory of Tradition' in 1949 (Popper 1963, pp. 61–72), Popper himself came to distance himself not only from the rationalists he explicitly criticizes, but also from his own earlier rationalistic stance. In 1949 he castigated rationalists in rather Burkean terms. They think that they have the means to correct and disparage traditions on the basis of pure reason, through their own brains, rather than confining their criticisms to cases where actual problems were apparent – but doesn't this picture apply at least to some degree to the ideal, thorough-goingly untraditional citizen of his earlier open society?

But by 1949, when he had appeared to qualify his rationalism in favour of a positive role for tradition, Popper had experienced the traditional societies of New Zealand and Britain, and he liked them. He valued many of their traditions, such as their unspoken and unargued-for tolerance, their respect for the law, and their concern for the individual and the underdog, things which go up to make up his own 'irrational faith in reason'. At around this time, in conversation at least, he also defended Hayek against critics from the political left. A liberal society, he said, requires a framework of conservative values.

What might that framework consist of? As Popper himself did not attempt to spell this out in any detail, it would not be inappropriate to end this essay by saying something here about the social and historical conditions which obtain in Britain and the United States and which form the context in which liberalism and openness exist and flourish there – to the extent they do. What is beginning to suggest itself is that more, much more, is required than an abstract commitment to reason or to the political process. A society committed only to engaging in discussion not only has no real means of resolving basic disagreements. It could very easily subside into continually quarrelling factions who may have no basis for agreement on fundamental points, and who feel no sense of community with each other.

To turn now to the world of the twenty-first century, rather than the era of the great dictatorships in and for which Popper was writing, a lack of prospect of agreement on fundamental points is what worries people in the West about large-scale

Islamic immigration, for in this case the newcomers and their descendents see themselves as belonging to political communities defined by their religion, rather than by the nationhood of the societies they are living in. Some of them also have, in accordance with their faith, a long term aim of bringing the places in which they live under Sharia law, and regard the institutions and customs of the West as decadent and infidel. It is not that what is said by Muslim groups who see themselves in this way is unintelligible or even necessarily wholly wrong; it is rather that having numbers of people within a liberal society who believe such things throws into sharp relief the substantive commitments of liberal societies. It also poses a significant problem for those societies and their institutions (such as schools), which for centuries have been unaccustomed to the presence within them of groups fundamentally at variance with their beliefs, commitments and practices. Popperian openness on its own is not enough, faced with such radically diverging views once those views are held by significant and vocal minorities.

The open society, in these circumstances, will also become increasingly subject to paradoxes brought about by its own commitment to openness, as we see daily in the West. For example, wanting to express in a meaningful way its commitment to freedom of speech – which it was felt was being eroded from within from fear of Islamic responses such as those following the publication of Salman Rushdie's *The Satanic Verses* – in 2006 a Danish newspaper published cartoons mocking the prophet Mohammed, and in the ensuing and predictable outcry, other papers re-published them. Is this what openness required? Some Muslims and some non-Muslims pointed out that in a tolerant and open society deliberately insulting people was at the very least bad form, and hardly conducive to living together peacefully and tolerantly – which are also, of course, virtues of Popperian openness. So what does openness require here: publication of calculatedly insulting material, to insist on freedom of speech, or restraint in the service of tolerance?

Other Muslims, however, reacted rather more aggressively. Using their right to free speech, which has been given them in the West, and which in abstract all proponents of openness would defend, they protested noisily and vigorously outside the Danish embassy in London. Inflammatory speeches were made, including calling for death to the blasphemers and also for British soldiers fighting in Muslim lands (but only calling for it). For this exercise of free speech some of the noisier protesters have now been sent to prison for a long time. Of course, all this is against the background of Islamic terrorist plots in London, where maybe calling for attacks on blasphemers and British soldiers comes close to plotting or instigating attacks, but there is more than a hint of contradiction about this episode. By imprisoning the protesters, we in the West show that we believe so strongly in freedom of speech that we lock up those who use freedom of speech to protest about freedom of speech (as *The Economist* rather glibly put it in its July 21st 2007 issue).

Popper himself argued that the open society had to defend itself against threats to openness, and that it should take strong measures to deal with those who would exploit the openness they were granted within an open society to undermine it. These measures would include in certain circumstances suppressing intolerant philosophies and ideologies (including, presumably, religions) by force, particularly

those philosophies which forbade their followers to listen to rational arguments and to use violence instead (cf. Popper 1945, Vol. I, p. 265). Terrorists and the like would have no right to complain when the open society moves to curb their freedoms, if they are using these freedoms to pursue illiberal ends (though whether this would extend to imprisoning people who simply say they would like other people to die is another question). Practically restraining those who use their freedoms to undermine freedom might be sensible, though I am unclear whether a fundamentalist cleric would fall under the Popperian stricture of forbidding his followers to listen to rational argument. There might also be a very fine line between a religious leader arguing against argument (a strategy often used by religious fundamentalists hostile to the pretensions of reason, and who are sometimes well aware of Humean and other philosophical scepticisms) and one who encourages his followers to use more direct means. This is not a purely theoretical matter as Western democracies strive to rid themselves of the 'extremist' clerics whose teachings they fear are influencing their followers to take up violence.

But raising the question of how to deal with enemies of openness, important as it is, hardly gets to the root of the problem. The problem is that openness is not in itself enough to keep a society together. Openness can flourish and function positively only given fundamental agreement among citizens, particularly, one suspects a fundamental agreement on the restraint we all need to observe for a free society to be possible. The worry about the presence of large numbers of Muslims in a European country is whether this means that there might be significant groups within the country who are not party to such fundamental agreement.

Against the type of individualism and cosmopolitanism advocated by Popper in *The Open Society and Its Enemies*, in the light of terrorist attacks on the open society, it is becoming increasingly clear that the freedoms of the individual valued by Popper and other advocates of openness and liberal democracy (to the extent that the two are the same thing) require a political community and a sense among the citizens that they belong to a political community, and that in a sense their freedoms derive from membership of that community.

Though these are not absolutely necessary conditions of the sort of community necessary for grounding an open society, a common language and culture would be a good basis for the sort of general basis of agreement within which liberty, tolerance and openness might flourish. This commonality of language and culture would ground a sense of historical and political identity, as well as a sense of common interest at some profound level, which would transcend political and other differences at a more superficial level. This sense of common identity would also, rather against Popperian and Kantian cosmopolitanism, give members of that society a sense that they have duties and responsibilities towards the society, and in the final resort, a sense that their society would be worth fighting for and defending, as we saw with Orwell.

It will be obvious that within the modern world the only states which fulfil these conditions are nation states. So the sense of community a nation state engenders may be extremely helpful towards setting up regimes of openness, in which there is enough shared interest among members of that state to use their freedoms in such

a way as not to pull the state apart. But it will also be obvious that nation states may not value individual freedom in the way presupposed by talk of openness; they could be nationalist and socialist or both, and they could in various ways elevate the collective above the individual. So nationhood is not a sufficient condition for openness and a regime of liberty. We also need a nation founded on what might be called personalism – a sense of the paramount and unconditional value of the individual person, and which in Popper is what we have called his humanitarianism and individualism.

We could debate as to whether personalism in this sense requires a religious and specifically a Christian underpinning. Of course there was such an idea in the European Enlightenment and in some, but not all, of the streams of thought stemming from that Enlightenment, and in Enlightenment thinking there was a repudiation of anything specifically Christian. It may, though, be that Nietzsche was right in believing that in the absence of its Christian underpinning the nineteenth century liberal sense of the value and sacredness of the individual was intellectually and morally vulnerable.

But, for the purposes of this article, we do not need to settle the Nietzschean question or even to pursue it further. What we need to observe is simply this: that the type of liberalism advocated by Popper requires for its implementation a tradition of thought and practice in which a community sustains, but does not swamp, a profound sense of the value of the individual, and in which individuals, in the main anyway, retain enough of a commitment to their community to enable the institutions and practices of openness to operate constructively and positively, and not destructively and negatively. In such a polity individuals will act as individuals, but there will be enough agreement in the society in a form of life, to allow that society to function cohesively.

It may be that it is liberalism itself which has tended to undermine society in this sense, in that individuals have tended to think of themselves in excessively individualistic terms. In contemporary Europe, along with atomisation of individuals in relation to the society as a whole has gone a growth of identity politics, in which many individuals are tending to see themselves in terms of smaller groups (ethnic, religious, etc.), in contrast to or even in opposition to the society as a whole. Part of my point against Popper is that he failed to anticipate that openness and liberalism themselves could actually contribute to the demise of the common culture which, I am arguing is necessary to keep a society, even or especially a liberal society together, to be replaced by a situation in which heterogeneous groups occupy the same physical space, but with nothing in common mentally or morally. To go back to Popper's own experience, it should by now be clear that in Britain and New Zealand and the United States, he found societies with the traditions and common cultures in which the openness he valued was able to flourish, and which were not yet the atomised societies vulnerable to problems of social disintegration.

So the implicit contrast of *The Open Society* is wrong. It is not that there are irrational communities, with no commitment to reason, and rational open societies with no basis in tradition. The open society, where and in so far as it exists, exists as part of a given tradition. One may object to this being seen in terms of an 'irrational' faith

in reason, and to return to a point made earlier, one might have preferred Popper to have located his ideas about freedom, individualism and openness in a more fully worked-out notion of human flourishing and human nature, so as to give his – and our – views on society and our tradition a more substantial basis than that of one faith confronting other faiths. Maybe he did not want to do this because he resisted talk of human good, rather than of human harm. But, this admittedly crucial point aside, one can also see some positive point of his talk of an irrational faith in reason if it leads us to see what is surely correct: that the open society itself is a substantive tradition, or a part of one. Openness in a society will be undermined if too many people in it do not share its values and traditions, including openness itself, but also values other than openness itself, which permit a society to see itself as a genuine community. This is a problem which concerns many in the West today, and which has no prospect of a solution so long as we think only in terms of the procedures of critical rationalism, and not in terms of the actual traditions and substantive beliefs which sustain those procedures. In this respect, Popper's cosmopolitan individualism is not just utopian; it is utopian in exactly the same way as contemporary neo-conservatism in its belief that, for the best of all reasons, a model of life might be best for all, quite irrespective of local traditions and values.

## References

- Orwell, George. 1984. Wells, Hitler and the world state. In *The Penguin essays of George Orwell*, 194–199. London: Penguin Books.
- Popper, Karl Raimund. 1935. *Logik der Forschung; zur Erkenntnistheorie der modernen Naturwissenschaft*. Vienna: Julius Springer.
- Popper, Karl Raimund. 1945. *The open society and its enemies*. 2 volumes. London: Routledge & Kegan Paul. (Fifth revised edition, 1966).
- Popper, Karl Raimund. 1963. *Conjectures and refutations*. London: Routledge & Kegan Paul.
- Warnock, Mary. 1998. *An intelligent person's guide to ethics*. London: Duckworth.



# Popper's Continuing Relevance

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**Abstract** Popper claims that error indicates what to avoid and there is no recipe for how to proceed. Most rationalist philosophers ignore his arguments and still try to justify their views instead of trying to improve upon them by criticizing them and conjecturing alternatives. In public discourse barren forms of justification are widespread. More and better critical institutions are required, and these require political compromise on shared aims.

Popper's central claim in all his work is that error prompts us on what to avoid but it does not prompt us where to go, still less how to get there. Where to go and how to get there are in the realm of conjecture. Our conjectures are only constrained negatively, not directed positively. Bartley christened this Popper's non-justificationism: Popper's view that for logical reasons justification of conjectures is both impossible and futile when our quest is for the truth (since the justified  $\neq$  the true) (Bartley 1962, pp. 146–155, 1964). Popper's non-justificationism has swingeing consequences for conventional academic philosophy and, more important, for thinking about the problems of today's world. His thought has had a mixed reception. Late in his life Popper gained considerable fame and was showered with honours. His works remain in print and are in the process of being consolidated into collected editions. The reach of his ideas in the public sphere is heartening. Less heartening, as I shall specify, is the tenacious strength of justificationism in academic philosophy and in public discourse. In discussing these matters I shall proceed in a critical rather than a justificationist manner, to learn from error but not pretend to have justification for conjectures offered for critical discussion.

First (Sect. 1) I shall show that a common way in which Popper's philosophy is read and construed is highly problematic. Second (Sect. 2) I shall discuss an example of the persistence of justificationism, namely its historicist variant. This is relevant both to current philosophical practice and to public discourse. Third (Sect. 3) I shall turn to academic philosophy and its troubled relationship to Popper's ideas. Finally (Sect. 4) I shall widen matters to a discussion of today's world.

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Popper made some mistakes in his dealings with his fellow philosophers, I shall contend, and those mistakes are connected to mistakes about today's world and especially about democracy. The mistakes were sociological, not philosophical; practical, not theoretical; yet they stem from a central deficiency in his thinking about science. This deficiency has a wider significance because he extended his model of science to public life in general. Learning from his mistake is important. We cannot learn thereby the correct path to follow, only which previously explored path was a dead end.

## The Non-unity of Popper's Philosophy

Popper had a long philosophical career. His first book appeared in the mid-1930s. He began university teaching in 1937 and continued uninterrupted until his retirement in 1969. Subsequently he was a frequent academic visitor and produced many important writings, including the major collection *Objective Knowledge* (Popper 1972), the autobiography and replies in P. A. Schilpp's, *The Philosophy of Karl Popper* (Schilpp 1974), *The Self and Its Brain* (Popper and Eccles 1977), *Die beiden Grundprobleme der Erkenntnistheorie* (Popper 1979), the *Postscript* (Popper 1983–1985), and *A World of Propensities* (Popper 1990). He claims that he achieved his major philosophical insight by 1919 at the age of 17 (Popper 1976, opening of Sect. 8). Even if that claim is taken with a pinch of salt, he had a long and fecund philosophical career working out the consequences of his central non-justificationist view. It seems likely that there would be changes and shifts in his ideas over that long span. This calls into question the practice of treating his philosophy as a unity.

When he published "The Aim of Science", in 1957 (it was a slice of the then-unpublished *Postscript* [Popper 1983–1985]) he presented its central aim of satisfactory explanation as all of a piece with his falsificationist philosophy. However the question immediately arose (I recall hearing Agassi raise it first) of whether aiming at falsifiable theories (as advocated in *Logik der Forschung* [Popper 1935]) and aiming at satisfactory explanation were not rather different aims and that, hence, he was inaugurating a new philosophy of science. In his contribution to the Schilpp volume Lakatos, half-facetiously, suggested that there were naïve and sophisticated versions of falsificationism and that, in some of his slogans, Popper had embraced the first, although of course he meant the second. Since those early suggestions there has been a good deal of discussion of what we might term the unity of Popper's philosophy. Was he, for example, always and consistently anti-inductivist, or did he have an inductivist phase when he was young? Was he always against psychology and for logic, or had he been grappling with psychological problems long before he transformed them into logical problems? Was he the author of a single, seamless package of ideas that he developed and refined over time, or were there inconsistencies in his views that required choices; were there changes not just of emphasis but of mind; were there errors discovered that led to

course corrections? Popper allowed that there were some cases of each; the issue is whether there were more cases than he indicated. Were these such as to call into question the unity of his philosophy? These and other disputes are matters of deep scholarship which need not be discussed in this paper. Instead, I propose a breakdown of Popper's philosophical career into three phases in a way that is, I hope, relatively uncontroversial, i.e. does not close any ongoing debates.

At a minimum we might distinguish a pre-critical Popper, as it were – whose works appear in Troels Eggers Hansen's collection *Frühe Schriften* (1925–1936) (Popper 2006) – from a critical or classical Popper of the years 1935–1945. The turning point is *Die beiden Grundprobleme* (Popper 1979) in which, Wettersten (Wettersten 1992) and Hacoheh (Hacoheh 2000, Chap. 5) argue, the Popper who we associate with *Logik der Forschung* can be seen emerging. What emerged was the Popper of the critical and falsificationist philosophy of science. One decade later *The Open Society and Its Enemies* (Popper 1945) articulated the more general philosophy of critical rationalism. What came after the critical or classical period of Popper's thought? There is no "post critical" Popper: there is a late Popper, which is a residual category, leaving open the question of the internal consistency between works from the different periods, but also marking a point where Popper looked back on his work and offered glosses and interpretations on it and on its reception, taking account above all of reactions by his logical positivist colleagues. If this threefold division of his thought is reasonable, then the practice of referring to Popper's philosophy as a monolith and citing texts from all periods on single topics is uncritical. Popper himself was insufficiently self-critical: he often commented upon or recalled the context of his own ideas, i.e. acted as his own interpreter. By his own account, such historical descriptions are controlled by overall interpretations that can be debated, but not tested and refuted (Popper 1945, Chap. 25). Thus an author may be a privileged interpreter but not necessarily reliable, infallible, the last word, or anything like that. That Popper regarded his own work as a unity is a fact but one that can be contested. Similarly, that he was prepared to concede some mistakes and changes of mind does not close the discussion on the possibility that there are others.

In my book *The Republic of Science* (Jarvie 2001) I mostly confined myself to the classical Popper of 1935–1945. I proposed there that Popper's emerging picture of science had an implicit sociology of science, one that played a crucial epistemological role. Once that insight was gained, it was possible to see that Popper had not developed or thought it through. For example, he chose to gloss his later idea of "objective knowledge", world 3, in metaphysical rather than sociological terms. Almost all of the objectivity of knowledge he wished to capture could be captured by viewing objective knowledge as a social institution. Furthermore, his idea that objectivity was constituted by a critical attitude and criticism that in turn depended upon friendly-hostile cooperation between seekers of the truth, clearly begged to be worked through at the level of institutions. Popper gestured toward institutions of learning, such as conferences, journals, seminars, but he did not discuss their adequacy or the desirability of their improvement. Moreover, as soon as the implicit sociology of science was exposed it could be seen to have weaknesses as well as

strengths. The brilliant sociological insights displayed in *The Open Society and Its Enemies* (Popper 1945) did not include an examination of science in its specific social framework as institutions. Instead Popper focussed on an idealised vision of science as a magnified Socratic seminar, and democracy as a magnified version of that. These are in my view serious, and of course fruitful, errors. According to his own sociological principles it is well-designed and well-manned institutions that make social cooperation effective and lasting.

## Justificationism and Historicism Continue to Flourish<sup>1</sup>

Conjecture: the great majority of analytic philosophers hold that there is something called knowledge, that it is a subclass of belief, and that it is characterized by the beliefs being true and justified, or true and reliable, or some variation. Case in point: the article on the analysis of knowledge in the ambitious *Stanford Encyclopedia of Philosophy* (2001; revised 2006) presents this view in a typically scholastic fashion: philosophers analyse the claim that “S knows p if and only if certain necessary and sufficient conditions are met”. No work of Popper is in the two bibliographies to the article, nor is the work of anyone working in the Popperian tradition. As far as the article is concerned Popper never wrote anything, never offered any argument that needs to be confronted. Such is the disregard of Popper to be found in an encyclopedia that is summarising the current state of scholarship and laying out the basics for the subject for students.<sup>2</sup>

On the rare occasions when analytic philosophers address Popper’s arguments they classify him as a sceptic. This is because he denies that knowledge has foundations or could have foundations. They could also ignore him because, according to his ideas, knowledge is not a kind of belief. If the quest for knowledge seeks a class of beliefs that can be ticked as justified, or reliable, or some other pedigree, then it would follow from Popper’s ideas that knowledge in that sense does not exist: all is a woven web of guesses. To characterize this as scepticism may be correct, but then, if scepticism is so characterized (Miller 2006, Chap. 7), what is amiss with it? It is not a nihilistic scepticism, because according to it the pursuit of truth is still considered worthwhile. It is not a fideistic or dogmatic scepticism that immunises a core of ideas. It is a healthy scepticism that tries to cope with the true epistemological situation.

On a narrower canvas we find the same situation with induction. If there is induction then it serves as empirical justification for science. So induction is a case of justification. Popper was quite adamant about induction. All inductive inferences are invalid. If we need induction for science then we could never have science. Reactions to this have ranged from incredulity to attempts to show its assertion as inconsistent, since it presupposes induction.<sup>3</sup> In this area, however, the situation is rather different. Quite a few working scientists have publicly endorsed Popper’s views, and in his anti-inductivism he was following a number of heavyweight philosophers of science such as Whewell, Pierce, Duhem, and Poincaré. Hence it

is commonplace for introductions to the philosophy of science to give some space to Popper even if, more commonly than not, his views are eventually set aside with flimsy excuses.

Another form of justificationism is historicism.<sup>4</sup> Historicism was the name Popper gave to the idea that historical inevitability shapes the course of events, that there are inexorable laws of human history. His major targets were Plato, Hegel, Marx, Spengler, Toynbee, and other practitioners of the grand vision of human history. There is no shortage of such speculations today, when we are said to be in a “postmodern” age, the successor to a “modernist” age; or when the “end of history” is at hand; or when Michel Foucault has been an academic best seller with his idea that long periods are dominated by “epistemes”, epistemological outlooks, interrupted from time to time by a “coupure” that moves us into the next (much like Collingwood's Absolute Presuppositions). As these examples indicate, contemporary historicism either recycles the ideas of the figures that Popper has attacked, or focuses on what one might call *Zeitgeist* theory, i.e. the theory that time comes in natural slices and that each slice has its dominant theme or spirit or character. Such claims show historicism as not only justificationist but also essentialist. Academic philosophy vacillates between two explanatory models of its changes of mind. In one it sees itself as making incremental progress, so that only the most recent literature, and, especially the most recent jargon, is respectable. The other is a historicist view of itself as subject to vast currents on the philosophical ocean, such as the rise of idealism, the demise of idealism, the rise of logical positivism, the eclipse of logical positivism, the rise of analytic philosophy, and its struggle with Continental obscurantism.

That justificationism in general and its historicist variant in particular continue to flourish may have many explanations, but at least part of the story has to do with mistakes made by Popper in the transmission and propagation of his ideas. These were mistakes of practice that conflicted with his sociology. They were also mistakes that he did not make consistently, for sometimes his practice did align with his sociology. His actions sometimes leave the impression that if you have great ideas and publish them then you have done all that you can or should do. We can and must learn from this mistake. Reading “objective knowledge” sociologically allows for the view that the fate of ideas may be influenced by social factors, not just by their objective merit in the pure (world 3) sense of merit.

Consider Popper's critique of historicism. Let us contrast the reception of two of Popper's ideas: on historicism and on conspiracy theories of society. The critique of historicism was a major theme aimed at countering a major intellectual liability. The critique of the conspiracy theory of society was undertaken as part of a critique of psychologistic views of society, coupled with the observation that the conspiracy theory is an *Ur*-version of historicism. Nowadays the phrase “conspiracy theory of society” is in wide use. Politicians mouth it without a second thought. It might seem then, that Popper exposed and labelled a flawed way of thinking about society and that people have learned from him more than from Richard Hofstadter (Hofstadter 1964). It may seem so, but it is not. Popper's point was not that there are no political conspiracies. His point was one of sociological method. There are far more

conspiracies than there are successful conspiracies, he observed. Therefore, the existence of conspiracy, even the suspicion of one, explains too little. What needs explaining and is not explained by the conspiracy theory is how this or that conspiracy succeeded, given that most fail. Such an explanation would not attribute automatic causal efficacy to any particular conspiratorial group. The weight of explanation would be on the social set up that the conspiracy was able to use to its advantage and on the unintended outcomes of intentional action, including that of conspirators, successful and unsuccessful. The name Popper invented circulates; his principal point does not.<sup>5</sup>

The case of public grasp of Popper's view of historicism is similar. If the conspiracy theory of society went to the heart of vulgar Marxism, historicism went to the heart of Marxism *tout court* and to Fascism and quite a few other political creeds as well. Historicism, to repeat, is the belief in inexorable laws of human destiny.<sup>6</sup> Despite the defeat of Fascism and the fall of the major bastions of Marxism-Leninism, historicism and historicist thinking are almost as pervasive, all-inclusive, and uncritical as ever they were. To see this one needs to look not at highly elaborated historical and philosophical systems but at, e.g., the constant claims of journalists that decades have a profile ("the Sixties" etc.), a spirit, an atmosphere. (Terms that describe periods, like "the jazz age", are innocuous, of course; the claim that they provide any explanation is what is historicist and pernicious.) Popper thought the doctrine of the Chosen People was an early version of historicism. Whether this is historically so or not, it is so today. From certain groups in Israel to the Bible Belt in the United States historicist claims of that kind are articulated and taken seriously – at least in the sense that some politicians endorse them at face value and constrain their actions to accommodate them. Historicism in its religious version is far from private and benign. It legitimates reckless, irresponsible politics. Belief in the inevitability of the triumph of democracy and free markets is as historicist as Plato's view that democracy inevitably decays into tyranny.

Popper's diagnosis and criticism of such views as barren historicism notwithstanding, they continue to flourish, as does the conspiracy version of historicism. When historicism is fused with power, the rational practice of politics becomes next to impossible. Popper writes:

We want to know how our troubles are related to the past, and we want to see the line along which we may progress towards the solution of what we feel, and what we choose, to be our main tasks. It is this need which, if not answered by rational and fair means, produces historicist interpretations. Under its pressure the historicist substitutes for a rational question: 'What are we to choose as our most urgent problems, how did they arise, and along what roads may we proceed to solve them?' the irrational and apparently factual question: 'Which way are we going? What, in essence, is the part that history has destined us to play?' (Popper 1945, Vol. II, Chap. 25, esp. Sect. III)

Nothing could be more relevant and important than the question of political rationality. I have argued that Popper's objections to historicism and historicist thinking have been circulated but they have not sunk in, they have not resulted in the kind of course correction that he considered urgent.

## Popper and Current Philosophical Debates

I turn to Popper's relations to current philosophical debates. This is a tricky matter. Let us glance backwards. What was Popper's attitude to current philosophical debates in his lifetime? His early publications and his classics are dense with footnotes to the work of his contemporaries, especially the logical positivists among them. His later publications seldom refer to his British or American colleagues. His attitude to them was detached, to say the least. He found most of their work to be of little interest, some of it trivial or verbal, some of it clinging defensively to indefensible philosophical systems, some, like the industry around the later Wittgenstein, simply incomprehensible.

Between the 1920s when Popper started out, and the 1950s when he had a legacy of his own, philosophy had become thoroughly professionalized. He had a well worked out critique of professional academic philosophy.<sup>7</sup> He found it unserious in outlook and with a built-in institutional tendency to scholasticism. Not unlike Wittgenstein, he sometimes discouraged students from entering philosophy. He deplored how much of it was pursued in ignorance of science. He also subjected it to ideology critique. From Plato to Hegel and beyond he found covert and even overt inhumanity and reactionary tendencies in the philosophical classics. He was not impressed by the tendency of students of those classics to become advocates and apologists for these philosophers, obscuring their deficiencies. He expected greater candour about Great Thinkers. He did not think academic intellectuals took their responsibilities as citizens seriously enough and he thought too many of them were too easily seduced by power. If I may be allowed a contemporary example. When the Bush administration, as part of its war on terror, sought legal justification for torture, even though it was explicitly banned by statute and by treaty, it was able to find academic lawyers willing to put their minds to the task. The best explanation of this is the seduction of power. The Popper of *The Open Society and Its Enemies* had already made this part of his sociology of intellectuals, his prime example being Plato and Platonists.

Popper's deepest critique of professional academic philosophy targeted its view of knowledge. Some of this has been discussed above. There is more. "Academic" for Popper signified treating knowledge as refracted through text books, syllabi, a view of knowledge as a justified body of ideas systematically divisible for teaching and research into "fields". Those in "fields" put effort into boundary-drawing and boundary-policing. The usual approach was to try to specify the essence of a field and to valorize professional training in it. This is a model that retains its grip on all academics in all academies. It is such an ingrained approach that even those of us who contest it have to be beware of falling into it. Popper's view, by contrast, was that knowledge existed only in relation to problems, that problems are no respecters of academic divisions, and that much of what we call knowledge is the story of errors and failures on the way to deepening our appreciation of problems.

So in rethinking Popper and the relevance of his philosophy to current philosophical debates it seems to me that we must be selective. Some current philosophical



debates Popper would judge some current Philosophical debates to be no better and no worse than those from which he detached himself during his own academic career. Some tendencies that he deplored have worsened. I would instance the scholasticism that seems to have grown with professionalization. In the “Preface 1958” to the English translation of *Logik der Forschung* (Popper 1935) (*The Logic of Scientific Discovery* [Popper 1959]) Popper makes two cardinal points: there is no method special to philosophy and no essence of it to be defined; and scientific knowledge should be at the centre of philosophical concerns with knowledge, especially with the growth of knowledge. Both the tendencies that he opposed are as strong as ever: professionalized philosophers take it for granted that philosophy has a subject matter and a method, hence a “training”. As for science, the heyday of philosophy of science has long passed and almost all discussions in epistemology utilise everyday knowledge and employ highly artificial thought experiments. Rereading that “Preface 1958” today one can with only minor adjustments treat it as a critique of current philosophical debates. Popper’s philosophy cannot be reconciled with or evaluated by the current preoccupations of philosophical academe without doing violence both to its letter and to its spirit. The question is, which philosophical approach is better? Which is more fruitful?

Given that Popper did not legitimate his concerns by reference to current academic fashion. I would say it behooves us not to try to do it on his behalf. To the extent that he was enmeshed in the philosophical debates going on when he was young it was because he deemed a small selection of them serious. There were thrilling new developments in logic and in physics, and both created intellectual crises, i.e. cascades of consequences and new problems. Popper was pulled into the discussion by intellectual excitement and not by interest in an academic career. This had fortunate effects for his independence and intellectual autonomy. He could legitimate his concerns by reference not to the rest of the philosophical world but to the intellectual situation – and as he viewed it from as purely an intellectual viewpoint as he could. Where he saw no such interest, as in the growth of the Heidegger industry in the 1930s or the later Wittgenstein industry in the 1950s, he ignored them.<sup>8</sup> He was never a team player; always a boat rocker. Most academics are bureaucrats and organisation men.<sup>9</sup> The boat-rocker needs a sinecure, but these are few and far between. Popper had a sinecure at the London School of Economics, but, alas, as time went on he used it partly as a redoubt.

Popper could and did point to role-models. Above all to Socrates<sup>10</sup> – or, at least, Socrates as reconstructed in his own rather partial reading of the historical evidence – as well as to loners like Einstein, Schrödinger, and their likes.<sup>11</sup> And though Popper had philosophical friends, some well placed, he did not develop a reciprocal network with them. His colleagues sent him students, but he not infrequently welcomed them with a cold shower.<sup>12</sup> He put lots of energy into writing and teaching. He did not put much into the construction of networks and reciprocity, institutions and centres. Or rather, I should correct this overstatement and say that he did a lot of that in New Zealand, and some of it in his early years in London – for example he was active to some extent in founding the British Society for the Philosophy of Science and its journal *The British Journal for the Philosophy of Science*.

But he soon dropped out of the academic game leaving a vacuum that Imre Lakatos noticed and filled; in a few short years of intrepid academic game-playing, set up an amazing world-wide range of contacts that included critics and opponents as well as students and disciples from the very same department. The disciples were of Lakatos, not of Popper.<sup>13</sup> Lakatos reassured the powerful that Popper was old hat. The London School of Economics provides concrete evidence of his success: it has a Lakatos building, but not a Popper building.

Mention of Lakatos brings up power once more. Another cost of institution-building, more difficult to articulate, is that you have to accept the realities of power, not only the reality of those who have it, but the reality that you have it, may need more of it, and must utilise it judiciously and responsibly, not squander it. I cannot quite make up my mind about Popper's attitude to his own power. His ideas, of course, have great power. He was an intense and passionate advocate, sometimes dazzlingly persuasive. He also had a bunch of talented students, many respectful colleagues who were devoted friends, and a platform in one of the world's top-rated academic institutions. Yet, when it came to reference letters he repeatedly declared that he had no power and that he was quite ineffective at "placing" students in appropriate jobs. He was exaggerating, but that is also telling. Such are the consequences of neglecting networking.

In Popper's defence, it cannot be denied that institution and network building have costs. You have to suffer fools, even knaves. You have to develop small talk and master what anthropologists call joking relationships. You sometimes have to suit means to ends. Time may be taken away from the pure pursuit of scholarship. My critique is not personal. I say that Popper made a scientific, a sociological mistake. In the scholarly world the propagation of ideas does not take care of itself. It needs the building of institutions and networks and if that is neglected or slighted then the growth of knowledge suffers. This is Popper's own view of philosophical schools (Popper 1958). Most schools preserve and defend, but a critical school is also possible. He conjectures that such was the Ionian school of Thales. If one wants one's ideas criticized, as Popper's philosophy mandated, and if intellectual endeavour is a friendly-hostile social cooperation, as he put it, then the forging of institutionalized reciprocal links with colleagues at home and abroad is the best policy. Otherwise, only dogmatic schools will exist.

An objection to my criticism is the argument from world 3, from the standpoint of objective knowledge. Once an item of knowledge is articulated abstractly, as a world 3 entity, it makes its way in the world, takes its chances, as it were. Certainly Popper occasionally paints a picture of the struggle between ideas as resembling nature red in tooth and claw; he even wrote of our ideas dying in our stead. That is one reading of world 3. Yet, Popper did make some socially effective efforts on behalf of his ideas. He tried to choose his platforms carefully. So, for example, he gave major papers as President of the British Society for the Philosophy of Science, President of the Aristotelian Society, as Annual Philosophical Lecturer to the British Academy, as a Herbert Spencer Lecturer at Oxford, as an Arthur Holly Compton Lecturer in St. Louis, to the First and Third International Congresses for Logic, Methodology, and Philosophy of Science, and so on. Other platforms

were denied to him. *Mind* would not publish “The Poverty of Historicism” nor his response to the criticisms of G. S. Kirk; the *Journal of Philosophy* turned down a criticism of Nelson Goodman in a dismissive way, that is, without deigning to explain.<sup>14</sup> He published with good presses: Routledge, Princeton, Hutchinson, Oxford. So there was nothing casual, or “let events take their course” about his treatment of his own work.

Moreover, Popper worked night and day on his manuscripts to make them as clear as he possibly could. That was his own rationale, his own account of the responsibility he felt towards his problems. By his own view of science, however, institution and network building is necessary, not optional. The process goes like this. You read me, I read you. Your students study my stuff, my students study yours. You hire one of my PhDs I hire one of yours. Ideas diffuse through this network and back through it filters criticism. To seek criticism and to answer it is to treat the critic with respect. To ignore critics and criticism is to treat them with something other than respect. Networks are networks of respect and mutual regard, even towards those one is sure are mistaken (or worse). These are necessary conditions of a critical school.

Admittedly, the grasp of one’s ideas amongst one’s colleagues and critics (even students!) may be deficient. But Popper’s own argument was that it is from the critical discussion of misunderstanding, of oversimplification, that better understanding and representation emerge. Only in this way will better criticism be generated. To work away at one’s manuscripts in isolation, without specifying and reviewing the many criticisms, especially those that people are beginning to say are telling, is misdirected effort, deficient intellectual sociology. Someone may respond: What does public opinion matter? Why should one attend to criticisms that others think are telling but you think miss the mark? After all, matters will be decided in world 3. The answer to this challenge comes from Popper’s philosophy and from his sociology. He argued that criticism and its assessment are cooperative endeavours and the inner dialogue of an isolated writer can become stale – recall his argument in *The Open Society and Its Enemies* about the Robinson Crusoe scientist figure cast away on an island who eventually loses his bearings for ongoing scientific research (Popper 1945, Chap. 23):

there is nobody but himself to check his results; nobody but himself to correct those prejudices which are the unavoidable consequence of his peculiar mental history; nobody to help him get rid of that strange blindness concerning the inherent possibilities of our own results which is a consequence of the fact that most of them are reached through comparatively irrelevant approaches. And concerning his scientific papers, it is only in attempts to explain his work to somebody who has not done it that he can acquire the discipline of clear and reasoned communication which too is part of scientific method

Popper concludes his discussion of Crusoe thus (Popper 1945, loc. cit):

To sum up these considerations, it may be said that what we call ‘scientific objectivity’ is not a product of the individual scientist’s impartiality, but a product of the social or public character of scientific method; and the individual scientist’s impartiality is, so far as it exists, not the source but rather the result of this socially or institutionally organized objectivity of science.

Although these passages are from his discussion of the objectivity of science I see no reason not to apply them to Popper's own tendencies towards being a Robinson Crusoe in philosophy. Especially given that, in the broad picture, Popper took science to be part of philosophy.<sup>15</sup> It is not just responsibility to one's ideas that enjoin institution and network building, it is also the sociological argument that you need to be in social contact with your colleagues and critics so as to get their feedback. However hard Crusoe tries to check himself, Popper argues, in the end his lack of sociality means the end of his work being scientific. What he lacks is a framework of institutions that embraces a critical audience. Only in that framework can he make a scientific contribution. This rather complicates the idea of world 3 status: it asserts that ideas cannot be gained and that status cannot be maintained except in a specific social matrix of critical intellectual communication.

For rhetorical emphasis I have exaggerated Popper's sociological deficiencies. He did some networking. He gave many public lectures. He did some broadcasting. He wrote in various genres, more popular and less. He carried out a voluminous correspondence. He gave his time most generously to students and to other visitors who wanted to discuss things and who brought him news of criticisms. And for someone who did not as a matter of routine read and review the current literature he kept remarkably well informed. Nonetheless, he remains recognisable under the exaggerations.

Another exaggeration I should correct by reiterating that he did engage his critics. He replied in various ways to critics of his *The Open Society and Its Enemies*.<sup>16</sup> He replied to Kuhn in a Pickwickian way by not citing him in the relevant paper (Popper 1975). His *Postscript* was an attempt to expound and defend, as well as to withdraw and correct, some features of his logic of research. He expended considerable effort on his "Replies to Critics" (Schilpp 1974, Vol. II). What then did he not do? He shrank from accumulating academic power at the London School of Economics or the University of London. He did not regularly attend learned society meetings after the early 1950s. When he was invited to speak he tried to keep to a minimum the socialising before and after, often pleading health concerns. He declined to be an academic entrepreneur. Yet both in business and in academe the entrepreneur is a creator of wealth, economic or intellectual as the case may be. Increase in the wealth of discussion is an increase in the general wealth. So sociologically Popper made mistakes, however good his own reasons seemed to him.

Despite his self-imposed isolation the force of his ideas was such (in the world 3 sense) that they were widely read and discussed when newly published. The *Logic of Scientific Discovery* made a much bigger splash than *Logik der Forschung* ever did, though neither reached the threshold established by *The Open Society and Its Enemies*. Notably, all his works are still in print in English. His main detractors are professional academic philosophers and the publicists among their followers. Something can be said about their view. Kuhn, Lakatos, Salmon, Grünbaum and others claim to have decisively criticized the central non-justificationist theses of Popper and theirs has become the received view in "the profession". The received view is the establishment view and of course comes with disincentives to students and faculty to invest intellectual effort in challenging it. Philosophy is

quite Kuhnian in this respect. Nonetheless, the establishment view needs to be challenged; channels to the establishment are needed if one is to be heard.

If Popper did not seek legitimation from his professional colleagues whence did he seek it? The legitimation Popper sought was intellectual, world 3, namely, the problem, the history of discussion around the problem, and its proposed solutions, the *cul de sacs* of error from which learning springs. But by his own account the problem, the discussions of it, and the discovery of error dwell only in public interchange, in mutual articulations, misunderstandings, and corrections.

Popper did not seek justification and he most certainly did not seek it in the opinion or practices of his colleagues. I once heard him ruefully predict that he would be forgotten after his death. He was mistaken about that, but it shows some awareness of his predicament. Bartley wrote (Bartley 1990, p. 196),

Popper and his followers are not true participants in the contemporary philosophical dialogue... Rather, he has ruined that dialogue and that is deeply resented... If he is on the right track, then the majority of professional philosophers the world over have wasted or are wasting their careers. And they know it...the gulf between Popper's way of doing philosophy and that of the bulk of contemporary professional philosophers remains as great as that between astronomy and astrology.

I would have phrased it, "wasting their time and ours". You can change careers, but you cannot retrieve lost time. How do you forge friendly-hostile social relations with peers who are quite capable of working it out that if you are correct they are wasting their time? I do not know the answer to that.<sup>17</sup> Still, I think the alienation from contemporary philosophical debates is to be regretted and it sits ill with Popper's view of knowledge coming from cooperation and dialogue. Or perhaps his model of cooperation and dialogue is wanting, in particular regarding politics.

## **Popper and Some Issues of Today's World (Science and Democracy)**

Popper was a very shrewd social scientist and we still have much to learn from him. But his faulty sociology of academe shows up vividly in his faulty sociology of science. In particular, he made some errors, or at least questionable assertions, about science and about democracy. What he said about the sociology of science, suggestive as it was, bore little resemblance to the facts. He said almost nothing of the politics of science, itself an error of oversight. Yet he used his sociology of science to draw up a model for democracy; democracy as a kind of Socratic debating society. This reminds me of no-one more than Rousseau who saw the ideal political unit as one where all the citizens could gather under the spreading branches of one big oak tree; sheltering there they could debate and decide issues, be effectively self-governing (Rousseau 1762, Book IV, Chap. 1).

Speaking in sociological and political terms, neither science nor mass democracy is much like a Socratic seminar or a Rousseauvian town meeting, although intellectually there may be some faint resemblance. That resemblance lies in that science

and direct democracy have problems to meliorate and solutions to debate and the best model of debate extant is critical rationalism. Within both science and direct democracy, institutional sectors operate in which a critical rationalist model makes ample sense. Take the seminars conducted by academics, for example. James Watson describes Cambridge's Cavendish labs in the early 1950s as such. Some political debates are like that, in party meetings, in committees, and in plenary bodies. But let us not get carried away. In *The Open Society and Its Enemies* Popper expressed the hope that we could forge political institutions that would foster the identification of error, admission of it, and corrections made as a result. This went too far: politics is not a seminar. Politics and power are inseparable.

What about the sociology and politics of science? Recall that Popper once asserted that Thomas Kuhn's description of the sociology of science was by and large correct (Popper 1970, p. 51). The main feature of that description was a distinction between mundane or normal science, which constituted the vast bulk of the work of the majority of scientists, and revolutionary science, which was rare and always of uncertain status. Popper added that it was a sociological situation he deplored because only the heroic revolutionaries were engaged in intellectually interesting science. This was a bit grudging, a bit partial, and rather romantic. It also neglected politics, one of the engines of debate. Most striking also is the claim that science progresses despite its institutions. By Popper's own view, how could that be?

There was some lack of sociological seriousness here. Not every scientist can be an heroic revolutionary any more than every businessman can be a titan billionaire. This is not for lack of wishing: it is systemic. The giant leaps sometimes stem from the quiet chipping away. It is not enough that we treat normal science with respect. On Popper's own sociological account of science as a collective activity, one has to acknowledge the necessity of normal science. If normal science, if intellectual stability, is necessary to the achievement of theoretical change, or of paradigm shift, then its role commands respect. This is not to say, however, that Popper should have endorsed Kuhn's account. Kuhn's uncritical model, his shifting the explanation to social psychology, is an error: critical institutions are important for science, and they function significantly even in normal science.

Kuhn's account was that as a theoretical system, a paradigm, falls into place, much useful work has to be done on its details, on its applications, and on its technology. This is its institutionalisation as normal science. Any theoretical system will have its flaws, some known right away; others only become apparent in the work of normal scientific research. But these flaws do not in themselves constitute reasons for upsetting the theoretical/institutional bandwagon. If all paradigms are flawed, then flaws are not reason enough to replace a paradigm. What is reason enough? In a curiously opaque account Kuhn struggles to reconcile the absurdity of counting flaws, or anomalies, with the historical fact that theoretical crisis crops up in idiosyncratic ways. Kuhn stresses turning points in expert opinion and, following Polanyi, he endorses the scorn expert opinion directs at premature renegades. This is his substitution of social psychology for the institutional process. By contrast, a satisfactory institutional account should include the politics of science as part and parcel of its rationality.



Take the institution of critical reasoning. Kuhn's model has almost no use for it. Yet scientific training is not just about facts; it is about theories and theoretical structures, about more promising lines of inquiry and less promising ones. Sometimes this knowledge is represented as scientific intuition as if a rational account of scientific intuition is impossible. But we can give a critical rationalist an account of it. Part of scientific intuition is simply logical and mathematical intuition, sensing quickly hidden inconsistencies or what will compute. Part of it is also a sense of the hierarchical structure of the science, what are the most basic, i.e. most general laws and corollaries, and what are of much narrower scope. A sense of this hierarchy can lead one to see whether an anomaly, a puzzle, or, to speak more plainly, a recalcitrant fact, bodes trouble for only a localised part of the theoretical system, easily unplugged and replaced, or whether it threatens to go deep and call some of the fundamental laws into question.<sup>18</sup> It seems to me that in the sea of anomalies it is quite rational not to venture a revolution when anomalies seem localised; it is irrational to shove aside anomalies that go deep (Bunge 1967). Critical rationalism as institution clarifies the situation. Both stability and change can be rationally explained.

My metaphors of depth, locality, and so on may be crude, but they are I hope suggestive. Change only makes sense against a background of stability. It is, as Popper says, breaches in the horizon of expectations that leap to our attention. Hence there should be an horizon of expectations. Such an horizon is theoretical. The horizon is the theoretical background, the paradigm, the innovations that once were novel and now are sediments. Anomalies that seem to go deep are ignored at our intellectual peril. If in this way we equate intuition with training, with grasp of the theoretical background, the way is open for rational debate about institutions. Such questions as the seriousness and range of a refutation are hardly subject to algorithmic calculation: they are matters of conjecture. Those with different intuitions about it, different conjectures, seek to be heard and to be debated. Kuhn's tendency to defer to the leadership speaks correctly to the power politics of science, where leaders and grant givers are kings. Yet any historian of science, including Kuhn, knows of significant cases where the leadership was part of the problem, not the vanguard towards a solution. What has Kuhn to offer in response? A curious, Hobbesian model of consensus. Hobbes thought that no revolt against the sovereign was to be commended except the one that succeeded – the success of the revolt only went to show that the sovereign was ineffective. Kuhn pours cold water on the idea that there are rational revolts against the sovereign, only with less cogency than Hobbes. What Popper's critical rationalism has to offer is a way to make rational sense of, rationally to reconstruct, scientific moments that Kuhn following Polanyi gives over to irrationalism.

How rational is the politics of science?<sup>19</sup> Training produces intuition and training includes getting familiar with the institutional structures of critical reasoning, language, and basic theory. Ideas are social institutions. Locke, Berkeley, and Hume thought of ideas as mental entities, as in the head. The problem of connecting the subjective ideas to the objective world became notorious. Popper argued that what is in the head is not strictly speaking of intellectual interest; beliefs are not interesting



and are neither necessary nor sufficient for knowledge, or *vice versa*. Knowledge emerges with public articulation, i.e. when thought is spoken or written in language and circulated. Language is thus the first or the presupposed social institution of knowledge in general and of science in particular.<sup>20</sup> Cooperation is the second such institution with the complication that it is probably cooperation or the aim of cooperation that is the driving force behind the development of all language.

Institutional cooperation takes many forms. There are forms of storage and retrieval, and these are connected to forms of presentation and circulation. Forms of interacting include teaching, debating, presenting, illustrating, and popularizing. Groups include societies, clubs, sessions, seminars, academies, invisible colleges, workshops, breeze shooting in the tea room or by the water cooler. All such groups and institutions have politics, meaning by politics no more than that groups and institutions, include or are taken to include diverse interests and hence individual aims that sometimes clash.

Politics is the use of coalition and compromise to prevent worse. The centre of scientific politics is differences over which lines of debate to pursue, which to neglect. If this is not the centre then it should be, and if it is not, then we need to subject the training institutions of science to criticism. Polanyi and Kuhn endorse the political slogan "follow the leader" or rather "follow the leadership". This is repugnant in politics in general, why should it be the best we can come up with in science? Both Polanyi and Kuhn gave as their reason efficiency in the deployment of resources. Efficiency considerations seem to assume there is some time pressure in science, some need not to waste effort. This is bogus: rationale not rationality. No doubt there are times when military, ecological, or medical necessity creates urgency. More frequently deadlines come from grantors and are bureaucratic. The stakes in science are the highest: getting it right. There is no substitute for this. Whenever getting it right *tout court* is at stake that must trump efficiency and pleas for haste. After all, the costs of haste, especially hurtful mistakes, not to mention malpractice, should not be left out of efficiency calculations. Hence, the slogan "follow the leadership" is imprudent as well as irrational. There must needs be institutional support and resource allocation sufficiently loose that those who argue that the leadership is part of the problem are not silenced. What is true of science is true of society in general. The institutions for error detection are part of the politics. We need to free ourselves from Plato and Rousseau's disdain for politics and to push for its improved rationality (Crick 1962).

Institutions and their politics are necessary, perhaps necessary evils; but then in democracies their politics can be criticized and improved. Historicism and justificationism overlook this, or at least play it down. This is their folly and it does much damage. Historicism preempts rational politics; justificationism seeks to preempt criticism, i.e. rationality itself. Popper's ideas on science and rationality provide a crucial component of the explanation of their persistence. His view of the open society is that all of its deficiencies, including attempts to overthrow it, are quite endemic and require ceaseless institutional innovation and ceaseless institutional vigilance. It seems we have a long way to go to grasp and implement his message.

## Conclusion

Part of the problem of the relation of Popper's ideas to current philosophical debates is that he did not set up enough institutions and networks to convey his ideas, to keep them plugged into the feedback loops. Hence the fate of his ideas is subject to the vagaries of *ad hoc* interest. They lack institutional support in academe. Their value in world 3 is obscured in worlds 1 and 2. Let us hope that this is not a case of "in the long run the truth will out" for, as Lord Keynes remarked, in the long run we are all dead.

I do not claim that this lack of institutional support, Popper's ideas not being plugged into enough feedback networks, fully explains the persistence of justificationism in general and of historicism in particular. It is part of the story, at most. If the failure is not to be repeated in the next generation, the broadcasting and critique of Popper's ideas should embrace institutions and networks to channel critical feedback and also build political alliances with others, supporters and critics alike. We need to learn from Lakatos and to devise institutions and networks and to engage in politics. The stakes are high. Popper's relevance could not be clearer.

## Notes

1. I follow here in the footsteps of Bartley (1990).
2. The same is true of the article "Epistemology" in Sarkar and Pfeifer (2006).
3. The *Stanford Encyclopedia* entry on induction uses a new tactic. There is a section on Popper; it reports his opposition to induction but not his actual criticisms of inductivism. With no comment or criticism it then passes on to long discussions on the intricacies of the views on induction of Carnap and the Bayesians. We might call this "dismissal by the allocation of space".
4. Which I have discussed extensively in Jarvie (2007).
5. A case where Popper's point was taken but not acknowledged is the official report on the 9/11 attack on the United States. The 9/11 plotters and their sponsors were the real conspirators. Their identities and their actions were quite quickly discovered. The official investigation, however, looked mainly at lapses in the control of the borders, on keeping track of suspect persons, and on lapses of airport security to explain how these conspirators had consummated most of their conspiratorial aims (Kean and Hamilton 2004). Their conspiracy was shown to have failed to crash the fourth aircraft into its intended target because of the passengers' initiative. So it was a conspiracy that did not achieve all of its goals. The utter confusion about the conspiracy theory of society and its importance is illustrated on almost every page of Coady (2006).
6. Scholars chided Popper for using the term "historicism" which had at the time limited circulation but was used to capture a somewhat different idea. This may have been an excuse to avoid discussion. Ryle relabelled it "the Juggernaut theory of history" and was able to discuss its merits without such pedantry (Ryle 1947).
7. It is my recollection that he resisted having a philosophy honours programme at LSE until he gave way to the wishes of his colleagues Wisdom and Watkins. He took pride in the fact that he was not a Professor of Philosophy but of Logic and Scientific Method. In *Unended Quest* he writes "I often began my lectures on scientific method with an explanation of why this subject is nonexistent". (Popper 1974, 1976, end of Sect. 27). In lectures he would elaborate

the joke, mock-boasting about being the one and only professor of this non-subject in the entire British Commonwealth.

8. The earlier Wittgenstein (Wittgenstein 1922) is another matter. Popper wrote long critical comments on it (Popper 1945, footnotes to Chap. 11). Typically, most commentaries on it ignore these: think of (Black 1964).
9. I am alluding to that classic text of sociology (Whyte 1956).
10. On Socrates see Vlastos (1994) and Nails (1995).
11. Popper explicitly says (Schilpp 1974, "Replies to My Critics", Sect. 5) his focus is on the heroic and romantic great scientists grappling with great problems and bold solutions. He maintains that this focus will be fruitful in explaining those to whom science is no more than a profession or a technique. It is unstated that Kuhn focussed on the latter and did not illuminate thereby the former.
12. Many of these were delivered when the newcomer read a paper to Popper's Tuesday afternoon seminar. Some flavour can be found in the reports of Agassi (Agassi 2008) being told he was a charlatan and of Bartley, who was told his prize essay was written by someone more interested in the effect he was producing than the truth of the matter (Bartley 1990, Chap. 9).
13. Lakatos told me that since Popper was not an effective propagator of his own ideas, he, Lakatos, would take on the task, out of dedication to their importance. Imagine my surprise to find that what Lakatos actually propagated was Lakatos, the Lakatos who had, it seems, set Popper's skewed ideas to rights. With the wisdom of hindsight and the work of Jancis Long (Long 1998) I now see that Lakatos told me, an adamant defender of Popper, a sanitized version of what he was up to. The aim being to pacify me, I suppose. He told people whatever would get them on his side or at least preempt their opposition.
14. The relevant letters are in the Hoover Archive at 28.11 and 243.16 where Popper is admonished by the editors of the *Journal of Philosophy* that his points are "not surprising to Professor Goodman nor unacceptable to him. They see the purpose of your paper as stating for the historical record why you hold that your own views on these matters are not affected by Goodman's "New Riddle." But in that case the paper should be somewhat refocused, so that you explicitly state in the text why Goodman is mistaken in alleging that your view is affected. The editors would be much interested in seeing a revised version".
15. Popper aimed his discussion on Robinson Crusoe against the traditional study of knowledge as answers to the question, how do I learn/ know? And replaced it with How do we learn/ know? The question mentioned above, "what are the necessary and sufficient conditions for asserting S knows that p", is a version of the first question.
16. Discussed interestingly in the Introduction and notes to Chap. 18 of Popper (2008).
17. An answer is perhaps necessary anyway. People often tell other people that they are wasting their time. For one thing, that is just your opinion. For another, people should be free to waste their time if they wish and can even reasonably demand to be left alone. For a third, a low opinion of what you do might be refuted if you come up with good results or even merely good arguments.
18. This is the importance – and revolutionary character – of the insistence on the basic character of the second law of thermodynamics that Planck and Einstein shared.
19. Agassi (2003) insists that there is politics in science and most of it consists of various forms of abuse of power. His aim is to bring this into the open for discussion, possibly reform. What is also required is: a survey of the different institutions involved in science, not just those peculiar to it. What makes (Latour and Woolgar 1979) interesting is the ethnographic detail. But their fake behaviourism and lack of institutional framework are frustrating, and their debunking of science as mystique is both tiresome and feigned. Some of Agassi's most telling contributions in this line are his papers on the history of Royal Society. Incidentally, the intrusion of general politics into science resulted in the interesting case of James Watson's institutional career coming to an abrupt end on account of some thoughts that he expressed (or perhaps misspoke). The scandal is gisted in the Wikipedia biographical article on Watson.
20. Popper has always argued that the use of language in science introduces a conventional element into it, and hence inductivism is false and, since the idea of the ideal language was given up as a bad job, it is hopeless.

## References

- Agassi, Joseph. 2003. The politics of science. In *Science and culture. Boston Studies* vol. 231 Dordrecht: Kluwer.
- Agassi, Joseph. 2008. *A philosopher's apprentice. In Karl Popper's workshop* (second edition). Amsterdam: Rodopi.
- Bartley, III, William Warren. 1962 (1964). *The retreat to commitment*. New York: Knopf.
- Bartley, III, William Warren. 1964. Rationality versus the theory of rationality. In *The critical approach to science and philosophy*, ed. Mario Bunge, pp. 3–31. New York: Free Press.
- Bartley, III, William Warren. 1990. *Unfathomed knowledge, unmeasured wealth: On universities and the wealth of nations*. LaSalle, IL: Open Court.
- Black, Max. 1964. *A companion to Wittgenstein's Tractatus*. Ithaca, NY: Cornell University Press.
- Bunge, Mario. 1967. *Scientific research: Strategy and philosophy*. Berlin: Springer.
- Coady, David. 2006. *Conspiracy theories. The philosophical debate*. Aldershot: Ashgate.
- Crick, Bernard. 1962. *In defence of politics*. London: Weidenfeld & Nicolson.
- Hacohen, Malachi Haim. 2000. *Karl Popper: The formative years, 1902–1945*. Cambridge: Cambridge University Press.
- Harré, Rom (ed.). 1975. *Problems of scientific revolution: Progress and obstacles to progress in the sciences*. Oxford: Clarendon.
- Hofstadter, Richard. 1964. The paranoid style in American politics. *Harper's Magazine*, November 1964, 77–86.
- Jarvie, Ian. 2001. *The republic of science: The emergence of Popper's social view of science 1935–1945*. Series in the Philosophy of Science of Karl R. Popper and Critical Rationalism. Amsterdam: Rodopi.
- Jarvie, Ian. 2007. Relativism and historicism. In *Handbook of the philosophy of science. Philosophy of anthropology and sociology*, eds. Stephen P. Turner and Mark W. Risjord, pp. 553–589. Amsterdam: Elsevier.
- Kean, Thomas Howard and Hamilton, Lee Herbert. 2004. The 9/11 commission report: Final report of the National Commission on Terrorist Attacks upon the United States, Washington, DC: USGPO.
- Latour, Bruno and Woolgar, Steve. 1979. *Laboratory life: The construction of scientific facts*. Princeton, NJ: Princeton University Press.
- Long, Jancis. 1998. Lakatos in Hungary. *Philosophy of the Social Sciences*, 28, 244–311.
- Miller, David. 2006. *Out of error*. Aldershot: Ashgate.
- Nails, Debra. 1995. *Agora, academy and the conduct of philosophy*. Dordrecht: Kluwer.
- Popper, Karl Raimund. 1935. *Logik der Forschung*. Wien: Springer.
- Popper, Karl Raimund. 1945. *The open society and its enemies*. London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1957. The aim of science. *Ratio*, 1, 24–35.
- Popper, Karl Raimund. 1958. Back to the presocratics. Reprinted as Chap. 5 of Popper (1963).
- Popper, Karl Raimund. 1959. *The logic of scientific discovery*. London: Hutchinson.
- Popper, Karl Raimund. 1963. *Conjectures and refutations*. London: Routledge.
- Popper, Karl Raimund. 1970. Normal science and its dangers. In *Criticism and the growth of knowledge*, eds. Imre Lakatos and Alan Musgrave, pp. 51–58. Cambridge: Cambridge University Press.
- Popper, Karl Raimund. 1972. *Objective knowledge*. Oxford: Clarendon.
- Popper, Karl Raimund. 1975. The rationality of scientific revolutions (Harré 1975, pp. 72–101). Reprinted as Popper (1994, Chap. 1).
- Popper, Karl Raimund. 1976. *Unended quest: An intellectual autobiography*. Glasgow: Fontana.
- Popper, Karl Raimund. 1979. *Die beiden Grundprobleme der Erkenntnistheorie*. Tübingen: J.C.B. Mohr.
- Popper, Karl Raimund. 1983–1985. *Postscript to the logic of scientific discovery*, 3 Volumes. London: Routledge.
- Popper, Karl Raimund. 1990. *A world of propensities*. Bristol: Thoemmes.
- Popper, Karl Raimund. 1994. *The myth of the framework*. London: Routledge.

- Popper, Karl Raimund. 2006. *Frühe Schriften (1925–1936)*, ed. Troels Eggers Hansen. Tübingen: J.C.B. Mohr.
- Popper, Karl Raimund. 2008. *After the open society: Selected social and political writings*, eds. Jeremy Shearmur and Piers Norris Turner. London: Routledge.
- Popper, Karl Raimund and Eccles, John Carew. 1977. *The self and its brain*. Berlin: Springer.
- Rousseau, Jean Jacques. 1762. *The social contract or principles of political right*.
- Ryle, Gilbert. 1947. Review (untitled) of Popper, *The open society and its enemies*. *Mind*, 56, 167–172.
- Sarkar, Sahotra and Pfeifer, Jessica (eds.). 2006. *The philosophy of science. An encyclopedia*. London: Routledge.
- Schilpp, Paul Arthur (ed.). 1974. *The philosophy of Karl Popper*. La Salle, IL: Open Court
- Steup, Matthias, 2001 (rev. 2006), *The analysis of knowledge*. In Stanford Encyclopedia of Philosophy. <http://plato.stanford.edu/>.
- Vlastos, Gregory. 1994. *Socratic studies*. New York: Cambridge University Press.
- Wettersten, John. 1992. *The roots of critical rationalism*. Amsterdam: Rodopi.
- Whyte, William Hollingsworth. 1956. *The organization man*. New York: Simon & Schuster.
- Wittgenstein, Ludwig. 1922. *Tractatus logico-philosophicus*. London: Routledge & Kegan Paul.

# Open Society and the European Union

Miloslav Bednář

**Abstract** Popper's concept of the open society articulates and further develops the classical paradigm of the liberal human condition. The radical challenge of totalitarianism as an alternative mode of human existence motivated Karl Popper to define freedom, the very heart of democratic civilization, as the standard and criterion of human dignity. Dictatorships and totalitarian regimes suppress human freedom, which is the basic characteristic of open democratic societies and states. Today, the European Union has apparently evolved into a sophisticated post-democratic entity with a highly-regulated, closed, non-liberal, irrational, non-humanitarian, and consequently undemocratic nature. This relatively new historical phenomenon thereby represents a new type of serious threat to the freedom of individuals and open societies alongside those that we already know from history.

## Popper's outline of open society

Karl R. Popper's succinct definition of closed versus open societies is that "the magical or tribal or collectivist society will also be called the *closed society*, and the society in which individuals are confronted with personal decisions, the *open society*." (Popper 1995, p. 173). More specifically, he compares a closed society at its best with an organism (cf. *ibid.*). Popper states that

the so-called organic or biological theory of the state can be applied to it to a considerable extent. A closed society resembles a herd or a tribe in being a semi-organic unit whose members are held together by semi-biological ties—kinship, living together, sharing common efforts, common dangers, common joys and common distress. It is still a concrete group of concrete individuals, related to one another not merely by such abstract social relationships as division of labor and exchange of commodities, but by concrete physical relationships such as touch, smell, and sight. (*ibid.*).

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In contrast, Popper views one of the most significant characteristics of the open society to be “competition for status among its members.” (*ibid.*, p. 174). He also points out that

personal relationships of a new kind can arise where they can be freely entered into, instead of being determined by the accidents of birth; and with this, a new individualism arises. Similarly, spiritual bonds can play a major role where the biological and physical bonds are weakened... our modern open societies function largely by way of abstract relations, such as exchange and co-operation. (*ibid.*, p. 175).

Popper then prophetically adds that

the transition from the closed to the open society can be described as one of the deepest revolutions through which mankind has passed... Thus when we say that our Western civilization derives from the Greeks, we ought to realize what it means. It means that the Greeks started for us that great revolution which, it seems, is still in its beginning—the transition from the closed to the open society. (*ibid.*).

Popper additionally speaks of “the new faith of the open society, its only possible faith, humanitarianism,” (*ibid.*, p. 183) which he defines as “man’s faith in reason.” (Popper 1996, p. 258). Within this context, his concept of rationalism consists of the following four components:

- *Impartiality*, which is based on the idea that “everybody is liable to make mistakes. It therefore suggests the idea that nobody should be his own judge” (*ibid.*, p. 238).
- *Tolerance*, which derives from the idea that “faith in reason is not only a faith in our own reason, but also—and even more—in that of others” (*ibid.*). Tolerance is thus grounded on the ability to learn “from criticism as well as from [one’s] own and other people’s mistakes and that one can learn in this sense if one takes others and their arguments seriously. Rationalism is therefore bound up with the idea that the other fellow has a right to be heard, and to defend his arguments” (*ibid.*).
- *Responsibility*, which denotes the idea that “we have not only to listen to arguments, but we have a duty to respond, to answer where our actions affect others” (*ibid.*).
- *Recognition of the necessity of social institutions to protect freedom of criticism, freedom of thought, and thus the freedom of men and women*. This involves “a moral obligation towards the support of these institutions” (*ibid.*).

In summary, *Popper’s outline of the open society* as one goal of historical development since the time of the ancient Greeks *expresses the classical paradigm of the liberal human condition*. The dramatic historical context of World War II prompted Popper to define the open society, understood as the very heart of democratic civilization, as the standard and criterion of human dignity. It was in fact the prelude to that war which inspired him to work on his *The Open Society and Its Enemies* (Popper 1995, p. viii).

Totalitarian regimes continue to exist and some of them, such as communist China, even flourish under the condescending care of their strikingly tolerant democratic rivals. However, it appears that it is not only dictatorships and totalitarian regimes that suppress human freedom, which is the basic characteristic of open democratic societies and their corresponding states today. The European Union itself – an entity formed from European democratic states – also poses a threat to human freedom.



## Critique of European Union's mainstream ideas in the light of Popper's notion of open society

In order to understand this issue properly, it is necessary to examine the specific set of ideas that provides the foundation for the ideology and politics of European federalism. The institutional starting point of this supranational mainstream of European integration is the *European Coal and Steel Community* of 1951, the ultimate outcome of the Schuman plan, which presents a *specific concept of effectiveness in political decision making* (cf., e.g. Pinder 1995, pp. 3–6). The primary normative view that implemented that peculiar concept of political efficacy was developed by Jean Monnet, considered by many to be the chief architect of European integration. As Monnet explained in his memoirs, he developed his concept of political effectiveness in antithesis to the notion of co-operation between sovereign national states (Monnet 1978, e.g., pp. 96–97). Monnet's evident opposition to the notion of peace based on national equality clearly arose from his experience with the manner of decision-making by unanimous agreement that was typical of the League of Nations, where he was appointed Deputy Secretary General upon its creation in 1919.

Monnet's strongly negative assessment of the League of Nations gave rise to a line of reasoning that led to the normative concept of European political union, which is essentially different from Popper's view of the open society. He writes that

We developed methods of co-operation among nations which hitherto had known only relationships based on power. We placed great hopes in the development of the League, and the difficulties we encountered acted as a stimulus. It was only later that I realized how we have underestimated them, or rather how we had failed to dig deep enough. At the root of them all was national sovereignty. In the League Council, this prevented the general interest's being seen. At every meeting, people talked about the general interest, but it was always forgotten along the way: everyone was obsessed by the effect that any solution would have on him—on his country. The result was that no one really tried to solve the actual problems: their main concern was to find answers that would respect the interests of all those around the table. In this way, the whole organization fell into the routine of mere co-operation. This was inevitable in a body subject to the unanimity rule. This rule seemed natural to even the best-intentioned of men... People's ideas had simply not evolved very far. The veto was at once the cause and symbol of this inability to go beyond national self-interest (*ibid.*).

This statement articulates Monnet's view of the basic contradiction he sought to resolve: On the one hand, there was the need to resolve actual problems, which he refers to as the general interest; on the other, there were the specific interests pursuant to national sovereignty of all those around the table, which culminated in the veto right held by each nation. As a simple and effective remedy for the resulting deadlock, Monnet introduced the idea of "an independent political body that can take a common view of the problem and arrive at a common decision" (*ibid.*, p. 87). However, the openness in Popper's terms of such an independent body, or its democratic accountability, was an issue that evidently did not concern Monnet. Why?

Monnet seems to have maintained that the goal of political effectiveness necessitated the sacrifice of the humanitarian and rationalist principles of the open society. He identifies this decisive supranational step towards a closed society as “the delegation of sovereignty to a common High Authority” (*ibid.*, p. 88). This indicates that the pattern of development which Monnet had in mind involved the principle of state sovereignty as governmental sovereignty that has been delegated by the people. Monnet’s supranationalism thus outwardly emulated the principle of representative democratic government. Monnet overlooked, however, the obvious reality that there is no such thing as a European people who might be capable of deciding to establish and construct their own state – there are instead different European peoples represented by their democratic governments. But these governments taken together cannot serve as a non-existent European people. Consequently, the basic normative concept of EU supranational institutions has been seriously flawed from its very inception by a substantial lack of legitimacy in terms of democratic accountability.

Furthermore, Monnet’s concept of a non-accountable, supra-national High Authority thereby contradicts in philosophical terms the basic characteristics of Popper’s humanitarianism, which is a rationalism that constitutes the basic features of his idea of the open society. The EU’s founding principle of political effectiveness therefore *cannot be humanitarian* in terms of Popper’s concept of rationalism as the faith of the open society – and it is thus *not morally legitimate*.

We will now consider Monnet’s basic ideas as stated above in respect to the fundamental characteristics of Popper’s humanitarian rationalism (cf. *ibid.*, notes 9–13).

First of all, Monnet’s notion of High Authority obviously does not meet the criterion of *impartiality*, which stipulates that “nobody should be his own judge” (*ibid.*, p. 238). Monnet’s idea of “an independent political body that can take a common view of the problem and arrive at a common decision” (*ibid.*, p. 87) unfortunately does not imply that it should be subject to any external criticism. Monnet’s principle of an independent supranational High Authority is thus an autonomous judge of its own deliberations and actions.

Nor does Monnet’s concept of High Authority correspond with the standard of *tolerance* as defined by Popper (*ibid.*). It does not comprise an authority engaged in rational open discussion, but rather a supposedly well-informed arbiter above the alleged squabbling and narrow-minded sovereignties of individual states. This conception thus rests upon an essentially anti-individualistic world view in respect to which the crucial tenet of tolerance, or the right to defend the arguments of others, in principle does not pertain.

A similar problem faces the notion of High Authority in regards to Popper’s rationalist criterion of *responsibility* (*ibid.*). The most flagrant case in point was the decision of the European Union subsequent to the rejection of the so-called European Constitution (cf. *Treaty Establishing a Constitution for Europe*) in the French and Dutch referenda of 2005 to avoid the ratification of its succeeding version by referenda (cf. *Draft Treaty Amending the Treaty on European Union and the Treaty Establishing the European Community*).

The individual citizens of the democratic member states of the European Union who would be affected by the proposed treaty were deliberately not consulted in this matter with the exception of Ireland, where a referendum was required for constitutional reasons.<sup>1</sup>

Such difficulties also extend to the moral obligation that social institutions must protect freedom (Popper 1996, p. 238). Both the concept and reality of Monnet's supranational High Authority contradict Popper's principle of the institutional protection of freedom of criticism, freedom of thought, and, consequently, freedom of individuals. By virtue of its very essence, Monnet's High Authority concedes the existence of *no counterbalancing institutions of free criticism and thought* that embody human freedom as such, let alone a "moral obligation" to support them that is rooted in an awareness of its fallibility (*ibid.*).

In summary, the uncritical and elitist nature of Monnet's High Authority renders it *irrational, anti-humanitarian, and antithetical to the open society* as defined by Popper.

There are also serious flaws in Monnet's underlying attitude, which blames European state sovereignty for the mere cooperation that was powerless to prevent the outbreak of World War II (cf., e.g. *ibid.*, pp. 95–98). A critical issue in this regard is that he ignored the fact that pre-war Europe was characterized by two distinct types of state sovereignty, namely, democratic states that approach Popper's notion of the open society and dictatorships. This oversight had consequences for the vision of EU integration, not least of all in that it concealed the essential differences between democracies and dictatorships in their respective approaches to war and peace. Whereas democracies tend to resolve their mutual problems through *rational* negotiation, dictatorships do not. Furthermore, dictatorships are in principle inclined to seek the elimination of democracies through war due to the mortal political danger that the latter pose to them by their very existence. Monnet's superficial analysis gives no consideration to how such political factors can be a source of war, such as was the case in Europe with German aggression during the twentieth century.

As a result, Monnet's account of the interrelationship between peace, war, and state sovereignty is profoundly deficient. Nevertheless, his irrational normative notion of political effectiveness in respect to a delegation of sovereignty to an independent political body that is capable of acquiring a common view of problems and arriving at a common decision has been a basic principle of the process of European integration since the 1950s. This has continued to be the case with the various drafts of the so-called European Constitution (cf. Treaty Establishing a Constitution for Europe) as well as the subsequent Lisbon Treaty (cf. Draft Treaty Amending the Treaty on European Union and the Treaty Establishing the European Community).

Another important issue is that today's European democracies differ significantly from each other in respect to their political traditions and character. This makes it senseless and unrealistic to endeavor re-making them according to a model that is not only irrational in terms of the open society, but would also replace their plurality and rationally grounded democratic sovereignty with an institutional entity

that would amount to an irrational all-European state. But it is precisely this type of unrealistic undertaking that has shaped the model of European integration that has increasingly been materialized since the early 1950s. Perhaps it has finally collapsed, however, with the failed ratification of both the *Treaty Establishing a Constitution for Europe* in 2005 and the *Treaty Amending the Treaty on European Union and the Treaty Establishing the European Community* in 2008.

What light does the preceding analysis cast upon the legitimacy of the supranational institutions of the European Union? Most obviously, the EU's so-called objective or external dimension, (Bellamy and Castiglione 2003, p. 5), which involves the delegation of sovereignty to a common High Authority, is completely incongruent with the principle of democratic accountability, because of the simple fact that there is no European people. Those institutions are thus *not democratic in nature*, and they *do not correspond with such formal and substantive norms as legality and human rights*. Furthermore, the obvious non-existence of a European people absolutely precludes any possibility of common *socially accepted norms* in respect to the so-called internal or subjective dimension of legitimacy (cf. *ibid.*).

This clearly undercuts any notion of common European customs and beliefs that might be able to support the existence of formalized processes of cross-European authorization upon the basis of various *indirect* forms of consent (cf. *ibid.*).

As Marcus Höreth states, the European Union's system of governance "has, in contrast to the political systems of the member states, no sufficiently democratic quality" (Höreth 2002, p. 16). He adds that "if the EU applied for membership in the EU, it would have to be turned down because of its non-democratic nature" (*ibid.*).

The key element in such EU governance is the so called "community method" developed by Jean Monnet, which consists of the exercise of power by the supranational, non-democratic, and, in Popper's terms, *highly irrational institutions* of the European Commission and the European Court of Justice. Their only source of legitimacy that is somewhat consistent with Popper's concept of humanitarianism, or rationality as an essential characteristic of the open society, is a particular type of indirect or borrowed legitimacy that has been termed *social legitimacy*. This is produced "by feedback between the actions of the European bodies on the parliaments of the member states" (*ibid.*, p. 18).

Höreth correctly observed that

the governments and parliaments of the member states continue to exist, and it is they that maintain political accountability. They are the only actors in European politics that can be effectively held politically accountable by voters in national elections. In contrast with the European level, the national level provides the structural and socio-cultural prerequisites for a lively and rational democracy. Consequently, both European rule as well as observation of the principles of political accountability are still dependent to a great degree on feedback from the member states... It is by virtue of the strong feedback between European politics and the democratic member state that the EU institutional arrangement eventually responds to the need to keep supra-state decision-making processes rational and democratic (*ibid.*).

Lüder Gerken and the former German President Roman Herzog add support to this view by arguing that the non-democratic and, in Popper's sense, irrational supra-state

institutions of the EU, such as the European Commission, in fact serve as a large-scale refuge for the blatantly undemocratic activities of the member states' executive appointees (cf. Herzog and Gerken 2007).

The evident non-existence of EU rationality and humanitarianism in Popper's terms, and its consequent illegitimacy in the terms of democratic civilization, continue to induce the proponents of EU supranationalism to resort to eccentric normative constructions in its defense. An instructive example is Jürgen Habermas' view of the history of European nation-states. He maintains that there are two lessons to be learnt from the history of the European nation-states. If the emergence of national consciousness involved a painful process of abstraction, leading from local and dynastic identities to national and democratic ones, why, firstly, should this generation of a highly artificial kind of civic solidarity – a “solidarity among strangers” – be doomed to come to a final halt just at the borders of our classical nation-states? And secondly: the artificial conditions in which national consciousness came into existence recall the empirical circumstances necessary for an extension of that process of identity formation beyond national boundaries. These are: the emergence of a European civil society; the construction of a European-wide public sphere; and the shaping of a political culture that can be shared by all European citizens (Habermas 2001, p. 16).

First of all, what Habermas described as a “painful process of abstraction” does not appear to be as artificial as some of its recent results. He unfortunately does not take into account the conspicuously early emergence of state consciousness at the beginning of the Middle Ages following the break-up of the Roman and Frankish Empires. That is to say that the preconditions for individual political nations, namely, peoples and their non-artificial states, have existed and developed in Europe throughout the various dynastic periods, not afterwards. This is why the modern European phenomena of nation states with their civic solidarities cannot be judged to be artificial, with the important exception of Germany and its essentially pluralistic state tradition. The “borders of our classical nation-states” are not merely modern artificial formations, but have rather arisen from deeply rooted traditional entities that developed over the course of many, many centuries. Habermas' astonishment at the stable borders of European democracies thereby appears to be without any justifiable foundation.

Secondly, insofar as the conditions necessary for national consciousness are by no means artificial, they do not recall, in contrast to Habermas' account, “the empirical circumstances necessary for an extension of that process of identity-formation” beyond the rationality of individual democratic states. In addition, Habermas obviously did not take into consideration the different *rational* political cultures, public spheres, and civil societies of individual European democracies, which differ from each other significantly. For such reasons, it is senseless and unrealistic to endeavor changing European democracies in accordance with an irrational model that would replace their plurality and democratic sovereignty by an institutional entity constituting an all-European state.

In conclusion, the European Union's institutional structure has evidently evolved into a sophisticated post-democratic entity that is irrational, regulated, closed, non-liberal, and undemocratic in nature. The European Union thus comprises a serious

enemy of the open society, in Popper's sense at the very least. In addition to the traditional adversaries that we already know, the European Union as a relatively new type of historical phenomena thereby poses a new type of sinister threat to the freedom of both individuals and open societies.

## Notes

1. It is noteworthy that the new treaty was rejected in the Irish referendum on 12 June 2008.

## References

- Bellamy, Richard. Castiglione, Dario. 2003. Normative theory and the EU: Legitimizing the Euro-Polity and Its Regime. <http://www.huss.ex.ac.uk/politics/research/readingroom/CastiglioneBellamyNormativeTheory.doc>. Accessed 10. July 2008.
- Draft Treaty Amending the Treaty on European Union and the Treaty Establishing the European Community. <http://consilium.europa.eu/uedocs/cmsUpload/cg00004re01en.pdf>. Accessed 10. July 2008.
- Habermas, Jürgen. 2001. Why Europe Needs a Constitution. *New Left Review* 11: 5–26.
- Herzog, Roman. Gerken, Lüder. 2007. Europa entmachtet uns und unsere Vertreter. *Die Welt* 13. 1. 2007.
- Höreth, Marcus. 2002. Das Demokratiedefizit lässt sich nicht wegreformieren. Über Sinn und Unsinn der europäischen Verfassungsdebatte. *Internationale Politik und Gesellschaft* 4: 11–38.
- Monnet, Jean. 1978. *Memoirs*. New York: Doubleday.
- Pinder, John. 1995. *European Community. The Building of a Union*. Oxford: Oxford University Press.
- Popper, Karl Raimund. 1995. *The Open Society and Its Enemies*. Vol. I. *The Spell of Plato*. London: Routledge.
- Popper, Karl Raimund. 1996. *The Open Society and Its Enemies*. Vol. II. *The High Tide of Prophecy: Hegel, Marx, and the Aftermath*. London: Routledge.
- Treaty Establishing a Constitution for Europe. <http://europa.eu.int/eur-lex/lex/JOHtml.do?uri=OJ:C:2004:310:SOM:EN:HTML>. Accessed 10. July 2008.

# Open Rationality: Making Guesses About Nature, Society and Justice

Alain Boyer

*Nous avons une impuissance de prouver invincible à tout le  
Dogmatisme; nous avons une idée de la vérité invincible à tout  
le Pyrrhonisme*

Blaise Pascal

**Abstract** We are problem solving active guess-workers, capable of an indefinite number of anticipations, which we have to criticize through the analysis of their deductive consequences. “Inductive inferences” are an illusion; either they are pure guesses, or enthymematic deductive reasonings, with an implicit “fair sample hypothesis.” Our aim is not justification or even consensus, but truth. And it is because our theories are true or truthlike that they have some successes. This can be generalized to the realm of moral and political concerns: our aim is not consensus, but the invention or safeguarding of just institutions. Popper’s theory of democracy appears as a deliberative theory of representative democracy. Its rationality is inscribed in its method, the liberal theory of public critical discussion. Consensus, after dissent, is only a fallible sign of our possible grasping of some part of what should be justice in an Open Society.

Critical rationalism is one of the rare contemporary systems to cover nearly all classical problems of philosophy, from cosmology to morals. As an open system should be, it claims to understand the world and our place in it, but it is open to criticisms, and I definitely do not assert that it is a true system. Only that it is a conception of the world quite interesting to challenge, and that it contains important truths. Anti-Popperians are welcome, if they try first to read Popper carefully and also to feel the appeal of his open system. I will submit three ideas, which owe everything to Popper, except perhaps their falsity content when they are stronger than anything that he said.

1. Popper was essentially right about verification and passive induction: the former is inaccessible, outside the formal sciences, and the latter is a myth: we are active

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and (more or less) creative guess-workers, equipped with an indefinite number of anticipations and prejudices, coming more often from our genetical make-up and from tradition than from direct experience. (Popper used the expression “horizon of expectations” from 1948 on [Popper 1972, Appendix 1].) But we can together criticize our guesses by testing their deductive consequences. Knowledge is a special kind of art, and “inductive inferences” are an illusion. Either they are guesses, or enthymematic deductive reasonings. As Russell said: “What is called induction appears to me to be either disguised deduction or a mere method of making plausible guesses” (Russell 1903, p. 11, footnote). One can interpret “plausible” as “close to our background knowledge”, and add with Popper that scientists had often to imagine implausible, “bold” guesses to respond to unanticipated difficulties. Remember Einstein’s problems with simultaneity, and Kepler’s with circular movement.

2. Our aim is not justification or even consensus, but truth. Moreover, it is because our theories are true or truthlike that they have some successes. In our understanding of Nature, we have been rather successful because “World I” (the set of all real physical states) is not a strategic gambler. This may explain why we produce general hypotheses that are tested only in a very small part of their scope, and then can be regarded as good candidates for truth as correspondence to reality. To say that means that methodology is not entirely neutral vis-à-vis metaphysics: it does not imply that there are strict laws, only our nomological theories do this, but that there is a possibly deep reality to discover, and not only the so-called empirical “given”.
3. The situation is a bit different in social sciences and in social engineering, because the social world, which is so to speak an intersection of World II (the mental states) with World III (the cultural products), has among its elements interactive conscious minds, which can react to publicized ideas about themselves. But, this does not undermine the necessity of trying to understand the social world by sophisticated methods of trials and errors: it just reinforces the “poverty of historicism”. Our quest for freedom and justice is also a process of trials and errors toward these aims, in the context of a deliberative and open representative democracy.

The “trilemma of justification” was first expounded by some Greek thinkers (probably some Sophists), as Aristotle stated it in his *Posterior Analytics* (I, 3). It was repeated by Sceptics (Agrippa, according to Sextus), and rediscovered in a different version by Fries, and then Popper (referring to Fries), and Popperians such as Hans Albert, who came back to the first version (“Münchhausen’s trilemma”). Any attempt to justify a proposition leads either to an infinite regress, or to a vicious circle, or to an arbitrary dogmatism. As David Miller has noted (Miller 1994, p. 57), this is rather a dilemma, since a vicious circle is an infinite regress of sorts. Aristotle, denying Plato’s innatism as defended in the *Meno*, offers his own solution in the last chapter of the *Post. An.*, combining mysteriously an empiricist inductivist “bucket theory of the mind”, in Popper’s words (Popper 1972, Chap. 2, Sect. 12), and a kind of theoretical intuitionism, thanks to the function he eventually ascribed

to the infallible “*nous*”. Popper described the “third” alternative as “psychologism” (in Fries’s version) (Popper 1959, Chap. V), alongside dogmatism and infinite regress. Psychologism, the doctrine that “statements can be justified not only by statements, but also by perceptual experience” faces the problem that Popper called the problem of the “transcendence inherent in any description”. This is because “immediate experience is only once ‘immediately given’, it is unique”. Universals cannot be “reduced to classes of experiences”. This problem is quite difficult, and I shall not pursue its investigation. Let us concentrate on the dilemma between infinite regress and dogmatism.

From Plato to Husserl and to Wittgenstein’s *Tractatus*, excepting the sceptical tradition, all philosophy is more or less “dogmatic”. Popper (as correctly interpreted by Bartley) initiated from the early 1930s a genuine “third” alternative (besides infinite regress and dogmatism), which he once called “dynamic scepticism” (Popper 1972, p. 99), situating himself in the Carneadean tradition: eschewing “justification”, and seeing the pursuit of truth as a matter of discussing critically some of the various relevant consequences of our (preferably multiple and incompatible) tentative solutions to the problems we face. The intersubjective, “friendly hostile”, and objective critical debate replaces justification as the mark of rationality. In consequence, “induction” no longer refers to a justifying process, but is seen as a misleading term for the element of generalization inherent in most of our guesses, incidentally always (to some degree) theory-laden. All we can do is to prefer a hypothesis to others among those we have provisionally been able to frame. And, we should prefer the hypothesis that resists our best criticisms better than the others do. If more than one is equally resistant to our critical scrutiny, we can, especially if we have to act, choose any of them. But, inductivists are not convinced by this critical deductivist turn, which still gives a positive and attractive idea of human beings as bold, imaginative and critical problem-solvers. They continue to claim that we persistently induce conclusions directly from a pure evidential basis. (Others speak, after Peirce, of “abduction”, or of “inference to the best explanation”. I am reluctant to speak of “inferences” with no rule, and I should argue that it is trivially true that we look for the best possible explanation [the true one] when we are involved in guess-working.) If this were so, Popperian theory would be born-refuted, as a silly scepticism, or an irrationalism. A falsificationist has to show that Russell was right, and that the so-called “inductive inferences” can be “simulated” by a conjecturalist-cum-deductivist position, in the same way that learning by passive instruction (and passive association) can be simulated by the better quasi-Darwinian two-stages process of imaginative creation and critical selection. (“Quasi-Darwinian”, because if evolution has no aim, knowledge has at least one, truth. It therefore contains also a “Lamarckian” element.)

Induction by enumeration is “ampliative”, because its conclusion transcends the premises. Inductive reasoning is not “monotonic”: it can always be expanded by introducing a new premise that permits the assertion of a contrary conclusion; the reasoning is non-robust. But, deductively invalid arguments can always be made deductively valid by reinforcing the premises. “Hume’s problem” has been differently regarded, according to the way one would choose to deal with the invalidity of

inductive arguments. Hume had proved that the conjunction: “evidence + deductive logic” is unable to permit us to predict anything more than that every theory compatible with the evidence is possibly true. And that every categorization (Goodman’s problem), if not arbitrary, must be somewhat *a priori*, but not *a priori* valid: in Popperian terms, evolutionary epistemology replaces Kantian transcendental “deduction” (*a priori justification* of our supposed *a priori* knowledge of Nature), criticized by Popper in his 1932 manuscript, published in German in 1979 (Popper 1979). Anyway, either one tries to vindicate the ambitious idea of constructing a formally satisfactory theory of partial validity, or one looks for a principle whose adjunction to the premises of the argument would render it deductively valid. The idea in the latter case is that “inductive arguments” are nothing but *enthymemes*. The most famous candidate for being the missing link was the “Principle of the Uniformity of Nature” (PUN) (Mill 1843, Book III, Chap. III, in a footnote, Mill admits the non-universality of the PUN, and states that he prefers local principles, but he does not systematize this idea.) But the PUN is vague and too strong to be true, and is shown to be false each time the conclusion of some induction is refuted, as Popper argued in his *Logik der Forschung* in 1934 (Popper 1959, expanded English translation). One must look for a weaker implicit premise.

Admitting for the sake of the argument the dubious idea that one starts only from the “evidence” (Popper rightly argues that we always start from a more or less complex problem-situation), let us call it the “sample”. I propose that the missing link has to do only with this sample, and not with what is not in it, and not with samples in general, but that it is such that its conjunction with the evidence yields the generalization as a deductive consequence. As no logical criticism can be raised against any enthymematic completion of an incomplete argument, I should insist that the construal be *psychologically plausible*. One idea that meets these requirements is the one on the *fairness* of a sample (see Cohen and Nagel 1939, Chap. 9, Sect. 2: “The Rôle of Fair Samples in Induction”). Let me call “Fair Sample Hypothesis” (FSH) the hypothesis according to which the particular sample constituting the evidence is (more or less) *representative* of the population we are interested in, even if it exhibits a diversity of types ( $x\%$  of white swans,  $y\%$  of black ones). The FSH is *local*, but it is comparatively *a priori*, because it can be asserted independently of the evidence and possibly *before* the evidence is gathered. It is mainly *about* the evidence, but not obtained *from* the evidence. It *pertains* to the facts, but cannot be *derived* from them, even “inductively”. It asserts something *about* the evidence, e.g., that the evidence has the property of being a (more or less) fair image of some larger class than itself. No one, even an enthusiastic inductivist, could claim it has been obtained by gathering information from experience: “*evidence itself never says about itself that it is representative of something else*”, so that “*no sequence of observations of the elements of a sample can tell us that we are faced with a fair sample*” (Popper 1983, p. 76). This is Hume’s result in a nice nutshell. The FSH is a guess. Whatever the reasons the “inducer” has for dreaming it up, it is possible to criticize it and to submit it to tests.

The generalization procedure is a less important matter than empiricists usually take it to be, but, still, it is something that cannot be denied, in everyday life as in

the history of science. According to Popper himself, the “demand for the highest degree of universality” is perfectly in order (Popper 1959, Sect. 36). He described that “trend” as “quasi-inductive” (*ibid.*, Sect. 85). So, my claim is that even if the inductive inference from repeated observations has no rule of its own, it can be plausibly *simulated* by a hypothetico-deductive mechanism, which would produce the “ampliation” without any ampliative rule. Given the significance of the underdetermination of universal theories by the evidence, I do not really see Bacon’s so-called “eliminative induction” (Popper is sometimes unfair to Bacon, as Koyré was) as an alternative to the “method” of conjectures and refutations. Recall that Duhem never said that the refutation of a theoretical system was impossible, but only that one cannot deduce from it *where* the mistake is in the system (“où gît l’erreur”). We have to try out many possibilities, avoiding the use of ad hoc and immunizing strategies, added Popper. The fact that refutation, which is itself a conjecture, is not an easy matter is trivial. A theory has also to be defended. But I should insist on the anthropological fact that consensus on falsifying observational hypotheses is easier to obtain than consensus on abstract theories. Otherwise, one could object that we can only state a contradiction, without being able to take a decision. It would be a bankruptcy of empiricism, as Popper objected to Neurath in 1934 (Popper 1959, Chap. V), even if he accepted his anti-foundationalist “boat” (but not his coherence theory of truth, also fiercely criticized by Russell).

The FSH proposal is not affected by the objection raised against the PUN: as it is on each occasion a different hypothesis, the refutation of a particular FSH says nothing about any other FSH. Concerning the psychological plausibility of the process which consists “à prendre son cas pour une généralité”, I can only confess that it seems to me at least as plausible as any other proposal I am aware of, for instance that we are equipped with some complex non-classical logic. Perhaps, it can be regarded as an echo of the idea that we cannot possibly cope with such a complex world without first simplifying it, for example in projecting our local knowledge on to the unknown. Besides, we are often wrong when estimating that our samples are sufficiently fair. This is why we must be quite critical of our selection procedures. The explanation of our extrapolative propensity could be that we sometimes think that the world of our experience is, from a certain point of view, a *microcosmos* of the larger world itself. Without this capability, we could not survive. We learn much when Nature gives a negative answer to our conjectures and permits us to revise them. We just suppose that Nature is not deceiving us when we try to impose on it some frame. Let us speak of the (metaphysical) hypothesis of the exclusion of any “Malin Génie”, using the Cartesian phrase, or of any “conspiracy theories”, in Popper’s words. This permits us to exclude the idea that all our samples are unrepresentative. I would call that metaphysical theory a conjecture about the “non-magical constitution of Nature”. It is not a *presupposition* of research, but a metaphysical explanation of the success of a methodological rule that I would call “methodological atheism”: “Conjecture about Nature as if God does not exist (especially a deceitful one) !”, and it seems to be also a consequence of all our best theories: they typically imply the non-existence of any hidden intentionality in Nature. If we act as if God does not exist, this has no theological consequence

at all, because an “as if” is free of any ontological commitment. However, it is the negation of the theist regulative idea proposed by Kant in what is now called “biology” and in history (Boyer 2004). This “atheist” methodological rule may explain why we do not hesitate to regard fairly selected samples of observations as probably representative of the infinite set of all possible observations in the domain, and then as good candidates for being true or truthlike. This would perhaps solve the problem of the conjectural link between real corroboration (non-infirmary, in spite of our best efforts to refute the theory) and verisimilitude (unfortunately at best an intuitive idea for the moment [Miller 1994, Chap. 10]). Our innate tendency to extrapolate from random samples would be a bad method of survival if the natural world was a magical one, deceiving us into believing that a severely tested hypothesis is possibly universally true, when it is true only in the finite domain where it has been tested (through independent tests). We would have to play with a strategic enemy. (Cf. Weber on “*die Entzauberung der Welt*”, and Einstein: “*Raffiniert ist der Herr Gott, aber boshhaft ist Er nicht.*”) Our hypotheses may have to be completed by auxiliary assumptions, including denials of the existence of causal factors other than those used in the theoretical model. These logically possible other factors are considered as nonexistent or negligible, which is an independently refutable hypothesis. As David Miller reminded me, this is not a *ceteris paribus* clause. (For a formulation of Newton’s theory of gravitation including this proviso, see Popper’s reply to Lakatos, in Schilpp 1974, Vol. II, p. 1008. On the fact that we *propose* nomological hypotheses that each imply the metaphysical proposition that there is at least one natural law, [see Popper 1983, p. 73], and against the use of *ceteris paribus* clauses, *ibid.*, p. 288.)

Truth, not justification, not consensus, is our main concern. Tarski’s theory of truth was regarded by Popper as one of the greatest achievements of modern logic, especially in view of its elegant and efficient clarification of a most important distinction, namely that between language and metalanguage(s), a distinction already envisaged by Russell in his Foreword to the *Tractatus*. Not all what Popper had to say about truth can however be reduced to the question of interpreting Tarski’s work and to the idea of eliminating the traditional objections against regarding truth as correspondence to “reality” (i.e., to something whose nature need not and cannot be settled by logic, which is only “the organon of criticism”). One can rather argue that his recurrent proposal to regard truth as something objective but not “manifest” (Popper 1963, Introduction) is the crux of his approach. Truth can be “superhuman” (Popper 1972, p. 159, on the “third world”), and yet be regarded as an effective “regulative idea” (*ibid.*, p. 318) for the fallible human cognitive progress. Truth is the main goal of the adventure of ideas, and it is not trivial that, without involving ourselves in insuperable difficulties and even more in dubious metaphysics, we can suppose that all we want to assert is true.

As Rawls aptly said, truth and justice are strikingly similar, occupying similar lexical priority in the search for knowledge on the one hand, and the building of good institutions on the other (Boyer 2005). According to Popper, we can learn, that is eliminate some mistakes we have rationally but erroneously made, and find better candidates, without being able positively to prove that we have captured the truth.

Certainty and all the other epistemological notions are definitely to be disconnected from questions of truth; and this separation of certainty and truth must be observed even at the observational level, the one of observational linguistic reports. If one is not in possession of a general criterion of truth, and if the latter is not self-evident, even in the eyes of a supposedly unbiased mind, a major breakthrough is made against the classical view that knowledge is analytically linked with certainty and justification. Locke, for instance, continued to insist on the necessity of that link. He thereby opened the door to idealistic objections based on his inability to show that we can legitimately propose bold theories about a mind-independent reality, as genuine candidates for the status of scientific knowledge, because we get absolute certainty only for our subjective “ideas” (“the veil of ideas”).

Along with the fallibilist turn, some theses emanating from the sceptical tradition have to be acknowledged as far from absurd, and Popper could find support for fallibilism in Gödel’s and Tarski’s results, which show that even in mathematics truth and provability are different matters. If truth, and not only prediction and technical domination of nature (“control”), a Baconian idea that Popper disliked, is the main *telos* of scientific research, then science and metaphysics are not different in their *ends*. And since falsifiability is not as a rule transmitted by deduction, some (meaningful) metaphysics follows from any scientific theory. More importantly, one has to find *means* of achieving progress. Truth is not manifest, as religion and the Enlightenment wanted dogmatically to assert, but falsity may be recognized, if only in a fallible manner, by means of the notion of contradiction, provided “we are lucky”. Whenever we think we have discovered a contradiction within the system of our theories, including certain well tested empirical reports, we can guess that there is *somewhere* a falsehood, a weak spot in the system, from which the rational critical debate may begin. Nothing can replace critical discussion and the united search for mistakes, in the light of the unending quest for truth. As Quine said (Quine 1950, Introduction), the aim of science is to classify propositions into true and false, and Miller added that this hard job is enough, and it is neither possible nor necessary to *certify* them. Truth is not, as it is claimed by Apel, a “transcendental pragmatical *presupposition*” of discussion, unless this claim is interpreted in a trivial manner: it is only too easy to show that every assertion implies that what is asserted is true. But almost every assertion could be false, and even a Peircean or Apelian “ideal community of free thinkers” could be wrong and fall short of the truth. Yet truth, like logical validity, is an ideal or a normative standard of all intersubjective discussion. No previous philosophy of science seems to have placed such importance on the notion of a real and passionate rational discussion (not to speak of an “ethics of discussion”), and this new emphasis is the result of the “negativist” answer Popper proposed to the problem of induction. In the absence of any “inductive method”, nothing is better for our finite understanding than to propose imaginative tentative solutions to our problems and to challenge others to criticize them with good deductive critical arguments; for a non-deductive “consequence” of what I claim cannot possibly be used against me. A non-deductive critical argument would simply be sophistry. (There is something “moral” in the responsibility we have to accept the deductive consequences of what we assert.) Note that it is not necessary



to presuppose that truth or “the one final truth” can in the end be captured: Popper argues (1983, p. 74) that one can look for something without presupposing the existence of that thing. The elimination of mistakes, which is not incompatible with the tentative use of methodological or even metaphysical regulative ideas, is our sole means towards our end, namely the truth, which is anyway “hard to come by”. Mistakes are not the result of an abnormal use of our cognitive capacities, as Kant still thought, but the normal way towards human knowledge, thanks to our willingness to detect and correct them in the light of the norm of truth. But, how is it possible for objective truth to be recommended as an ideal? Is it not a ludicrous and idle metaphysical idea, supposedly given up by Wittgenstein (II) as well as by Heidegger? In order to be able to assess the impact of Tarski’s semantics on Popper (as well as on Carnap and Hempel in the early 1940s), one has to remind oneself of the rejection in the early twentieth century by so many philosophers of the very idea of truth as correspondence to a normally non-linguistic reality: that idea was indeed rejected by pragmatists as well as by logical positivists, and especially by Neurath, not to speak of the Nietzschean genealogy, which amounted to thoroughgoing doubt about the idea of searching for an objective and absolute truth. Even Bergson did not hesitate to give his support to James’s views on truth, views that Russell tried in vain to debunk.

Popper never renounced his adherence to an absolute and correspondentist conception of truth. Close to Russell on that point, he refused coherentist, pragmatist and verificationist conceptions, maintaining that we need not abandon the classical view implicit in the Aristotelian tradition, even if it has to be cautiously reformulated. Russell held the same, without using Tarski’s work much, whereas Popper decided in 1935 that this work provided an immense breakthrough in rehabilitating the idea of correspondence (something cautiously asserted by Tarski himself, who spoke of “doing justice to the intuitions which adhere to the classical Aristotelian conception”). It should be recalled that in 1934, just before meeting Tarski in Vienna, Popper had written that his own approach rendered it “possible to avoid using the concepts ‘true’ and ‘false’, their place (...) taken by logical considerations about derivability relations” (Popper 1959, Sect. 84). Yet, contrasting corroboration and truth, he maintained at the end of that section that “we should not normally say of a theory that it is hardly true at all so far, or that it is still false”. (A footnote [1959] then explains that Tarski has shown correspondence to be reducible to “satisfaction”). Further, it is worth citing a passage written in 1946 (Popper 1963, Chap. 9, p. 214) in which Popper held that “facts are something like a common product of language and reality; they are reality pinned down by descriptive statements”. The important idea seems to be the Bühlerian notion of a descriptive function of language. This older position of Popper’s could seem less “naïve” than the one adopted later, according to which truth is simply correspondence with the facts, as if these were preestablished atomic constituents of the world, a Wittgensteinian (I) position Popper criticized fiercely in the text just mentioned. It appears that Popper did not see any variation in his development, since he himself quoted this older text of his in his reply to Bronowski in the Schilpp Volume (Schilpp 1974, Vol. II, p. 1095), maintaining that “we must not allow ourselves to suffocate the important processes



of the world under an overwhelming plethora of facts". (Popper's ontology is Heraclitean–Whiteheadian: Reality as Processes.) Without speaking of very deep and abstract theoretical statements, let us remember that even an apparently simple factual statement such as "Snow is white" is, "in fact, deeply impregnated by theory". I should just add that the distinction between facts and events is still unclear and needs to be elaborated much more than Popper elaborated it.

Anyway, World I appears to fit rather nicely the idea that it is not a strategic gambler. In the social sciences, and especially in the context of an open society, the situation is different: the primary objects of the inquiry are intelligent agents (always situated in a natural and institutional environment). This means that a tentative description of their behaviour and the effects of these (the core of "methodological individualism", the Popperian version of it being an institutional one: Agassi 1960) can be understood by them, and consequently their behaviour can change. That is a case of the phenomenon aptly and ironically called "Œdipus effect" by Popper ("ironically", because it means implicitly that Freudian psychoanalysis, one of whose main tenets is none other than the Œdipus complex [and not the particular testable theory of paranoia], often has its predictions "confirmed" through a suggestion effect). On a particular application of Popper's theory of the Œdipus effect, see List and Pettit 2004. We must maintain a "methodological atheism", in particular in history, because the methodological theism embraced by Kant leads to historicism, in Popper's sense ("History has a plot"). But we have to take into account the fact that people can be influenced by predictive theories or ideas: Marxism underestimated the rôle of ideas in society, supposed to belong only to the "superstructure", but it was itself a refutation of this idea. Marxism has had a big influence on modern history, but with effects different from its predictions. As a political theory, it is both refuted and self-refuting.

The more open is the society, the more likely is it that its everlasting changing complexity will defy our methods of prediction, reenforcing Popper's arguments against historicism. The uncertainty of our future is the price to pay for the openness of our world, even if science grows in such a way as to permit us to predict events better and better. But the very existence of a predictive science, via technology, itself complicates our world, and diminishes science's own capacity to predict social events. To come back to Œdipus effects, it is clear that we may hope to be able to build corroborated theories in the social field only if we take them seriously. Contrary to Nature, especially non-living matter, we have to introduce the possibility of the influence of what we say on what we say it about, that is, human beings. But this only leads us to an even a more anti-inductivist attitude. The possible influence of Gallup polls on elections is an example of the fact that we have to be very cautious in our extrapolations: the *global analysis of the situation* (Popper 1983, p. 306) is a much better way of dealing with social complexities than with any inductivist procedure.

Politics also is a matter of cautious procedures of trials and errors, with the big proviso that we must not regard citizens as pure objects of experimentation, but as "ends in themselves". Our aim is not absolute consensus, but the invention or safeguarding of just institutions, insofar as we are able to minimize avoidable

sufferings. The tentative prediction of previously unexpected consequences of our actions is one of the main aims of social and political science, a kind of theory of “the resistance and resilience of the social material”, in Popper’s words. We need governments, but it is much better if we can replace them without any violent revolution. Popper’s “eliminative” theory of democracy (Popper 1966, vol. I, Chap. 7) could be read as an elitist theory à la Pareto or Schumpeter, but with its inscription in the more general Popperian theory of rationality, it appears clearly as a deliberative theory of representative democracy. All its rationality is inscribed in its method, the liberal theory of public critical discussion and of “checks and balances”. In that process, no “induction” has value, excepted if it is construed as a process of guesswork, augmented by fair sample hypotheses, open to criticism, and taking seriously into account the importance of Oedipus effects. Consensus, after a time of stimulated dissent (Boyer 1995), is necessary, especially about the principles of open discussion and equality before the law, but it is not our aim, and is no more than a sign of the improvement that we desire towards freedom, peace and justice.

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## References

- Agassi, Joseph. 1960. Methodological Individualism. *The British Journal of Sociology*, II, 11, reprinted in O’Neill John (ed.), *Modes of Individualism and Collectivism*, London: HEB, 1973.
- Boyer, Alain. 1995. Democracy and Disagreement. *Ratio Juris*, Bologna and Oxford, 8(1): 1–8.
- Boyer, Alain. 1997. Induction as Fairness. *Cahiers du CREA*, Paris: Ecole Polytechnique.
- Boyer, Alain. 2004. *Kant et Epicure*, Paris: PUF.
- Boyer, Alain. 2005. Is an Open Society a Just Society? Popper and Rawls. *Learning for Democracy*, Critical Press, 1(2): 7–27.
- Cohen, Morris and Nagel, Ernest. 1939. *An Introduction to Logic and to Scientific Method*, London: Routledge.
- List, Christian and Pettit, Philip. 2004. An Epistemic Free-Riding Problem? In *Karl Popper, Critical Appraisals*, eds. Catton, Philip and Macdonald, Graham, London: Routledge, Chap. 7.
- Mill, John Stuart. 1843. *A System of Logic Ratiocinative and Inductive*, London.
- Miller, David. 1994. *Critical Rationalism. A Restatement and Defence*, Chicago, IL/La Salle, IL: Open Court.
- Miller, David. 1995. How Little Uniformity Need an Inductive Inference Presuppose? *Critical Rationalism, Metaphysics & Science. Essays for Joseph Agassi*, Vol. I, Dordrecht: Kluwer. Now Chap. 8 of his (2006).
- Miller, David. 2006. *Out of Error. Further Essays on Critical Rationalism*, Aldershot, England: Ashgate.
- Popper, Karl Raimund. 1959. *The Logic of Scientific Discovery*, London: Hutchinson.
- Popper, Karl Raimund. 1963. *Conjectures and Refutations*, London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1966 (First edition 1945). *The Open Society and Its Enemies*, London: Routledge.
- Popper, Karl Raimund. 1972. *Objective Knowledge*, Oxford: Oxford University Press.

- Popper, Karl Raimund. 1979. *Die beiden Grundprobleme der Erkenntnistheorie*, ed. Troels Eggers Hansen, Tübingen: Mohr.
- Popper, Karl Raimund. 1983. *Realism and the Aim of Science*, London: Hutchinson.
- Quine, Willard Van Orman. 1950. *Methods of Logic*. New York: Holt.
- Russell, Bertrand. 1903. *The Principles of Mathematics*, London: Allen & Unwin.
- Schilpp, Paul Arthur (ed). 1974. *The Philosophy of Karl Popper, The Library of Living Philosophers*, La Salle, IL: Open Court.

# Logic and *The Open Society*: Revising the Place of Tarski's Theory of Truth Within Popper's Political Philosophy

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**Abstract** This chapter retraces the way in which the Austrian philosopher Sir Karl Popper came to accept a Correspondence Theory of Truth from the work of the Polish logician and mathematician Alfred Tarski. It is argued that Popper's use of Tarski's semantic theory of truth reveals crucial insights into the fundamental characteristics of Popper's social philosophy. Quite deceptively, arguments based upon Tarski's theory of truth appear implicitly throughout the text of *The Open Society and Its Enemies* (1945). It is then demonstrated how Popper integrated a correspondence theory of truth into a theory of the functions of communicative language that he received from Karl Bühler.

## Introduction

The exchange of ideas between Karl Popper (1902–1994) and Alfred Tarski (1902–1983) is more complex than it appears at first sight. It is characterised by an innovative yet problematic application of Tarski's thought to other theoretical fields of inquiry by Popper, and a resulting absence of criticism by Tarski in relation to Popper's interpretations of his work. As a result this study perceives the need to subject Popper's use of Tarski's theory of truth to further scrutiny by critically evaluating whether Popper was correct, in his intellectual autobiography *Unended Quest* (1974), correct to claim that: *it was clear that we could learn from Tarski's analysis how to use, with a little care, the notion of truth in ordinary discourse, and to use it, moreover, in its ordinary sense – as correspondence to the facts?* This claim is investigated in relation to Tarski's theory of truth as presented in *The Concept of Truth in Formalized Languages* (1931).<sup>1</sup> On top of this concern, this paper explores what influences Tarski's theory of truth can be seen to have had upon Popper's social and political philosophy, irrespective of the theoretical correctness of its use.

It is argued here that Popper was able to integrate a formal language semantic theory of truth from Tarski into an *empractic*<sup>2</sup> semiotic philosophy of language

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that he had received from his supervisor Karl Bühler (1897–1963) when Popper was a psychology student in Vienna.<sup>2</sup> Out of a synthesis of Bühler's and Tarski's linguistic theories arise many philosophical concerns, the most pressing of which are explored in the different sections of this chapter. Firstly a brief background to the importance of Tarski for Popper's thought is given. Section 3 explores, the problem arising out of an application of Tarski's semantic formal language theory of truth to that of scientific statements. Section 4 looks at the problem of holding a truth predicated for scientific statements and other modes of ordinary language alike. Section 5 deals with the increasing distinction in Popper's latter thought between defining truth objectively as 'satisfaction' or subjectively as 'conviction'. In Section 6 an examination is made of integration of Tarski's semantic theory of truth into an evolutionary, action orientated Bühlerian theory of language. Finally, the implications of Popper's use of Tarski's thought for political philosophy is discussed.

## Background to the Popper-Tarski Connection

This study builds upon the groundbreaking work of David Miller in identifying the importance of Tarski's theory of truth for Popper's work. In *Popper and Tarski* (1999) Miller made the striking observation that it was Popper's political work *The Open Society* that deals most with the work of Tarski. In this paper Miller referred to Tarski's shock at being quoted in the indexes almost as often as Marx.<sup>3</sup> What Miller's work indicates is that scholars of Popper's political thought who have hitherto avoided discussing Tarski in relation to *The Open Society* can no longer do so without neglecting an important feature of his argumentation. What is most striking is the minimal direct reference to Tarski in the body of *The Open Society*, which has undoubtedly been deceptive to scholars of Popper's political philosophy in the past. However, the remarkable extent to which Tarski is referred to in the endnotes reveal the extent to which Tarski's thought underpins much of this political tract.<sup>4</sup> By raising the above mentioned four problems related to Popper's treatment of Tarski, this chapter will show that political philosophers must take the Popper–Tarski nexus seriously in order to treat the theoretical support of the arguments in *The Open Society* with the attention that it deserves.

The way Popper appropriated Tarski's work provides the groundwork to attitudes concerning definitions and argumentation such as his "negativism", that is, his unwillingness to give positive definitions to theoretical concepts, as well as the related notion of *diarrhesis* ('division' or 'distinction') by which meaning is created objectively through the activity between individuals engaged in a particular argument. As these linguistic problems are central to Popper's political philosophy, an investigation into the linguistic theory behind such standpoints is warranted. Joseph Agassi has pointed out that Popper defended liberty through the criticism of theories rather than the criticism of concepts; this can be seen as a result of his admittedly unfortunately named 'negativism', which is his unwillingness to

define concepts positively.<sup>5</sup> If this is the case then Popper's appropriation and understanding of Tarski's theory of truth ought to be subjected to greater scrutiny for the bearing it has upon our understanding of his political thought. In this way a renewed relevancy and direction for scholarly research into *The Open Society and its Enemies* can be gained.

## Applying Tarski's Formal Language Truth Predicate to Ordinary Language Use

Tarski's theory of truth is a semantic theory developed for statements in formal language. The intention of which was to find *a definition of truth which would do justice to the institutions which adhere to the classical Aristotelian conception of truth*.<sup>6</sup> The Aristotelian conception of truth from the *Metaphysics* states that:

To say of what is that it is not, or of what is not that it is, is false, while to say of what is that it is, or of what is not that it is not, is true.<sup>7</sup>

In modern philosophical terminology this is understood as the *correspondence theory of truth*, which was according to Popper, 'rehabilitated' by Tarski. The correspondence theory is defined by Tarski as one in which:

The truth of a sentence consists in its agreement with (or correspondence to) reality.<sup>8</sup>

David Miller has pointed out that there is a problem for critical rationalists in that *when a speaker utters a declarative sentence in ordinary discourse he does more than use it to say what it says*.<sup>9</sup> As Tarski's notion of truth was developed for the use of formal language in logic, it cannot be applied without problems to a study of natural languages in general. According to Jan Wolenski, Tarski himself was sceptical of the possibility of a formal semantics of natural language which includes the application of the semantic theory of truth to natural languages.<sup>10</sup> For Tarski, the object of his investigation into the problem of *the definition of truth* requires a definition which is *a materially adequate and formally correct definition of the term 'true sentence'*. Tarski states that when this definition is applied to colloquial language, the results are *entirely unproductive*. For Tarski with respect to colloquial or ordinary language, not only does the definition of truth seemed to be impossible, but even the consistent use of this concept in conformity with the laws of logic seem also for Tarski to be untenable.<sup>11</sup>

Applying the semantic theory of truth to natural or colloquial language was exactly what Popper did. How exactly Popper convinced himself of this possibility remains obscure as the logical distinction is, according to Hacoen, *put to rest* as all statements are made non-verifiable and hypothetical.<sup>12</sup> In his intellectual autobiography we can see that Popper understood very well that Tarski's notion of truth was developed for and limited to, formal languages, which are the constructed languages for 'science', namely formal disciplines such as mathematics. In a given statement of a formal language there consists two languages: the *object language*

and the *meta-language*, only the latter higher order language could contain the truth predicate. Tarski summarised his results in thesis (A):

For every formalized language of finite order a formally correct and materially adequate definition of true sentence can be constructed in the metalanguage, making use only of expressions of a general logical kind, expressions of the language itself as well as terms belonging to the morphology of language, i.e. names of linguistic expressions and of the structural relations existing between them.<sup>13</sup>

In an obscurely presented argument, Popper in his autobiography states that; *despite Tarski's restrictions it was clear that from Tarski's analysis we could apply this to ordinary language and that the application of this was made clear by Tarski himself*.<sup>14</sup> This claim by Popper takes great liberties with Tarski's insistence in *Logic, Semantics, Metamathematics* (1956), in which he argued that such an application to an ordinary language was not possible. From what Popper elucidates, it appears that since a formal theory of truth can be obtained for a semantic meta-language in which this language can be seen to correspond directly to reality, the jump to claim that ordinary language when speaking of reality can also be shown to be formally true is made.<sup>15</sup>

The consequences of Tarski's understanding of the notion of truth in relation to colloquial language may have had a stronger influence on Popper's social and political philosophy than has generally been recognised. In *The Concept of Truth in Formalized Languages* (1931) Tarski stated that a definition of truth for colloquial language is impossible. Not only can we not hold a positive definition of this concept in ordinary language but even the consistent use of this concept in conformity with the laws of logic is impossible.<sup>16</sup> Tarski was adamant about the impossibility of *defining* truth, particularly in ordinary language, but this does not mean that the semantic conception of truth cannot have any bearing on ordinary human languages, particularly as they appear in scientific statements. Popper was clearly observant of this fact and saw that we could for test statements (that is our scientific hypotheses) and other assertions of ordinary language "proceed with caution". If under Tarski's proscription a test statement is clear in its content, that is, materially adequate as well as correct in form, we can assert that a statement is consistent with the meaning 'x is a true sentence'. This holds as long as the premises are intuitively clear and do not, in Tarski's words, lead to obvious contradictions as is the case with the *antinomy of the liar*.<sup>17</sup> Based upon these minimal requirements Popper had grounds to assert the applicability of this semantic theory of truth beyond the realm of semantics into the realm of everyday language of scientific statements and statements of a political and social nature. However, this raises further questions. If Popper correctly identified that within Tarski's work such implications for a formal semantic theory of truth were possible in the realm of ordinary language, how exactly should one go about applying this? More seriously how is the semantic concept of truth related to other psychological conceptions of truth that we commonly use in everyday speech?

Within Tarski's prohibition of holding a *definition* of truth in the 'object language' and the possibility of saying of a statement in ordinary language that it is 'true' that is, it satisfies the minimal requirements of being materially adequate and formally correct, lies much of the tension and particular characteristics of Popper's political philosophy. A direct result of this can be seen in Popper's belief in the



impossibility of holding fixed definitions for political and social theories, which separates the social from the natural sciences. In a letter to Hayek, Popper describes himself as *a conscious and determined enemy of definitions*.<sup>18</sup> Popper replaced definition with a notion he called *diarrhesis* that he appropriated from Plato's *Laws* (932e), which effectively means a 'division' or 'distinction'. Explaining the advantage of *diarrhesis* over the definition of a word in that *definitions are attempts to lay down some 'absolute' meaning of a term in advance*, knowledge of which for Popper is impossible. *Diarrhesis*, however, holds that the meaning of a concept is always *ad hoc* and pertains to the current problem under discussion.<sup>19</sup> Thus, the distinctions developed and terms used can only be understood in regard to the argument into which they are situated.

For Popper words contain meaning relative to the argument in which they appear. Popper states that *distinctions (diarrhesis) may be always refined, that is, carried one step further; but one should only do so if the needs of the discussion require it*.<sup>20</sup> The implication of Popper's understanding of meaning as *diarrhesis* extends past the problems of definition of terms and the positive understanding of notions. It also increases the difficulty of linking the various aspects of Popper's thought into a whole as one cannot simply relate the logical relationship of arguments to certain constant terms, which may evolve in terms of their content but nonetheless refer to the same thing. For example, there is no relationship whatsoever between the notion of 'world' in *The Open Society* to the same notion in *Knowledge and the Mind-Body Problem* (1994b), the term is once again understood differently in *A World of Propensities* (1990).

From an understanding of this notion of *diarrhesis* we can gain important clues into Popper's understanding of objectivity and how objective knowledge grows conjecturally through the 'argumentative' or 'explanatory' function of language.<sup>21</sup> As Tarski emphasised the logical impossibility of the consistent use of a concept in colloquial language, Popper turned to a development of Bühler's theory of language function to develop a theory of objectivity which adheres to the logical prohibitions espoused by Tarski. It is not argued that Popper's 'negativism' is the result of the direct influence of Tarski, rather it is seen as being based upon the presupposition that knowledge grows, yet we cannot hold any positive concepts or definitions as being true in the most absolute sense. Tarski's role is best seen as enabling an epistemological optimism within a sceptical framework. As such, it provides a central component of his critique of the authoritarian understanding of the ability to possess a known conception of truth from which one can remodel humanity and society.

## **The Distinction between Scientific Statements and Other Modes of Ordinary Languages**

The acceptance of Tarski's semantic theory of truth raises a further problem for Popper's developing thought system. The problem is of applying a meta-linguistic theory designed for formal mathematical languages to ordinary human languages.

A further problem arises when ordinary language is further broken down into the ordinary language of scientific statements and other instances of human utterances, which for Popper are evolutionarily interrelated. One can see that the second mode of ordinary language, that of daily utterances (and perhaps other non-linguistic expressive forms of communication) were not really what Popper had in mind when applying Tarski's theory to ordinary language, but it nonetheless became a necessary adduction as his epistemology increasingly took on an evolutionary character. However, these daily utterances and expressions as they are a critical part of Popper's evolutionary theory must also be treated in a manner consistent with scientific statements. It is also interesting to note the way that Popper applied a theory from one field of inquiry to that of another.

Tarski's theory of truth makes the distinction between colloquial language in Section 1 of *The Concept of Truth in Formalized Languages* and the formalised languages of the deductive sciences in Sections 2, 3, 4 and 5 of this work. It ought to be understood that Popper's work also makes similar distinctions between the deductive psychological work of his early years (in which he aimed to recapture in all of his epistemological and methodological writings) and his latter work in political and social philosophy which develops arguments through analogous methods of inference. Of the latter *Schöpferische Selbstkritik in Wissenschaft und Kunst* (1979b) provides the most poignant example, as the title indicates, analogy from the field of science is used to develop an aesthetic theory. Popper's evolutionism is another example of this latter trend in which evolutionary arguments from biology are extended to develop a theory of human institutions which can be seen in his letter of correspondence with Hayek as dated, 28 May 1944.<sup>22</sup> The epistemological distinction that Popper made between the positive possibility of attaining knowledge in the different fields of inquiry led to the differing modes of evidential corroboration and possibilities of falsification. The conclusions that Tarski arrived at to the problem of the *definition of truth* deserve to be analysed further for the restrictions they placed on Popper's ability to develop theories. Thus, we must once again return to the question, to what extent was Popper's theorisation subject to the logical arguments developed by Tarski in relation to the problem of the definition of truth?

A definition of truth for ordinary languages of every day usage is not possible according to Popper, yet he rightly saw that with caution Tarski's semantic theory can confer theoretical benefits on the understanding of human communication beyond the narrow field of semantics. In 1944 Tarski even stated that: *I happen to believe that the semantic conception does conform to a very considerable extent with the common-sense usage.*<sup>23</sup> As mentioned above, in *The Open Society*, Popper enlarged the sphere of this semantic theory and its notion of truth to the field of ethics by attempting develop valid norms which could be used proscriptively in our moral actions.<sup>24</sup> The belief in the successfulness of this method could also have proven to be an effective enough method which would inspire later applications of theories from science and logic to social and political problems. This is a noteworthy instance in Popper's thought in which an argument based upon what Rudolf Carnap understood as an *implication from analogy* is made.<sup>25</sup> In other words, if an

argument is successful in one field of inquiry, it is reasonable to assume that the argument and empirical evidence also carries validity when applied to another field of inquiry.

## Defining Truth as Objective ‘Satisfaction’ or Subjective ‘Conviction’

It is clear that in Popper’s methodological thought that he understood the notion of truth as consisting of properties that were also sufficiently expressed in the notions of ‘corroboration’ and later ‘verisimilitude’. In this sense Popper spoke of truth much in the way Tarski did, even though its application was seen to operate in modes other than that of a formal meta-language. Tarski focused on the notion of ‘satisfaction’ rather than on the notion of truth *per se*.<sup>26</sup> When Popper spoke of truth in statements that are not convictional, the meaning of this term seems to closely reflect the way Tarski spoke of satisfaction. However, for Popper truth was seen in relation to a criterion of satisfaction for a hypothesis within the method of falsification. In this sense the criterion for truth or satisfaction cannot be seen as identical for Popper as it was for Tarski. Tarski’s notion of truth as Popper developed it may seem restrictive in that it is an objective semantic theory that does not allow for subjective and unutterable convictions and feelings to be admitted as sources of truth in themselves. Why did such a defined and constricting notion of truth appeal to an individual such as Popper whose later works extended into the realm of the subjective? It is clear that both Tarski and Popper both understood very well the complexity and plurality of this notion. Tarski himself claimed that he hoped that nothing he said would be interpreted as a claim that the semantic conception of truth is the “right” or “only possible” one.<sup>27</sup> Tarski understood that in every day language there are many concepts that share the same word ‘truth’, and that these are often based upon various psychological criteria which allow a particular concept of truth to be said to exist in relation to a particular state of affairs.

In Popper’s only theoretical writings concerning the notion of truth, he only ever referred to the correspondence theory as the one that he supported. This, however, does not mean that Popper understood philosophical concerns with the conception of truth from this theoretical perspective alone. His extensive use of the term ‘conviction’ as in the expression “it is my conviction that ...” or the term ‘principles’ such as can be seen in the ‘Ethical Principles’ and ‘professional ethics’ in *The World of Parmenides* (1998) attest to an appreciation of the non-objective sources of truth, despite the tendency to reserve the name ‘truth’ to the correspondence conception.<sup>28</sup> Popper’s language concerning the notion of truth is problematical, and the words he often uses to describe this notion can easily be used to denote different concepts. Popper wrote with such conviction of the goodness that exists in scientific discovery, music and art that it is evident that there is a subjective sense in which the individual can speak of the existence of certainty or truth, however this is not what Popper meant by the term *absolute truth*. Popper’s *absolute truth* is not absolute in

the truest sense of the word; it is what he refers to as a regulative ideal<sup>29</sup> and even as a “heuristic fiction” (*echte Fiktionen*).<sup>30</sup> However, the forcefulness of conviction he displayed in matters pertaining to instances of subjectively perceived law-like certainty show that he must have had an understanding of truth capable of encapsulating the non-rational and non-objective.

## Combining Bühler’s Linguistic Theory and Tarski’s Truth Theory

In *Popper and Tarski* (1999) Miller raises two problems that complicate Popper’s treatment of language in relation to Tarski’s theory of truth. These problems are the *phenomenon of vagueness* and the *problem of selectivity*.<sup>31</sup> The former problem admits of the vagueness inherent in language as ordinarily used and the way that this is in a state of conflict with the world which is not vague. This then provides a problem in finding a correspondence between a true statement and the world of facts. Miller explains Tarski’s solution to the “convictional” holding of a correspondence between a ‘vague statement’ and the world as follows:

The statement “Snow is white” is doubtless vague, but according to Tarski it is true if and only if snow is white. That does not postulate a vague aspect to the world, but states – vaguely, of course – a condition on the world that has to be satisfied if the statement “Snow is white” is to be true.<sup>32</sup>

Indeed, Tarski’s lack of a criterion for truth can be seen along with his proscription for the minimal conditions of *satisfaction* of certain minimal requirements. Once again we come to the problem of the discontinuity of speaking of ‘satisfaction’ and of ‘truth’, of which Popper despite Miller’s statement of the problem of vagueness, is inclined to speak in the bold language of ‘truth’ rather than the more cautious ‘satisfaction’.

Miller’s second concern is that of selectivity. This problem put simply is that which describes the distortion in our knowledge of the object of inquiry as we specialise, focus or simplify the ‘whole situation’ into something manageable. Miller makes reference to Popper’s writings on situational models in *The Myth of the Framework* (1994) in which Popper stated that he did not believe that models whether in physics or the social sciences can be said to be true.<sup>33</sup> This is due to the process of oversimplification in which we choose to focus on certain facts while ignoring others. Miller concludes his paper by emphasising that Tarski’s objectivist theory of truth makes obvious the existence of a dualism of facts and decisions. This dualism, as Miller so eloquently put it, “patrols the boundary of the realm of facts itself.”<sup>34</sup> If this remained the case for Popper, and Miller has shown that it could not have been otherwise, then how could Popper overcome this dualism of facts and decisions, of a vague language and a world which is not vague in order to be able to speak confidently of truth? That is deciding upon what sort of objective knowledge enables us to speak of intellectual enterprises or claims as being true and what sort *ought not to be taken too seriously*.<sup>35</sup> From *The Open Society*

it appears that Popper accepted this irreducible dualism, nevertheless he stated *the idea of absolute truth – of correspondence to the facts – as a kind of model for the realm of standards*. Tarski enables us to *seek* for absolutely true propositions in the realm of facts or at least for propositions which come nearer to the truth.<sup>36</sup> We can never *find* a proposition that is true for all time and places, Tarski did not provide us with a *criterion* for stating that a proposition is true in any sort of absolute sense and neither does Popper.

I propose that the way Popper was able to overcome this dilemma in order to fulfil his convictional need to speak of truth, and thereby avoiding the moral and political dangers of relativism lies in the way he integrated Tarski's theory of truth into Bühler's psycho-linguistics. Thus, it is not only necessary to speak of Popper and Tarski in relation to the problem of truth but also of Bühler's influence upon Popper as well. Bühler's theory was the only other theory concerning language that Popper took to heart. If truth is a notion for Popper that is inherently linked with communication, it is necessary to view this notion as Popper would have, that is, in relation to his understanding of linguistics, or more precisely the extension he made to Karl Bühler's Theory of Language Function.<sup>37</sup> Hence, the way that Popper received Tarski's work on a Semantic Theory of Truth would become for Popper the needed common sense notion of truth which could also be integrated into Bühler's Theory of Language Function.<sup>38</sup> This enabled the survival and evolution of Bühler's work into what would become Popper's late "World Three" ontology. It appears that when Popper appropriated Tarski's criterion for truth he did so with Bühler's notion of the relationship of truth to communicative language in mind rather than as Tarski himself intended the notion to be developed.

Following Bühler's *empractic* theory of language (that is built into praxis) Popper came to see the act of communication on a practical common sense problem-solving level as the realm in which truth operates. For Popper, it was the evolutionary and survival significance of communication that gives meaning to the truth or falsity of our assertions. It is one thing for us to accept Tarski's semantic theory of truth at the "descriptive" level of Bühler's theory of language function, it is quite another to accept it at the higher "critical" or "argumentative" functions. The truthfulness of a proposition as an object of knowledge pertains to the descriptive level of linguistic communication. However, there is also a practical component to the problem of truth and objectivity. At the higher "critical" or "argumentative" level of communication Miller's points concerning the problems of selectivity and vagueness become increasingly important as individuals seek to accept a statement as true over another as a result of personal or political inclinations. A statement may be said to be descriptively true as corresponding to the facts, however, in relation to a particular social problem situation where there are countless facts, a descriptive correspondence theory becomes unfeasible. In such circumstances truth can only be secured objectively through inter-subjective agreement or action, and for this Popper chose the criterion of harm reduction as a test of truthfulness.

Geoffrey Stokes in *Popper: Philosophy, Politics and Scientific Method* (1998) argued in Chapter 8 that despite the antagonism of the so called *Positivist Dispute* there are many areas of convergence between Popper and his critical rationalism and

the critical theorists of the Frankfurt School.<sup>39</sup> This convergence is seen by Stokes in relation to Jürgen Habermas's and Theodor Adorno's subjectivist theories of truth which is seen to have affinities with Popper's work. Stokes argued that Habermas' consensus theory of truth converges with the Popperian concept of objectivity as an inter-subjectivity leading to consensus. The importance of such an argument is that it demonstrates that fruitful research into problems concerning truth and objectivity need not limit itself to either the discursive practices of the analytical or the continental philosopher. In this way Stokes provides a case in which the analytical and continental philosophical divide is overcome. Stefano Gattei, however, in his review of Stokes' book in *The Ethical Nature of Karl Popper's Solution to the Problem of Rationality* (2002) argued that Stokes in making this argument "confuses the concept of truth with that of corroboration, thus misplacing him alongside Habermas."<sup>40</sup> Gattei correctly identifies that the discussion of truth is "crucial" for Stokes's attempt at reconciling these traditions; however, Gattei himself did not take into account the ways in which the notion of truth is seen to operate within Popper's understanding of communication.

According to the Popper–Bühler linguistic schema, at the "critical" or "argumentative" level of language function, a theory of truth would secure objectivity through inter-subjective consensus in an effort to eliminate error and to reduce harm. Descriptively a proposition may be true, that is, it may correspond to the facts,<sup>41</sup> however, in relation to a given problem the same proposition may turn out to be untruthful in light of Miller's concerns mentioned above of the *phenomenon of vagueness* and the *problem of selectivity*.<sup>42</sup> In other words, at the descriptive level of language function, a proposition may be objectively true, as in corresponding to the facts, however, at the "critical" or "argumentative" function of language, a proposition requires at this level an inter-subjective component to secure its objectivity, as well as an alternative criterion of truthfulness from correspondence to the facts, such as a practical instance of harm reduction or error elimination. In this way we can see the importance of the lasting impact that Bühler's theoretical work on communication had on Popper's latter arguments, which is evident in Popper's latter ontology and social theory and is also central to our understanding of his philosophy of science and methodology. Once we take this into account we can appreciate the distinction between the levels of language function and the way truth and objectivity operate within this theory of communication. Eventually, the link between language and evolutionary necessitated problem-solving which are seen in our ability to eliminate error and reduce harm developed to the point that Popper saw a unity of method that drives the search for truth from the amoeba to Einstein.

## Conclusion

An understanding of Popper's intellectual response to his exposure of Tarski's ideas from 1935 onwards provides salient clues for re-interpreting the argumentative oddities in his later thought. From the philosophical standpoint, Popper keenly

observed that Tarski's work on the problem of finding a definition of truth for semantics had implications that went well beyond the field of semantics or science at large and could be used, with a measure of care, to shape arguments in political and social philosophy as well. The presuppositions associated with this argument contributed to the formidable nature of *The Open Society and Its Enemies*. Despite the care and attention in which Popper achieved this integration of a notion of truth from logic to the philosophy of science, the implications for his political philosophy and his sociology of knowledge cannot be said to be unproblematic. The epistemic restrictiveness of holding an objective notion of truth would lead to a strange use of linguistic terminology in the framing of arguments in his latter theorising on aesthetics, institutions and tradition. This indicates the greater importance that ought to be focused upon certain linguistic idiosyncrasies in his non-scientific thought. It can be seen that terms such as 'principles', 'intuitive satisfactoriness', 'conviction', or 'not to be taken too seriously' have implications for our understanding of the justification behind Popper's political arguments. The use of an objective notion of truth, in relation to the various conceptions denoted by this word (and similar words such as conviction) in Popper's political and methodological discourse is evidence of a highly sophisticated understanding of the role of truth for human knowledge.

The strength of the naturalistic analogical method for Popper's political philosophy can be seen in its ability to provide new criticisms of concepts of truth and freedom. By adhering to a strict *negativism*, Popper's political philosophy was most effective in critiquing existing political doctrines rather than subjecting any of his social theories to the requirements of falsification. However the knowledge gained through the criticism of existing doctrines can be seen to lead to new knowledge through the elimination of erroneous assumptions inherent in a particular political doctrine. Tarski's theory of truth, as applied analogously from formal or mathematical language to everyday language usage, gave Popper the argumentative ability to assert whether we can say of a theory that it is 'false' if it does not meet certain methodological standards. It also enables us to talk of a theory or concept as being 'true' (even if only to a certain degree) even when it has failed to measure up to falsification as it conforms to our common daily use of this term.

## Notes

1. This article was presented by J. Łukasiewicz to the Warsaw Scientific Society on 21 March 1931, and published in Polish 2 years later. A German translation was published as "Der Wahrheitsbegriff in den formalisierten Sprachen," in *Studia Philosophica*, 1936, Vol. 1. This article appears in English in Tarski, 1956.
2. *Empractic* in this sense refers to "built into praxis".
3. Miller, David. 1999. "Popper and Tarski", in Ian Jarvie and Sandra Pralong (eds.), *Popper's Open Society After Fifty Years*. London/New York: Routledge, p 56.



4. David Miller (1999), shows how the positions in Chapter 8 of *The Open Society* "The Philosopher King" are a philosophical application of Tarski's theory of truth.
5. Agassi, Joseph. 1993. *A Philosopher's Apprentice: In Karl Popper's Workshop*. Amsterdam: Rodopi B. V. 115. For an understanding of Popper's understanding of the definition 'negativism' see Letter of Correspondence, Popper to Hayek.
6. Tarski, Alfred. 2001. "The Semantic Conception of Truth and the Foundations of Semantics", in A. P. Martinich (eds.), *The Philosophy of Language*, 4th edn. New York: Oxford University Press, p 70.
7. Tarski, Alfred. [1944], 2001. "The Semantic Conception of Truth and the Foundations of Semantics." p 70.
8. Tarski, Alfred. [1944], 2001. p 70.
9. Miller, David. 1999. p 65.
10. Wolenski, Jan. 1989. *Logic and Philosophy in the Lvov–Warsaw School*. Dordrecht/Boston/London: Kluwer, p 180.
11. Tarski, Alfred. 1956. *Logic, Semantics, Metamathematics: Papers from 1923 to 1938*. pp 152–153, 165. Where Tarski concludes Section 1 with: the very possibility of a consistent use of the expression 'true sentence' which is in harmony with the laws of logic and the spirit of everyday language seems to be very questionable, and consequently the same doubt attaches to the possibility of constructing a correct definition of this expression.
12. Hacothen, Malachi H. 2000. *Karl Popper – The Formative Years, 1902–1945, Politics and Philosophy in Interwar Vienna*. Cambridge: Cambridge University Press, p 230.
13. Tarski, Alfred. 1956. "The Concept of Truth in Formalized Languages", in *Logic, Semantics, Metamathematics*. New York: Clarendon, p 265.
14. Where Popper states in: *Unended Quest: An Intellectual Autobiography*, [1974], 1993, p 99, that: ... all these precise methods were confined to formalized languages, and could not, as Tarski had shown, be applied to ordinary language (with its "universalistic" character). Nevertheless it was clear that we could learn from Tarski's analysis how to use, with a little care, the notion of truth in ordinary discourse, and to use it, moreover, in its ordinary sense – as correspondence to the facts. I decided in the end that what Tarski had done was to show that once we had understood the distinction between an object language and a (semantic) meta-language – a language in which we can speak about statements and about facts – there was no great difficulty left in understanding how a statement could correspond to a fact.
15. Tarski, Alfred. 1944. "The Semantic Conception of Truth and the Foundations of Semantics", in *Philosophy and Phenomenological Research*, Vol. 4, pp 341–376.
16. Tarski, Alfred. 1956. *Logic, Semantics, Metamathematics: Papers from 1923 to 1938*. Oxford: Clarendon, p 153.
17. The paragraph that enables the cautious use of the semantic theory of truth in ordinary language, which is the most likely source for Popper's stance regarding this can be seen in the English publication of Tarski's 1931 paper, *The Concept of Truth in Formalized Languages*, 1956, p 157: sentences which are analogous to (3) and (4) seem to be clear and completely in accordance with the meaning of the word 'true' which was expressed in the formulation (1). In regard to the clarity of their content and the correctness of their form they arouse, in general, no doubt (assuming of course that no such doubts are involved in the sentences which we substitute of the symbol 'p' in (2)).
18. Letter of Correspondence: Popper to Hayek, 20 October 1964, p 1.
19. Letter of Correspondence: Popper to Hayek, 20 October 1964, p 1.
20. Letter of Correspondence: Popper to Hayek, 20 October 1964, p 2. This stopping point of the minimum needs of a problem situation, reflects Popper's position of restricting his ontology to "three worlds" rather than allowing for an ontology of worlds 4, 5, etc. See Popper, Karl. 1994. *Knowledge and the Mind-Body Problem: In Defence of Interaction*. London/New York: Routledge.
21. For Popper's development of Bühler's theory of language function see: Popper, Karl. 1994. *Knowledge and the Mind-Body Problem: In Defence of Interaction*. London/New York: Routledge, p 84.

22. Letter of Correspondence: Popper to Hayek 28 May 1944.
23. Tarski, Alfred. [1944] 2001, 2001e. "Semantic Conception of Truth and the Foundations of Semantics", in *Philosophy and Phenomenological Research*, Vol. 4, pp 341–376. See A. P. Martinich (eds.), p 81.
24. Popper, Karl. 1962. *The Open Society and Its Enemies*. Vol. 1, 64, note 5(2).
25. See Carnap, R. 1962. *Logical Foundations of Probability*. London: University of Chicago Press, p 207. This is another instance in which Joseph Agassi was correct in asserting that Popper's thought contains a "whiff of Inductivism"; see Agassi, J. 1993, p 118. In the letter from Popper to Hayek 20 October 1964, Popper raises the similarity of the distinction between abstract and concrete societies (the problem in which the notion of *diarrhesis* was related to) with formalised projective geometry, which reveals much about the way Popper used analogy to support his arguments in political philosophy.
26. In "The Semantic Conception of Truth and the Foundations of Semantics" (1944) Tarski states in (1) that the main problem is giving a *satisfactory definition* of the notion of truth. The satisfactoriness of this definition rests upon its formal correctness and material adequacy. See article in A. P. Martinich (ed.), 2001. *The Philosophy of Language*, 4th edn. New York: Oxford University Press, pp 69–91.
27. Tarski, Alfred. [1944], 2001. "The Semantic Conception of Truth and the Foundations of Semantics." p 78.
28. Popper, Karl. 1998. *The World of Parmenides: Essays on the Presocratic Enlightenment*, Arne F. Petersen and Jørgen Mejer (eds.). London/New York: Routledge, p 129.
29. For Popper's use of the notion of "Absolute truth" as a "regulative ideal" as distinct from the semantic or correspondence theory of truth see: Popper, Karl. [1963], 1989. *Conjectures and Refutations: The Growth of Scientific Knowledge*. London: Routledge, p 229.
30. Popper, Karl. 1979. *Die beiden Grundprobleme der Erkenntnistheorie*. Tübingen: J. C. B. Mohr (Paul Siebeck), p 244.
31. Miller, David. 1999. pp 66–67.
32. Miller, David. 1999. p 66.
33. Popper, Karl. 1994. *The Myth of the Framework*. London: Routledge, p 172.
34. Miller, David. 1999. p 68.
35. Besides Popper's letters of correspondence, particularly with Hayek, examples of this can be found in Popper, Karl. 1979. *Schöpferische Selbstkritik in Wissenschaft und Kunst*, Reder zur Eröffnung der Salzburger Festspiele, particularly page 29 where it is stated; *Ich möchte betonen, daß alles, was ich darüber sagen werde, spekulativ ist, aus Vermutungen besteht*. Also see, Popper Karl. 1984. "Bücher und Gedanken: Das erste Buch Europas", in *Auf der Suche nach einer besseren Welt: Vorträge und Aufsätze aus dreißig Jahren*. Piper Verlag GmbH, München, 2004, p 120. English unpublished version titled *Unintended Consequences: The Origin of the European Book*. In this lecture, Popper communicates for the first time the thesis that European culture began with the first publication of the works of Homer in book form. He then proceeds to give an account of the effect of this on the publication of the works of ensuing thinkers in ancient Greece. It is interesting to note that at this stage of Popper's career he is happy to develop hypotheses that are non testable yet are and therefore states *should not be taken too seriously*, yet they are seen to be both meaningful and important (Meine Hypothese ... ist natürlich nicht überprüfbar. Historische Parallelen sollte man nie zu Ernst nehmen). The constant use of the expression *I conjecture (ich vermute)* also aides in this illusion, however, one can be assured that this theorising is just as serious as his scientific thought.
36. Popper, Karl. *The Open Society and Its Enemies*. Vol. 2, p 385. We may contend that Popper's understanding of the term "absolute truth" changed from his more ambitious understanding of it as "correspondence" to the facts in *The Open Society* to as a "regulative ideal" in *Conjectures and Refutations*. From this we can see the concept of truth evolved in its meaning throughout Popper's work.
37. For Popper's elucidation of, what I call the Popper–Bühler Linguistic Schema, see Popper, Karl. 1994. *Knowledge and the Mind-Body Problem: In Defence of Interaction*. London/New York: Routledge, p 84.

38. For the importance of the work of the Würzburg School, in particular the thought of Karl Bühler upon Popper's thought see; Alt, Jürgen August, 1982. *Die Frühschriften Poppers*, Frankfurt am Main: Peter Lang. Also see Berkson, William and John, Wettersten. 1984. *Learning from Error: Karl Popper's Psychology of Learning*. La Salle, IL: Open Court.
39. Stokes, Geoffrey. 1998. *Popper: Philosophy, Politics and Scientific Method*. Cambridge: Polity, p 144.
40. Gattei, Stefano. 2002. "The Ethical Nature of Karl Popper's Solution to the Problem of Rationality", in *Philosophy of the Social Sciences*, Vol. 32, pp 249–250.
41. That Popper did in fact relate the notion of truth into Bühler's theory of language function at the "descriptive level" can be seen in Petersen, Arne Friemuth, 1992, "On Emergent Pre-Language and Language Evolution and Transcendent Feedback from Language Production on Cognition and Emotion in Early Man", in J. Wind et al. (eds.), *Language Origin: A Multidisciplinary Approach*. The Netherlands: Kluwer, p 451. Petersen in turn attributes his diagram to one used in a little known paper by Popper titled: "The Place of Mind in Nature," 6 October 1981. Speech (not delivered in person), Nobel Conference XVII, Gustavus Adolphus College, St. Peter, Minnesota. Typescript original, published in *Mind in Nature*, Richard Q. Elvee (ed.), 1982. San Francisco, CA: Harper & Row, pp 31–59. However, the constant mentioning in Popper's written work including his letters of correspondence of positions that he held as convictions to be true as well as the understanding of truth as a regulative ideal in *Conjectures and Refutations* is evident that a notion of truth was seen by Popper to operate at the higher "critical" or "argumentative" function of language. However, like much of Popper thought on the philosophical problems concerning language it was not systematically developed.
42. Miller, David. 1999. pp 66–67.

## References

### Karl Popper Archives, University of Klagenfurt, Austria.

354–359. Letters, Tarski, Alfred. 1935–1981

3005.13. Letters. Hayek, F. A. 1940–1947

### Published Sources

Agassi, Joseph. 1993. *A Philosopher's Apprentice: In Karl Popper's Workshop*. Amsterdam: Rodopi B. V.

Alt, Jürgen August. 1982. *Die Frühschriften Poppers*. Frankfurt am Main: Peter Lang.

Berkson, William and John, Wettersten. 1984. *Learning from Error: Karl Popper's Psychology of Learning*. La Salle, IL: Open Court.

Carnap, Rudolf. 1962. *Logical Foundations of Probability*. London: University of Chicago Press.

Gattei, Stefano. 2002. "The Ethical Nature of Karl Popper's Solution to the Problem of Rationality," in *Philosophy of the Social Sciences* Vol. 32, pp 240–266.

Hacohen, Malachi, H. 2000. *Karl Popper – The Formative Years, 1902–1945, Politics and Philosophy in Interwar Vienna*. Cambridge: Cambridge University Press.

Miller, David. 1999. "Popper and Tarski", in Ian Jarvie and Sandra Pralong (eds.) *Popper's Open Society After Fifty Years*. London/New York: Routledge, pp 56–70.

Petersen, Arne Friemuth. 1984. "The Role of Problems and Problem Solving in Popper's Early Work on Psychology", in *Philosophy of the Social Sciences* Vol. 14, No. 2, pp 239–250.

Petersen, Arne Friemuth. 1992. "On Emergent Pre-Language and Language Evolution and Transcendent Feedback from Language Production on Cognition and Emotion in Early Man", in J. Wind et al. (eds.) *Language Origin: A Multidisciplinary Approach*. The Netherlands: Kluwer, pp 449–464.

Popper, Karl. [1945], 1962. *The Open Society and Its Enemies*. London/New York: Routledge.

- Popper, Karl. 1979a. *Die beiden Grundprobleme der Erkenntnistheorie*. Tübingen: J. C. B. Mohr (Paul Siebeck).
- Popper, Karl. 1979b. *Schöpferische Selbstkritik in Wissenschaft und Kunst*, Reder zur Eröffnung der Salzburger Festspiele. Copyright 1979 Karl R. Popper.
- Popper, Karl. [1963], 1989. *Conjectures and Refutations: The Growth of Scientific Knowledge*. London: Routledge.
- Popper, Karl. 1990. *A World of Propensities*. Bristol: Thoemmes.
- Popper, Karl. [1974], 1993. *Unended Quest: An Intellectual Autobiography*. London: Routledge.
- Popper, Karl. 1994a. *The Myth of the Framework*. London: Routledge.
- Popper, Karl. 1994b. *Knowledge and the Mind-Body Problem: In Defence of Interaction*. London/New York: Routledge.
- Popper, Karl. 1998. *The World of Parmenides: Essays on the Presocratic Enlightenment*, Arne F. Petersen, and Jørgen Mejer (eds.). London/New York: Routledge.
- Popper, Karl. 2004. "Bücher und Gedanken: Das erste Buch Europas", in *Auf der Suche nach einer besseren Welt: Vorträge und Aufsätze aus dreißig Jahren* (1984). München: Piper Verlag GmbH.
- Tarski, Alfred. 1944. "The Semantic Conception of Truth and the Foundations of Semantics", in *Philosophy and Phenomenological Research* Vol. 4, pp 341–376.
- Tarski, Alfred. 1956. "The Concept of Truth in Formalized Languages", in *Logic, Semantics, Metamathematics: Papers from 1923 to 1938*. Oxford: Clarendon.
- Tarski, Alfred. 2001. "The Semantic Conception of Truth and the Foundations of Semantics", in A. P. Martinich (eds.) *The Philosophy of Language*, 4th edn. New York: Oxford University Press, pp 69–91.
- Wolenski, Jan. 1989. *Logic and Philosophy in the Lvov–Warsaw School*. Dordrecht/Boston/London: Kluwer.

# Popper and Communitarianism: Justification and Criticism of Moral Standards

Harald Stelzer

**Abstract** In this paper I start with a critical discussion of the communitarian justification of moral standards with reference to communally shared moral convictions. I criticize a reconstructed communitarian ethical position on two levels: an individual level, focusing on the possibility to criticize and reject the moral standards of one's own community, and a socio-cultural level, emphasizing the intracultural moral pluralism and intercultural contact. Finally, on the basis of this criticism of the communitarian view, the thesis of a critical-rationalist ethical position will be elaborated. Such a position can be derived from the fact, that all life can be seen as a process of problem solving based on the method of trial and error. Hence, moral standards can be interpreted as attempts to solve different problems, resulting from social life. Linking this interpretation to the method of critical discussion and the willingness to learn from our mistakes, a critical-rationalist ethical position is consistent with intercultural discussion, interpretation and evaluation of different moral systems and allows for improvement of moral standards.

## The Communitarian Position in Ethics

The political and social state of modern democratic societies still presents an important challenge for liberal positions. Even though Popper contributed substantially to the development of liberalism after the Second World War with his conception of an 'open society', his social philosophy has had almost no relevance for the liberal-communitarian debate. Nevertheless, an analysis of the communitarian position from the view of critical rationalism can open up new perspectives and thus contribute to the discussion of the political and ethical foundation of modern democracies (Stelzer 2006). Here I will concentrate on some aspects of the ethical dimension of this debate.

A distinctive analysis of communitarianism has to consider the fact that because of the different approaches and intellectual backgrounds, communitarianism can-

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not be seen as a unified and well-defined philosophical school. Only concerning thematic similarities among the different approaches of so-called communitarian authors like Alasdair MacIntyre, Charles Taylor, Michael Sandel, Michael Walzer, Benjamin Barber, Daniel Bell, Robert Galston, Amitai Etzioni or Robert Bellah, allow the construction of something like a communitarian philosophy. Even though there is nothing like a unified communitarian moral position, I will try to outline the ethical dimensions of communitarian thought by focusing on five points:

1. The rejection of liberal universalism
2. The stress on communally shared moral standards
3. The criticism of liberal individualism
4. The constitution of the identity of the individual by the community
5. The dependence of moral criticism on communally shared moral standards and intuition

ad 1. Communitarians criticize liberal universal principles as empty and insufficient for moral orientation. Furthermore, for them the promise of generally accepted universal principles, putting aside all concrete circumstances of individuals, their preferences, their culture, their community and their life situations, is an illusion, based on the mistaken assumption of an external point of view. The attempt to reach universal principles may well end up in moral relativism, since the constant critical analysis leads to an alienation from communally shared standards, the only actual resources for moral reasoning and action. What remains is 'moral chaos' as the basic moral situation of modern society (MacIntyre 1981, pp. 2–5).

ad 2. Instead of searching for universal principles, moral philosophy and reasoning should be based on communally shared moral standards, values and norms, which need to be strengthened against the relativistic consequences of liberalism (Bell 1993, p. 7). Liberal morality is confronted by the emphasis on the morals (Sittlichkeit) found in certain communities with their particular moral standards, ties, solidarities, as well as their social and cultural habits and particularities. Here individuals can find specific rules and instruction for their actions and the foundation of their moral judgments.

ad 3. In this respect communitarians criticize liberal universalism for its underlying individualistic conception of the moral self, based on the assumption that the universal principles are chosen by abstract individuals as free and rational persons, detached from all communal peculiarities and ties. Therefore communitarians conclude that what matters above all in liberal universalism are not the ends we choose but our capacity to choose them (Sandel 1982, p. 6). For communitarians such a conception of the moral self does not correspond with the self-understanding of human beings, since their ends, values and obligations always belong to their self-perception. Hence, liberal individualism is criticized from a communitarian perspective as a misconception of the moral self. This misconception cannot lead to a deeper self-understanding of one's own ends, values and convictions, based on the membership in certain communities.

ad 4. Accordingly, communitarians refer to the empirical fact that most humans draw their moral standards from the communities they belong to. In general, people

do not come to their moral standards by autonomous thinking and rational choice. Form a communitarian point of view their convictions, ends and moral standards are not the products of free choice, but human beings are endowed with a moral orientation due to their socialization in certain communities (Mulhall and Swift 1992, p. 111). Thus, communitarians infer to the constitutive function of communally shared convictions and moral standards for the identity of the individual (Sandel 1984, p. 6). The social ties and obligations to other community members, the communally shared ends and values, the history and traditions of the community are important parts of the individual identity. An attempt at total detachment from these resources would lead to the dissolution of the person, to the renunciation of individual responsibility and to the arbitrariness of moral choice.

ad 5. Therefore, communitarians argue that the individual cannot simply leave or extinguish all characteristics and particularities of the community he/she belongs to. The dissociation of the self from the values, ends, convictions and social ties of the community always remains precarious. The individual can place certain communally shared values and norms in question, revolt against them, develop new cultural elements or combine traditional ones in different new ways. However, those transformations take place not in a moral vacuum but depend on a shared cultural background. For this reason communitarians conclude that moral criticism depends on communally shared moral standards and intuitions (Walzer 1983, p. 29). The justification of ethical judgments always takes place in the context of the common tradition and a shared cultural understanding (Bell 1993, pp. 66–67). Consequently, the standards for the evaluation of values, norms, moral standards and practices are drawn from the moral convictions and intuitions of communities and not from an external, impersonal and detached point of view (Mulhall and Swift 1992, pp. 141–143).

These central communitarian ethical assumptions can be criticized from a critical-rationalist perspective in different ways, drawing on concurrent standpoints of liberal criticism. Here I want to focus on (a) the impossibility to reject communally shared moral standards on the individual level, and (b) the narrow limits of cross-cultural criticism on the socio-cultural level.

## **The Impossibility to Reject Communally Shared Moral Standards**

The communitarian argumentation is of importance as it refers to the fact that the starting point for moral judgments, and the critical reflection on values, norms, and practice is not to be found in a moral vacuum but in communally shared moral standards and intuitions (Buchanan 1998, pp. 470–471). However, the social embedment of our moral judgments is quite compatible with the liberal concept of autonomy (Kymlicka 1989, p. 15). Liberals do not assume that individuals always experience themselves as the authors of their own rules, values and norms. Moral



standards are typically adopted from tradition, religion or society, and are often not subject to critical evaluation by the individual. Furthermore, it is also not asserted by liberal positions that the adjustment of behavior to communally shared values and norms must be immoral. Rather, communally shared moral standards have, just as traditions, an important orienting function. Therefore the liberal concept of autonomy does not presuppose that we are completely independent from our experience, the information provided by the social environment of moral standards, the beliefs of the communities in which we grow up or live in, and other conscious and unconscious influences on our thinking while we make our moral decisions. It would be hard to make sense of such a kind of independence.

Just as the individual depends on the social world in determining his/her identity, also autonomy develops only in confrontation with the social environment. As little as we are born as an 'I', but have to learn that we have an 'I', so as little are we born as autonomous moral beings. Still, we can change our self-conception as well as we can gain more moral autonomy. Therefore, autonomy is not to be confused with absolute independence. It is rather a question of degree, an option more or less taken up by each individual. As human beings we have the ability to reflect critically on our moral decisions and thereby we can sometimes successfully distance ourselves from parts of the moral framework we live in. Despite their social embedment, individuals are capable of critically analyzing shared moral standards and practices, to try to change them if appropriate or to leave them if necessary (Kymlicka 2002, pp. 227–228).

Autonomy is precisely reflected in the fact that no particular challenge to us by society, no communally shared moral standard, and no cultural practice or end has authority over our own judgment and are therefore exempt from possible re-examination. This reflects the deepest meaning of freedom, the freedom of the mind to choose (Kautz 1995, p. 193). The main point for our moral autonomy from the perspective of liberal individualism "is the insistence on respect for each individual's capacity to understand and evaluate her own actions, to make judgments about the value of the communal and cultural circumstances she finds herself in" (Kymlicka 1989, p. 254). Therefore, not only must we always decide whether to accept a moral standard or not, but we are also the creators of our standards by proposing, discussing, and adopting them (Popper 1966b, p. 385). Only through this assumption can individuals be held responsible for the standards they adopt and the actions they carry out based on these choices.

Liberal individualism reflects the fallibility of our moral reasoning. We can err in our motives and ends, and therefore we need to have the possibility to reflect critically and change our conception of the good as well as have the chance to learn from our mistakes. Liberals like Mill, Rawls, Dworkin, Nozick, Lomasky or Raz argue for a right of moral independence not because our ends are arbitrary or fixed, but because our goals can be wrong and because we can revise and improve them (Kymlicka 1989, p. 18). This entails the liberal concern for the social condition necessary to question freely and rationally our commitments. So it makes a difference if education and cultural socialization opens up or closes off the possibility of revising our ends. Even though the concept of liberal autonomy rests on the assumption

of the possibility to distance oneself from communally shared values and beliefs, liberal individualism is not an expression of an anthropological atomism, but the result of a specifically modern reflection and emancipation process. Therefore the predominant question is not regarding human nature or how we normally arrive at our moral judgments, but the nature of the social conditions necessary for people to choose freely for themselves. In this sense, liberalism is primarily based on normative individualism and not on ontological individualism (Phillips 1993, p. 177). The possibility of liberation from one's community is itself a moral good and the indispensable condition of freedom (Kautz 1995, p. 196).

## **The Narrow Limits of Cross-Cultural Criticism**

Not only on an individual level can the communitarian approach be challenged but also from a cultural level can one criticize the emphasis on the dependence of moral criticism on the communally shared moral understanding. The communitarian assumptions, that criticism always takes place on a shared common background and must fit the communally shared understanding, has a great impact for the criticism of other communities. Based on these claims, criticism would be understandable for members of the criticized community only to the extent that it addresses their own moral convictions and shared intuition (Mulhall and Swift 1992, pp. 143–144). This brings about serious problems for moral evaluation in cases where communities do not reflect on the convictions of one's own value in a sufficient way. If a criminal community, like a slaveholding society, could be criticized only on the basis of its shared moral standards, its agreement to have slaves would be morally correct for this society if it would coincide with principles of their moral system. The only critique that could be brought up against it would be that we do not agree with this decision. Our differing view would only reflect our own agreement to disagree with their agreement and underlying moral standards (Gewirth 1994, p. 29).

But one does not have to select such an extreme example. The fundamental problem of this approach is likewise present within Walzer's distinction of different social spheres, which have to be judged according to their rules of distribution practice and their own common view of the good (Ferrara 1990, p. 23). The basic problem of the communitarian limitation of criticism is to be found in the consequence that the evaluation of cultures, societies, communities or social spheres cannot be meaningfully made from the outside by other communities or according to community transcending standards (Phillips 1993, p. 183). Thus, there is no escape from the normative relativism of communal or cultural conventions (Avineri and de-Shalit 1992, p. 4). I think this restriction can be criticized for several reasons:

First, the existence of moral standards and their acceptance in a community must not be confused with the question of their moral justification. Also the deepest common convictions have to be critically analyzed on the ground of their general moral justification (Kymlicka 1993, p. 214). Such a justification cannot be based on communally shared standards alone, but must aim for moral standards with general validity.

The liberal quest for universal principles can be understood as the search for standards that can be generally and reciprocally justified to all human beings. Persons are not only members of certain communities with corresponding rights and obligations, but as human beings they are also moral persons with certain rights and obligations of acknowledgment in principle to 'each and every' person. This dimension of moral justification and moral acknowledgment referring not to members of a certain community but to the 'universal context' of mankind is neglected in communitarianism.

Secondly, the development of moral systems is not an isolated process as one would expect on grounds of the communitarian restriction on meaningful moral criticism, but an open and dynamic one, determined by intercultural contact and intracultural differentiation. Here, one can refer to Popper's thesis of 'cultural clash' and its meaning for the development of the critical dualism of facts and standards (Popper 1994, p. 43). For Popper, contact with other cultures shows clearly the social origin of moral standards and refers thereby to their variability and changeability (Popper 1966a, p. 294). Especially in a globalized world, a multiplicity of pluralistic societies interact with each other. Moreover, people frequently learn from the moral standards of another society. Even existing moral conflicts between societies are sometimes resolved when the members of one society change their moral outlook and come to share at least some of the moral standards of another society. Members of a society come to believe that some moral standards of another society are better in some respects than their own previously accepted standards. For example, the international human rights movement shows substantial moral agreement across different cultures. Moral standards regarding human rights and equal rights for women have been successfully adopted by many societies upon integration into the international community, in stark contravention of traditional roles, norms, values and practices (Buchanan 1998, pp. 468–469).

Third, modern societies and also many of their included communities do not exhibit some form of homogeneity and separateness but are characterized to a large extent by complexity, heterogeneity and discord. Since modern societies are so highly differentiated, homogeneity for them is neither fundamental nor attainable. Therefore, the communitarian conception seems inappropriate for modern societies. Communitarianism in this respect has been criticized for social-romantic tendencies (Rosenblum 1989, p. 215). The communitarian thesis of a common ground of shared convictions, moral standards and ends cannot be maintained. Rather, individuals are confronted with heterogeneous traditions, communities and social practices and find themselves in relation to different ethical demands, sometimes contradicting each other. They are thus forced to select between the different obligations and moral standards and therefore look for principles which transcend the particularities of concrete communities and their shared moral standards rather than uncritically presume the common ground traditions of their community.

Fourth, the limitations of cross-cultural criticism seem to be based on the assumption that moral systems of different cultures and communities are too dissimilar to step into a fruitful discussion with each other. This can be linked to Popper's criticism of 'the myth of the framework'. The different proponents of the myth of the framework commonly refer not only to the context dependence of our opinions and convictions, but also state that the different frameworks are too

dissimilar, so that representatives of different frameworks cannot understand each other and that therefore a fruitful and rational discussion is not possible (Popper 1994, p. 33). Not only does this introduce unrealistically high standards of mutual understanding prone to fail but the acceptance of incommensurability works also as a self-fulfilling prophecy (Popper 1974, p. 1152). If one assumes that there are insurmountable barriers, then these barriers are stabilized. At the same time the framework forms a border as a reference system, at which criticism recoils (Albert 2000, p. 163). Thus, relativism is suitable as a defense strategy, an instrument that protects one's own opinions and practices against criticism. Moreover, it can lead to the retreat from discussion often linked with the ascription of a privileged status to one's own culture, race or class. This transforms the myth of the framework into a doctrine with ethnocentric, authoritarian and imperialist inclinations. In this perspective, some liberal critiques pointed out the danger that the communitarian understanding of communally shared moral standards could contribute to exclusion, intolerance, or marginalization of minority groups and to an elimination of differences (Phillips 1993, p. 168; Kymlicka 2002, p. 259).

To put it in a nutshell, the key question between liberals and communitarians concerning the individual as well as the cultural level is not the question of where to begin the philosophical enterprise, but where that enterprise must end up. Both schools seem to agree that we start with local and particular ethical opinions. While for some communitarians the notions of community and shared values mark the limits of practical reason, for liberals these moral claims are the point of departure (Kymlicka 1989, pp. 67–68). Consequently, liberal positions have to give reasons for certain (more or less universal) moral principles and argue for the possibility of a rational choice between competing moral claims, if they do not want to fall into relativism. As I will point out in the following, by rethinking some of Popper's ethical assumptions, one can give an adequate answer to this challenge.

## **Rethinking Popper for a Critical-Rationalist Position in Ethics**

A critical-rationalist ethical position must adopt a middle ground between absolutism and relativism, taking into account the fact that our moral standards are tentative, changeable and not final but still permit a rational decision between different standards and moral systems and their defense in critical discussion. Furthermore, this position has to rest on four basic assumptions: critical dualism of facts and standards, the plurality of standards, the moral autonomy of the individual and a teleological consequentialist orientation based on the concept of 'negative utilitarianism'. Additionally, it has to allow for the possibility of cross-cultural critical discussion, interpretation and evaluation of moral standards and their further improvement.

The development of such a critical-rationalist ethical position can be derived from the fact, that all life can be seen as a process of problem solving and that all

problem solving is based on the same method, the method of trial and error. This unites not only rationality in theory and practice but also takes into account basic assumptions of evolutionary theory. In evolution the method of trial and error can be found in the mechanism of natural selection based on variation and elimination. In the course of evolution human beings have developed a more active orientation behavior. Beside the biological mechanism of adaptation to changing environments, there is cultural evolution based on specific human dispositions like creativity, criticism, abstract thinking or complex language, especially its descriptive and critical function. As human beings we are not only able to develop hypotheses and test them empirically. We also have the ability to modify our culturally formed anticipations and expectations and the resulting behavior if they turn out to be insufficient solutions to our problems. Thus, the development of norms, values and practices is to be seen as an achievement of adaptation, stimulated by problems, forcing us to adjust our convictions and expectations as well as our moral standards.

From such a perspective, different moral standards appear as attempts to answer different problems, resulting from social life. They can therefore be interpreted as undogmatic suggestions for the regulation of social behavior, subject to change and development and based on the method of trial and error. Even in the realm of our moral standards, we make guesses, and by improving upon our guesses, by criticism and discussion we can develop our standards further and grow not only intellectually but also morally. As Popper has pointed out, the possibility of progress in our standards is based on the fact that we can learn here by our mistakes and by criticism just as well as in the realm of facts (Popper 1966b, pp. 381, 386). What progress entails in both fields is a critical and self-critical orientation, the readiness to learn from mistakes and to correct them, as well as the willingness to enter into rational discussion with each other. Such an approach should get us closer to truth and right moral standards (Popper 2008b, p. 324). Therefore, fallibilism seems to be relevant not only for science but also for ethics.

As Popper and other critical rationalists pointed out, a fallibilist approach gives up the assumption that our cognitive claims have to be justifiable or proven in order to be genuine knowledge. It is sufficient if we are able to argue that some theories are better than others on the basis of severe critical examination given the evidence and arguments available so far (Hayes 2001, pp. 14–16). Not justification but criticism helps us to decide between our cognitive claims. For Popper, “criticism invariably consists in pointing out some contradiction; either a contradiction within a theory criticized, or a contradiction between the theory and some other theory which we have some reason to accept, or a contradiction between the theory and certain [...] statements of fact” (Popper 1963, p. 316).

These three types of criticism (logical, theoretical and empirical) are also possible in ethical reasoning. It is clear that not all sets of moral rules are equally logically consistent. Furthermore, we are able to compare different moral standards, practices and institutions with reference to the underlying problem situation (Albert 2000, p. 75; Popper 1994, p. 46). Analogous to the competition between scientific theories, their rational discussion, their empirical examination and their critical elimination, it should also be possible to improve our moral standards

(Popper 1976, p. 195). In doing so, we have to discuss different moral standards critically, question their consequences and eliminate those parts of our moral conceptions that prove to provide no adequate solution to social problem situations. Even though it is not possible to deduce an 'ought' from an 'is', there are ways, as Hans Albert and Calvin Hayes pointed out, to criticize and even reject moral standards if they contradict certain statements of fact. We can use *reductio ad absurdum* arguments as a special case of moving from 'ought' to 'is'. We can go "from 'ought'-propositions (or principles, prescriptions) to 'is'-statements and therefore by modus tollens refute the ought-premise(s) if we can refute the is-conclusion(s)" (Hayes 2001, p. 119). To put it in a more logical form: " $r \rightarrow d \rightarrow o \rightarrow c \rightarrow i$  but  $\sim i$  therefore  $\sim r$ , where  $r$  can designate rights, reason, responsibility, religion, revelation or even virtue and utility" (p. 197).

This is a good argument for dealing with moral standards which demand actions we are not able to carry out or lead to consequences we are not willing to accept. Still, the possibility that some of our moral claims are after all conjectural is not to be confused with the assumption that we can settle all moral disagreements or make all our moral decisions by simply pointing to some conjectures between different underlying moral standards and some statements of fact. Often we have to choose between different disliked consequences. And there are, as Popper pointed out himself, "always various and even opposite decisions possible with respect to a certain relevant fact." Therefore he is right to emphasize that "it is impossible to derive a sentence stating a norm or a decision or, say, a proposal for a policy from a sentence stating a fact" (Popper 1966a, p. 64). Then again this does not entail that our moral decisions are totally arbitrary and that we cannot criticize them by looking for their implications and consequences. Furthermore, some of our moral claims will survive criticism better than others and are therefore rationally preferable with no guarantees that they will remain forever so.

Fallibilism as a meta-theoretical principle enables us to make a rational choice not only between different theories in science but sometimes also between competing moral standards. Even though it cannot provide us with an absolute criterion for moral rightness, it seems a good answer to relativism, perhaps the best we have so far.

Human fallibility means that we may err, and that we must not rely on what appears to us as true, or as morally right, because it may not be true, or morally right. But this implies that there is such a thing as truth, and that there are actions that are morally right, or very nearly so. Fallibilism certainly implies that truth and goodness are often hard to come by and that we should always be prepared to find that we have made a mistake. On the other hand, fallibilism implies that we can get nearer to the truth or to a good society. [...] What all this teaches us is that we must never stop our critical – a highly critical – search for truth, always trying to learn from those who hold a different view. [...] And precisely for this reason, we must reject relativism. (Popper 2008b, pp. 320–321)

The basic assumptions of a critical rationalist ethical position introduce a notion of intersubjectivity to morality, since this position is grounded in the method of critical discussion and the rational examination of the consequences of our moral standards and decisions. This does not lead to a justification for the claim of any particular standard or moral assertion to be true. But the same goes for our scientific knowledge



(Popper 2008a, p. 10). Because our moral reasoning rests on the method of critical discussion, it should be based on certain presuppositions such as the reciprocal consent and readiness for discussion, the recognition of plurality, a positive idea of tolerance, an anti-absolutist and anti-dogmatic attitude, a self-critical and open position and the respect for the persons involved in discussion and for their opinions.

These prerequisites are also important for cross-cultural discussion, interpretation and evaluation of different moral systems. Such discussions rest on the assumption, that despite the existence of cultural and linguistic barriers, persons with extremely different cultural backgrounds can enter into fruitful discussion with one another and break out of their cognitive prisons (Popper 1966b, pp. 387–388). Given these presuppositions people from different cultural backgrounds should be able to compare their moral standards by the method of critical discussion in relation to the different problem situations and their possible consequences. Considering the fact that human beings face similar problems in different societies, such intercultural discussions also seem promising for the discovery of basic moral standards. Popper himself expressed the hope for such discoveries in the 1961 Addenda to the *Open Society*, by pointing to the avoidance of cruelty and the importance of the Golden Rule (Popper 1966b, p. 386). Even though he remained very vague in these matters (Minogue 1995, p. 83) these basic moral standards are supported by significant empirical evidence of considerable moral agreement across different societies and cultures. For example, the role-reversal test implied by the Golden Rule has been prominent beyond Western traditions, e.g. in *The Way of the Bodhisattva* of the Indian Buddhist philosopher Shāntideva, *The Analects* of Confucius, and elsewhere.

The search for basic moral standards corresponds to some communitarian authors, as far as they do not try to establish morality on the foundation of certain existing traditions but aim to overcome the cultural relativity of moral standards. Accordingly, Michael Walzer speaks of a ‘minimal and universal moral code’ reflecting fundamental prohibitions – to murder, to betray, to commit cruelties – that are acknowledged in practically every human society. For Walzer, these prohibitions can be represented as philosophical discoveries or inventions. They are the product of always tentative, intermittent, and unfinished conversations, of the work of many years, of trial and error, of failed, partial, and insecure understandings (Walzer 1987, p. 23). Likewise, Henry Tam tries to formulate certain values of a general nature, based on the fact that these can be found in almost every culture (Tam 1998, pp. 15, 235). These values – love, wisdom, justice and self-fulfillment – go well together with critical rationalism. Both Walzer and Tam stress that those universal values and norms are too general to be able to provide an answer to every moral question. Therefore those norms and values must be supplemented by communally based moral standards. The function of these universal values and norms can be found in setting up a framework within which cultural- and community-specific values can be pursued (Walzer 1987, p. 23; Tam 1998, p. 238). Here one can point out an analogy to liberalism where the fundamental principles of justice likewise establish a framework for the pursuit of different conceptions of the good life, although on an individual and far more abstract level.



From a critical-rationalist perspective as well, the aim of moral discussion is not an agreement on all moral questions but the further development of our own moral standards (Yoshida 2006, p. 257). Cultural differences are not only fruitful stimuli for the advancement of our moral standards, but the encounter with other moral traditions and systems is of great importance because it can change the perception of one's own tradition and culture. As Hans Albert has pointed out, the contact with other cultures can break the natural immunity against criticism of one's own worldview (Albert 2000, p. 37). This is promising only if we do not take our own culture as universally superior, because if we do so, there is no more impulse to analyze it critically in comparison with others (Popper 1994, p. 51). At the same time, the culturally relativistic claim that all value systems are of equivalent validity would also make the encounter with diverse opinions, moral standards and practices unnecessary. If all opinions, moral standards and practices are recognized as equal, then there is no reason to criticize them or one's own positions and thereby to develop them further.

Nevertheless, the assertion of the equal validity of different positions is challenging. Considering the plurality of opinions, moral standards and practices in different cultures, relativism must be accorded a core of truth. There exists neither a general truth criterion nor a criterion of absolute rightness (Popper 1966b, pp. 373, 386). Based on this assumption it is often argued that if we are confronted with rival and competitive demands of different cultures and traditions, we have no good reasons to prefer one tradition, since such a preference would only reflect the unjustified inclination of a standpoint of one particular tradition (MacIntyre 1988, pp. 351–352). However, as pointed out above, from a critical-rationalist perspective the absence of final criteria does not mean that the choice between competitive theories or moral standards is arbitrary. It only means that we can err and that we have to stay open to criticism. Still, there can be good reasons to prefer theories, ideas, moral standards and even traditions if one is willing to argue and to question the different approaches critically.

Popper and other critical rationalists not only developed criteria to decide among different theories, but also on a more general level Ian C. Jarvie and Joseph Agassi introduced a distinction of three levels of rationality, setting apart a rational-empirical, self-critical style of thinking manifest in modern science (Agassi and Jarvie 1979, pp. 353–354). Parallel to the development and differentiation of rationality, cultures can also achieve quite different stages in respect to their level of moral reflection. Those differences, for example the openness to self-criticism and plurality, the willingness to learn from mistakes or the acceptance of critical dualism, can be interpreted as results of learning processes (McCarthy 1989, pp. 263–264). Also the liberal demand for the general validity and reciprocity of moral standards based on the autonomy of the individual and the acknowledgment of human beings as ends in themselves, can be interpreted in this way. The justification of these basic liberal demands therefore rests not on the claim that they are universal in their origin – even though they aim for universality in their application – but that they are important achievements of cultural development and products of a long process of trial and error. Therefore, we should not fall back behind these ideas but rather try

to develop them further and to realize them. Even though our standards are always subject to criticism and discussion, we should not simply accept some form of a 'lax tolerance' built upon the relativistic assumption that we cannot defend moral standards (Popper 1966a, p. 265, 1984, p. 191). At the end we have to take a stand for the moral standards we have found in discussion. As human beings we are left with the decision and the responsibility for what standards we are willing to accept, even though these decisions are always tentative and not final.

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## References

- Agassi, Joseph and Jarvie, Ian. 1979. The Rationality of Dogmatism. In *Rationality Today*, ed. Theodore Geraets, pp. 353–362. Ottawa: University of Ottawa Press.
- Albert, Hans. 2000. *Kritischer Rationalismus*. Tübingen: J.C. Mohr.
- Avineri, Shlomo and de-Shalit, Avner. 1992. Introduction. In *Communitarianism and Individualism*, eds. Shlomo Avineri and Avner de-Shalit, pp. 1–11. Oxford: Oxford University Press.
- Bell, Daniel. 1993. *Communitarianism and Its Critics*. Oxford: Oxford University Press.
- Buchanan, Allen. 1998. Community and Communitarianism. In *Routledge Encyclopedia of Philosophy*, ed. Edward Craig, pp. 464–471. London: Routledge.
- Ferrara, A. 1990. Universalism: Procedural, Contextualist and Prudential. In *Universalism vs. Communitarianism. Contemporary Debates in Ethics*, ed. David Rasmussen, pp. 11–37. Cambridge, MA: MIT Press.
- Gewirth, A. 1994. Is Cultural Pluralism Relevant to Moral Knowledge. In *Cultural Pluralism and Moral Knowledge*, eds. Ellen Frankel Paul, Fred D. Miller, Jr., and Jeffrey Paul, pp. 22–43. Cambridge: Cambridge University Press.
- Hayes, Calvin. 2001. *Fallibilism Democracy and the Market: The Meta-Theoretical Foundations of Popper's Political Philosophy*. Lanham: University Press of America.
- Kautz, Steven. 1995. *Liberalism and Community*. Ithaca, NY: Cornell University Press.
- Kymlicka, Will. 1989. *Liberalism, Community and Culture*. Oxford: Clarendon.
- Kymlicka, Will. 1993. Some Questions About Justice and Community. In *Communitarianism and Its Critics*, ed. Daniel Bell, pp. 208–221. Oxford: Oxford University Press.
- Kymlicka, Will. 2002. *Contemporary Political Philosophy: An Introduction*, 2nd edition. Oxford: Oxford University Press.
- MacIntyre, Alasdair. 1981. *After Virtue. A Study in Moral Theory*. Notre Dame, IN: University of Notre Dame Press.
- MacIntyre, Alasdair. 1988. *Whose Justice? Which Rationality?* London: Duckworth.
- McCarthy, Thomas. 1989. Contra Relativism: A Thought-Experiment. In *Relativism: Interpretation and Confrontation*, ed. Michael Krausz, pp. 256–271. Notre Dame: University of Notre Dame Press.
- Minogue, Kenneth. 1995. Politics and Morality in the Thought of Karl Popper. *Government and Opposition* 30(1), 74–85.
- Mulhall, Stephen and Swift, Adam. 1992. *Liberals and Communitarians*. Oxford: Blackwell.
- Phillips, Derek. 1993. *Looking Backward. A Critical Appraisal of Communitarian Thought*. Princeton, NJ: Princeton University Press.
- Popper, Karl Raimund. 1963. *Conjectures and Refutations. The Growth of Scientific Knowledge*. London: Routledge.
- Popper, Karl Raimund. 1966a. *The Open Society and Its Enemies – Volume I – The Spell of Plato*, 5th revised edition. London: Routledge.

- Popper, Karl Raimund. 1966b. *The Open Society and Its Enemies – Volume II – The High Tide of Prophecy: Hegel, Marx and the Aftermath*, 5th revised edition. London: Routledge.
- Popper, Karl Raimund. 1974. Replies to My Critics. In *The Philosophy of Karl Popper – Volume II*, ed. Paul Arthur Schilpp, pp. 961–1197. La Salle, IL: Open Court.
- Popper, Karl Raimund. 1976. *Unended Quest: An Intellectual Autobiography*, 2nd edition. Glasgow: Collins/Fontana.
- Popper, Karl Raimund. 1984. *In Search of a Better World: Lectures and Essays from Thirty Years*. London: Routledge.
- Popper, Karl Raimund. 1994. The Myth of the Framework. *The Myth of the Framework: In Defence of Science and Rationality*, ed. Mark Amadeus Notturmo, pp. 33–64. London: Routledge.
- Popper, Karl Raimund. 2008a. Optimist, Pessimist and Pragmatist Views of Scientific Knowledge. In *After The Open Society. Selected Social and Political Writings*, eds. Jeremy Shearmur, and Piers Norris Turner, pp. 3–10. London: Routledge.
- Popper, Karl Raimund. 2008b. On Toleration. In *After The Open Society. Selected Social and Political Writings*, eds. Jeremy Shearmur, and Piers Norris Turner, pp. 313–328. London: Routledge.
- Rosenblum, Nancy. 1989. Pluralism and Self-Defence. In *Liberalism and the Moral Life*, ed. Nancy Rosenblum, pp. 207–226. Cambridge, MA: Harvard University Press.
- Sandel, Michael. 1982. *Liberalism and the Limits of Justice*. Cambridge: Cambridge University Press.
- Sandel, Michael. 1984. *Liberalism and Its Critics*. Oxford: Blackwell.
- Stelzer, Harald. 2006. Popper and Communitarianism: Ethical and Political Dimensions of Democracy. In *Karl Popper: A Centenary Assessment – Volume I*, eds. Ian Jarvie, Karl Milford, and David Miller, pp. 231–240. Aldershot: Ashgate.
- Tam, Henry. 1998. *Communitarianism*. New York: New York University Press.
- Walzer, Michael. 1983. *Spheres of Justice. A Defence of Pluralism and Equality*. Oxford: Robertson.
- Walzer, Michael. 1987. *Interpretation and Social Criticism*. Cambridge, MA: Harvard University Press.
- Yoshida, Kei. 2006. Rationality and Other Cultures. In *Karl Popper: A Centenary Assessment – Volume III*, eds. Ian Jarvie, Karl Milford, and David Miller, pp. 251–261. Aldershot: Ashgate.

# Popper's Communitarianism

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**Abstract** In this chapter, I argue that Karl Popper was a communitarian philosopher. This will surprise some readers. Liberals often tout Popper as one of their champions. Indeed, there is no doubt that Popper shared much in common with liberals. However, I will argue that Popper rejected a central, though perhaps not essential, pillar of liberal theory, namely, individualism. This claim may seem to contradict Popper's professed methodological individualism. Yet I argue that Popper was a methodological individualist in name only. In fact, methodological individualism faded from Popper's vocabulary as he moved institutions and situational analysis more firmly to centre-stage. Popper's focus on institutions and situations constitutes what I call his communitarianism. If my interpretation is correct, then theorists in the sociology of scientific knowledge and communitarian epistemology should reconsider their long-standing distrust of Popper's philosophy. Indeed, they may have much to gain by treating Popper as a friend rather than a foe.

## Introduction

Karl Popper's political philosophy is usually placed squarely within the liberal tradition. John Gray, for example, writes that Popper's political works "contain a defence of liberalism (the most formidable to be found anywhere in 20th-century thought)" (Gray 1976, p. 339). Michael Lessnoff and Anthony Quinton have both argued that Popper's political philosophy is a natural extension and refinement of the liberal doctrines of John Stuart Mill (Lessnoff 1980, p. 105; Quinton 1975, pp. 148–149; see also Gray 1976, p. 354). Alan Ryan makes the strong claim that Popper's interest in science was itself motivated by a more general aim to defend political liberalism (Ryan 1985, p. 102). Such readings of Popper's philosophy gain in credibility when set alongside the fact that Popper too had described himself as an "old-fashioned liberal" (Stokes 1998, p. 72; see also Magee 1973, p. 80).

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Yet not everyone agrees that Popper was a liberal. Geoffrey Stokes argues that Popper “shares little with the classical liberals” and is more closely aligned with European social democracy (Stokes 1998, p. 72). Jeremy Shearmur, utilising Philip Pettit’s distinction between republican and liberal understandings of freedom, suggests that Popper is best treated as a republican thinker (Shearmur 2001, pp. 37–38; cf. Pettit 1997). Bryan Magee contends that Popper’s philosophy is best applied in the service of democratic socialism (Magee 1973, p. 81). Given that these scholars must disagree with Popper himself on this point, they seem committed to arguing that Popper did not fully understand the nature of his contribution to political thought.

In this paper, I shall adopt a similar position. A key element in such an approach is a consideration of the situation in which Popper wrote his main political works, *The Open Society and its Enemies* and *The Poverty of Historicism*. Popper composed these texts during the Second World War, and his problem was to defend European civilization against its own dark tendency towards imperialistic fascism. Disillusioned by the failure of social democrats to unconditionally oppose the Nazis, Popper naturally turned to a defence of what seemed to him the only viable alternative, liberal democracy (Magee 1973, p. 80). With the horror of Nazism spreading through Europe in the early 1940s, there could have been little time for nuanced analysis. In the first decade of the twenty-first century, on the other hand, the comparatively peaceful circumstances of Europe provide room for more subtle reflections. In this paper I shall attempt to re-think Popper’s contribution to philosophy, and I aim to demonstrate that, despite his own views to the contrary, Popper cannot be categorized as a liberal thinker in any conventional sense.

Indeed, I hope to make a case for reading Popper as a communitarian philosopher. If I succeed in this, then important consequences follow not just for our understanding of Popper’s political philosophy but, as I hope to also demonstrate, for our understanding of his philosophy of science as well. I argue that Popper’s communitarianism points up his affinities with the sociology of scientific knowledge and also with recent work in communitarian epistemology. This may come as a surprise to some readers. Although sociologists of scientific knowledge have acknowledged a superficial resemblance between Popper’s philosophy and their own position, they have consistently argued for their own originality on certain matters of key importance. I believe that they may have been mistaken to do so. There is more harmony between their work and Popper’s philosophy than has heretofore been acknowledged. I suggest that the recognition of these affinities may clear new ground for productive collaboration between the sociology of scientific knowledge and communitarian epistemology, on the one hand, and Popperian political philosophy and philosophy of science, on the other.

In Section 2, I briefly consider the debate between liberals and communitarians. As will be seen, liberalism, in particular, is not easily constrained by precise definition. I suggest, however, that individualism has played a historically important, if not essential, role in liberal theory. In Section 3, then, I treat Popper’s methodological individualism as a corner stone of his alleged liberalism. In fact, Popper’s methodological individualism is not above criticism, and it proves especially problematic in light of the considerable emphasis Popper also placed on institutions. Indeed, it turns out that Popper’s work on institutions makes a far more important

contribution than does his problematic espousal of methodological individualism. In Section 4, I explore in detail Popper's theory of institutions, arguing that it provides the foundation for his communitarianism. In Section 5, I outline the ways in which Popper's institutionalism underpins his theory of objectivity. This leads into a discussion of the sociology of scientific knowledge and of communitarian epistemology, in both of which institutional theories of objectivity also figure prominently. All the more unfortunate, then, that theorists in these fields have tended to misunderstand, and on that basis to reject, Popper's theory of objective knowledge. I conclude the chapter with some very tentative reflections on how the sociology of scientific knowledge and communitarian epistemology might benefit from a renewed consideration of Popper's philosophy.

## Liberalism and Individualism

Liberalism is, by definition, an approach to politics which puts liberty at its centre. Yet different people at different times have meant different things by liberty.<sup>1</sup> As a consequence, it is exceedingly difficult, if not impossible, to identify a stable extension for the term 'liberalism.' As a practically engaged, historically situated phenomenon, liberalism would seem to have no fixed essence (Geuss 2001, pp. 69–73). Indeed, Paul Gottfried has written that the history of liberalism is a history of "growing semantic confusion" (Gottfried 1999, p. 3).

This creates a difficulty for critics of liberalism. Not only does liberalism present an amorphous and indeterminate target, it has also proven remarkably capable of accommodating a wide range of criticism without giving up its claim to the liberal label (Neal and Paris 1990, p. 431). Because there is no fixed essence determining its meaning, liberalism is always open to significant re-interpretation and revision.

A case in point is the communitarian critique of liberalism which emerged in the 1980s, largely in response to the new liberal theory advanced by John Rawls in his 1971 book *A Theory of Justice*. It must be said that the label "communitarian critique" is, in this context, somewhat artificial. The authors typically gathered under this label often have divergent interests, and many of them do not even identify themselves as communitarians.<sup>2</sup> What they do have in common, and what motivates their being labelled communitarians, are worries about a certain liberal conception of the self. The main target of their arguments is Rawls, especially his notions of "the original position" and "the veil of ignorance." The original position is one in which free, rational actors deliberate and agree upon the principles of justice which are to structure their society. In order to keep their deliberation fair, Rawls suggests that it must happen behind a veil of ignorance which prevents actors from knowing anything whatsoever about their society, much less about their own place in that society and their relationships with others (Rawls 1971, p. 136ff).<sup>3</sup> Communitarians argue that this conception of the self places too much emphasis on the rational autonomy of the individual and not enough on the fundamentally constitutive role community plays vis-à-vis individual identity. This has proven a powerful and

effective line of attack on liberal inattentions to the importance of community and public values.<sup>4</sup> Yet it has hardly delivered a fatal blow to liberalism *as such*. Indeed, Amy Gutman has argued that the communitarian critique is “properly viewed as supplementing rather than supplanting basic liberal values” (Gutman 1985, p. 320). Will Kymlicka argues that value neutrality, not individualism, is the core concept of liberalism (Kymlicka 1989). In turn, William Galston, whilst rebuffing critics who would classify him as a communitarian, argues that liberalism “is a form of community” and he argues for “a non-neutral, substantive liberalism committed to its own distinctive conception of the good” (Galston 1991, pp. 43–44).<sup>5</sup>

The moral here is a Popperian one: namely, that there is little point in niggling over the correct application of terms. In the next section, I will critically examine the doctrine of individualism in Popper’s work. I shall argue that this doctrine is a failure, and that Popper at least tacitly recognised this fact and subsequently came to place more emphasis instead on institutions. This development in his philosophy is what I refer to as Popper’s communitarianism. Those readers who feel especially strongly about liberalism as a theory of the individual will likely take me to be ascribing (rightly or wrongly) an anti-liberal position to Popper. Those more convivial towards liberal theories of community will likely take me to be characterising Popper as a liberal reformist who acknowledges the fundamental role of community in political life.<sup>6</sup> I can offer no great objections to either interpretation. My goal in this chapter is not to remove Popper from the liberal tradition entirely, but only to divorce him from the mainstream ranks of liberal individualism. If I succeed in demonstrating that Popper’s individualism is inessential to his philosophy, then I will have fulfilled that goal.

## Methodological Individualism

Popper drew a distinction between “methodological individualism” and “methodological holism,” and he argued for the former and against the latter (Popper 1966, p. 91). Popper’s suspicions about holism were well founded. He rejected the romantic celebration of such abstract concepts as a general will, a national spirit, or a group mind, citing with approval Mill’s claim that “[m]en are not, when brought together, converted into another kind of substance” (cited in Popper 1966, p. 91). Popper correctly recognized that the elevation of such concepts as ‘national spirit’ or ‘*das Volk*’ to the level of ontologically independent and authoritative phenomena was both morally pernicious and logically incoherent. However, in defending individualism against holism, Popper elided the concept of the individual with the concept of autonomous subjectivity. In other words, he did not sufficiently acknowledge the fact that one can reject holism without also giving up a concept of intersubjectivity, that is, without also rejecting the sociological insight that no individual can be a person except in relation to other persons.



Having made this observation, however, it must be immediately emphasized that, *in practice*, Popper challenged the doctrine of autonomous subjectivity through his acknowledgement that individuals exist in "situations," and in relation to others. Accordingly, as both Steven Lukes and A. F. Chalmers have pointed out, it was rather misleading of Popper to have insisted on labelling his position one of methodological individualism (Lukes 1970, p. 86; Chalmers 1985, p. 73).<sup>7</sup> The reason he did this, however, seems to be that, in dividing the field exclusively into *either* methodological individualism *or* methodological holism, Popper was left with no other option but to declare himself a methodological individualist. Yet, in marrying himself to this rigid distinction, Popper was forced to struggle with what Alasdair MacIntyre correctly identified as a "false dichotomy" (MacIntyre 1960, p. 219). As MacIntyre puts it,

the choice posed is utterly misleading. What is important is that no individual can be characterised except in terms applicable to other individuals. To characterise an individual by any predicate at all is to exhibit that individual as a member of a class. (loc. cit.)

In other words, to speak meaningfully of an individual is to presuppose her membership in a group. Lukes also rejects Popper's distinction, pointing out that "just as facts about social phenomena are contingent upon facts about individuals, the reverse is also true" (Lukes 1970, p. 80). The concepts of an individual and of a group cannot be divided in the way Popper's dichotomy suggests. It should be immediately clear that this poses a problem, not just for methodological individualists, but for methodological holists as well. After all, at least according to Popper's interpretation, holists claim that a group can be considered separately from, and so elevated above, the individuals who constitute it. In other words, holists tend to attribute an ontological independence and authority to the group as a definite whole, rendering it a kind of "super-individual."

The fact that individualists and holists alike share this problem demonstrates that, in an important sense, they occupy the same position. Indeed, as Raymond Geuss has argued, liberalism's celebration of the self derives from the very Romanticism which Popper decried as holistic and politically dangerous (Geuss 2001, p. 102). This point speaks directly to the situation in which Popper developed his critique. Jeffrey Herf, in his study of inter-war Germany, demonstrates how the *völkisch* ideologists of German Romanticism promoted "the accentuation of individual subjectivity combined with a sense of being subjected to a fate and destiny beyond one's control" (Herf 1984, p. 15). In setting out to demolish the historicism of methodological holism, Popper succeeded in extracting only one of its two fangs. The second persisted in his endorsement of methodological individualism, an unfortunate circumstance forced upon him by the false dichotomy he adopted in his analysis.

Yet it needs to be emphasised once again that Popper's philosophical instincts were simply too sharp for him to have been irrevocably boondoggled by a misleading analytical distinction. As I noted earlier, Popper was a methodological individualist in name only. With his emphasis on intersubjectivity and his in-practice repudiation of autonomous subjectivity, he in fact did complete his demolition of

political Romanticism. This can be seen most clearly in the emphasis he placed on the role of institutions in explaining human actions.

## Institutions

Lars Udehn instructively compares Popper's methodological individualism with "Austrian" methodological individualism. The latter originated in the Austrian School of Economics and is characterised by its emphasis on subjectivity (Udehn 2001, p. 87). What distinguishes Popper's methodological individualism from the Austrian School's is its rejection of this subjectivism (Udehn 2001, p. 210). This is most evident in his attack on psychologism. Contending that an appeal to psychological propensities alone cannot explain scientific and industrial progress, Popper recommends that the theory of psychological propensities be replaced with an institutional analysis of the conditions upon which such progress depends (Popper 1961, p. 154). He argues that the psychological, or subjective, properties of individuals are dependent upon the institutional conditions in which those individuals exist. Only through the analysis of the institutional conditions in which human actions are embedded can one achieve an adequate theory of progress (Popper 1961, p. 154).

Popper's use of 'institution' is rather broad, encompassing laboratories, periodicals, congresses and conferences, universities and other schools, books, printing presses, and even writing, language, and, at least in part, scientific method. All of these are, Popper writes, *social institutions* (Popper 1961, p. 154). He argues, furthermore, that "what is usually called '*scientific objectivity*' is based, to some extent, on social institutions" (Popper 1961, p. 155). Indeed, Popper even goes so far as to make the general claim that "our actions cannot be explained without reference to our social environment, to social institutions and to their manner of functioning" (Popper 1966, ii, p. 90). Furthermore, far from what one might expect from a methodological individualist, Popper often treats institutions as being distinct from individuals. For example, writing about traditions, he maintains that they hold an intermediate position between persons and institutions (Popper 1965, p. 133). Describing his method of "situational analysis," he argues that "[i]t makes room for giving full weight [...] not only to individuals but also to institutions" (Popper 1969, p. 199).

Popper's "situational logic" provides further evidence of the distance which lies between his philosophy and individualism. As Joseph Agassi observes, situational logic is Popper's answer to problems arising from the individualistic principle of "psychologistic rationality" (Agassi 1975, p. 153). This principle states that individuals will act in ways adequate to their aims, given their circumstances (Agassi 1975, p. 146). In other words, it treats individuals as rational agents, and is committed to the explanation of social institutions in terms of the aggregated rational actions of individuals. Yet Popper points out that this explanatory method founders in cases where the actors lack full awareness of the circumstances in which they act (Popper 1969,

p. 199). The institutions which develop in such cases are not consciously designed, but have “just ‘grown’, as the undesigned results of human actions” (Popper 1961, p. 65, emphasis removed). Even those institutions which emerge as the result of the fully conscious and intentional actions of individuals will be, as a rule, “the indirect, the unintended and often the unwanted by-products of such actions” (Popper 1966, ii, p. 93, emphasis removed).<sup>8</sup> It follows from this that institutions cannot be explained solely in terms of the aggregated psychological characteristics of individuals. Hence, the principle of psychologicistic rationality fails.<sup>9</sup>

Yet Popper is also careful not to grant too much explanatory power to institutions. Indeed, when he introduces his method of situational analysis, he does so explicitly as an alternative to a form of analysis which attempts to explain historical events by reference to abstract entities – the spirit of a nation or an age, for example – which operate according to their own law-like principles, independently of the actions of individual persons. In short, Popper introduces situational analysis in opposition to methodological holism, as he understands it. Popper's institutions serve, in part, as concrete replacements for the abstract, indeed metaphysical, phenomena posited by holists. Hence, he makes frequent arguments of the sort that institutions “must be analysed in individualistic terms – that is to say, in terms of the relations of individuals acting in certain situations” in order to assuage any worries that he is slipping towards methodological holism (Popper 1966, ii, p. 324, n. 11). However, the explanatory move in the cited passage is not from institutions to individuals *as such*, but rather from institutions to individuals in relationship with one another. In other words, Popper's explanatory focus is on *groups* of interacting persons. The sense of ‘group’ being used here is commensurate with Popper's notion of institution. A group is no more an abstract and autonomous metaphysical “whole,” a substance which can be separated from the individuals making it up, than is, let us say, a pair of socks an independent substance which can be separated from the two socks making it up.

Yet a pair of socks nevertheless possesses properties which cannot be reduced to the properties of two socks considered independent of one another. A pair of socks might be characterized, for example, by the complementary relations (of colour, size, etc.) which exist between two socks. This complementarity can only be understood when the two socks are taken together. Likewise, a group (or institution) cannot be reduced to the private characteristics of its individual members. Such a reduction would amount to psychologism, and, as Popper argued, would thus fail to explain such phenomena as the rational progress of science.<sup>10</sup> The rationality of science, on Popper's account, is a characteristic of scientists working together, that is, working as a group. Put another way, the rational progress of science can be explained, not in terms of the rational propensities of individual scientists, but rather in terms of the rationality of the scientific community *as such*.<sup>11</sup>

Here it must be stressed that the rationality of the scientific community should not be regarded as a propensity of that community taken as a “whole.” To put matters this way would be to reproduce the problematic principle of psychologicistic rationality at the community level. It would, in other words, be to treat the community as a kind of “super-individual.” The crux of this argument is the claim that holism,

understood in the Popperian sense, is really just another version of individualism. This recalls MacIntyre's argument, canvassed above, that the dichotomy between methodological individualism and methodological holism is a false dichotomy.

Prickly metaphysical issues potentially emerge here, but these should not delay us for long. It is perfectly intelligible to describe a group of scientists as rational. We might even say of the group that it has the property of being rational. But, on the institutional account, the group is not an individual substance, so its property of being rational cannot belong to it in the way that a property belongs to a substance. Substance ontology will thus be of little service here.<sup>12</sup> Such an ontology seems suitable for both methodological individualism and methodological holism, insofar as both treat entities individually, and because substance ontology supports an understanding of entities as individuals. But the present considerations call for a different methodology, one that allows for an explanation of social events in terms of the properties of both individual and (non-individual) institutional phenomena. At a minimum, it seems that this would have to be a pluralistic methodology. An ontology which could support this methodology would thus have to be pluralist too. I do not feel able to elaborate any further on what such an ontology might look like, nor am I convinced that doing so would be either prudent or desirable. In any case, we can sufficiently explain social events by reference to persons and institutions without having to rely upon an ontology which would successfully capture the respective "natures" of individuals and institutions at the metaphysical level. We need only keep firmly in mind that individuals are not institutions, and institutions are not individuals.

Given the argument so far, it does seem rather peculiar that Popper insisted on using the label 'methodological individualism.' In fact, as Udehn notes, Popper's interest in this doctrine seems to have waned as his career progressed. When Popper returned to the methodology of the social sciences in the 1960s,

the theme of methodological individualism has almost disappeared. The few scattered remarks there are about anything resembling methodological individualism are consistently ambiguous. Institutionalism, however, remains important, but most important is situational analysis, which is now launched as *the* method of social science. (Udehn 2001, p. 2006)

It would appear, then, that institutionalism and situational analysis were, at the end of the day, more important for Popper than was methodological individualism. I have argued that situational analysis – as a method which focuses on individuals interacting with one another in some common context – is most intelligibly described as a method oriented towards communities. In this way, it forms the core of what I call Popper's communitarianism. Note, however, that this is a communitarianism which need not support the dogma that only communal phenomena exist. On the contrary, I have attempted to maintain a practical ambivalence towards such metaphysical generalizations and advocated methodological pluralism instead. Communities are made up of interacting individuals. One of the most important characteristics of communities is their possession of properties which cannot be explained reductively in terms of the private properties of their participating individuals. In the next section, I will discuss Popper's analysis of one such irreducible and communal property, namely, objectivity.

## Objectivity

As already cited in the previous section, Popper argued that “‘scientific objectivity’ is based, to some extent, on social institutions” (Popper 1961, p. 155). Furthermore, he contended that social institutions play a central role in preserving the objectivity of science (Popper 1961, p. 156). Thus, for Popper, scientific objectivity necessarily depends upon institutions, not only for its genesis, but also for its continuance. The implication of this is that there can be no objectivity where there are no institutions.<sup>13</sup> One immediately obvious consequence of this position is that objectivity cannot solely be the private characteristic of individual persons. In other words, Popper holds that, to some consequential degree, objectivity is a public phenomenon, that is, a characteristic of groups.

In his 1972 book *Objective Knowledge*, Popper presents his most comprehensive discussion of objectivity. There he distinguishes three “worlds”: the world of physical objects and states; the world of mental states; and the world of the objective contents of thought (Popper 1972, p. 106). Popper calls this lattermost world the “third world” (Popper 1972, p. 106). Among the entities of this third world, Popper includes problems, conjectures, theories, arguments, and the contents of journals, books and libraries (Popper 1972, p. 107). Popper is careful to distinguish between two different conceptions of knowledge, that in the subjective sense and that in the objective sense. This distinction correlates to his distinction between the “second world” of individual mental states and the third world of objective contents. Popper argues that objective knowledge is “totally independent” of an individual knower’s subjective mental states. He writes that “[k]nowledge in the objective sense is *knowledge without a knower: it is knowledge without a knowing subject*” (Popper 1972, p. 109).

A position which states that objective knowledge can exist independently of any particular knower seems a far cry from epistemic individualism. Indeed, one might construe Popper as asserting that individuals no longer play any role at all in the existence of objective knowledge.<sup>14</sup> Such a move seems to motivate Anthony O’Hear’s description of Popper as a Platonist (O’Hear 1980, p. 194). Yet, as Alan Musgrave (1974) has pointed out, such an interpretation, though possible, lacks charity. In particular, it ignores Popper’s central claim that knowledge “grows.” If the objective contents of knowledge possess eternal features, that is, if they are construed in Platonist terms, then it becomes difficult to see how they might still grow (Musgrave 1974, p. 587). In any case, Popper explicitly rejected the Platonist interpretation of his theory (Popper 1972, p. 106). Yet this rejection was a qualified one. Popper notes that his third world has much in common with Plato’s forms and also with Hegel’s objective spirit, but emphasises that it differs radically from both in important ways (Popper 1972, p. 106). What is striking here is that both Plato and Hegel were subjected to fierce attack by Popper in his *Open Society and Its Enemies*. Their crime was holism, in Popper’s sense of the term. Hence, Popper’s withdrawal from individualism in *Objective Knowledge*, and his qualified endorsement of the philosophies of Plato and Hegel, may give the impression that Popper has entered the camp of the holists.

But, as was argued in Section 3, the dichotomy between methodological individualism and methodological holism is a misleading one. Popper's institutionalism includes features of both whilst avoiding their worst shortcomings. I suggest that the most charitable reading of Popper's third world would interpret it in light of his theory of institutions. From this point of view, it should be quite clear that Popper's theory of objective knowledge does not exclude individuals *as such*, but only individuals as conceived independently of their relations to others, that is, independently of their membership in a community. Nor, in turn, does the theory exclude holistic phenomena *as such*, but only holistic phenomena as conceived independently of a community of interacting individuals. On this basis, then, it seems quite reasonable to construe the objective inhabitants of Popper's third world as being the inhabitants of a *social* world.

The sociologicistic threads running through Popper's third world of objective knowledge can be drawn out further by considering the anti-psychologism of the theory. Recall from Section 4 that Popper argued that the growth of institutions cannot be explained solely in terms of the psychological properties of individuals. From this it would seem to follow that, if objective knowledge is institutional in character, then it too cannot be explained by sole reference to individual psychology. Indeed, in *Objective Knowledge* Popper emphasises that the objective phenomena of world three cannot be explained reductively in terms of the mental phenomena of world two. In fact, he insists that the direction of explanation runs entirely the other way around. He argues that it is only through a study of the objective products of the third world that we can hope to understand the processes of production (Popper 1972, p. 114). The idea here seems to be that exclusive attention to the psychological properties of individuals will fail to explain the objective products of production, because processes of production include irreducible individual and institutional features. Only by reasoning back from the objective effects of production to their psychological and institutional causes might one achieve a sufficient explanation of the production process (Popper 1972, p. 115). Only in this way, in other words, can one develop a complete understanding of production as the actions of individuals interacting with one another in a specific situational context.

Popper's observation that objective products are always the result, in part, of institutional causes has not always been appreciated. The sociologist of scientific knowledge, David Bloor, for instance, seems to entirely overlook Popper's theory of institutions when he characterises Popper's account of objectivity in *Objective Knowledge* as individualistic and atomistic (Bloor 1991, p. 62). Bloor writes, furthermore, that

[t]he Popperian style of mystification is to endow logic and rationality with an a-social and, indeed, transcendent objectivity. Thus in his recent work Popper talks of objectivity as forming a 'world' in its own right, to be distinguished from the world of physical and mental processes (Bloor 1991, p. 77)

and also that "the opaque ontology of the so-called 'objectivity approach' paralyses the imagination and stultifies research" (Bloor 1974, p. 76).

Granting that Popper's presentation of his theory of an objective third world is more opaque than it might have been, it still need not follow that the theory is



an a-social and transcendental one. Lack of perspicuity does not entail mystification. Popper indeed distinguished the objectivity of his third world from physical (i.e. behavioural) and mental (i.e. psychological) processes, but he did not similarly distinguish it from social processes. Popper's institutions were, after all, *social* institutions.

What seems to have misled, and no doubt provoked, Bloor was Popper's explicit dismissal of sociology as a "subjective" or "second-world" form of explanation (Popper 1972, p. 114). According to Popper, sociologists try to explain objective phenomena reductively in terms of the subjective characteristics of individuals. But this only amounts to a criticism of a particular sociological method, not an outright denial of the causal relevance of social phenomena. In fact, Popper's criticism is directed at an old form of the sociology of knowledge, exemplified by Karl Mannheim (Popper 1966, ii, p. 213). According to this view, sociologists explain failed science by interpreting it as the product of distorting social processes which corrupt the otherwise impartial and objective methods of science. Popper criticises this approach for painting objective science as "a process in the mind or consciousness of the individual scientist, or perhaps as the product of such a process" (Popper 1966, p. 217). In other words, Popper charges Mannheimian sociology of knowledge with *psychologism* vis-à-vis scientific objectivity. In contrast, Popper argues that

'scientific objectivity' is not a product of the individual scientist's impartiality, but a product of the social and public character of scientific method; and the individual scientist's impartiality is, so far as it exists, not the source but rather the result of this socially or institutionally organized objectivity of science. (Popper 1966, ii, p. 220)

Popper argues that impartiality, construed as the psychological characteristic of individual scientists, cannot explain the objective products of scientific enquiry. If we are to understand these products, we should not start with the processes of production, since they are precisely what is in need of clarification. Only by starting with the objective products of science, and reasoning backwards from effect to cause, do we arrive at the insight that scientific objectivity is, to a consequential extent, an institutional phenomena (Popper 1972, p. 115). Most importantly, the impartiality of the individual scientist is, Popper argues, a consequence of her conformity to the institutional constraints governing the objectivity of science. Popper argues that "this social aspect of science is almost entirely neglected by those who call themselves sociologists of knowledge" (Popper 1966, ii, p. 217).

Those who are familiar with Bloor's "strong programme" in the sociology of scientific knowledge will recognise that Popper's criticism does not threaten his approach.<sup>15</sup> Indeed, Bloor shares Popper's criticism of Mannheimian sociology of knowledge, and he too argues that scientific objectivity is a social phenomenon. It seems that Bloor took Popper's attack on sociology as a broad attack on the very idea of a sociological analysis of scientific knowledge, rather than as a narrow attack on a particular methodological approach present within the history of the sociology of knowledge. In other words, he took Popper to be a foe when he was, in fact, a friend. This leads Bloor to perform some unnecessary and rather fanciful interpretive footwork. Although he recognises that there is much in Popper's work that he can agree with, because he assumes that Popper's eschews all sociological



analysis, he interprets these features of Popper's work as appearing in defiance of the author's own stated interests. Thus Bloor claims that he must apply a "transformative method" of analysis (as developed by Feuerbach) to Popper's third world of objective knowledge. The "conjectural rule of transformation" to be applied is: "For 'Third World' read 'Social World'" (Bloor 1974, p. 70). Yet, as I have already demonstrated above, this "transformation" is really nothing of the sort. Popper never denied that his third world was the social world. Indeed, he insisted throughout his career that objectivity is a social phenomenon. Bloor's transformative method is nothing more than smoke and mirrors obscuring this otherwise clear fact.<sup>16</sup>

This misinterpretation of Popper's theory of objectivity has also found a life in recent philosophy. Martin Kusch (2002) has remonstrated against Popper's third world in his own development of a "communitarian epistemology." His attack is short and to the point. It concerns Popper's characterisation of knowledge as a human artefact, and focuses on the first half of this passage from *Objective Knowledge*:

A wasp's nest is a wasp's nest even after it has been deserted; even though it is never again used by wasps as a nest. [...] Similarly a book remains a book – a certain type of product – even if it is never read (as may easily happen nowadays). (Popper 1972, p. 115)

Kusch uses Popper's observations about the wasp's nest as evidence that "Popper explicitly accepts the implication that knowledge can survive even when all humans disappear from the face of the earth" (Kusch 2002, p. 165). Yet this conclusion only follows if one falsely assumes, with Bloor, that Popper's third world of objective knowledge is a-social and transcendent. There is nothing in Popper's analogy to the wasp's nest which could support this assumption. The wasps do not disappear from the face of the earth, they merely desert the nest. Popper's point was that the objective significance of an artefact cannot be accounted for in terms of its specific use by individual agents. A book is a book even when it is never read, because the objective significance of books is a product of their being situated within a particular institutional structure. The social status of a book *qua* book is quite independent of whether or not some individual has read it. On the other hand, this social status is not independent of the institutional circumstances wherein books are endowed with their conventional meaning. As Popper puts it:

It is its possibility or potentiality of being understood, its dispositional character of being understood or interpreted, or misunderstood or misinterpreted, which makes a thing a book. And this potentiality or disposition may exist without ever being actualised or realized. (Popper 1972, p. 116)

Needless to say, if all human beings – or, perhaps more to the point, if all capacity to understand or interpret, or misunderstand or misinterpret books – disappeared from the face of the earth, then, on Popper's account, one could scarcely speak any longer of the objective contents of books. To put the point more generally, there appears to be nothing in Popper's account of objective knowledge which contradicts the core tenet of Kusch's communitarian epistemology: that knowledge is a social status (Kusch 2002, p. 166). For Kusch, as for Bloor, Popper is a friend rather than a foe.

In sum, I have argued in this section that Popper's theory of objective knowledge, because it conceptualises objectivity in institutional terms, is committed to the claim that objectivity is the characteristic of a concretely situated, interacting group of individuals, that is, a community. This claim flows naturally out of Popper's consistent emphasis on the fundamental role of institutions in the development and preservation of knowledge.

## Conclusion

My chief concern in this chapter has been to highlight the communitarian threads running through Popper's philosophy. This feature of Popper's work seems to conflict with his strongly professed methodological individualism. However, as I hope to have shown, Popper's consistent emphasis on the interaction between individuals and the role of situational contexts vis-à-vis individual behaviour seems to undercut his early claim to be a methodological individualist. Indeed, Popper's references to individualism fade off in his later philosophy, while his emphasis on institutions intensifies and moves more firmly into the centre of his work. In particular, Popper's theory of the third world of objective knowledge has a powerful institutional basis. For Popper, objectivity is, in the first instance, a characteristic of communities, not of individuals.

The communitarian aspects of Popper's work have not always been properly understood. I have considered two examples of such misunderstanding, one from the sociology of scientific knowledge and another from communitarian epistemology. Both fields have tended to approach Popper as a foe who touted a philosophy which is in fundamental opposition to their own. But this treatment of Popper, based as it is on a misconstrual of his philosophy, is both unjustified and counter-productive. It may be that much of benefit could grow from more friendly relations between Popperian philosophy and such fields as the sociology of scientific knowledge and communitarian epistemology. After all, they each represent a marginalized, but nevertheless important, critical position within the mainstream of intellectual culture. Further exploration of the affinities running between Popperian philosophy and approaches such as these would, I should think, be far more attractive and constructive than a continuation of the misunderstandings which currently divide their houses.

Certainly one area warranting further attention is the position of communitarian theories of knowledge with respect to contemporary political theory and practice. At the beginning of this chapter, I expressed my doubt that Popper can be comfortably accommodated within the liberal tradition. His communitarianism runs counter to the individualism which seems so common in liberal thought. Yet, there are reasons to doubt that individualism is a necessary component of liberalism. One could conceivably argue that Popper espoused a kind of "liberal communitarianism." In any case, proponents of the sociology of scientific knowledge and of communitarian epistemology have tended towards reticence when it comes to the broader political

consequences of their theories.<sup>17</sup> More attention to the way Popper's communitarian analysis of knowledge goes together with the powerfully prescriptive elements in his social and political philosophy could prove enlightening for such theorists, leading to a more comprehensive development of their projects, and a richer understanding of those projects' social and political implications.

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## Notes

1. For a short and informative historical overview of the remarkably diverse political structures which have been developed in the name of liberty see Cranston (1967).
2. Key authors include Alasdair MacIntyre (1981), Michael Sandel (1982), Charles Taylor (1979, 1982), and Michael Walzer (1983). Daniel Bell cites evidence to the fact that none of these four authors labels himself a communitarian (Bell 1993, p. 17, n. 14). Stephen Mulhall concurs on this point, and also notes the lack of conceptual unity between their works (Mulhall 1987, p. 270).
3. A deeply unsympathetic, but nevertheless insightful, discussion of Rawls' concept of the veil of ignorance can be found in Geuss (2005).
4. For Rawls' own response to these criticisms, see Rawls (1985). Sibyl Schwarzenbach (1991) makes the interesting and provocative claim that the communitarian critique of Rawls is beside the mark because Rawls already is a communitarian thinker (in the spirit of Hegel, no less).
5. Galston (1991, p. 43) endorses Kymlicka's characterisation of him as a "worried liberal."
6. Harald Stelzer presents such a reading of Popper, but distinguishes more sharply than I do between liberalism and communitarianism, arguing that Popper provides a liberal solution to the problem of citizen participation highlighted by communitarians. Stelzer's argument depends, however, on an unsubstantiated and rather hostile characterisation of communitarianism as leading, in its strongest forms, to "authoritarian and totalitarian consequences" (Stelzer 2006, p. 232). Stelzer furthermore fails to explain how a communitarian emphasis on citizen participation might square with its alleged authoritarianism. Charles Taylor offers a more coherent position in this regard, glossing the communitarian notion of freedom as "participatory self-government" and contrasting it with despotism (Taylor 1989, p. 171).
7. In addition, Robert Ackermann observes that "[t]here is a noticeable lack of argument for methodological individualism in Popper" (Ackermann 1976, p. 166).
8. In a similar passage in another text, Popper (1961, p. 158) notes that "'unintended' in this context does not perhaps mean 'not consciously intended'; rather it characterizes repercussions which may violate all interests of the social agent, whether conscious or unconscious."
9. Note, in this regard, Chalmers' argument that Popper's "rationality principle" is at odds with his situational analysis, and tied to his methodological individualism (Chalmers 1985, p. 82).

Chalmers further suggests that the rationality principle is constituted by Popper's denial of holism (Chalmers 1985, p. 83). Yet a denial of holism does not entail individualism. Popper's institutionalism will also do the job, as I argue in the next four paragraphs.

10. Matt Brown accuses me here of ignoring normative issues (Brown 2008, p. 3). He argues that the communitarian's descriptive ontology of rational agency does not yet include the additional normative claim made by communitarians that communal values should be preferred to rational self-determination. But Brown is confusing things here. If, as the communitarian argues, individuals are rational only in virtue of their membership in a community, then their individual acts of rational self-determination are necessarily constituted by the communal norms which prevail in that community. The communitarian does not reject outright the notion of rational self-determination, but reformulates that notion in communitarian terms. Hence, I am not here ignoring normative issues. An ontological description of rational agency is *eo ipso* a normative claim about what counts as rational.
11. For this reason, it seems to me that Popper could not endorse Rawls' notions of the "original position" and the "veil of ignorance." Rawls presumes that individuals can function rationally in the absence of all knowledge of their situation and relations to others. According to Popper, such individuals can act rationally only in virtue of their personal situations and relations to others. Hence, Rawls' method would seem, on Popper's account, to systematically prevent actors from recognizing the very conditions making their rational deliberation possible. Surely this method, with its apparent disregard for the phenomenology of political practice, provides a less than sound basis for the principles of justice meant to govern society.
12. This accords well with Mill's claim, cited earlier and endorsed by Popper, that "[m]en are not, when brought together, converted into another kind of substance." We can now see how this statement might be true without limiting the field of existents to individual persons.
13. I use the terms 'scientific objectivity' and 'objectivity' interchangeably, assuming that this will make no difference to my argument.
14. Ackermann also doubts that Popper can sustain methodological individualism in the face of his account of world three (Ackermann 1976, p. 167).
15. At least on the surface. In fact, there is some reason to worry that Bloor courts trouble when he attempts to explain institutional phenomena reductively in terms of the biological characteristic of individuals. For a criticism of Bloor on this point, see Martin Kusch (2004a). For Bloor's response and Kusch's reply, see Bloor (2004) and Kusch (2004b).
16. Interestingly, Bloor elsewhere recognises and endorses Popper's earlier discussion, in *The Open Society and Its Enemies*, of the social nature of scientific objectivity (Bloor 1997, pp. 108–109; cf. Popper 1966, ii, pp. 218–220). Hence, in rejecting Popper's later account in *Objective Knowledge*, Bloor is forced to posit a "retreat" by Popper resulting in "a transfigured and mystified apprehension of the social world" (Bloor 1997, p. 137). However, Bloor offers no explanation for Popper's alleged turnaround. My own interpretation, by contrast, tells a story of continuous progress rather than dramatic change, and I have offered an explanation for that progress, namely, Popper's wiggling free from the methodological individualism which was a drag on his institutional and situational analyses.
17. In a recent article, which has only just come to my attention, Bloor (2007) takes much of the punch out of this observation. He forcefully demonstrates the way in which a communitarian might effectively condemn the use of torture, as well as the despicable practice of "extraordinary rendition." Hats off.

## References

- Ackermann, Robert John. 1976. *The Philosophy of Karl Popper*. Amherst, MA: University of Massachusetts Press.
- Agassi, Joseph. 1975. Institutional Individualism. *The British Journal of Sociology* 26(2): 144–155.

- Bell, Daniel. 1993. *Communitarianism and Its Critics*. Oxford: Clarendon.
- Bloor, David. 1974. Popper's Mystification of Objective Knowledge. *Science Studies* 4(1): 65–76.
- Bloor, David. 1991. *Knowledge and Social Imagery* (second edition). Chicago, IL: University of Chicago Press.
- Bloor, David. 1997. *Wittgenstein, Rules and Institutions*. London: Routledge.
- Bloor, David. 2004. Institutions and Rule-Scepticism: A Reply to Martin Kusch. *Social Studies of Science* 34(4): 593–601.
- Bloor, David. 2007. Epistemic Grace: Antirelativism as Theology in Disguise. *Common Knowledge* 13(2–3): 250–280.
- Brown, Matthew J. 2008. Comment: 'Popper's Communitarianism'. Commentary presented to the Canadian Philosophical Association, June 3rd, 2008, Vancouver.
- Chalmers, A. F. 1985. Methodological Individualism: An Incongruity in Popper's Philosophy. In *Popper and the Human Sciences*, eds. Gregory Currie and Alan Musgrave, 73–87. Dordrecht: Martinus Nijhoff.
- Cranston, Maurice. 1967. Liberalism. In *The Encyclopedia of Philosophy*, Vols. 3 and 4, ed. Paul Edwards, 458–661. London: Macmillan and Free Press.
- Galston, William A. 1991. *Liberal Purposes: Goods, Virtues, and Diversity in the Liberal State*. Cambridge: Cambridge University Press.
- Geuss, Raymond. 2001. *History and Illusion in Politics*. Cambridge: Cambridge University Press.
- Geuss, Raymond. 2005. Neither History nor Praxis. In *Outside Ethics*, idem., 29–39. Princeton, NJ: Princeton University Press.
- Gottfried, Paul Edward. 1999. *After Liberalism: Mass Democracy in the Managerial State*. Princeton, NJ: Princeton University Press.
- Gray, John N. 1976. The Liberalism of Karl Popper. *Government and Opposition* 11: 337–355.
- Gutman, Amy. 1985. Communitarian Critics of Liberalism. *Philosophy and Public Affairs* 14(3): 308–322.
- Herf, Jeffrey. 1984. *Reactionary Modernism: Technology, Culture, and Culture in Weimar and the Third Reich*. Cambridge: Cambridge University Press.
- Kusch, Martin. 2002. *Knowledge by Agreement: The Programme of Communitarian Epistemology*. Oxford: Clarendon.
- Kusch, Martin. 2004a. Rule-Scepticism and the Sociology of Knowledge: The Bloor-Lynch Debate Revisited. *Social Studies of Science* 34(4): 571–591.
- Kusch, Martin. 2004b. Reply to My Critics. *Social Studies of Science* 34(4): 615–620.
- Kymlicka, Will. 1989. Liberal Individualism and Liberal Neutrality. *Ethics* 99(4): 883–905.
- Lessnoff, Michael. 1980. The Political Philosophy of Karl Popper. *British Journal for the Philosophy of Science* 10(1): 99–120.
- Lukes, Steven. 1970. Methodological Individualism Reconsidered. In *Sociological Theory and Philosophical Analysis*, eds. Dorothy Emmet and Alasdair MacIntyre, 76–88. London: Macmillan.
- Magee, Bryan. 1973. *Karl Popper*. New York: Viking Press.
- MacIntyre, Alasdair. 1960. Breaking the Chains of Reason. In *Out of Apathy*, ed. Edward Palmer Thompson, 195–240. London: Stevens & Sons.
- MacIntyre, Alasdair. 1981. *After Virtue*. Notre Dame: Notre Dame University Press.
- Mulhall, Stephen. 1987. The Theoretical Foundations of Liberalism. *Archives Européennes de Sociologie* 28(2): 269–295.
- Musgrave, Alan. 1974. The Objectivism of Popper's Epistemology. In *The Philosophy of Karl Popper* (vol. one), ed. Paul Arthur Schilpp, 560–596. La Salle, IL: Open Court.
- Neal, Patrick and David Paris. 1990. Liberalism and the Communitarian Critique: A Guide for the Perplexed. *Canadian Journal of Political Science/Revue canadienne de science politique* 23(3): 419–439.
- O'Hear, Anthony. 1980. *Karl Popper*. London: Routledge & Kegan Paul.
- Pettit, Philip. 1997. *Republicanism*. Oxford: Clarendon.
- Popper, Karl Raimund. 1961. *The Poverty of Historicism* (third edition). New York: Harper & Row.

- Popper, Karl Raimund. 1965. *Conjectures and Refutations* (second edition). New York: Basic Books.
- Popper, Karl Raimund. 1966. *The Open Society and Its Enemies*, 2 vols (fifth edition). London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1969. A Pluralist Approach to the Philosophy of History. In *Roads to Freedom: Essays in Honour of Friedrich A. von Hayek*, ed. Erich Streissler, 181–200. London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1972. *Objective Knowledge: An Evolutionary Approach*. Oxford: Clarendon.
- Quinton, Anthony. 1975. Karl Popper: Politics Without Essences. In *Contemporary Political Thinkers*, eds. Anthony de Crespigny and Kenneth Minogue, 147–167. New York: Dodd, Mead and Co.
- Rawls, John. 1971. *A Theory of Justice*. Cambridge, MA: Belknap of Harvard University Press.
- Rawls, John. 1985. Justice as Fairness: Political not Metaphysical. *Philosophy and Public Affairs* 14(3): 223–251.
- Ryan, Alan. 1985. Popper and Liberalism. In *Popper and the Human Sciences*, eds. Gregory Currie and Alan Musgrave, 89–104. Dordrecht: Martinus Nijhoff.
- Sandel, Michael. 1982. *Liberalism and the Limits of Justice*. Cambridge: Cambridge University Press.
- Schwarzenbach, Sibyl A. 1991. Rawls, Hegel, and Communitarianism. *Political Theory* 19(4): 539–571.
- Shearmur, Jeremy. 2001. Popper and Classical Liberalism. *Economic Affairs*, December: 37–40.
- Stelzer, Harald 2006. Popper and Communitarianism: Ethical and Political Dimensions of Democracy. In *Karl Popper: A Centenary Assessment*, Vol. 1, eds. Ian Jarvie, Karl Milford and David Miller, 231–240. Aldershot: Ashgate.
- Stokes, Geoffrey. 1998. *Popper: Philosophy, Politics and Scientific Method*. Cambridge: Polity.
- Taylor, Charles. 1979. Atomism. In *Powers, Possessions, and Freedom*, ed. Alkis Kontons, 39–61. Toronto, ON: University of Toronto Press.
- Taylor, Charles. 1982. The Diversity of Goods. In *Utilitarianism and Beyond*, eds. Amartya Sen and Bernard Williams, 129–144. Cambridge: Cambridge University Press.
- Taylor, Charles. 1989. Cross-Purposes: The Liberal-Communitarian Debate. In *Liberalism and the Moral Life*, ed. Nancy L. Rosenblum, 159–182. Cambridge: Harvard University Press.
- Udehn, Lars. 2001. *Methodological Individualism: Background, History and Meaning*. London: Routledge.
- Walzer, Michael. 1983. *Spheres of Justice*. Oxford: Basil Blackwell.

# Re-examination of Popper's Portrayal of Socrates

Herzl Baruch

**Abstract** Among “the great generation”, Popper claims, Socrates has contributed more than any other intellectual to the new faith of the open society and even died for it (Popper 1945, Vol. 1, pp. 128, 189). Bearing on the ‘Socratic problem’, Popper insisted that the historical Socrates, especially in the *Crito* and the *Apology*, didn’t have any metaphysical theory nor made any effort to theorize (*ibid.*, pp. 301–302). While also acknowledging that the figure of Socrates in the later early period dialogues, in *Gorgias* for example (*ibid.*, pp. 302–303), gradually becomes more positive and assertive, these theories are attributed to Plato. However, Popper didn’t discuss more systematically Socrates’ theories in the early dialogues, which according to later commentators (Vlastos 1991; Prior 2004) play a significant role. Do these theories justify a different understanding of Socrates? Trying to answer this question I will re-examine Popper’s portrayal of Socrates by focussing on his figure as a claimer of knowledge in three prominent early dialogues: the *Apology*, the *Protagoras* and the *Meno*. I suggest that, while all his claims of knowledge are compatible with ‘Platonist’ doctrines, the ‘Socratic’ principles are due to the explicit and implicit criticism of these claims through the dialogue. In this respect whereas the ‘Socratic’ principles aren’t manifested in the *Apology*, although reminded in the trial, they are better manifested in the *Protagoras* and the *Meno*, at least partly.

## Introduction

Popper emphasized the uniqueness of Socrates’ moral intellectualism, particularly the realization how little does he know. Bearing on the ‘Socratic problem’, he maintained that the historical Socrates, especially as portrayed in the *Apology* and the *Crito*, didn’t make any effort to theorize (Popper 1945, Vol. 1, pp. 301–302). On the contrary, “Socrates seems to have kept away from metaphysical theories as much as he could.” (*ibid.*, p. 190) For example, the fact that Socrates repeat-

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edly presents his thoughts about the concept of the soul. But while agreeing with Burnet (*ibid.*, *ibid.*) that it was Socrates who created the conception of the soul, he suggests that rather than putting forward a metaphysical theory of the soul Socrates talks about the intelligence of a self-sufficient individual, in short a moral theory of individualism.<sup>1</sup> Accordingly, later metaphysical elaborations of a theory of the soul, as given for example in the *Meno*, are Platonic.<sup>2</sup> If Socrates has *some* theories they are moral theories, and apart of his theory of individualism, also his equalitarian theory of justice and some moral maxims are mentioned (*ibid.*, pp. 189–190). Challenging the passage in the *Crito* where Socrates admits that he always described Sparta and Crete as well-governed (52e), he suggests that it was put incorrectly by later editing, may be by someone who tried to settle the *Crito* with the *Republic* (*ibid.*, pp. 304–305).<sup>3</sup> By these explanations Plato's historical account of Socrates, especially in the *Apology* and the *Crito* (*ibid.*, p. 304), is accepted as reliable and the positive philosophies incorporated gradually in *later* early period dialogues are regarded as Plato's ideas. The idea that there is a deep gap between the portrayal of the historical Socrates and the creation of the 'Platonic' Socrates, the 'older solution' to the Socratic problem, is contrasted in *The Open Society and its Enemies* (OSE) with Burnet's interpretation. Burnet (together with Taylor among others) rejected the idea that there is a deep gap between the Socratic and the Platonic and he demanded that "we should proceed from the assumption that *Plato really meant what he said*, and that when he made Socrates pronounce a certain doctrine, he believed and wished his readers to believe, that this doctrine was characteristic of Socrates' teaching" (in Popper 1945, p. 307; italics in the origin).<sup>4</sup> But while Popper agrees with Burnet that the *Apology* and the *Crito* are, as explained above, the most reliable historical accounts of Socrates, he rejects it when metaphysical and political theories are put into Socrates' mouth in later early period dialogues (*ibid.*, pp. 306–307).

Popper's dichotomy between Plato's Socrates having metaphysical and social theories and Socrates evading them and adopting the elenctic method (or inquiry by refutation) has also close relationship with their moral and political approaches. While Plato is considered the greatest enemy of open society, Socrates, knowing little or (almost) nothing at all, is "the champion of the open society, and a friend of democracy." (*ibid.*, p. 191) Vlastos, may be the most reputed mainstream commentator of Plato's since the 1950s of the last century, advances this interpretation more explicitly and systematically, but, alas, without mentioning Popper. He distinguishes between Socrates of the early dialogues ( $S_e$ ) and Socrates of the middle dialogues ( $S_m$ ), and he claims that  $S_e$ , especially in the sub-group of the *elenctic* dialogues, is exclusively a moral philosopher, while in the sub-group of the transitional dialogues he is beginning to present theories in some other areas too (Vlastos 1991, pp. 46–48).<sup>5</sup> It is only Socrates of the middle dialogues who is also a philosopher in every other area (*ibid.*, *ibid.*). Vlastos, like Popper, draws from the early dialogues, especially the elenctic dialogues, that Socrates is *demophilic*, though not strictly *democratic*, as he doesn't offer any political theory (*ibid.*, p. 18). And like Popper he would also interpret Socrates' thoughts about the soul in the early dialogues as expressing his individualistic way of life rather than having a metaphysical theory

of the soul (Popper 1945, p. 302; Vlastos 1991, pp. 54–55). But unlike Popper he doesn't deny that Socrates as a moral intellectualist *has* a comprehensive moral philosophy, not only *some* moral theories and maxims. Some commentators (like Allen 1971; Kraut 1999; Benson 2000; Prior 2004) have critically challenged the idea that Socrates can have moral theories without having any metaphysical or social theories. Prior (2004, p. 2) in his article *Socrates Metaphysician*, which is dedicated to the life work of Vlastos, claims that in the early period elenctic dialogues Socrates has a philosophy and he is theorizing in varied fields of philosophy. Moreover, in many cases he practices elenctic arguments on, and at the expense of, his interlocutors. One of the major implications to be drawn from these works is that the controversy between Popper and Burnet and between Vlastos and others, the 'old' and 'new' solutions to the Socratic problem is still a challenge.

In this article I will focus on a relatively neglected issue of Socrates' theories: his claims of knowledge (rather than his claims of opinion) in three representative early dialogues, the *Apology*, the *Protagoras* and the *Meno*.<sup>6</sup> Does Socrates' claimer of knowledge necessarily exclude Socrates' claimer of ignorance who applies the elenctic method? Does he use the elenctic method also on himself and at his own expense, and not only on others and at their expense? Or in short, did Plato invent a 'Platonic' Socrates? Drawing on Popper's dichotomy between Socrates and Plato's Socrates the answer seems to be obvious. Socrates' claims of knowledge in the early dialogues are Plato's creation. And following Popper's methodological principle (see Notes 4 and 5) it might be conjectured that one will find few marginal claims of knowledge in the *Apology* while he will find more in the *Protagoras* and the most in the *Meno*, as expected from a transitional early period dialogue. And that in the later dialogues there are less elenctic arguments. But as I will try to show such answer isn't obvious. Trying to answer these questions I will introduce in the next section Popper's account of Socrates as a friendly critique of democracy in the *OSE*. Then, in the three subsequent sections I will present and discuss Socrates as a claimer of knowledge in each of these three dialogues. And finally, I will rethink Popper's solution to the Socratic problem in the *OSE* from the perspectives of Socrates' claimer of knowledge and some insights from Popper's evolutionary epistemology (Popper 1979, 1994, 2002).

## Popper's Account of Socrates in the *OSE*

Among "the great generation", Popper says, Socrates has contributed more than any other intellectual to the new faith of the open society and even died for it (Popper, 1945, pp. 128, 189). He mentions great conservatives, like Sophocles and Thucydides, on one side, and ardent democrats, like Pericles, Protagoras, Democritus and the school of Gorgias, on the other side, and those who were wavering, like Euripides and skeptical, like Aristophanes (*ibid.*, p. 185), but he doesn't say where Socrates stands. Unlike Pericles, the most distinguished political leader of Athenian democracy and unlike Protagoras, a theorist of the open society and the first humanist philosopher,

Socrates criticized the Athenian democracy and its democratic institutions by holding the belief in the human individual as an end in itself. His criticism was friendly, not hostile (*ibid.*, p. 189), although it was abused by some of his pupils and was used against democracy (*ibid.*, pp. 191–193). For example, Socrates criticized the politicians for their lack of intellectual honesty, reminding them of their intellectual limitations, and for their obsession with power-politics (*ibid.*, pp. 190–191).

Popper rebukes those who do not distinguish between friendly and hostile criticism of democracy. And he adds: “Democrats who do not see the difference between a friendly and a hostile criticism of democracy are themselves imbued with the totalitarian spirit.” (*ibid.*, p. 189) How does Popper explain Socrates’ mixing with anti-democrats like Alcibiades, his pupil who for a time went over to the side of Sparta, and two of Plato’s uncles, Critias, who later became the ruthless leader of the 30 tyrants and Charmides who became his lieutenant? Popper raises the question and claims that if Socrates took interest in a promising youth he wasn’t deterred by their oligarchic family connections. As a moralist and individual he would take a real interest in them and he will make the difficult effort to convert them. Popper suggests that these connections were the cause of his death (*ibid.*, p. 192). Critical of the motives of the young aristocrats who used the language and teaching of Socrates against the open society, Popper suspects that Plato’s motive at the time was of a similar kind (*ibid.*, p. 194).

Socrates’ different approach to open society is emphasized by Popper:

In his defense he insisted that he had no sympathy with the policy of the Thirty, and that he actually risked his life by defying their attempt to implicate him in one of their crimes. And he reminded the jury that among his closest associates and most enthusiastic disciples there was at least one ardent democrat, Chaerephon, who fought against the Thirty. (*ibid.*, p. 193; see also *ibid.*, pp. 128–132)

Only from these reasons in favor of Socrates do we also learn that Popper is aware of the fact that Socrates stayed in Athens during the tyranny. Popper doesn’t question this fact: Why did Socrates stay in Athens during the rule of the Thirty, as he was probably forced to be silent and passive most of the period?<sup>7</sup> Although Plato, and Popper after him, elegantly evades this issue, it is reasonable to conjecture that the issue (as part of the larger issue that Popper called “the unfortunate effects”) occupied his mind and the minds of many Athenian citizens. And later on, due to Socrates’ declaration that he is going to continue his mission as the gadfly of Athens in the reestablished democracy, Popper doesn’t ask how Socrates tried to resume his teaching during that period (between 403–399 b.c.) and what were the difficulties that he encountered, within himself and in with others. Popper, so it seems, is aware of the fact that Socrates mentions those who like conversing with him and practice his method of cross-examination, among them Crito and Plato who were present in court (33e–34a), and his concern is that Socrates is going to be abused again (*ibid.*, p. 194).

There is however a prominent issue that Socrates in the *Apology* tells the audience in court and Popper doesn’t take into account: Socrates’ single and dangerous experience during former democracy (32a–32c). This case is given just before his dangerous experience of disobedience in the last days of tyranny (32c-d). He starts

with the claim that “the true fighter for justice, if he is to survive for even a short time, should remain a private individual and not go into public life.” (32a) Then he tells the jury that as a member of the council representing his tribe he was the only one to vote against the majority of the council’s resolution to put on trial the ten generals who failed to pick up the survivors from the sea battle. He thought that the majority of the council acted unconstitutionally and that he ought to take his chance on the side of the law and justice. As a consequence he was almost arrested as many politicians were set to bring an immediate action against him. Then, after the presentation of both experiences, in democracy as well as in tyranny, his rhetorical question is: “Do you think that I have survived all these years if I had taken part in political life...?” (32e). His conveyed message, I think, is that both democratic and autocratic politics are dangerous. While Popper’s protagonist of Socrates neglects the evidence of Socrates’ dangerous experience in democracy he emphasizes the evidence of Socrates’ dangerous experience in tyranny together with the friendship he had with one ardent democrat, Chaerephon, as cited in the passage above.

However, Popper doesn’t challenge the difficulties between Socrates and democratic leaders by casuistic argument, as many mainstream commentators do, according to which the leaders of the reestablished democracy and many Athenian citizens weren’t courage enough to examine their lives. The aim of the prosecutors, he suggests, wasn’t to punish Socrates for the unfortunate political events of the past but “to prevent him from continuing his teaching, which, in view of its effects, they could hardly regard otherwise than as dangerous to the state.” (*ibid.*, p. 193) Anytus, the democratic leader who backed the prosecution aimed to exile Socrates, not to make a martyr of him. Popper also says that “this plan was defeated by Socrates’ refusal to compromise his principles” and he adds: “He simply fought for what he believed to be right, and for his life work. He had never intended to undermine democracy. In fact, he had tried *to give it the faith it needed*. This has been the work of his life. It was, he felt, seriously threatened.” (*ibid.*, *ibid.*; my italics) What was the faith and how giving democracy the faith it needed by Socrates – who due to his bad experience isn’t a political activist, nor a leader (like Pericles) or a theorist of open society (like Protagoras) – is not explained. And finally, Popper also doesn’t question why Socrates didn’t mention any theoretical or practical lesson to learn, particularly concerning his former pupils some of whom he considered in the past to be promising.<sup>8</sup>

Assuming, with Popper that the *Apology* represent the most reliable historical account of Socrates, I will examine Socrates’ claims of knowledge in the *Apology* as an evidence to examine Popper’s solution to the Socratic problem.

## The Theory of the First Accusers in the *Apology*

Starting with the declaration that “there wasn’t a word of truth (*alethes*) in what they [the accusers at court] said” (17a) and adding that “whereas from me you will hear the whole truth (*alethes*) – certainly not a piece of polished rhetoric like theirs...” Socrates then puts forward his major<sup>9</sup> claim of knowledge in the *Apology*, his theory

of the first accusers. The first accusations against him, beginning in the former democratic period (before 404 b.c.), were and are more effective and dangerous in spreading prejudices than the present accusations in court because they cannot be addressed. In this respect he mentions the known comedy of Aristophanes as less dangerous, compared to the unknown anonymous instances of spreading prejudices (18c–d; 19c). Many of the jury members at court have in fact heard these lies about him since their childhood when they had no capabilities to resist them (18b).

The major prejudices are those in which he is described as rhetorician, pretending to have knowledge. Among them the most dangerous is the prejudice that he undertakes to educate people, which he can't and doesn't (19e), because he lacks the knowledge that is needed for teaching (20b–c).<sup>10</sup> The anonymous accusers spreading these prejudices are from varied social sectors in the Athenian society: writers, orators, politicians and craftsmen. When he was conversing with them, he showed them that they didn't know what they have pretended to know, and as a consequence they became his enemies. Socrates, as portrayed by Plato,<sup>11</sup> doesn't explicitly appeal to a conspiracy theory, although he emphasizes the rhetorical skills of his accusers (17a–d). At least it's not a conspiracy of the few and powerful implementing a secret plan, as both former and present accusers aren't wise.<sup>12</sup> Rather, the accusations are described as uncontrolled reactions of the many (*hoi polloi*) anonymous citizens, pretending to be political and moral experts, to his critical conversations (19d).<sup>13</sup> As already mentioned, there were also some "young people (who) follow me of their own free will" (23c) who entertained these conversations and cross-examinations, and even liked him and imitated him by cross-examining others. But he doesn't say in his defense whether some of them tried to do anything to prevent and even refute the prejudices against him.<sup>14</sup>

Former antidemocrat pupils, among them the leaders of the tyranny aren't mentioned in his speech not even as those who may have abused his life work. It isn't clear whether, when and how his associations with them continued or stopped and what was it like through different periods. But when he mentions Chaerephon, an ardent democrat associate, he emphasizes the fact that "(h)e was a friend of mine from an early age, and a friend of most of *you*. He shared *your* recent exile, and returned from exile with *you*." (21a; my italics) It is clear that Socrates doesn't belong to the camp of the ardent democrats, and as Popper (1945) suggests, as a practicing individualist he didn't belong to any other camp, not even that of the oligarchs. But it is implied from this example (and many other examples in other early dialogues) that his *beliefs* are different from the views of ardent democrats and are closer to the conservatives' beliefs. We don't know what the accusers thought, and Meletus answering Socrates' questions is in fact the extension of Socrates' own argument.

## Claiming Knowledge in the *Protagoras*

In the *Protagoras* Socrates' claims of knowledge (314a–b) are given as a part of a theory (312a–314c2), at the beginning of the dialogue. The theory comes as a guide and a warning to the enthusiastic Hippocrates, eager to meet Protagoras who came

to Athens. While walking in the yard, before going to meet Protagoras, Socrates asks Hippocrates what is he going to Protagoras for and what is Protagoras' expertise. And also: What does he expect to be after learning from Protagoras? Hippocrates doesn't know. Socrates helps him to elicit an answer by analogy of learning medicine and sculpture: What would you go to the famous Hippocrates as or to Policleitus and Pheidias as? He would go to the famous Hippocrates as a doctor and to Policleitos and Pheidias as sculptures. What then he takes Protagoras to be? The answer is a sophist. Does he wish to become a sophist? Hippocrates replied with blush that if it is like the previous cases then the answer is yes. Socrates immediately asks him whether he won't be shameful to present himself as a sophist to the Greeks. Hippocrates admits that he will. Socrates tries another possibility: Does he expect something else from Protagoras, to be educated and not to be a professional expertise, like going to the language master or the harp teacher? Responding to Hippocrates' positive answer, Socrates then acknowledges him that in such case, unlike buying bodily food, he is submitting his soul to a sophist without knowing whether the results will be good or evil. Continuing the analogy, Socrates explains that in many cases the seller of bodily food will pretend to know about the good qualities of his merchandize and sometimes even deceive the buyer only to sell his merchandize. Also, the buyer isn't always sure what the qualities of the merchandize are, but he will be cautious and will usually consult an expertise, before buying the food, and more importantly, before eating it. But then, Hippocrates, probably like many other young Athenians, is sure that he knows to whom he can entrust his soul and what the results will be (312c). The crucial issue in this theory, that Socrates is confident of, is that *what is learned (mathemata)* cannot be carried away in a separate vessel before one consumes it into his soul (313c). Therefore, as Protagoras "is a sophist; and as to what a sophist really is..." meeting and hearing what he has to say is dangerous to his soul. Socrates then suggests that in order to unravel such a serious question and before one entrusts his soul to a newly arrived foreigner, the aid of the elders is needed, including other men of wisdom, like Hippias and Prodicus (314b7–314c1). In this part of the dialogue, it seems to be clear that Hippocrates needs some guide and protection and it is expected that Socrates will be his guide and protector.

When they meet Protagoras, Socrates asks him: What would Hippocrates gain by joining your classes? Protagoras answers that he will become a better man (318a6) and by this he means that Hippocrates will become a good citizen (319a3–4). The first important disagreement between them has to do with the question whether this 'goodly accomplishment', to become a good citizen, can be taught. Unlike Protagoras, Socrates claims that it cannot be (319b1; also not the excellence of state administration; 319d4–5). Later on, their conversation shifts to some theoretical questions: What is virtue (from 329c)? What is the proper education of man (from 339a)? What is virtue relating to knowledge (from 349b)? During this conversation they have agreed on some issues, disputed over others, changed some of their views and what is more surprising is how Socrates concludes their conversation:

For I know that, were it once plain, that other question on which you and I have argued at such length on either sides – you maintaining and I denying that virtue can be taught – would be cleared up satisfactorily. Our discussion, in its present result, seems to me (as)



though it accused and mocked us like some human person; if it were given a voice it would say: "What strange creatures you are, Socrates and Protagoras! You on the one hand, after having said at first that virtue cannot be taught, are now hot in opposition to yourself, endeavoring to prove that all things are knowledge – justice, temperance, and courage – which is the best way to make virtue appear teachable: for if virtue were anything else than knowledge, as Protagoras tried to make out, obviously it would not be teachable; but if as a matter of fact it turns out to be entirely knowledge, as you urge, Socrates, I shall be surprised if it is not teachable. Protagoras, on the other hand, though at first he claimed that it was teachable, now seems as eager for the opposite, declaring that it has been found to be almost anything but knowledge, which would make it quite unteachable! (361a1–c2)

The conversation held between the two is probably the most outstanding critical debate between two philosophers in Plato's writings. It is free from stereotypes of 'Sophists', as given in the first part of the dialogue (as well as in many other dialogues). Rather, their conversation is one in which two systems of opinion are in interaction, sometimes ending with agreement, sometimes with disagreement, and what is more significant these opinions are reconstructed while trying to challenge a critical examination. And finally, they end the conversation with *Aporia*.

We don't know from the dialogue how the conversation between Socrates and Protagoras is related to the theory and guidance given by Socrates to Hippocrates in the earlier part of the dialogue. How do Socrates' claims of knowledge and his theory in the first part of the dialogue relate to the conversation in the second part? Have both their opinions been absorbed in Hippocrates' soul? Were also the changes in the opinions absorbed? And finally, what is the something learned (*mathemata*) in the context of competing opinions in flux?

## Claims of Knowledge in the *Meno*

In the *Meno* Socrates' claims of knowledge are presented in the most elaborated manner among the early period dialogues. The first and most important claim of knowledge is given as an answer to Meno's practical opening question: How is virtue acquired, whether it comes from teaching or in some other way? (71a1–2). Socrates says that he doesn't know at all (*to parapan*, 71a7) what virtue is and therefore cannot answer the question how it is acquired or whether it can be taught. To this he adds: "And if I don't know what something is, how could I know what that thing is like?" (71b3–4). In other words, only by the ascertainment of what virtue is can its qualities be truly established. On one hand, Socrates says that he *doesn't know* what virtue is which is a claim of ignorance, and on the other hand that he *knows* that only by knowing what virtue itself is can one really know what sort of thing it is. So if one is interested whether virtue is teachable, one should know first what virtue is. To illustrate his claim he asks a rhetoric question: Is it possible that if someone doesn't know Meno at all, that he should know whether he is beautiful or rich or whether he is well-born? (71b).<sup>15</sup> Therefore, in order to give an answer to Meno's practical question (how to acquire virtue?) one should first give an answer to the theoretical question (what is virtue itself?).



Socrates then proposes to inquire with Meno what virtue is by defining it, as knowing what virtue is has to do with having a perfect definition of it. After Meno's three attempts to define what virtue is Meno learned to admit ignorance (in the sense that he doesn't claim to know what virtue *itself* is) and to engage with inquiring what virtue is by learning to be a definer. Socrates' questioning following Meno's two first efforts at definition aimed to search for weaknesses and inadequacies in order to help Meno examine his definition critically and encourage him to formulate a better definition (72a–74b). So were Meno's questioning when Socrates made two efforts to define the concept of the shape before Meno's third effort at definition (75a–76b). But in his response to Meno's third and best attempt at definition Socrates doesn't try to refute the definition. Rather, he makes the effort and succeeds to persuade Meno to accept the maxim that if one really knows what virtue is he cannot do wrong willingly (77b–e). This maxim is in fact drawn from Socrates' first claim of knowledge and is related to his famous opinion that "virtue is knowledge". Towards the end of this part of the dialogue, Socrates tells Meno that no progress was obtained and he is still totally ignorant of what virtue is, as he was in the beginning of the conversation (80c–d). Based on the dichotomy between *knowledge* and *ignorance* a non-perfect definition has to be regarded as ignorance. The terms opinion and true opinion aren't introduced yet.

At this stage Meno is wondering for the first time what does Socrates mean by knowledge. Following Meno's question (How will we know what is the target of our inquiry and to identify that it is knowledge if we reach there? (80d6–10)) Socrates tries to show him that one can inquire into knowledge even when he doesn't know what it is. In this context his second claim of knowledge is presented: learning resembles recollection of knowledge that is innate in the soul in its eternity (85b–86c). He also tries to prove his claim by referring to priests and priestesses and divinely inspired poets, but more important by the illustration with the slave. Based on this illustration, he detects the process of learning that the slave has gone through, and he generalizes its conclusion into a theory of learning. Learning begins with the slave who supposes that he knows (82e; 84a). He was confident and didn't think he was perplexed, but after some questions (by Socrates) and trials (his answers) he began to think that he is perplexed and he acknowledges his ignorance. Being perplexed he then began longing for knowledge (*episteme*) (85a5–b1). At first the slave is succeeding with drawing out his *own* opinions (*doxas*), not only opinions that are inculcated in him or borrowed. Learning then proceeds to true (*alethes*) opinions (85c8). And only "(i)f some one asks him many times and in many ways about the same things as now, you may be sure that he will end up with knowing (*epistesetai*) them as precisely as anyone does." (85c10–d1) Meno, who according to Socrates is in the stage of perplexity, should follow the same process concerning what virtue is (86b2–4).<sup>16</sup> Such shift from a dichotomist account of knowledge to a gradualist account of knowledge implies a withdrawal from Socrates' first claim of knowledge. The reason for this is that according to the first claim of knowledge one cannot advance from opinion (*doxa*) to knowledge (*episteme*), as opinion, like technical knowledge (*techne*) and in general knowledge of the qualities of a thing necessarily depends

on the knowledge of the thing itself, which is taken in the dialogue as synonym with *episteme*.

After persuading Meno that knowledge exists in the eternal soul and can be reminded by recollection, Socrates then tries to show that if we don't have knowledge of virtue we cannot teach it. The third claim of knowledge determines that only if virtue is knowledge it is teachable (86d). It is also rephrased later in a rhetorical question to which Meno gives an 'of course' answer: "But if virtue *is* some sort of knowledge, clearly it *must* come from teaching?" (87c5–6; italics in the origin) Socrates' third claim can be drawn from his first one: if someone doesn't know what virtue is he couldn't know whether and how it can be taught. And those who pretend to know what virtue (itself) is also pretend to know that they can teach it. The examples of the Protagoras and Gorgias are given as teachers of virtue (91d; 95b)<sup>17</sup> and the examples of ardent distinguished democrats, like Themistocles, Aristides, Pericles and Thucydides (93a–94e) as parents who tried and failed to be teachers of their own virtues, as they also failed to find teachers of virtue to their sons. However, in all these cases they didn't fail to train their sons and find them teachers who know the arts of athletics or horsemen.

What is unique to Socrates' third claim of knowledge is that later on he *explicitly* withdraws from it, by suggesting that in the absence of knowledge *guiding* someone is possible based on right opinion (96e).<sup>18</sup> He gives Meno the example of guiding someone who doesn't know the way to Larisa. In such case, guiding based on right opinion is no less beneficial than teaching based on knowledge. In his fourth claim of knowledge, which he emphasizes that he speaks of knowledge not of what looks likely to him, Socrates asserts that he is sure that knowledge and right opinion are different kind of things (98b). He explains: "True opinions too are fine things and altogether good in their effects so long as they stay with one, but they won't willingly stay and instead run away from a person's soul, so they're not worth much until one ties them down by reasoning out the explanation. And that is recollection, Meno my friend, as we agreed earlier. And when they've been tied down, then for one thing they become items of knowledge, and for another, permanent" (98a1–6).

The hidden implication from Socrates' withdrawal from the perfect teaching-knowledge relationship to the possible guiding-right opinion relationship is that in our "cavy" world, as Weiss (2001, p. 7) puts it, it is true opinion that we should really search for in our moral inquiry and reasoning, as knowledge (*episteme*) is impossible to achieve. At the end of the dialogue, Socrates rephrases his first claim of knowledge: "But we shan't have clear knowledge about it (virtue, H.B.), until before searching for how it comes to people, we first *try* to search out what it is in itself." (100b4–6; my italics) Here I think Socrates shifts the emphasis from claims of knowledge to claims of opinion, since human trials result at best with correct or true opinions. Moreover, he is hinting at the end of the dialogue that the theoretical inquiry would focus on opinion rather than knowledge, as the later is impossible to accomplish. Therefore, before one tries to answer the question "how is virtue achieved?" one would better try to answer the question "what is virtue?" assuming that the best opinion achieved will lead the inquirer to a better understanding and guidance. But even though knowledge is a necessary ideal for Socrates, as he

believes that one should crave for knowledge as an ultimate goal or something divine, it cannot bind the inquiry as suggested in the first claim of knowledge at the beginning of the dialogue. If one doesn't know what virtue is, and no human does, he still can opine about its nature; i.e., it's most general qualities.

## Rethinking Popper's Account of Socrates in *OSE*

Is Socrates' claimer of knowledge Plato's construction and therefore Socrates is being misrepresented or even abused by his pupil? If one follows Popper's major methodological principle (see Notes 4 and 5) then the answer would probably be yes. But regarding the *Apology* such solution undermines the reliability of its historical account. Taking such central theory, as the theory of the first accusers, to be a construction of Plato, it implies that the *Apology* isn't reliable as a historical account. But if it is historically reliable, as there aren't good reasons to think otherwise, then Socrates' claims of knowledge aren't Plato's invention and the dichotomy between Socrates and Plato's Socrates dissolves.<sup>19</sup> Contrary to Popper's interpretation, from the perspective of Socrates claimer of knowledge and the evidence concerning his prominent experiences, Socrates in the *Apology* has explicit ideas about democracy and tyranny and implicit ideas about the relations between them. What is diagnosed in the *Apology* is complemented with prognoses in the middle and late dialogues, especially the explicit political theory elaborated in the *Republic* (564a). I therefore suggest that from the perspective of Socrates claimer of knowledge and the evidence added later on, the *Apology* (and the *Crito*) although written earlier as the best historical account, has a closer relationship with the middle period *Republic* (Books ii–x) and the *Laws* than with later early period elenctic or transitional dialogues.

The evidence given in the *Apology* doesn't support the theory that Socrates was a friendly critique of democracy while only his pupils were hostile critiques of, and some of them hostile activists against, democracy. Due to his most important belief, that the best way to conduct public life in a state is by moral and political *experts*, his critique of democracy is understandable. The problem of "the prejudices and malice of the many" (28b), including the "popular assembly", makes it almost impossible to advance such expertise, if it is possible at all. It is a problem that cannot be reasonably dismissed in any open democratic society. Socrates, then, was a vigorous and important critique of democracy. But he *was* hostile too. Moreover, he *had* good reasons to be hostile. After all he believed that prejudices are spreading in the democratic life by many citizens and leaders who pretend to have political and moral knowledge which they don't really have. And if the promotion of political and moral experts is partially possible, one may better search for the few promising young men, as Popper rightly suggests. In this respect, Socrates' view of the superiority of the best 'few' is closely connected with his second claim of knowledge (as mentioned in note 9), that those who are superior deserve the obedience of the inferior, never otherwise. Popper, therefore, is wrong to assume that Socrates'

important and vigorous criticism of democracy (may be radical democracy) is necessarily involved with a friendly approach. One may also add that those promising best few, like Alcibiades, perhaps betrayed and abused Socrates, but by being able to do so they also refuted his theory. It isn't possible to predict who will be the few best promising leaders and accordingly to know who is superior or inferior and who should obey whom.

The reminded portrayal of Socrates in court who for many years had practiced the eclectic method is probably a reliable historical account. However, it is in the *Protagoras* and the *Meno*, among the dialogues analyzed in this article, that he is portrayed *exercising* it, not only mentioning it; moreover, exercising it, explicitly or implicitly, on his own ideas not only on others. Both dialogues, although probably containing more literal and less historical elements than the *Apology*, refer to earlier events in his life and in the history of Athens. In these dialogues his claims of knowledge are doubted, indirectly and implicitly in the *Protagoras* and perhaps in the most elaborated and explicit manner in the *Meno*. His implicit and explicit withdrawals of his claims of knowledge cannot be explained by Popper's dichotomist interpretation in the *OSE*, and I suggest that it would better be interpreted by Popper's later evolutionary epistemology (Popper 1979, 1994, 2002).<sup>20</sup>

In the *Protagoras* there are two philosophical problems and three practical problems.<sup>21</sup> The first theoretical problem, "How is the soul nourished?" is answered by Socrates' metaphysical theory according to which the soul is nourished by knowledge and that what is learned (*mathemata*) cannot be carried in a separate vessel. There are also two practical problems in his conversation with Hippocrates: What is Hippocrates expecting to learn from Protagoras? How to protect Hippocrates' 'soul'? The first practical problem is also addressed to Protagoras: What is he intending to teach Hippocrates? Later on also the theoretical question of "What is virtue?" in more than one formulation is discussed. But while in the first conversation with Hippocrates Socrates' trial of theory (or his metaphysical theory) is comprised, among other opinions, of the claim of knowledge according to which what is learned is necessarily absorbed into the soul, in the second conversation with Protagoras' two competing theories, presented *only* as claims of opinion, which are two different attempted solutions to the theoretical problem of virtue, are introduced. Both Socrates' and Protagoras' theories were critically examined and later on reformulated. However, Socrates' claims of knowledge in the *Protagoras* were not examined in an explicit manner, as they weren't mentioned later on in the dialogue. But I think that following the conversation between Socrates and Protagoras, Socrates' claims of knowledge are indirectly and implicitly doubted and they are opened to a potential examination. For example, how is the soul fed when there is no knowledge to impart? A possible answer that isn't given explicitly, but nonetheless implicit in the conversation with Protagoras and in Socrates' epilogue, is that the psychological soul is fed with opinions. And opinions, unlike knowledge, aren't necessarily absorbed when they are learned and therefore they *can* be carried by the learner in a separate vessel, if they aren't forgotten.

In the *Meno*, in response to Meno's practical question ("Does virtue come from teaching?") Socrates raises a theoretical question ("What is virtue?") which he

cannot answer, as he doesn't know at all what virtue is. But, what he is sure of at the beginning of the conversation is his first and major conviction that without having knowledge of what virtue is, Meno's practical question cannot really be answered. However, following Meno's paradox Socrates shifted from a dichotomist to a gradual account of knowledge. And this first error elimination, which Meno is unaware of leads in the end to rephrase his first and major claim in terms of opinion and as an opinion, assuming that knowledge (*episteme*) is only an ideal or a divine dispensation. Therefore, the final answer to Meno's practical question is that virtue cannot be taught (in Socrates' sense), probably only be guided. I think that what is significant to the *Meno* is that although it portrays Socrates' with the most elaborated system of claims of knowledge among the early dialogues, it is also the most elaborated elenctic dialogue. Additional to the practice of the elenctic method on Meno's two first efforts to define the concept of virtue, on Socrates two efforts to define the concept of shape and the slave's process of learning geometry, Socrates contrary to Prior's argument (see p. 307) also practices it on himself and at his own expense.

## Conclusions

In this article, I have re-examined Popper's answer to the 'Socratic problem' in the *OSE* by examining three early dialogues in which Socrates is a claimer of knowledge. Contrary to Popper's dichotomist solution, I have emphasized that Socrates *is* having metaphysical and social theories by focussing on those that he claims to know. I have suggested that some of them are elaborated by Plato in the middle and late dialogues, his theory of the soul for example. From the perspective of Socrates claimer of knowledge the best account of Socrates' elenctic method would be found in those early dialogues in which his claims of knowledge are critically challenged, both explicitly and implicitly. While in the *Apology* they aren't in the *Protagoras* and the *Meno* they are. My analysis rejects Popper's solution to the Socratic problem in the *OSE* and corroborates some of Burnet's solution by appealing to insights of Popper's later evolutionary epistemology. In this respect, Plato didn't create a Platonist Socrates. Rather, he has documented and then systematically developed his master's tentative original ideas.

## Notes

1. Popper also adds: "Socrates' saying 'care for your souls' is largely an appeal for *intellectual* honesty, just as the saying 'know thyself' is used by him to remind us of our intellectual limitations." (*ibid.*, p. 190; italics in the origin).
2. According to Popper, in Plato's conception the soul exists by 'nature' and man's 'nature' is the same as his 'soul' (*ibid.*, pp. 75 and 78). Assuming a clear distinction between Socrates and Plato's Socrates Popper draws the conclusion that the idea of the soul as explained by Socrates in the *Meno*, that "the soul of man is immortal" (81b), is Plato's.

3. Compare with Kraut (1999, pp. 35–38) who tries to explain “how Socrates can both be a philosophical maverick and a citizen who feels obliged to respect his city’s legal system”? What then did he have in mind when he praises Sparta and the cities of Crete (in 52e)? According to Kraut it is the value of conventional moral training that Socrates must have in mind. In an advanced stage of moral development he will examine the traditional beliefs that were inculcated in him and attempt to see the defects of that first stage (*ibid.*, p. 42). See also Socrates’ praise of Spartans and Cretes as lovers of wisdom in the *Protagoras* (342a–343c) which Popper doesn’t mention. Vlastos (1991, p. 136) suggests that it is “irony put to a very special use: mockery elaborately played out in sly concealment of its mocking intent.”
4. Assuming that Xenophon’s account of Socrates is independent of Plato’s writings, Popper, following the traditional solution to the Socratic problem, suggests the following principle: “the similarity between the Xenophontic Socrates and the Socrates of the ‘Socratic’ group of dialogues, and the dissimilarities between the Xenophontic Socrates and the ‘Socrates’ of the Platonic group of dialogues.” (*ibid.* 306) Therefore, “(t)he metaphysical theory of Forms or Ideas, more especially was usually considered Platonic” (*ibid. ibid.*). Burnet rejects these assumptions and claims that Xenophon knew some of Plato’s dialogues before writing the *Memorabilia*. However, although Popper thinks that Burnet’s views are untenable he nevertheless takes them to be valuable and stimulating. And he explains: “A bold theory of this kind, even if it is false, always means progress;” (*ibid.* 307).
5. The sub-group of the elenctic dialogues includes among others, the *Apology*, the *Crito*, and the *Protagoras*. The transitional dialogues include among others, the *Gorgias* and the *Meno* (Vlastos 1991, pp. 46–47). While for Popper the *Apology* is in the main ‘Socratic’ the *Laws* is in the main ‘Platonic’. Accordingly, at least with the most important dialogues, Popper adopts a methodological principle by which the order of the dialogues is determined by tendencies to similarity with the Socratic *Apology* that decreases and that with the Platonic *Laws* that increases (1945, p. 310). Therefore he suggests that the *Euthyphro* comes after the *Crito* and the *Apology* and together with them is part of the core historical dialogues; and the *Protagoras* precedes the *Meno* and the *Meno* precedes the *Gorgias*, which for Popper is probably the latest early period dialogue (*ibid.*, pp. 208 and 310; compare with Vlastos 1991, pp. 46–47). See also Brandwood (1990) and Penner (1994) for contemporary accounts of the chronological order of the dialogues.
6. As a claimer of knowledge, unlike a claimer of opinion, Socrates might declare that he is telling “the whole truth”, as he does in the *Apology* (17c); use rhetorical questions which imply what he is sure of, at least during parts of the conversation, as he does for example in the *Protagoras* (313a) and the *Meno* (72b; 89a) and he might simply say that he speaks out of knowledge as he does both in the *Apology* (29b) and the *Meno* (98b).
7. The case of Socrates’ disobedience happened in the last days of the tyranny, as he explains: “I might perhaps have been put to death for that (his refusal to participate in fetching Leon as ordered by the oligarchs), if their power had not been brought to an end soon after.” (32d)
8. Popper however mentions one mistake of Socrates: “He was mistaken when he considered himself a politician; he was a teacher.” (*ibid.*, 191) This is an interesting remark. By teacher Popper doesn’t mean that Socrates was imparting knowledge, only that he practiced his elenctic method and conversations through cross-examinations with his pupils. But did Socrates consider himself to be a politician? I think the above evidence (p. 308–309) based on the *Apology* (32a–e), which Popper doesn’t mention (except Socrates’ disobedience), shows that Socrates didn’t. He tried, according to this testimony, to keep away from politics and public life, in democracy as well as in autocracy.
9. Another significant claim of knowledge in the *Apology* is given in 29b: “As for me, gentlemen, perhaps here too – in this one particular – I am different from most people. And if I did claim to be in any way wiser than anyone else, it would be in this, lacking any certain knowledge of what happens after death, I am also aware that I have no knowledge. But that it is evil and shameful to do wrong, and disobey one’s superiors, divine or human, that I do know.” (Italics in the origin).



10. The assumption that in order to educate (and I may also add, to practice politics, both as a leader and as an active citizen) one has to have knowledge is claimed and discussed by Socrates in many other dialogues, including the *Protagoras* and the *Meno*. And as Popper, and many other commentators try to show, Socrates' teaching as a form of midwifery, as illustrated in the *Meno* for example, is still teaching, although a different kind of teaching (1945, p. 129).
11. In Xenophon's version of the *Apology* Socrates doesn't introduce a theory of the first accusers.
12. See Socrates' claim in the *Hippias Minor* that only the good and the expert can do bad and false things by their will (375a–376b). Compare with his conviction in the *Meno* that one cannot do wrong knowingly and therefore willingly (77c–78d).
13. In the *Crito* Socrates rejects the morality based on what “the many” (48c) or “most people” (49b) think and do, but he says that it is his duty to obey “the Athenians” (48b; 48e) and the laws of the city (50b–52c) according to which “the Athenians” have convicted him. Even if the multitude or majority is wrong by misusing the laws, the laws themselves, personified as stable and a-historical manner, aren't.
14. It is in this context that “the standard accusations made against all philosophers” (23d) is mentioned. Here, I think, Socrates perceives himself as one of the philosophers and he hints on well known accusations made against philosophers, such as Anaxagoras and Protagoras, who were also blamed for impiety. This may be the reason why in the *Apology* Socrates doesn't include the philosophers as spreading prejudices against him, together with the politicians, writers, orators and craftsmen.
15. On some problems with this analogy see my discussion (Baruch 2004).
16. I think that Meno was in the stage of perplexity at the beginning of the dialogue. And while in his second trial at definition Meno repeats ideas of Gorgias, it isn't clear whether in his third attempt he merely repeats the idea of the poet or uses the poem to bring his own ideas. Another question concerning the analogy has to do with the goal of knowledge. In Meno's case it is moral knowledge of virtue and in the slave's case it is knowledge of a geometrical principle. Is the knowledge of what virtue is analogous to the knowledge of the principle that the slave had acquired? I think it isn't. The knowledge of what virtue is would better be compared with the knowledge of what shape is, for example.
17. Meno denies that Gorgias professes to teach virtue (95c).
18. One may also mention that Socrates *explicitly* doubts his own opinion that virtue is knowledge (87c8–10). But as with his conversation with Protagoras it is less surprising that he may doubt his opinion. However, while in the *Protagoras* he challenges his opinions and introduces new trials, in the *Meno* he doesn't go further than his doubts.
19. Also his praise of the Spartan laws in the *Crito* should be regarded as a historically reliable account and it cannot be simply dismissed as a later editing, as Popper suggests.
20. According to Popper an inquiry begins with a problem (P1). Then tentative solutions are attempted (TS) and then through a critical discussion there are attempts at error elimination (EE). And finally the inquiry ends with a new problem (P2) (Popper 1979, pp. 241–244; 2002, pp. 4–15). On the distinction between a philosophical problem and a scientific problem, particularly focusing on the possibility of refutability, see Popper (1963, pp. 193–200). In this respect I think that what Popper calls a “philosophical theory” (*ibid.*, 193) is introduced in Socrates' claims of knowledge in the *Protagoras* and the *Meno*. His claims of knowledge in the *Apology* are sociological and historical, and therefore scientific. On Popper's distinction between a scientific and a practical problem see Popper (1994, p. 11).
21. On the distinction between a philosophical problem and a scientific problem, particularly focusing on the possibility of refutability, see Popper (1963, pp. 193–200). In this respect I think that what Popper calls a “philosophical theory” (*ibid.* p. 193) is introduced in Socrates' claims of knowledge in the *Protagoras* and the *Meno*. His claims of knowledge in the *Apology* are sociological and historical, and therefore scientific. On Popper's distinction between a scientific problem and a practical problem see Popper (1994, p. 11).



## References

- Allen, Reginald. 1971. Plato's Earlier Theory of Forms. In *The Philosophy of Socrates*, ed. Gregory Vlastos, pp. 319–334. New York: Garden City.
- Baruch, Herzl. 2004. Knowing and Knowledge in the *Meno*. Delivered at *Contemporary Issues in Epistemology*, Soochou University (Tai Pe) (11–13 June).
- Benson, Hugh. 2000. *Socratic Wisdom: The Model of Knowledge in Plato's Early Dialogues*. Oxford: Oxford University Press.
- Brandwood, Leonard. 1990. *The Chronology of Plato's Dialogues*. Cambridge: Cambridge University Press.
- Kraut, R. 1999. Socrates and Democracy. In *Plato 2 – Ethics, Politics, Religion and the Soul*, ed. Gail Fine, pp. 34–53. Oxford: Oxford University Press. (A former version, 1985, under the same title see in: *Popper and the Human Sciences*, eds. Gregory Currie and Alan Musgrave, pp. 85–203. The Hague: Martinus Nijhoff Publications.)
- Penner, Terry. 1992. Socrates and the Early Dialogues. In *The Cambridge Companion to Plato*, ed. Richard Kraut, pp. 121–169. Cambridge: Cambridge University Press.
- Plato. 1997. *Complete Works*, ed. John Cooper. Indianapolis, IN: Hackett.
- Popper, Karl Raimund. 1945. *The Open Society and Its Enemies*, Vol. 1. London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1963. *Conjectures and Refutations*. London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1979 (first edition 1972). *Objective Knowledge*. Oxford: Oxford University Press.
- Popper, Karl Raimund. 1994. *Knowledge and the Body–Mind Problem*. London: Routledge.
- Popper, Karl Raimund. 2002 (first edition 1999). *All Life Is Problem Solving*. London: Routledge.
- Prior, William. 2004. Socrates Metaphysician. *Oxford Studies of Ancient Philosophy* xxvii: 1–14.
- Vlastos, Gregory. 1991. *Socrates: Ironist and Moral Philosopher*. Cambridge: Cambridge University Press.
- Weiss, Roslyn. 2001. *Virtue in the Cave: Moral Inquiry in Plato's Meno*. Oxford: Oxford University Press.
- Xenophon. 1996. *The Shorter Socratic Writings: "Apology of Socrates to the Jury", "Oeconomicus" and "Symposium"*, trans. and ed. Robert Bartlett. Ithaca, NY: Cornell University Press.

# The Moral Underpinnings of Popper's Philosophy

Noretta Koertge

**Abstract** Popper's writings in both philosophy of science and political philosophy are sprinkled with exhortations: "Severely test your hypotheses!" or "Change society through piecemeal engineering!" But his famous books do not present a systematic moral philosophy. However, in the collections of talks and occasional papers published posthumously Popper speaks more openly about the ethical foundations of his philosophy. Perhaps, it is not surprising to find that he says: "The idea of truth as the fundamental regulative principle ... can be regarded as an ethical principle." But he also elaborates on less obvious moral virtues such as modesty and optimism.

This paper comments on the relevance of Popper's moral philosophy for our contemporary attempts to provide a normative sociology of science.

## Personal Introduction

According to Popper's account of observation, what we take away from an encounter depends in part on our state of mind as we approach it. Perhaps, then, it is not surprising that when I first encountered Popper's philosophy of science I took little cognizance of its moral underpinnings. My postgraduate work in history and philosophy of science began in the 1960s with Heinz Post at Chelsea College (London). The *Logic of Scientific Discovery* was a required text and we were also urged to attend Popper's lectures at the London School of Economics. The high point of the week, of course, was the famous 'Popper Seminar' in which, both students and visitors offered up papers for critical discussion.

Wellmer's *Methodologie als Erkenntnistheorie* (1968) had not been published yet, but his title nicely sums up the complex of topics that we postgraduate students had discussed: Did Popper's account of scientific inquiry as a sequence of conjectures and refutations really give us an adequate account of scientific knowledge? What was the status of basic statements? Did his theory of corroboration capture the key considerations that enter into the empirical evaluation of scientific hypotheses?

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And later, there was a host of logical and philosophical puzzles surrounding the concept of *verisimilitude*.

If you had asked me at that time about Popper's moral philosophy and its relationship to his philosophy of natural science, I would have not had much to report. Yes, Popper would sometimes exhort scientists to test their hypotheses severely, but that could have easily been interrupted as a bit of instrumentalistic advice: if you want highly corroborated theories, then you have to subject them to severe tests. He also frequently stressed the importance of expressing oneself clearly (like Bertrand Russell), but that could also be viewed as an instrumental virtue – clarity helps criticism – that had been reinforced by his antipathy for a lot of traditional German philosophy.

A more promising place to look for moral content, it might have seemed to me then, would have been in his writings on political philosophy. But, although there are parallels between Popper's endorsement of a government in which dissatisfied citizens can vote out their representatives in a peaceful fashion and his recommendation that theories be refuted by congeries of contrary experimental data, he is definitely not calling for 'Revolution in Permanence' within the political sphere. Quite the contrary, the recommendation of piecemeal engineering, which seems at odds with the doctrine of severe testing, is based on the argument that social changes should be adopted a bit at a time because it is easier to track the effect of new policies and reverse them if they have harmful unintended consequences. Popper's defence of democracy certainly has moral implications, but it would have appeared to me that the connections to his account of scientific inquiry were rather tenuous.

I now view the matter quite differently and the rest of this essay will spell out in detail my present interpretation of the moral concomitants that underlie all of Popper's philosophical works. There are two major factors that triggered this new perspective. First of all, at the end of Popper's career he published three volumes of essays that contain English translations of a number of talks that he gave in Germany and Austria (1994a, 1, 1999a). Here, he makes some of the moral commitments already present in his earlier writings more explicit. Secondly, because of my concerns about the so-called Science Wars and current postmodernist attacks on science (cf. Koertge 1998), I am now better able to appreciate his remarks on the moral responsibilities of intellectuals. While it is of course heartening in the struggle against obscurantism to find as eloquent an ally as Popper, of more enduring interest, perhaps, is the insight that a delineation of Popper's moral precepts provides insight into his writings on other topics.

## Popper's Credo and Commandments

In the *Open Society* Popper wrote in italics two lines that he later described as his moral credo: "*I may be wrong and you may be right, and by an effort, we may get nearer to the truth*" (Popper 1994m, p. xii). He had hoped that this motto would

help people interpret his theses in the *Open Society* not as dogma, but as an appeal to reason. In the Introduction to *Myth*, he elaborates what he describes as his 'confession of faith': "[A] faith in peace, in humanity, in tolerance, in modesty, in trying to learn from one's own mistakes; and in the possibilities of critical discussion" (Popper 1994m, p. xiii).

In an essay in which he criticizes what he calls 'the myth of the framework,' by which he means the doctrine that dialogue is fruitless unless the participants share basic assumptions, Popper argues that the idea that a shared intellectual framework is required is not only mistaken but also dangerous, for it may lead people to believe that if they do not agree on basics, then their only recourse is violence. However, he also emphasizes that there are *attitudes* which may well 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 somebody else" (Popper 1994p, p. 35).

This blending of empirical and moral critiques is quite common in Popper's less formal essays and speeches. In this same article, he shows through historical examples that fruitful discussions have in fact taken place between proponents of what Kuhn might call 'incommensurable' frameworks. He then draws the moral implications of failing to recognize the possibility of dialogue even in cases of extreme divergence of views. By implication, he urges other people to adopt the attitude spelled out in his *credo*.

In other places, Popper's moral exhortations are more emphatic. For example, on the occasion of being awarded an honorary doctorate at the University of Frankfurt, he first endorses the conception of fallible knowledge put forward by Socrates in Plato's *Apologia*, and then lists duties of intellectuals:

[K]nowledge is guesswork disciplined by rational criticism. This turns the struggle against dogmatic thinking into a duty. It also makes the utmost intellectual modesty a duty. And above all, it makes a duty of the cultivation of a simple and unpretentious language: the duty of every intellectual. (Popper 1994g, p. 40)

Once we recognize the moral dimensions of critical rationalism, it becomes easier to understand why the discussions between Popper and his opponents or critics sometimes became so heated. As Popper remarks, "an epistemological contrast ... can result in contrasting ethico-political objectives and requirements" (Popper 1994g, p. 33). For example, the doctrine that *the truth is manifest* may be taken to imply that anyone who disagrees with us is biased or prejudiced, a position that poisons political discussion (Popper 1994h, p. 152). More is at stake when we do epistemology than how we are to understand the growth of science!

## Piecing Together Popper's Moral Framework

Popper rarely provides a systematic discussion of his moral precepts. However, he frequently makes remarks about the moral or ethical significance of various intellectual practices and sometimes draws connections between his epistemology and the

moral recommendations that he proposes. Below is my attempt to outline Popper's moral philosophy. (This essay was in its final stages before I learned of Niemann's tremendously useful *Lexikon des Kritischen Rationalismus* (2004). Various articles therein, especially the ones on Popper's conception of intellectual virtue and vice, provide additional valuable examples and alternative perspectives.)

## *Primary Values*

The fundamental value that Popper invokes most frequently is personal freedom in the spirit of Kant (1999h) coupled with the project of *self-emancipation through knowledge* (1994d). His critical rationalist epistemology thus immediately takes on a deep moral significance – it provides the methodology by which we can carry out self-emancipation: “[E]thical principles form the basis of science. The idea of truth as the fundamental regulative principle ... can be regarded as an ethical principle” (Popper 1994j, p. 199). A despotic government is evil on two counts: it not only limits each individual's freedom of action, but also interferes with the communal discussions in which we learn by criticizing each other's solutions to problems. Thus, there are epistemic as well as political reasons for promoting tolerance. Popper freely translates Voltaire's formulation:

Toleration is the necessary consequence of realizing our human fallibility: to err is human, and we do it all the time. *So let us pardon each other's follies.* This is the first principle of natural right. (Popper 1994j, p. 190)

Another core value is the minimizing of suffering. (Popper claims this to be a much more important and practical goal than maximizing happiness.) Here technology, which has been developed using the methods and resources of science, becomes a crucial tool for carrying out this moral project. To the extent that they are anti-science, Popper sees various ‘Green’ political groups as foolishly attempting to negate our best resource for saving the environment (Popper 1999c, p. 100). He calls for reasoned and critical discussion of proposed policies to relieve suffering; often it is science that will provide key insights both into the problems and their possible remedies.

Popper opines that if objective truth is perhaps the greatest value there is, cruelty is the greatest evil (Popper 1994f, p. 5). Thus the imperative of avoiding cruelty, both physical and mental, is a prime responsibility. He mentions as examples bringing into the world unwanted children who often face a life of abuse and children infected with the AIDS virus (Popper 1999a, p. 138). Some cruelty is caused by thoughtlessness and stupidity. Popper, unlike Bertrand Russell, thinks that, in general, people's bad conduct is a result of stupidity, not a deficient moral sense (Popper 1994k, p. 213, 1999b, p. 111). This provides the link between his epistemology and the moral project of avoiding cruelty. As in the projects of avoiding suffering and striving for self-emancipation, a better understanding of how the physical and social world works gives us the means to improve the world.

## *Epistemic Virtues*

Because of the central position of epistemology in Popper's moral philosophy, behaviours that interfere with or enhance a community's ability to engage in efficient problem-solving take on moral significance. Writing in a pompous, convoluted, obscure style is seen as more than just annoying. It impedes the communal search for knowledge and may lead impressionable individuals to adopt a position without really understanding it. A simple, clear style for Popper is more than a matter of etiquette; it is a basic obligation of an intellectual: "[I]t is the duty of every intellectual to write as simply and clearly as he can, and in as civilized a manner as he can" (Popper 1994e, p. 185). "[T]he style of big and obscure words, of words bombastic and incomprehensible ... is intellectually irresponsible. It destroys healthy common sense; it destroys reason" (Popper 1994j, p. 191).

Popper frequently extols the virtue of intellectual modesty (1994n, 1999d). Since all knowledge claims are fallible, it behooves us to adopt a moderate stance towards the proposals we make. There is no place for dogmatism in Popper's epistemology. But, he also has in mind diffidence with regard to our own cleverness. At one point, he remarks that geniuses with unique ideas may play an important role in art, but not in science or philosophy. (Nevertheless, he speaks with great admiration of Kepler and Einstein, Socrates and Kant.) If intellectuals were more modest, they would be less able to exert undue influence upon their students or followers:

[U]nfortunately, it is all too common among intellectuals to want to impress others and, as Schopenhauer put it, not to teach but to captivate. ... [T]he true Enlightenment thinker, the true rationalist, never wants to talk anyone into anything ... He seeks not to convince but to arouse—to challenge others to form free opinions. (Popper 1999i, 1999h p. 85)

It is somewhat surprising to find that Popper also places a high premium on optimism and hope. He even says that it is a *duty* to avoid thoroughgoing pessimism (Popper 1999j, p. 125, 1994k, p. 213). As we will see in more detail below in the discussion of his views on cynicism and historicism, Popper strongly believes that the future is not fixed by the past, but is radically open to human interventions. By maintaining a hopeful attitude, we are better able to undertake action today that is intended to bring about a better world tomorrow:

The future is open. ... When I say "It is our duty to remain optimists", this includes not only the openness of the future but also that which all of us can contribute to it by everything we do: we are all responsible for what the future holds in store. Thus it is our duty, not to prophesy evil but, rather, to fight for a better world. (Popper 1994m, p. xiii)

## *Dangerous Ideologies*

Popper identifies several doctrines that he holds to be not only false, but also dangerous because they lead to seriously immoral consequences. The first,

relativism, grows out of a confusion between the notions of truth and certainty. He calls relativism, the idea that opposing views can each be right within their own framework, a “betrayal of reason and of humanity” and a crime (Popper 1994f, p. 5). To drop the goal of objective truth can “open the way to evil things, such as a propaganda of lies inciting men to hatred” (Popper 1994f, p. 5). Thus, he faults this philosophical doctrine not only for its deleterious effect on our quest for knowledge, but also for its tendency to increase suffering: “[T]he thesis of relativism leads to anarchy, to unlawfulness; and to the rule of violence” (Popper 1994j, p. 191).

Popper produced book-length critiques of historicism, the doctrine that there are laws of history that allow us to predict the future, which then enjoins people to work towards the inevitable next stage of social development. Popper shows that historicism is false. In addition to the fact that past history does not conform to any of the laws proposed by people such as Comte and Marx, Popper makes a logical argument: History is influenced by ideas, but no system today can predict what radically new ideas humans may come up with in the future. Of special interest to us, here is Popper’s moral indictment of historicism. By pretending its prophecies have scientific status, it places constraints on the options open to its adherents in our communal search for a better world.

A related concern is what Popper calls ‘utopian planning,’ the attempt to lay down detailed rules for a perfect society. Utopians may even rejoice when things go badly because they hope that current hardships will motivate people to work for the revolution that will supposedly solve all social problems. Popper recognizes that many utopians have good intentions, but faults them for their misunderstanding of the fact that our world is unpredictable because of its openness to new ideas. Especially reprehensible is the practice of encouraging people to sacrifice their lives for a utopian ideal (Popper 1994f, p. 28).

Cynicism also comes in for scrutiny. Popper admits that there is some plausibility to the doctrine that human actions are motivated by greed, such as, for example, the ruthless quest for gold and oil (Popper 1999b, p. 105). But he criticizes this theory severely, first by citing examples where there has been moral progress. Not only does technology provide us with the means to get rid of hunger and abject poverty, people are now more willing to move in that direction. And although atomic weapons are an enormous threat to civilization, Popper believes that the creation of peace-keeping organizations, such as the League of Nations and the United Nations, is an indication that people now endorse the idea of world peace in a much more immediate way than ever before. For these reasons, Popper describes it a *duty* to maintain hope instead of falling prey to the purveyors of cynicism and nihilism, which would destroy our motivation to work for a better world (Popper 1999f, p. 149). Furthermore, he argues that if one assumes that a man’s opinions are always determined by his self-interest (and if we apply this principle to ourselves), it makes rational discussion impossible and the breakdown of even the possibility of dialogue leads to disaster (Popper 1994j, p. 181).



## An Ethical Code for Intellectuals

Most of Popper's ethical counsel lies in scattered remarks and the occasional paragraph. But, there are two places where he writes at greater length and more formally about the responsibilities of professionals. I begin with his call for a new professional ethics, which was formulated in a lecture entitled 'Toleration and Intellectual Responsibility' (Popper 1994j). Popper points out that traditional codes assume that professionals can have infallible knowledge in their disciplinary area. To be an expert is taken to mean that one simply should not make mistakes. As a consequence, there is a temptation not to admit one's own mistakes nor to point out errors made by colleagues. As Popper puts it, the old professional ethics "leads (especially in medicine and in politics) to the covering up of mistakes for the sake of protecting authority: mistakes will be swept under the carpet" (Popper 1994j, p. 200).

A non-justificationist code of ethics would be much less rigid. Once we recognize that it is impossible to avoid all mistakes and that we learn from our own and each other's mistakes, then our automatic condemnation of error will become more discriminating. Of course, Popper emphasizes, it is our duty to avoid unnecessary mistakes. It requires integrity and courage to simultaneously try our best to avoid error and yet freely admit it when it occurs. Popper also remarks on the limits of even fallible expertise. These days, it is not practically feasible to be informed about every aspect of even a circumscribed special field of research.

I would add to Popper's proposal for a new professional ethics that how cautious we are in avoiding error will depend on whether we are working in theoretical physics or in medical practice. As Popper says elsewhere, when only ideas are at stake, our best strategy is to make mistakes as rapidly as possible, thus his call for severe testing, namely, the conducting of experiments in the domains where we anticipate the theory is most likely to fail. Clearly that would not be a good advice in instances where human safety is involved. One might well say that it is in the areas of applied science and practical action that Popper's response to the problem of induction is itself severely tested! If Popper's new code were to be widely adopted, it would appear that the legal prosecution of malpractice suits would also need to be modified.

However, an important additional component that mitigates some of the concern expressed above about an ethics that may appear too 'soft' on mistakes is presented in an earlier essay called 'The Moral Responsibility of the Scientist' (Popper 1994o). Here, Popper says that formerly scientists had a special responsibility to seek for the truth, but in general had little occasion to worry about how their discoveries would be applied. However, he says, "[t]his happy situation belongs to the past. Today not only all pure science may become applied science, but even all pure scholarship" (Popper 1994o, p. 121). Scientists have now become inextricably involved in the application of science. Because they are privy to relevant information, they should take on the additional responsibility to try to foresee any dangerous unintended consequences of their work and work to counteract them. He sums up this position with the phrase *sagesse oblige* (Popper 1994o, p. 128).

Popper gives examples of special obligations deriving from special knowledge. Some are commonplace – foreseeing difficulties in disposing of atomic waste or problems concerning population increases and the consumption of natural resources. He also emphasizes the obligations of social scientists to draw our attention to less visible developments that may endanger freedom directly, such as tools for mass-manipulation.

Placed in the context of his other statements about the duties of intellectuals, Popper's codes offer comprehensive guidance for scientists and academics in general. Many elements, such as the responsibility to pursue truth and avoid harm through rational decision making, are familiar – at least they would have been before the current rise of postmodernism! But, the details of Popper's epistemology introduce novel perspectives, such as his call for a thorough-going recognition of fallibilism in professional life and the way his theory of the openness of the physical and psychological world to the influence of new ideas entails the duty to maintain a hopeful attitude.

## Biographical Perspectives

Popper uses stories from his own life to illustrate parts of his moral theory. Interesting in their own right, they also exhibit the passion with which Popper approached questions of intellectual morality. Let us begin with the episode in which Viennese police killed young Marxist demonstrators shortly after the end of World War I. In *Conjectures and Refutations*, Popper mentions that this tragedy started him thinking about the status of Marxism and whether it really was a scientific theory as claimed. The contrast between Marxism and psychoanalytic theory, on the one hand, and Einstein's theory of relativity on the other led him to the problem of demarcation, which he "really fell in love with" (Popper 1999e, p. 160).

It is only later that we learn about the close personal connections between Popper and the demonstrators (see Popper 1999b and j). Out of school at the time, Popper was working as an errand boy for a local Marxist group that had sponsored the 'young comrades' who were gunned down. He describes how the anger he felt at the police was mixed with his personal guilt because his participation had, in a way, encouraged others to be swayed by the Marxist program (Popper 1999e, p. 133). In lectures given late in life (1991 and 1992), Popper paints a vivid picture of how he found his intellectual honesty being eroded by the powerful ideology of the group:

I was from the beginning somewhat sceptical about the paradise resulting from the revolution ... [and] I felt worried about the Party's obvious intention to arouse in its followers what seemed to me murderous instincts against the class enemy. I was told this was necessary, and in any case not meant quite so seriously, and that in a revolution only victory was important, since more workers were killed every day under capitalism than would be killed during the whole revolution. I grudgingly accepted that, but I felt I was paying heavily in terms of moral decency. (Popper 1999i, p. 133)

He describes the deceitful leadership and how their historicist claims about the inevitability of the forthcoming revolution for a time overrode his scepticism:

When a youngster is taken in by the proof of the historical necessity of socialism, he feels a deep moral obligation to offer his help—even if he sees, as I did, that the communists often lie and employ morally reprehensible means. For if socialism *must* come about, it is obviously *criminal* to fight its coming. Indeed it is everyone's duty to further the coming of socialism, so that what must come will encounter as little resistance as possible. Since you are not strong enough as an individual, you have to go with the movement, with the Party, and give it your loyal support, even if this means you support or at least swallow things you find morally repulsive. *This is a mechanism that must lead to personal depravity.* (Popper 1999b, p. 107, my italics)

Popper broke loose from what he calls 'the Marxist trap' after only 8 weeks, around the time of his 17th birthday (Popper 1999b, p. 108). But we can detect echoes of this early experience throughout his philosophical corpus: his work on the demarcation problem, his critique of historicism, his belief in the importance of ideas and our responsibility to adopt a critical attitude towards them, his fervent individualism and his repeated calls for modesty in the pronouncements of intellectual leaders.

The second biographical theme that I wish to mention is more difficult to recount, partly because it is much less clear what was going on and partly because it presents Popper in a less sympathetic light. I am referring to his public repudiations of German philosophy. As Popper explained on the occasion of receiving the Kyoto Prize, he did not set out to become a professional philosopher (Popper 1999e). His formal training centred on science pedagogy and he wrote his dissertation in psychology with Karl Buehler (whose ideas about the various functions of language are frequently cited by Popper). Yet as we have seen above, Popper was deeply interested in philosophical questions from an early age. Given the fact that he was largely self-taught, it is perhaps not too surprising that Popper seems to have had some difficulty gaining recognition from professional philosophers. He remarks more than once on the fact that he was not invited to attend meetings of the Vienna Circle (cf. Popper 1994e, p. 176). In 1930, Herbert Feigl encouraged him to write a book and *Logik der Forschung* was published in 1934. It led to invitations to lecture in England and in 1937 he emigrated to New Zealand (Popper 1999e, p. 159).

So, although Germans and Austrians played a dominant role in early twentieth century philosophy of science, Popper was only on the periphery of that community. Despite his quarrels with the prevailing Oxford ordinary-language philosophy, he gained recognition in England. Shortly after moving to the London School of Economics, he was given a chair and was eventually knighted by Queen Elizabeth II. Like many immigrants he found much to admire about his new country. He praised Bertrand Russell's elegantly clear writing style. He felt that the English on the whole had "learnt to respect opinions that differ from their own, and to be sober and realistic in their political aims" (Popper 1994d, p. 147). He agreed with Trollope's characterization of the *moral sensitivity* of British public opinion, although he emphasized that this ability to intuitively recognize and react to injustice could be lost (Popper 1994h, p. 154).

From these simple biographical facts alone it would not be surprising to find a certain coolness in Popper's attitude towards German philosophy. And indeed, during the so-called *Positivismusstreit*, outright hostilities broke out. (For a scholarly summary, see David Frisby's introduction to the English translation and Popper's retrospective account of the affair added on at the end [Adorno 1976].) But what is of interest here are Popper's off-the-cuff remarks about the state of German philosophy as expressed in a letter that appeared without his permission in a German newspaper, under the title 'Wider die grossen Worte (Against Big Words)'. When Popper included it in his 1994 anthology, he admitted that it has an aggressive tone. I refer to it because its unusually blunt prose gives us insight into the importance that he attaches to values that some of us might consider somewhat secondary.

Popper begins his answers to his interlocutor with a general statement about the importance of clear expression and intellectual modesty:

The worst thing that intellectuals can do—the cardinal sin—is to try to set themselves up as great prophets vis-à-vis their fellow men and to impress them with puzzling philosophies. Anyone who can not speak clearly and simply should say nothing and continue to work until he can do so. (Popper 1994b, p. 83)

He then compares certain contemporary leftist philosophers unfavourably with Marx and Lenin. As we have seen from the episode above, Popper has no sympathy with the content of Marxism but he emphasizes that both of them wrote in a 'simple and direct manner.' He continues: "What would they have said of the pomposity of the Neo-Dialecticians? They would have found harsher words than 'pomposity'" (Popper 1994b, p. 83). Indeed, Popper himself has harsher words:

[M]odern left-wing nonsense is generally even worse than modern right-wing nonsense. What have the neo-Dialecticians learnt? They have not learnt how hard it is to solve problems and come nearer to the truth. They have only learnt how to drown their fellow human beings in a sea of words. Consequently, I do not like squabbling with these people: *they have no standards*. (Popper 1994b, p. 86) [my italics]

He continues the explanation of why he has not grappled with the writings of Marcuse, Adorno and Habermas:

I am unwilling to criticize these philosophers. To criticize them would be (as my friend Karl Menger once said) to plunge after them, sword drawn, only to sink with them. (Popper 1994b, p. 87)

The letter next provides a list of quotations from Adorno and Habermas in one column and Popper's 'translation' in a parallel column. The contrast between the rather convoluted passages of prose in the left column and Popper's reductive paraphrases on the right is intended to provide a telling illustration of the claim that these authors have such low intellectual standards that it would be a waste of time to criticize their claims. The letter ends with a couplet from Goethe's *Faust*:

Men do believe, if they hear words,  
There must be thoughts that go with them. (Popper 1994b, p. 94)

One could surely take issue with Popper's evaluation of these writers. Certainly Hans Albert, who supports Popper's philosophy of critical rationalism, has found

it worthwhile to provide detailed criticism of the claims of a variety of German philosophers, in part because they are so influential (e.g., Albert 1994). Others have found it valuable to compare some of Habermas' later writings with Popper's ideas. In fact, one could make the case that some of the most interesting debates about Critical Rationalism today are actually centred in Germany!

There may well be a bit of pique interwoven with Popper's indictment of terrible writing styles. He was quite upset because his article did not appear first in the proceedings of a conference where his was the lead-off paper. Popper also resented the fact that the misleading label of 'Positivist' was applied to his position. But, what I would have us focus on is the emphasis Popper places on following the norms for productive intellectual inquiry. By saying that words do not matter and that we need not have a shared framework in order to have a productive dialogue, and by placing little stock in disciplinary boundaries, Popper might be interpreted as a proponent of a rather easy-going, coffee-shop approach to philosophy. Even the opening words of his credo, "I may be wrong and you may be right ...," might seem to connote a laid-back, free-flowing intellectual atmosphere. But as the Credo continues, it is by an *effort* that we may draw closer to the truth. Speaking clearly and simply is a crucial part of making that effort. This episode reminds us that for Popper, intellectual inquiry is a deadly serious enterprise. Referring to the erroneous, but influential, philosophical views he had criticized in his *Open Society*, Popper remarks: "I am convinced that we, the intellectuals, are to blame for almost all misery, because we do not strive hard enough to achieve intellectual honesty" (Popper 1994b, pp. 91–92).

## **An Evaluation of the Moral Theory Extracted from Popper's Writings**

Since Popper himself never laid out a theory of morality and since what I have presented above is a reconstruction of the precepts that appear to inhere in his work, it would hardly be appropriate to fault Popper for leaving out important moral virtues. For example, he says relatively little about justice (although he does criticize Plato's account). Ian Jarvie remarks that the question of what really constitutes justice is ignored: "Popper treats 'justice' as a label for a series of demands for equal treatment for all citizens, and equal sharing of responsibility amongst all citizens" (Jarvie 2001, p. 161). Presumably, different societies make different demands, which then must be critically discussed. He describes himself as a liberal in the nineteenth century sense of the term – from various passing remarks his views seem to match up pretty well with the political philosophy of contemporary American Libertarians:

It is easy to see that the state must be a constant danger, or (as I have ventured to call it) an evil, though a necessary one. For if the state is to fulfil its function [of protecting the weak against the strong], it must have more power ... than any single private citizen of public corporation ... [I]t seems that most men will always have to pay for the protection of the

state, not only in the form of taxes but even in the form of humiliation suffered, for example, at the hands of bullying officials. The thing is not to pay too heavily for it. (Popper 1994h, p. 155)

Although the relief of suffering is a primary moral goal, he is also concerned that the welfare state may be breeding dependency and indicates that the primary role of government is to protect individual freedom. So, self-avowed liberals in the United States would want to add to Popper's theory of morality an emphasis on the economic aspects of equality and fairness.

But, what I found most interesting was the strong overlap between the truth-seeking values that dominate his critical rationalism and the civic virtues that are so important for a liberal democracy. (This theme is explored further in Koertge 2005.) From the single goal of self-emancipation through knowledge, much follows: a dedication to communal problem solving, honesty, openness to criticism, tolerance for other views and a society that supports freedom of expression. When we add the imperatives to relieve suffering and avoid cruelty, we have the building blocks for a pretty adequate moral philosophy.

Where would such a moral theory fit into Popper's Three Worlds ontology? (For a brief summary of this ontological view, see Popper 1999g.) The suffering and cruelty we are trying to avoid lie in both World 1 and World 2, the pain and anguish in World 2 and the open wounds in World 1. The truths (and falsehoods) that we propose, as well as the arguments and counter arguments that generate our tentative conclusions, lie in World 3, as do our conceptions of our duties and responsibilities. My interpretation of Popper's moral theory also lies in World 3; hopefully it resides in close vicinity to Popper's actual theory! But where should we locate our *commitment* to these goals, our *positive attitudes* toward truth-seeking, our *moral outrage* against cruelty?

Popper has always struck me as wanting to down-play World 2, at least as far as his epistemology is concerned. 'I don't believe in beliefs,' he used to say with a grin. Although he realized there were often feelings of 'friendly-hostile competition' amongst scientists, what was really important was the evaluation of ideas, a process that should be strictly separated from the judgement of the folks who held them. To criticize someone's theory was a great compliment, for it meant they had proposed something that was an interesting possible solution to an important problem – something worth discussing. Thus, his approach to understanding the history of science was to look primarily at the scientist's *objective* problem situation. The most important aspects of language were its descriptive and argumentative capabilities, not its expressive function. He even proposed a 'Transference Principle' whereby the principles of logic would be used as a heuristic guide for psychology (Popper 1972).

So what are we to make of the attitudes and commitments that seem to energize Popper's moral stance? Certainly claims about what is the right thing to do can be critically discussed – Popper gives examples of moral progress in World 3 (e.g., our rejection of the theories that tried to justify slavery). But in which realm does our repudiation of slavery reside? I would argue that the conviction that slavery is wrong, our moral *sensitivity* to this evil, to use Popper's term, is something like a

belief, an almost visceral response in World 2 that gives value to propositions in World 3. If I am right about the centrality of moral convictions to our understanding of Popper's philosophy of science and his political philosophy, then we may need to admit that Popper *did* believe in beliefs after all! He remarks that the phrase *love of truth* is not a mere metaphor (Popper 1994i, p. 74) and he calls his Credo an attempt to "summarize a very central part of my moral articles of faith" (Popper 1994m, p. xii).

If Popper were to comment, I think he might direct us to his call for a rational theory of tradition (Popper 1963). Some of our traditions are merely habits at the individual level and conventions at the societal level. A trivial example is the custom of wearing a watch on the left hand. At the other extreme of importance is the meta-tradition of discussing and criticizing our traditions. For example, at the time of the English Revolution, the English began building a tradition of religious tolerance, an attitude that was codified in the procedures of the Royal Society (cf. Popper 1994d, p. 147). In politics, there has gradually evolved a tradition of government by discussion. But although Popper strongly endorses the idea of submitting our traditions to critical scrutiny, he also emphasizes the necessity for continuity:

We should be anxious, terrified, and frustrated, and we could not live in the social world, did it not contain a considerable amount of order, a great number of regularities is perhaps more important than their peculiar merits or demerits. They are needed as regularities, and therefore handed on as traditions, *whether or not they are in other respects rational or necessary or good or beautiful or what you will.* (Popper 1962, pp. 130–131) [my italics]

He goes on to fault those rationalists who criticize the emotional intolerance towards innovation of so-called traditional societies. We may well object to the content of some of their traditions – perhaps they lack tolerance – but we should never try to work for a clean slate. "[A]ll social betterment must refer to a framework of social traditions, of which some are criticized with the help of others ..." (Popper 1962, p. 132). The necessity of having a considerable degree of continuity of traditions is yet another reason for his call for piecemeal engineering in the social arena.

In an essay written at about the same time, he says that the most important of all traditions is what he calls 'the moral framework': "This incorporates the society's traditional sense of justice or fairness, or the degree of moral sensitivity it has reached" (Popper 1994h, p. 157). The framework can change, but it does so comparatively slowly. "Nothing is more dangerous than the destruction of this traditional framework, as it was consciously aimed at by Nazism. In the end its destruction will lead to cynicism and nihilism, i.e. to the disregard and the dissolution of all human values" (Popper 1994h, p. 157).

I conclude that Popper's own intellectual moral framework had a dual ontological status: It could be articulated in World 3 terms, argued for and criticized. This procedure of rational appraisal is exactly what Popper executed throughout his writings. But, the communal search for emancipation through knowledge was also something that he had internalized into his way of life, something he was committed to, and he could become anxious and frustrated if he believed this moral quest to be in jeopardy. At that point his prose style or his demeanour in seminars would become more heated – he might interrupt speakers or refuse to continue a discussion



if he felt his protagonists had violated the intellectual's code of ethics. If Popper erred, it was in thinking that everyone else, if they would only take pause, would surely opt to operate within this same moral framework. Popper's moral philosophy had one foot in World 3 and the other in World 2. Perhaps, it was that combination which sustained his lifetime of philosophical productivity.

This ends my reconstruction of Popper's moral theory and its integration with the rest of his philosophy. What directions for future work does it suggest? One could obviously work at a practical level to fill in gaps and search for further applications. For example, what about the problem of intellectual credit? In World 3 terms, it should hardly matter who was first to propose a conjecture or make a refutation. Yet, scientists and philosophers are notoriously touchy about such matters and Popperians are no exception. What, if anything, does critical rationalism tell us about good ethical rules for giving credit and how we should react if they are violated?

More interesting are meta-ethical issues about the status and evaluation of moral claims. Niemann (1993) develops the idea of treating ethical claims as solutions to specifically ethical problems that can be appraised in a manner similar to Popper's approach to metaphysical claims. Ongoing work on normative sociology of science, such as that of Agassi, Jarvie and Wettersten, will also be relevant (see Jarvie 2001 for sample references). Which rules for communal inquiry best promote our intellectual aims? How do they evolve? How can people be persuaded to adopt them? The current prospects for elaborating and improving Popper's moral philosophy look promising indeed.

## References

- Adorno, Theodor Wiesengrund et al. 1976. *The Positivist Dispute in German Sociology* [G. Adey and D. Frisby trans.]. London: Harper & Row.
- Albert, Hans. 1994. *Kritik der reinen Hermeneutik: Der Antirealismus und das Problem des Verstehens*. Tübingen: Mohr Siebeck.
- Jarvie, Ian Charles. 2001. *The Republic of Science: The Emergence of Popper's Social View of Science 1935–1945*. Amsterdam: Rodopi.
- Koertge, Noretta ed. 1998. *A House Built on Sand: Exposing Postmodernist Myths About Science*. New York: Oxford University Press.
- Koertge, Noretta ed. 2005. *Scientific Values for a Civil Society*. New York: Oxford University Press.
- Niemann, Hans-Joachim. 1993. *Die Strategie der Vernunft: Rationalität in Erkenntnis, Moral und Metaphysik*. Braunschweig: Vieweg.
- Niemann, Hans-Joachim. 2004. *Lexikon des Kritischen Rationalismus*. Tübingen: Mohr Siebeck.
- Popper, Karl Raimund. 1963. Towards a rational theory of tradition. In Karl Raimund Popper. *Conjectures and Refutations*. London: Routledge & Kegan Paul, pp. 120–135.
- Popper, Karl Raimund. 1972. *Objective Knowledge: An Evolutionary Approach*. London: Oxford University Press.
- Popper, Karl Raimund. 1994a. *In Search of a Better World Lecture and Essays from Thirty Year* [L.J. Bennett trans.]. London: Routledge.
- Popper, Karl Raimund. 1994b. Against big words (A letter not originally intended for publication). In Karl Raimund Popper. *In Search of a Better World Lecture and Essays from Thirty Year* [L.J. Bennett trans.]. London: Routledge, pp. 82–98.

- Popper, Karl Raimund. 1994c. Creative self-criticism in science and art (Stolen from Beethoven's sketch books). In Karl Raimund Popper. *In Search of a Better World Lecture and Essays from Thirty Year* [L.J. Bennett trans.]. London: Routledge, pp. 223–232.
- Popper, Karl Raimund. 1994d. Emancipation through knowledge. In Karl Raimund Popper. *In Search of a Better World Lecture and Essays from Thirty Year* [L.J. Bennett trans.]. London: Routledge, pp. 137–150.
- Popper, Karl Raimund. 1994e. How I see philosophy (Stolen from Fritz Waismann and from one of the first men to land on the moon). In Karl Raimund Popper. *In Search of a Better World Lecture and Essays from Thirty Year* [L.J. Bennett trans.]. London: Routledge, pp. 173–187.
- Popper, Karl Raimund. 1994f. Knowledge and the shaping of reality. In Karl Raimund Popper. *In Search of a Better World Lecture and Essays from Thirty Year* [L.J. Bennett trans.]. London: Routledge, pp. 3–29.
- Popper, Karl Raimund. 1994g. On knowledge and ignorance. In Karl Raimund Popper. *In Search of a Better World Lecture and Essays from Thirty Year* [L.J. Bennett trans.]. London: Routledge, pp. 30–43.
- Popper, Karl Raimund. 1994h. Public opinion and liberal principles. In Karl Raimund Popper. *In Search of a Better World Lecture and Essays from Thirty Year* [L.J. Bennett trans.]. London: Routledge, pp. 151–160.
- Popper, Karl Raimund. 1994i. The logic of the social sciences. In Karl Raimund Popper. *In Search of a Better World Lecture and Essays from Thirty Year* [L.J. Bennett trans.]. London: Routledge, pp. 64–81.
- Popper, Karl Raimund. 1994j. Toleration and intellectual responsibility (Stolen from Xenophanes and from Voltaire). In Karl Raimund Popper. *In Search of a Better World Lecture and Essays from Thirty Year* [L.J. Bennett trans.]. London: Routledge, pp. 188–203.
- Popper, Karl Raimund. 1994k. What does the West believe in? (Stolen from the author of *the Open Society*). In Karl Raimund Popper. *In Search of a Better World Lecture and Essays from Thirty Year* [L.J. Bennett trans.]. London: Routledge, pp. 204–222.
- Popper, Karl Raimund. 1994l. *The Myth of the Framework: In Defence of Science and Rationality*. London: Routledge.
- Popper, Karl Raimund. 1994m. Introduction. In Karl Raimund Popper. *The Myth of the Framework: In Defence of Science and Rationality*. London: Routledge, pp. xii–xiii.
- Popper, Karl Raimund. 1994n. Science: problems, aims and responsibilities. In Karl Raimund Popper. *The Myth of the Framework: In Defence of Science and Rationality*. London: Routledge, pp. 82–111.
- Popper, Karl Raimund. 1994o. The moral responsibility of the scientist. In Karl Raimund Popper. *The Myth of the Framework: In Defence of Science and Rationality*. London: Routledge, pp. 121–129.
- Popper, Karl Raimund. 1994p. The myth of the framework. In Karl Raimund Popper. *The Myth of the Framework: In Defence of Science and Rationality*. London: Routledge, pp. 33–64.
- Popper, Karl Raimund. 1999a. *All Life Is Problem Solving* [P. Camiller trans.]. London: Routledge.
- Popper, Karl Raimund. 1999b. Against the cynical interpretation of history. In Karl Raimund Popper. *All Life Is Problem Solving* [P. Camiller trans.]. London: Routledge, pp. 105–115.
- Popper, Karl Raimund. 1999c. All life is problem solving. In Karl Raimund Popper. *All Life Is Problem Solving* [P. Camiller trans.]. London: Routledge, pp. 99–104.
- Popper, Karl Raimund. 1999d. Epistemology and the problem of peace. In Karl Raimund Popper. *All Life Is Problem Solving* [P. Camiller trans.]. London: Routledge, pp. 36–44.
- Popper, Karl Raimund. 1999e. How I became a philosopher without trying. In Karl Raimund Popper. *All Life Is Problem Solving* [P. Camiller trans.]. London: Routledge, pp. 150–161.
- Popper, Karl Raimund. 1999f. Masaryk and the open society. In Karl Raimund Popper. *All Life Is Problem Solving* [P. Camiller trans.]. London: Routledge, pp. 145–149.
- Popper, Karl Raimund. 1999g. Notes of a realist on the body-mind problem. In Karl Raimund Popper. *All Life Is Problem Solving* [P. Camiller trans.]. London: Routledge, pp. 23–35.
- Popper, Karl Raimund. 1999h. On freedom. In Karl Raimund Popper. *All Life Is Problem Solving* [P. Camiller trans.]. London: Routledge, pp. 81–92.

- Popper, Karl Raimund. 1999i. The collapse of communism: understanding the past and influencing the future. In Karl Raimund Popper. *All Life Is Problem Solving* [P. Camiller trans.]. London: Routledge, pp. 126–138.
- Popper, Karl Raimund. 1999j. Waging wars for peace. In Karl Raimund Popper. *All Life Is Problem Solving* [P. Camiller trans.]. London: Routledge, pp. 116–125.

# Critical Rationalism and Ethics<sup>1</sup>

Jeremy Shearmur

**Abstract** This paper examines Popper's views about ethics and metaethics, drawing on a wide variety of sources. It notes the presence of Kantian and utilitarian themes, and discusses some ideas about how they might be interpreted and inter-related. It argues that there are various problems about Popper's views – notably that his Kantian-influenced ideas about the significance of the individual conscience would appear to conflict with the emphasis on inter-subjectivity in his more general epistemology. The author suggests that it is not likely that a resolution to the issues which he raises will be found in Popper's own work, and advocates, as a research programme for critical rationalism, the adoption of a strong form of ethical realism, and argues that Popper's own treatment of reduction suggests the legitimacy of exploration of these matters in a non-reductionistic manner, prior to the attempting of reductions.

## Ethics and the Open Society

As Alan Ryan notes in his 'Popper and Liberalism' (Ryan 1985), there are two strong but contrasting ethical themes in Popper's *Open Society*, of a Kantian and a utilitarian character. More specifically, one might say that on the one side, there is Popper's 'protectionism'. This has a strongly Kantian, or as one might say today, 'republican' flavour to it.<sup>2</sup> Popper's protectionism involves the protection of individual liberty in a manner reminiscent of liberalism. However, despite the fact that Popper has sometimes referred to his views in ways that would invite their assimilation to liberalism,<sup>3</sup> I think that we need to note the distinctive character of his views here. Not only does Popper see the protections that he favours as something that has to be created and enforced by the state in response to our moral demands. (That is, there is no hint of a suggestion that they are to be understood in terms of either moral practises – as in Locke – that pre-date the state, or as, after the fashion of J. S. Mill, something that is rooted in utilitarianism.) But it is clear from a comment that Popper

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made in his (unpublished) lectures at Emory University in 1956, that for Popper it is not acceptable that people are not, say, killed because of the good character of others; rather, they need to have rights protected by the power of the state.<sup>4</sup> (Compare also Popper's discussion of similar themes in his 'Public Opinion and Legal Principles'.<sup>5</sup>) In addition, Popper stresses that there should be the protection not only of the negative rights favoured by classical liberals, but also that they should not be subject to economic exploitation. Indeed, in the course of *The Open Society* and elsewhere, Popper favoured the idea that people should have an income guaranteed out of taxation; while in a discussion comment at the first meeting of the Mont Pélerin Society, Popper suggested that this should be at the level of the average wage.<sup>6</sup>

To these ideas, one might also relate Popper's passionate ethical individualism – something that he emphasised, in a manner that contemporary 'communitarians' might well consider, should be distinguished from egoism. What Popper favoured, here, was a concern for each individual, and for the idea that what was desirable was a combination of individualism and altruism.

On the other side, Popper is well-known for commending, as an approach to public policy, what has become known as 'negative utilitarianism'. Popper here urged that, rather than pursuing the aim of making people happy, government might, instead, be concerned with an agenda of the relief of suffering. It should be noted, at once, that this is not something that Popper was arguing which should be the only concern of public policy. Accordingly, Ninian Smart's amusing criticism of negative utilitarianism that from such a perspective, we should kill people painlessly; see (Smart 1958) does not hit Popper's approach; no more than does John Stuart Mill's earlier development of the same idea, in (Mill 1864).

There is, however, rather more to all this that meets the eye. For while Popper was clearly concerned about the relief of suffering – a response to which is a deep-seated theme in his work<sup>7</sup> – and while Popper seems to me to ignore the problem of what the extent of our responsibilities to others should be, on this score it is not altogether clear that the character of these ideas is quite utilitarian in its spirit. The reason for making this perhaps strange-sounding claim, is that Popper set out, in a paper called 'Public and Private Values', which seems to have been written in 1946,<sup>8</sup> an argument which clarifies ideas on this score already to be found in his *Open Society*. The paper is interesting, because in it Popper starts from the problem that people – his concerns seem to have been, especially with humanitarian democrats<sup>9</sup> – may bring to politics attachments to conflicting ideals, points at issue between which cannot be resolved rationally. (The pursuit of any one of which, he criticized as 'utopian'.) In the face of this, Popper proposed the suggestion that, in the formulation of an agenda for public policy, they should concentrate on what they could agree upon, and suggested, more specifically, that they consider what is *unacceptable*: concrete evils 'such as starvation, pain, humiliation, injustice, exploitation' (Popper 2008, p. 119). This, he was arguing, should furnish an agenda for politics. He further suggested that this might make it possible for those who would be in disagreement about positive ideals – Popper here refers to liberals, socialists, utilitarians and Christians – to discover an agenda that they could agree to.

How would politics itself function? Here his ideas about ‘piecemeal social engineering’ come to the fore. We start by way of the characterization of an agenda for government policy, along the lines indicated above, in which suggestions are advanced which, in principle, are open to inter-subjective assessment as to whether there is an agreement that they are acceptable. Next, ideas about how these are to be addressed are formulated – implicitly, by an elite. (Popper does not say this explicitly, but the very material in which he stresses that criticism may be offered by anyone, also seems to suggest that not everyone will necessarily be capable of formulating cogent policy suggestions.<sup>10</sup>) This is then presumably opened to criticism. What is crucial, from Popper’s perspective, is that experiments be tried out in such a way that we can expect to learn from them; it is striking that, for example, in correspondence with Rudolf Carnap, Popper seems ready to accept that this could even include limited experimentation with the socialization of the means of production.<sup>11</sup> Ideas then need to be assessed both for their effectiveness, and also as to whether they have given rise to problematic unintended consequences. Popper seemed, here, to favour assessment by the public at large – he quoted both Pericles, at the start of Chap. 1 of his *Open Society*, and Burke immediately before the Preface to the First Edition of *The Open Society* on the significance of assessment by ordinary people. He refers, in this context, also to the theme of the ‘rational unity of mankind’ – in which mankind share a unity, in respect of their reason, which for Popper is a matter of their being able to exercise criticism. In addition, Popper valued elections as a way in which the efforts of politicians could be assessed by the people at large, and he was critical, on this score, of list-based forms of proportional representation for making it more difficult to vote people out of power.<sup>12</sup> In addition, Popper favoured the idea that there should be independent specialists who would be employed by government to conduct such assessments.<sup>13</sup>

If we turn back to what all this implies for the philosophical character of Popper’s ‘negative utilitarianism’, what this means, or so it seems to me, is that while the substance of these ideas may be identified as negative utilitarian in their character, one can equally well take Popper’s emphasis to be procedural or methodological in its character. That is, he is offering us a procedure by means of which reasonable democrats who might otherwise be attached to contrasting ideals, between which rational decision-making is not possible,<sup>14</sup> might nonetheless be able to agree. In this sense, one might look at it as suggesting the kind of view that one finds, more recently, in Rawls’ later ‘political’ approach, or in Cass Sunstein’s ideas about ‘incompletely theorized agreements’ cf. (Sunstein 1996).

If one takes this view, there opens up, as I have suggested elsewhere (Shearmur 1996a) and (Shearmur 1996b), an interesting link between these two strands in Popper’s work. It relates to the idea upon which I have earlier remarked: ‘the rational unity of mankind’. For clearly, if one wishes that people should be able to offer critical feedback – of which we stand in need if we are concerned about truth, about the validity of our moral ideas, or about the cogency of our ideas concerning public policy – then it is important that they enjoy autonomy. The argument that John Stuart Mill developed in his *The Subjection of Women* here applies with full force: unless people enjoy autonomy, including, one might stress, material

autonomy, then it is not clear that they will be in any position to voice criticism. Accordingly – though I am *not* suggesting that this was Popper's own motivation – and there is a risk that it might, even if cogent, be seen as 'the wrong kind of argument'<sup>15</sup> – there is a way in which we might link Popper's negative consensus and criticism approach to public policy, with those aspects of the individual which would benefit from his 'protectionism'. For it would not be implausible to suggest that one might find people pursuing Popper's 'negative utilitarian' methodology, arriving at substantive results which serve to secure the autonomy of those people who are the objects of their moral consideration. Clearly, the possibility of their making a contribution to dialogue as cognitive agents could not be the only reason for protecting people. It is obvious enough that Popper would, for example, press that we especially protect, say, those people who would not be capable of making a contribution as what one might term critical, Popperian moral agents; not least because of his deep concern about suffering. But the link seems to me interesting and suggestive, and it has well-known parallels to Mill's argument from a truth-seeking fallibilism to toleration.

## Metaethics and the Open Society: Brief Remarks

I will here explore (only very briefly for reasons that I will explain in the next section), Popper's views about the status of ethical claims in *The Open Society* and elsewhere.<sup>16</sup> Three issues are here worth noting.

First, Popper strongly emphasises the autonomy of ethics, by way of an emphasis on the dualism of facts and decisions. As I have argued elsewhere, his concern here seems to me driven by a wish to resist ethical naturalism, and attempts to collapse ethics into sociology or to offer a historicist form of ethical futurism. Indeed, one could see a key motif running through this discussion of Popper's to be the autonomy of ethics, and a desire to defend it against various kinds of heteronomy. This, however, leads to a difficulty, which is that while Popper stresses that our decisions are not to be seen as necessarily arbitrary, he does not offer us an account of that by which our ethical decisions are to be understood as constrained. There is a risk, there, that because of Popper's wish to stress the autonomy of ethics, Popper's views sometimes come over as highly decisionistic in their character, and almost existentialist in their spirit (although Popper was critical of existentialism).

In a lecture that he delivered in New Zealand as part of a series which contained material that went into *The Open Society*, Popper clearly took the view that it was the individual's conscience which was 'the ultimate court of appeal'.<sup>17</sup> The conscience is also stressed in *The Open Society*. It is, however, worth noting that at the end of a broadcast lecture which Popper delivered in 1954, in which he addressed Kant's philosophy, Popper's discussion concluded with a discussion of Kant's ideas about autonomy. Popper here stressed the Kantian theme of – in the face of a command by God – its being 'our responsibility to judge whether the command is moral or immoral'. This Popper continues to redescribe as 'a man's



conscience is his moral authority', and then to describe aspects of Kant's ethics as offering a formulation of 'what our conscience may demand from us' (See Popper 1963, Chap. 7, Sect. 6). It would thus seem plausible to interpret Popper's own stress on the individual conscience, as being in a similar Kantian spirit.

What might we say about Popper's views concerning how these things might work? In one interesting passage in *The Open Society* (Popper 1945, Chap. 24, Sect. iii), Popper discusses the way in which, in George Bernard Shaw's *St Joan*, a figure who had been calling for the death of Joan of Arc breaks down, when confronted with the actual reality of what he had been calling for. He was thus led to repudiate his earlier views, when he was confronted with their consequences.

At the same time, it is by no means clear that a person's conscience has sufficient autonomy from their substantive ethical views, to generally play this role. In our own day, Peter Singer has offered a hard-hitting criticism of appeals to individual ethical intuitions, noting that these may carry the residues of mistaken philosophical or religious ideas, or issues relating to the quirks of our personal upbringing (Singer 1974).<sup>18</sup> Further – or so it seems to me – one might use against reliance upon the individual's conscience the very argument that Popper himself used in his *Logic of Scientific Discovery* against subjectivistic epistemology: that there is no reason to take individual subjective certainty as indicating truth. (See Popper 1959, Sect. 8; the quotation is from p. 46.) If I may edit Popper's statement there slightly, eliminating references to a scientific conscience, it would seem to me to apply perfectly well to the judgements of our conscience:

No matter how intense a feeling [I may have] it can never justify a statement. I may be utterly convinced of the truth of a statement...overwhelmed by the intensity of my experience: every doubt may seem to be absurd. But does this afford the slightest reason... to accept my statement. Can any statement be justified by the fact that K. R. P. is convinced of its truth? The answer is, 'No'; and any other answer would be incompatible with the idea of...objectivity.

The reference here to 'truth' in the context of Popper's ideas about ethics might seem far-fetched. But in fact, already in places in *The Open Society*, there are various passages which seem suggestive of an ethical objectivism.<sup>19</sup> However, Popper also criticized there explicitly both the idea of a science of ethics, and took issue with the idea of our making judgements on others – something that, it seems to me, would be needed if we were to endorse an objectivistic approach.

It is, however, worth noting that in Popper's 'Emory Lectures' of 1956 (which exist only in a fragmentary form<sup>20</sup>) Popper discusses in passing the theme of natural law.<sup>21</sup> He is critical of it, but because of the aspect to it which suggests that it was instituted, once and for all, by a god. Popper also indicates that he disagrees with moral positivism. He offers instead an account in which we criticize in the light of *developing* intuitions about justice – his account of which, while sketchy, seems to me to parallel his more general ideas about epistemology.

Finally, there is, of course, Popper's 1961 Appendix to *The Open Society*, 'Facts, Standards and Truth: A Further Criticism of Relativism'. Most of this is concerned with general issues in epistemology. But it is interesting that in Sect. 5, when he discusses fallibilism and the growth of knowledge, he illustrates his discussion

of the discovery of mistakes as constituting real advance in our knowledge by reference also to ‘the known examples of miscarriage of justice’ (Popper 1966, Vol. II, Sect. 5), which already suggests a continuity between his discussion of factual and of moral issues. However, rather than offering a straightforward parallel between ourselves as engaged in the quest to discover both factual and moral truth, Popper initially seems to suggest that criticism in the moral sphere is in the light of ‘standards which we have decided to adopt’, and he concludes his detailed discussion by saying ‘the logical situation of the regulative ideas, of “right”, say, or “good”, is far less clear than that of the idea of correspondence to the facts’ (Sect. 13). However, he also then goes on to write:

we may take the idea of absolute truth – of correspondence to the facts – as a kind of model for the realm of standards, in order to make it clear to ourselves that, just as we may *seek* for absolutely true propositions in the realm of facts or at least for propositions which come nearer to the truth, we may *seek* for absolutely right or valid proposals in the realm of standards – or at least for better, or more valid, proposals.

He then goes on to say, more substantively:

As in the realm of facts, we can make discoveries. That cruelty is always ‘bad’; that it should be avoided where possible; that the golden rule is a good standard which can perhaps even be improved by doing unto others, wherever possible, as they want to be done by: these are elementary and extremely important examples of discoveries in the realm of standards. These discoveries create standards, we might say, out of nothing: as in the field of factual discovery, we have to lift ourselves by our own bootstraps. This is the incredible fact: that we can learn; by our mistakes, and by criticism; and that we can learn in the realm of standards just as well as in the realm of facts.

This, however, is about as much as Popper says. I will explore something of why this is the case, in the next section. But it seems to me that there is enough by way of explicit objectivism here, to offer a basis for my earlier critique of the judgments of an individual conscience, in the name of a Popperian stress on inter-subjective appraisal.

## Ethics and the Bounds of Reason

Given that Popper wrote extensively about epistemology, and also about moral and political issues, one might therefore expect that one would find, in his writings, a well-developed epistemology and meta-ethics. This, however, is not the case. Indeed, writing to an otherwise sympathetic correspondent who suggested that there was a ‘hole’ in his argument at this point, Popper responded that there indeed was:<sup>22</sup>

a hole in my argument, very loosely filled by a few hints (to which you refer) and by the addendum to vol ii [of *The Open Society*] (to which you also refer). There is a reason for this: I did not want to be drawn into a discussion of meta ethics. Almost all philosophic discussions in those days were of meta ethics and they seemed to me endless and not profitable. There were other reasons too. I preferred to make my ethical position clear in discussing (criticizing) opposing positions. However, I have lectured on Ethics, both in

New Zealand and in London (one course, on request, at the L.S.E.). But I have never felt that I should publish my views on meta ethics – on what you call ‘the logic of moral discovery’. Thus the hole of which you complain does exist.

This, however, was not half of the matter. For it is important to bear in mind, when approaching Popper’s work, an important issue concerning his ideas about rationality. It is this. In *Logik der Forschung* – and thus the text of his *Logic of Scientific Discovery* – Popper, while recognizing that he held various metaphysical theories (e.g. that he was a realist; that he had particular views about the aims of science; that he favoured an ‘Aristotelian’ correspondence theory of truth), did not have to hand a theory about what made such views rational. Accordingly – and understandably – in *Logik der Forschung*, ideas about the aim of science played a role, but their status was made a matter of decision. That is to say, while it is clear what Popper’s own views were about the preferred aims for science, and while the substantive approach offered in the book was written with an eye to those aims, at bottom the issue of what view should be taken of the aims of science (and thus also of what methodology should be adopted) was left as a matter of decision. The book was also written in such a way that it was possible not – in considering its contents – to refer to truth. The issue of the aim of science – which plays a key role in the book – is thus explicitly made, by Popper, a matter of decision. (Popper makes clear that it is open for people who do not share his views as to the aims of science – conventionalists, for example – to prefer different methodological rules to the ones which he is recommending.)

Popper himself has explained this issue in notes added to the English translation of *Logic of Scientific Discovery* and, for example, in his *Objective Knowledge*, where he wrote (Popper 1972, p. 40, Note 9):

In my *Logik der Forschung*... I describe myself as a metaphysical realist. In those days I identified wrongly the limits of science with those of arguability. I later changed my mind and argued that non-testable (i.e. irrefutable) metaphysical theories may be rationally arguable...

Now, the reason why I have stressed this point is as follows. While Popper’s views about this issue changed (as I will describe shortly), the change took place *after* he wrote his *Open Society*. This I believe to be of considerable importance if we approach the text of that book. For it serves to explain several important features of it. In particular, if we consider that, for Popper at the time at which he wrote, it is only the empirical, the formal and – *given* a view as to what our aims should be – the methodological that falls within the scope of the rational, it becomes clear why Popper’s discussion of ethical issues is rather strange. For while one might say that – as in *Logik der Forschung* – aspects of Popper’s actual views keep breaking out, there is an oddly ‘decisionistic’ feel to a lot of what he wrote.

In 1948, Popper gave a paper, ‘Prediction and Prophecy in the Social Sciences’, at a conference in the Netherlands. While he was there, he heard a paper delivered by a Professor L. J. Russell from Birmingham. The paper, ‘Propositions and Proposals’ suggested to Popper the important idea that proposals can be discussed; and it was of such significance for him that he added a reference to Russell’s paper in a footnote to his ‘Prediction and Prophecy’, when it appeared in 1949.<sup>23</sup>

He subsequently referred to it, in his *Open Society*. At the same time, while Popper revised his *Open Society* for the first American edition (in 1950) he did no more, on this score, than add some references to Russell. He also used the terminology of 'proposals' in his appendix 'Facts, Standards and Truth'.

Subsequently, two developments took place. The first of these was the idea – which formed part of Popper's *Postscript* – that the rational discussion of meta-physical ideas was possible, if they were understood and evaluated as solutions to a problem. These ideas were first published as his 'The Problem of the Irrefutability of Philosophical Theories', which was given as a talk for Radio Free-University Berlin, and first published in *Ratio* in 1958. This marks the generalization of his views about the empirical evaluation of theories in terms of their testability, to the wider idea of the discussion of theories in terms of their inter-subjective appraisal. Popper, however, applies these ideas – both in the *Ratio* piece, and also in the fuller discussion in the *Postscript* – to metaphysical rather than to ethical theories.

The second theme related to a striking idea in Popper's *Open Society*. This was the notion that rationality itself was a matter of commitment – that the rationalist was involved in an irrational commitment to reason. This is an idea that seems to have an echo in Popper's personal history. As Bartley and Hacoen (Hacoen 2000) have explored, a first draft of Popper's *Unended Quest* seems to suggest that, at the point when, as a result of people dying in a demonstration, he lost faith in Marxism, he seems also to have lost faith in reason. He read Kierkegaard, pondered issues about Kierkegaard's treatment of Abraham's near-sacrifice of his son, Isaac, and seems to have come under his baleful influence – to the point, Bartley argues, of adopting a Kantian ethics on something like Kierkegaardian grounds (Bartley 1989).

Now, if one reads this into Popper's brief comments in *The Open Society* about an irrational commitment to reason, one gets onto the ground of Bartley's concerns, in his *Retreat to Commitment*, about rationalism, commitment and the 'tu-quoque' argument. As Bartley argued in his *Retreat to Commitment* (Bartley 1962), there is a problem facing the rationalist if rationalism is itself to be understood as involving 'an irrational *faith in reason*' (as Popper put it in the first edition of his *Open Society* (Popper 1945, Chap. 24, Sect. II, final page). The problem is that it would mean that the rationalist could not then criticize some other kind of fideist – whether religious or political – for *their* irrational leap of faith. In the fourth edition of the book in 1962, Popper modified the language of Chap. 24 in the light of Bartley's criticism, and also added his Appendix 'Facts, Standards and Truth: a further criticism of relativism'.

As has been explained in material that has been reproduced and commented on in Mariano Artigas's *The Ethical Nature of Karl Popper's Theory of Knowledge* (Artigas 1999), Popper has said that the changes that he made – with acknowledgement to Bartley – in *The Open Society* were, in fact, written by Bartley. Popper – and Artigas, who defends him on this point – seems to have been left unconvinced that there was a genuine philosophical problem here. But it seems to me that there were two, both related to the fact that *The Open Society* often seems decisionistic in its view of ethics.

The first problem – with which Bartley was concerned – relates to the question of whether or not what Popper had to say about ‘faith in reason’ was implicitly fideistic. I will not discuss this issue here, other than to say that it seems to me that Bartley was correct in seeking to bring out the way in which Popper’s work replaced justification by openness to criticism, and that in this context the issue of whether critical rationalism was, itself, open to criticism then becomes a serious issue.

The second problem relates to the status of ethics. Here, I have suggested that there are some problems. For understandable enough reasons, Popper’s treatment of ethics in *The Open Society* seems decisionistic. At the same time, Popper’s stress on our ability to learn in ethics, and the parallels that he draws with his epistemology, would seem to suggest an ethical realism. Popper, as I have indicated, had an aversion to addressing this topic, and more generally, to ethical theory. As he wrote in ‘Public and Private Values’ (see Popper 2008, p. 120):

For a long time I have been dissatisfied with the speculations of most of our philosophers on matters of Ethics. Especially the discussions of such problems as the Nature of Good or of the Objectivity of Values etc. appeared to me as hopelessly barren. It appeared to me that such a simple principle or imperative as ‘help those who are in distress’ or ‘fight for those who have suffered injustice’ or, if you like, the Golden Rule, was capable of covering at least nine tenths of what was needed in ethics. But the philosophy of this sort had practically nothing whatever to do with these simple principles or imperatives. On the other hand, I do not wish to suggest that abstract thought in ethical matters is superfluous or that it must be barren. On the contrary, I do feel that it is very important to think about these matters, as long as we do not forget what our thoughts should yield. They should yield a better understanding of such practical principles or imperatives as those mentioned before, and specially a kind of hierarchy of urgency among those principles, that is to say, a hierarchy which can serve as a guide in case of conflicts between such principles.

But this reaction while understandable in personal terms, and while intellectually understandable when Popper did not have to hand an expansive theory of the character of rationality, is surely insufficient. There is an intellectual *problem* here: can we give an account that can make sense, cosmologically, of the kind of common-sense realism which is part of our day-to-day ethical attitudes?<sup>24</sup>

## Critical Rationalism and Ethics

The situation with which all this leaves us, seems to me not altogether satisfactory. There is an immense amount of really interesting material in Popper’s work which relates to ethical issues (on which my remarks here have only touched). But it would seem to me clear that Popper’s own early ideas about the limited scope of reason, together with his personal aversion to writing on issues of metaethics, meant that he did not produce much by way of either an extended discussion of his own views about ethics, metaethics, or the epistemology of ethics.

It would in principle have been possible for him to offer such a thing, from the perspective of his later ideas. But although he admitted – in his letter to Sharratt – that there was such a gap in his work, it is clear that it was not a gap which he had

any inclination to fill. More might be extracted, to try to fill the gap, from remarks scattered through Popper's work. But it is not clear that there is any magic bullet to be found.<sup>25</sup> While as far as I can see, any attempt to do so would face the problem that it would be inconsistent with some of the things that Popper has written, because of the terms in which he wrote about these matters in his *Open Society*.

## What Is To Be Done?

Here I will offer some brief and speculative suggestions.

What is needed would seem to me to have three aspects to it. First, there are important problems about Popper's substantive ethical views. At one level, the picture is clear enough. He favours a liberal/republican protection of the individual. Second, he favours an approach to public policy which addresses – in an experimental manner – a negative utilitarian (or perhaps better, consensually agreed) negative agenda.<sup>26</sup> Third, he favoured the pursuit of positive values about the good life by individuals and their friends. (This would seem open also to what one might call private-collective arrangements as in the pursuit of a particular positive vision of life in a cooperative or a commercial setting. See, in this context, the Disney Corporation's town of 'Celebration' (cf. Shearmur 2002).)

What seems to me to be left open as a problem by all this is the *extent* to which we should be committed to relieving the suffering of others. Here, Peter Singer's 'Famine, Affluence and Morality' raises this issue in a stark manner (Singer 1972). For if the relief of famine and, more generally, the remedying of the dire conditions in which others are currently living features on our moral agenda, the key problem is: just what should our commitment to this amount to? Singer's own response – in which we are faced with the prospect of living our lives as joyless slaves to distant need – is clearly unacceptable.<sup>27</sup> What seems clearly called-for, is an account of how different ethical concerns are to be properly inter-related.

Second, we stand in need of an epistemology of ethics. And, finally, we need a coherent metaphysical defence of the kind of moral realism that a critical rationalist approach to ethics would involve. Each of these would merit a much fuller discussion than I can offer here, but let me say a very little about them, in turn.

Popper, as I have indicated above, drew certain parallels between his ordinary epistemology and ethics. It seems to me that this needs to be developed in much more detail. If we were to do so, however, we would need to make one significant change to Popper's ethical views. For if we are to take a 'Popperian' view of objectivity in ethics, then we would, surely, have to adopt his own emphasis, in this context, on objectivity as being the product of inter-subjective discussion. This, in turn, would mean that we would have to qualify the role that Popper gives to the individual's conscience. Indeed, as I suggested above, not only might an individual's view simply be wrong, but it may also be influenced by mistaken general theories. We have – as Popper has stressed – no means of detecting if this is the case on our own (compare, in this context, his important discussion of objectivity in



*The Open Society*, Chap. 23). While our own subjective convictions here, however strong, don't provide us with a means of detecting that we are right, we may learn by means of the critical judgements of others.

Here, Adam Smith's *Theory of Moral Sentiments* offers us an interesting model, if it is read not just as description, but rather as a theory directed towards the attainment of moral truth – this being what an impartial spectator would approve of. On Smith's account, we learn by way of the (fallible) moral judgements of others upon our conduct.<sup>28</sup> But there is something more specifically Popperian upon which we might draw, as well. It is Popper's theory of the 'empirical basis' – in which we take, as a guide to what is true, those particular judgements upon which we can, for the time being, reach an inter-subjective consensus. (I have offered an account of how I think that this is best to be understood, and a defence of it against some critics, elsewhere (See Shearmur 2007).)

In what I wrote above, I referred to 'particular judgements', and I did so advisedly. For while I have no reason to doubt that there may be general ethical truths, a knowledge of these would seem to me more of what we would expect to discover at the *end* of moral enquiry, rather than something that can sensibly be invoked as having authoritative status in the course of such enquiry. As in science, so in morality: we can surely take our tentative 'basic statements' as things that can be used to evaluate general moral claims, rather than as things the correctness of which is to be settled by invoking general moral claims. To be sure, we may wish to refer to general claims; but their value would seem to me restricted to their character as tentative generalizations about other cases, rather than to have any kind of authority in their own right – after all, from where could such authority stem, epistemologically?

What all this would amount to, is a kind of inter-subjective *methodological* ethical particularism. We would, thus, see what our intuitions are with respect to a particular case. If we were in disagreement, we would draw attention to the features of the action etc. that seemed to us to be significant, ask for clarification from the other people, and would argue by analogy with other cases actual or hypothetical. In the course of this, claims of a general character might be made; but they would then be open to testing in other cases rather than things the invocation of which could be expected to settle such cases. It would resemble a form of reflective equilibrium, but in which less weight is given to general statements or principles, and which is envisaged as an actual social process, in which we are engaged with other people who are typically in disagreement with us.

Two further comments are worth adding here.

The first is that, as compared with Popper's account, it would mean that the individual's conscience cannot be considered the final court of appeal. We can endorse, fully, Popper's concern for the autonomy of ethics. But his own parallel with truth surely points us towards the significance for ethics of his own critique of the epistemological significance of subjective certainty, and towards an inter-subjective theory of objectivity.

Does this mean that we must discount the significance of the individual conscience entirely? I think that three arguments may be offered for according it some significance.



First, we may find that we are simply not able to get any measure of inter-subjective agreement about some issues, despite our attempts to learn from one another. (After all, Popper's own discussions in 'The Myth of the Framework' and of the Bohr-Einstein arguments in his 'On Toleration' brought out the way in which learning may take place without our being able to reach agreement, in some areas.<sup>29</sup>) What this would suggest is an obligation on our own part at least to listen to the views of those who disagree with us and to see what we can learn from them. But if we discover that we are operating in areas in which the possibilities of inter-subjective agreement are limited, we should then still bear in mind that the strength of our subjective convictions is not to be equated with truth. Where there are persisting disagreements, this recognition of fallibility – in a context in which it may be difficult to correct our errors – should incline us towards compromise. But at the same time, it surely indicates that there are advantages to, as far as possible, allowing people who disagree to consensually make their own arrangements with those who agree with them, rather than trying to force a single way or proceeding on everybody. (One might, say, explore whether there could be separate hospital systems for those who favour or abhor euthanasia.)

Second, if there is a fair measure of dialogue and learning, we may, with an eye to the conscience, take heart from fallibilism. Just because there seems to be very wide discussion-based agreement on something, this does not show that what people have agreed on is correct. In such a setting, people may clearly retain their conscientious judgement that the preponderant view is false. This view would have a status comparable to that of a metaphysical research programme, as distinct from a scientific theory.<sup>30</sup> This means that one should be aiming to develop arguments that, in the end, others should find telling, and that one should not confuse the status of one's programmatic ideas with something for which there are intersubjectively telling arguments. The idea that everyone else is out of step with the truth, is not a view to be assumed lightly.

Third, we may accord people freedom of conscience not for reasons relating to truth, but out of respect for their subjective feelings. If someone is, individually, deeply unhappy about something, it would seem churlish to disregard their feelings even if they cannot offer any telling arguments for their view (or if we feel that such arguments that they do offer, are poor). On the face of it, provided that their actions largely concern just themselves, we can surely respect their feelings, as we would a matter of purely subjective, but deeply felt, taste.

The second issue relates to a complexity of the material with which we are dealing. I have suggested that a critical rationalist approach to ethics should take up a methodological form of ethical particularism, but one in which the actions of each individual are, in principle, open to critical scrutiny from all other moral agents (in the sense, here, of people who have the capacity to evaluate them). I should, thus, aim to act in such a way that the action that I am taking would be judged as the right thing for me – a named individual, at a particular point in time, in particular circumstances – to do.

Now circumstances are, clearly, important here. If I am a member of a particular hunter-gatherer society in certain particular circumstances, and where the society has

in place certain particular traditions and conventions, what it may be right for me to do may be very different from how a different 'I' should act in other circumstances. The setting must be taken account of by those judging the action, as must, say, differences between what is obligatory and what is supererogatory. We also need to recognize that it is morally possible for societies to organize themselves in different, systematic, ways. That is, there are clearly different, morally possible, ways in which modern Western societies may be organized – compare, say, Sweden or the U.S. While each of these societies may be open to moral criticism and improvement, the kind of differences in the sort of individualism to which they give prominence are, surely, morally viable options; and the same is surely true of various other social formations. Further, what our history has been – both at a social and an individual level – may introduce significant path-dependency. We can also surely sympathize with Thomas Jefferson's judgement that slavery may be wrong, but not seeing what could have been done about it in the situation that he was facing, and with the kinds of cases that Thomas Nagel discussed in his 'Moral Luck' (Nagel, 1979), in which people may or may not face difficult moral issues simply as a consequence of how chance has affected their personal circumstances.

But what of the metaphysical status of ethics? The key features of the account that seems to me suggested by Popper's work, are, first its objectivism: we clearly may learn things, morally – both individually, and in terms of the history of mankind. This seems to me difficult to deny, yet – as McNaughton has argued in his *Moral Vision*<sup>31</sup> – to be a key argument in favour of moral realism. How should we approach such a view? I would suggest that we should look at it in cosmological terms; i.e. in terms of our offering an account of what the world might be like, such that there could be moral truths.

At the same time, we need first to appreciate the complexity of what is involved. As I have suggested, what is right for us to do will differ depending on not just the specifics of an agent's situation, but also on the kind of society in which we are living, and the sort of social and moral institutions that have been adopted within it (where clearly, various different kinds of systematic choices may have been made, or various different institutions and customs may have been inherited). Not only may this mean that *different* actions may be morally mandatory, depending on the institutional system in which someone happens to be living. But the fact that people may be living within a system which has undesirable features to it, may mean that people may recognize that there are some things which it would be right to do, but which, given their circumstances, it would require an act of almost reckless supererogation to actually undertake. All this – in line with the epistemological approach suggested above – suggests that our focus should be on actions in specific situations, and that it could well be the case that there are no true *general* moral principles.

But let us turn from this, back to the metaphysics of morals more narrowly understood. I suspect that any theory which would be adequate to our common-sense experience, and to the phenomenon of learning, needs to have the following features. First, there must be truth-makers. Second, these must have what one might call a Platonic rather than a Humean relation to our motivation – i.e. that if we

have the appropriate capacities to appreciate it, then our discerning of a moral truth brings with it a predisposition to act in accordance with it. At the same time, there are other things involved, too – our various other sentiments, concerns for our own well-being, aesthetic feelings, and so on. Inter-subjective appraisal, however, can operate so as to tell us what, all told, we should do – or, more typically, about the relative acceptability of different kinds of action, judgement etc.

It might be thought: but merely to set out such ideas shows that they are obviously untenable. And, clearly, it might turn out that such views will prove to be unsustainable (e.g. that an ‘error’ theorist such as J. L. Mackie might be right), or that all that can be sustained is a much less radical view (e.g. the kind of ethical realism that has been defended by Michael Smith (see Smith 1994, 2000)).

However, I think that we should not be premature in dismissing the more radical form of these ideas. Compare, in this context, Popper’s suggestion – from his ‘A Realist View of Logic, Physics and History’ – that we should fluff Plato’s beard before attempting reductions.<sup>32</sup> Popper’s argument – made in the context of metaphysics – was that we should be careful lest we adopt a premature reductionism (and especially a ‘philosophical’ reductionism, which gets rid of problems and issues by simply finding ways to avoid talking about them). Rather, we should initially explore matters in a realist mode, with an eye to grasping their full *prima facie* character, prior to attempting any form of reduction.

From such a perspective, a priority should be given to actual moral engagement, and to moral epistemology. As with Popper’s non-moral epistemology, we should concentrate on the growth of our knowledge – on what has been learned (and, I would suggest, in what circumstances). We might also treasure such diversity as currently exists, not so much for its own sake, but as offering different perspectives from which, if we approach matters in a spirit of critical rationalism – recognizing our own fallibility and that we might hope to learn from one another – we might, indeed, learn.

What, however, of wider epistemological and metaphysical issues? What, say, of J. L. Mackie’s arguments, in his *Ethics*, against exactly the kind of theory that I am suggesting that we should be seeking to develop? This is not the place for an extended discussion, but three comments seem to me worth making.

First, my argument here suggests that the time to engage fully with Mackie is not now, but only after substantive efforts have been put into the substance and epistemology of ethics, along the lines of the critical rationalist approach suggested above. While there is clearly no harm in philosophical discussion of matters of ontological issues taking place at any point, we can – I would suggest – only expect to get close to the truth about these things, when we know a lot more than we do, currently.

Second, part of the argument would be connected with Popper’s own case for *prima facie* emergence, more generally. That is, while I think that Popper’s arguments on this theme are best interpreted in the context of his ideas about fluffing Plato’s beard, and thus as tentative and as subject to subsequent attempts at reduction, Popper has argued more broadly for a non-reductionist view of cosmology, in which there is genuine emergence. If this can be sustained, then the idea that there

are indeed values with the kinds of characteristics that ethical realism would require them to have, would look less 'queer'.

Third, Mackie offers an epistemological argument against the existence of such values. If they existed, he asks, how could we have the knowledge of them? But his argument would seem to me to depend crucially on our willingness to adopt an empiricist epistemology of exactly the kind that Popper's work has furnished so many telling arguments against. This aspect of Mackie's case, it seems to me, does not pose a serious problem for the ideas being suggested here.

## Conclusion

In this paper, I have tried to survey and where necessary to reconstruct some features of Popper's approach to ethics. I have suggested that, while interesting, there are some problems which should be of concern to those interested in 'critical rationalism', and that there is a lot of work to be done. This relates to the substance of a critical rationalist ethics, to its epistemology, and also to its metaphysics.

In the latter parts of the paper, I have offered some suggestions as to the direction in which I think that critical rationalism should go. While I have made use, here, of some ideas from Popper's work, the substance is very much my responsibility, rather than anything for which Popper should be blamed. Of the arguments that I have offered, I would take the case for extending the inter-subjectivity of Popper's more general approach into ethics, to be the most important. The fate of the other, more speculative, suggestions I suspect may be less likely to survive intersubjective assessment.

## Notes

1. I would like to thank participants in the 'Rethinking Popper' conference for their comments on an earlier version of this paper, and also members of the graduate seminar on Popper's Political Thought at the ANU in our first semester in 2008, notably Cavit Hacıhamdioglu and John Shellard, for discussion on related issues.
2. For the Kantian parallels, see (Reiss 1970). For the recent revival of republicanism, which has some striking parallels to this strand in Popper's work, see (Pettit 1997) and Skinner (1998).
3. Cf., for example, his 'Public Opinion and Liberal Principles' in (Popper 1963).
4. Karl Popper, Emory Lectures 1956, Popper Archive, Hoover Institution, Stanford University, Disk 11, side 2. Tape 10, side B. The theme is similar in spirit to Pettit's concerns about the slave with an indulgent master. It should, however, also be stressed that Popper wished to minimize the role of the state, and that he also mentions the theme that the existence of the state is a source of humiliation to us – a decidedly un-republican thought! I have discussed these lectures more fully in (Shearmur 2004).
5. See (Popper 1963, Chap. 17, Sect. 3, p. 350). I would like to thank Alain Boyer for drawing my attention to this material which – including Popper's reference to angels – is very similar to that in the Emory Lectures.
6. Hoover Institution Archive, Mont Pèlerin Society Archive, Box 14.

7. Compare, in this context, Sect. 2 of (Popper 1976), and his impatience with non-realist philosophy in the face of human suffering (see the start of Chap. 2 of (Popper 1972)), and the end of Sect. 26 of (Popper 1976) – a view perhaps not unexpected from someone who had been involved in the translation into German of (Lenin 1927). See Popper Archive, 504.17. That this may have made an impact, may be seen also by Popper's 'Berkeley as a Precursor of Mach and of Einstein' in (Popper 1963).
8. Popper indicates in a letter to Ernst Gombrich on October 24th, 1944 that it was a paper that he was planning to write (Popper Archive 300.3); a version has now been published in (Popper 2008).
9. This is important in thinking about the feasibility of Popper's ideas; it also relates closely to concerns in his letters at the time; see, in this context, the material brought together under the title of 'Uniting the Camp of Humanitarianism', in (Popper 2008).
10. Compare the remark that he quotes from Pericles at the start of Chap. 1 of (Popper 1945): 'Although only a few may originate a policy, we are all able to judge it.'
11. See Popper's letter to Carnap of January 6th, 1947, now included in (Popper 2008).
12. See (Popper 1988), now reprinted as 'On Democracy' in (Popper 2008).
13. See Popper's note 'On the New Liberty', in (Popper 2008).
14. Popper, of course, emphasised the importance of discussion, and criticized what he called 'The Myth of the Framework' cf. (Popper 1994); but he also stressed that while we may learn from such discussion, we should not expect that it will lead to consensus. Cf., on this, his 'On Toleration' in (Popper 2008).
15. Cf. Bernard Williams' various criticisms of utilitarianism as being, sometimes, able to save certain phenomena of moral life, such as the significance of friendship, from within a utilitarian perspective, but by offering a rationale for them which was of an unacceptable kind.
16. I have also discussed these issues, with extensive documentation, in (Shearmur 1996a).
17. See Popper's 'Science and Religion', now in (Popper 2008).
18. I would emphatically not wish to endorse what seems to me the quasi-Cartesian resolution of this problem, which Singer himself advances in this paper.
19. See, for the identification and discussion of these, (Shearmur 1996a).
20. All that exist are notes, and some limited recordings.
21. See Popper's Emory Lectures, Tape 6, side A.
22. Popper to Michael Sharratt, 24th October 1974; the quotation is now included in the editorial introduction to (Popper 2008).
23. Compare, in this connection, his reference in material subsequently added to Note 5 of Chap. 5 of Popper, 1945, to (Russell 1949), and the idea that proposals can be discussed. Popper first cited Russell's paper in his 'Prediction and Prophecy in the Social Sciences', which appeared in the proceedings of the same conference, and has since been reprinted in (Popper 1963).
24. I would like to thank Hatha McDivitt for extensive discussion on this issue, upon which she is currently writing her Ph.D. thesis (from a perspective critical of the realism that I would wish to endorse).
25. As I argued in (Shearmur 1996a), Note 27 to Chap. 4, it is not clear that the interesting paper 'A Non-Psychological Justification of the Categorical Imperative' (which one might argue to offer what he should have said), is by Popper. We have notes from his New Zealand lectures on ethics; these, while interesting, are historical – and were given at a time when Popper's own views about the scope of reason would preclude his contributing much of his own. I do not know if anything remains from Popper's lectures on these issues at the L.S.E. There is also a range of material relating to ethics included in (Popper 2008), but it does not seem to me to add much to our substantive concerns.
26. Popper was willing to extend this – under the probing of Lord Boyle – to include wider enabling measures (see his response to Boyle in [Schilpp 1974]), while in his 'For a Better World', he suggested that the government might encourage workers' participation in industrial enterprises, experiments in community life by young people, and experimental schools. See (Popper 2008, Chap. 32, pp. 294ff).
27. See, for what seems to me an acute criticism (McGinn 1999).

28. I would thus suggest that it is Smith's *Theory of Moral Sentiments*, rather than Hegel (contrast, here, (Benhabib 1986)), who should be looked at as the key figure in the development of an account which stresses intersubjectivity – and thus as a precursor of both Popper and the later Habermas.
29. See 'The Myth of the Framework', now (Popper 1994), and 'On Toleration', in (Popper 2008).
30. Compare, for a brief account (Popper 1976, Sect. 33).
31. (McNaughton 1988). McNaughton's approach to moral realism seems to me, along with Adam Smith's *Theory of Moral Sentiments*, a key starting-point for a critical rationalist approach to ethics, despite his dalliance with Davidson.
32. Karl Popper, 'A Realist View of Logic, Physics and History', in (Popper 1972).

## References

- Artigas, Mariano. 1999. *The Ethical Nature of Karl Popper's Theory of Knowledge, Including Popper's Unpublished Comments on Bartley and Critical Rationalism*. Bern: Peter Lang.
- Bartley, William Warren. 1962. *Retreat to Commitment*. New York: Knopf.
- Bartley, William Warren. 1989. Rehearsing a Revolution. Unpublished.
- Benhabib, Sayla. 1986. *Critique, Norm, and Utopia: A Study of the Foundations of Critical Theory*. New York: Columbia University Press.
- Hacohen, Malachi. 2000. *Karl Popper: The Formative Years*. Cambridge: Cambridge University Press.
- Lenin, Vladimir. 1927. *Materialism and Empirico-Criticism*, London: Lawrence.
- McGinn, Colin. 1999. Our Duties to Animals and the Poor. In *Singer and His Critics*, ed. Dale Jamieson, pp. 150–161. Oxford: Blackwell.
- Mill, John Stuart. 1864. *Utilitarianism*. London: Longmans.
- McNaughton, David. 1988. *Moral Vision*. Oxford: Blackwell.
- Nagel, Thomas. 1979. Moral Luck. In *Mortal Questions*, ed. Thomas Nagel, pp. 24–38. Cambridge: Cambridge University Press.
- Pettit, Philip. 1997. *Republicanism*. Oxford: Clarendon.
- Popper, Karl Raimund. 1959. *The Logic of Scientific Discovery*. London: Hutchinson.
- Popper, Karl Raimund. 1963. *Conjectures and Refutations*. London: Routledge.
- Popper, Karl Raimund. 1966. *The Open Society and Its Enemies*, fifth revised edition. London: Routledge.
- Popper, Karl Raimund. 1972. *Objective Knowledge*. Oxford: Clarendon.
- Popper, Karl Raimund. 1976. *Unended Quest*. London: Fontana.
- Popper, Karl Raimund. 1988. Popper on Democracy: The Open Society and Its Enemies Revisited. *The Economist*, April 23: 111–119.
- Popper, Karl Raimund. 1994. *The Myth of the Framework*, London: Routledge.
- Popper, Karl Raimund. 2008. *After the Open Society*, London: Routledge.
- Reiss, Hans. 1970. Introduction. In *Kant's Political Writings*, ed. Hans Reiss, pp. 1–40. Cambridge: Cambridge University Press.
- Russell, Leonard James. 1949. Propositions and Proposals. In *Library of the Xth International Congress of Philosophy, volume I: Proceedings of the Tenth International Congress of Philosophy Amsterdam August 11–18, 1948*, eds. Evert Beth and Hendrik Pos, pp. 618–620. Amsterdam: North-Holland.
- Ryan, Alan. 1985. Popper and Liberalism. In *Popper and the Human Sciences*, eds. Gregory Currie and Alan Musgrave, pp. 89–119. Dordrecht: Nijhoff.
- Schilpp, Paul (ed.). 1974. *The Philosophy of Karl Popper*. La Salle, IL: Open Court.
- Shearmur, Jeremy. 1996a. *The Political Thought of Karl Popper*. London: Routledge.
- Shearmur, Jeremy. 1996b. *Hayek and After*. London: Routledge.

- Shearmur, Jeremy. 2002. Living with a Marsupial Mouse. *Policy Winter*: 19–22. See also <http://www.cis.org.au/policy/winter02/polwin02-4.htm>. Accessed 10 July, 2008.
- Shearmur, Jeremy. 2004. Popper's Emory Lectures. Unpublished. Delivered at 'Philosophy: Problems, Aims Responsibilities' conference at the University of Warwick in September 2004.
- Shearmur, Jeremy. 2007. Popper and the Empirical Basis. In *Karl Popper: A Centenary Assessment, Part 3: Metaphysics and Epistemology*, eds. Ian Jarvie, Karl Milford and David Miller, pp. 197–210. Aldershot: Ashgate.
- Singer, Peter. 1972. Famine, Affluence and Morality. *Philosophy and Public Affairs* 1(3): 229–243.
- Singer, Peter. 1974. Sidgwick and Reflective Equilibrium. *The Monist* 58: 490–517.
- Skinner, Quentin. 1998. *Liberty Before Liberalism*, Cambridge: Cambridge University Press.
- Smart, Ninian. 1958. Negative Utilitarianism. *Mind* 67: 542–543.
- Smith, Michael. 1994. *The Moral Problem*, Oxford: Blackwell.
- Smith, Michael. 2000. Moral Realism. In *The Blackwell Guide to Ethical Theory*, ed. Hugh LaFollette, pp. 15–37. Oxford: Blackwell.
- Sunstein, Cass. 1996. *Legal Reasoning and Political Conflict*. New York: Oxford University Press.



# Popper's Insights into the State of Economics

Joseph Agassi

**Abstract** Popper's ideas on economics are interesting regardless of his philosophy in general. They were taken as test-case for his theory of the demarcation of empirical science. This chapter begins with the popular criticism of his theory of science and proceeds to discuss economics within the framework of his views.

## Critique of Popper Left and Right

The Vienna Circle demarcated science as verifiable, Popper demarcated it as refutable. They launched against him the first popular criticism of his criterion of demarcation of science. It was general. It is also uninteresting, as it rests on a willful<sup>1</sup> distortion: they ascribed to him the new, Wittgenstein-style idea that he was demarcating the language of science. (Wittgenstein identified meaningful sentences with the body potential of scientific knowledge and the verified ones with actual science.) Popper, however, was following tradition here and discussing science within some given language. Since a reasonably rich language includes the negation of every sentence within it, refutability and verifiability are usually identical as demarcations of meanings there, so that by the distortion, Popper's demarcation was identical with that of the targets of his criticism. Indeed, both within tradition and within Popper's system, the negations of scientific theories are not scientific; by contradistinction, Wittgenstein and his Viennese followers took it for granted that as the negation of a meaningful sentence is meaningful, it is also scientific. Wittgenstein's fans have meanwhile gracelessly withdrawn their Wittgenstein-style views on language (for reasons that are irrelevant here); consequently, the early popular criticism of Popper's criterion of demarcation of science has silently sunk into oblivion. What remained is the idea, usually ascribed to Carl Hempel, that just as verification is impossible, so is refutation. This way it is possible to keep the idea that a theory has the same scientific status as its negation, even though the rationale

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for it is gone. Moreover, this idea is a way to dodge the observation that an observation report relevant to a theory can contradict it or follow from it, and this makes the observation report either disprove the theory or support it but not prove it. This is so whether the observation is true or false. Hempel, however, discusses the case of a true observation report, and stresses that this is never certain. This, obviously, is true but proves them in error, not him. For, it is the verification that they demanded that should be irrevocable and is not. As to the possibility of revoking refutations, such cases do exist, rare though they are; and they obviously tip the scale against the Vienna Circle, not against Popper.<sup>2</sup>

The current popular criticism of Popper's criterion of demarcation of science is different from the one that the members of the Vienna Circle launched against him decades ago. It concerns *ad hoc* hypotheses. I will soon discuss this issue. Here suffice it to notice that Popper proscribed *ad hoc* hypotheses and his popular critics declare them kosher. Hence, if these critics are right, then their criticism is of his proscription of *ad hoc* hypotheses, not of his demarcation of science. These critics say, since any refuted hypothesis can be mended *ad hoc*, there is no refutation. Not so: since they recognize the need for amendment, they already recognize the refutation.

Are *ad hoc* hypotheses kosher? The argument in favor of their liberal use is that researchers systematically do so. Their paradigm-case is from economics. This raises the question, are the users of *ad hoc* hypotheses behaving properly by the book?

Before going into specifics, let me place this discussion on the map of current philosophy. This is advisable, in order to avoid the confusion that Sir Alfred Ayer introduced when Sir Karl Popper gave his presidential address to the Aristotelian Society in 1958. We do not need you to tell us to be self-critical, he said plaintively. Assuming that this is true, that the received view of criticism is favorable, the complaint against Popper is the false supposition that his view of openness to criticism as a sufficient criterion for (scientific) rationality is also the received opinion. It is not: it is interesting and contested, and Ayer is one of those who contested it aloud. Popper's critics from the camp that Ayer was representing, from the left, deem criticism insufficient; in their view, being open to criticism is not (yet) rational, much less scientific. This is quite understandable: the tradition of science was always on the radical left; Popper's less demanding view, his middle-way, is hard for many even to consider, much less deliberate on, let alone adopt. The attack from the right is that criticism is not always good, much less necessary: one can, and often rightly does, deflect it by *ad hoc* hypotheses (or by some other ploy). The first person to launch this idea was Michael Polanyi, whom Adolf Grünbaum considered irrational. This is understandable, but somewhat exaggerated, since Polanyi argued well when he presented this view. Grünbaum's epithet better fits those who used Polanyi's ideas extravagantly: Kuhn, Lakatos and Feyerabend; they have indeed gone too far (Agassi 1999).

Of these three, Lakatos deserves the attention most, as he has referred to the practice of economists as his test case. He declared it the most glaring refutation of Popper: economists deflect criticism regularly, and they do so by employing *ad hoc* hypotheses and they express their license to do so by the famous expression,

*ceteris paribus*: when all things are equal. Their ideas hold, they promise, when all things are equal. Famously, they never are.

Since the scientific tradition is radical and thus extremely hostile to the use of *ad hoc* hypotheses, Popper's critics from the left should dismiss offhand this attack on him from the right as hyperconservative. They do not: today all criticism of Popper is viewed favorably, be it from the right or from the left, be it interesting or not. Let me not go further into the radical criticism here, as I have already done so in too many of my writings (Agassi 1977). Here let me dwell for a while on Lakatos' criticism from the right.

## The Place of *Ad Hoc* Hypotheses in Science

The best advocacy of the use of *ad hoc* hypotheses is still that of Henri Poincaré, although it is a century old. He said a few important things on it. First, he said, keeping one's theories vague so as to allow *ad hoc* modifications of their meaning renders them irrefutable.<sup>3</sup> Hence, he added, one also deprives them this way of empirical confirmation.<sup>4</sup> Lakatos was ignorant of all this. Second, Poincaré said, one has to be honest about one's strategy: pulling an *ad hoc* hypothesis out of a hat to save the day is deception, and so it becomes only pseudoscience. Third and last, he said, *ad hoc* hypotheses are useful means for the rescue of important theories like Euclidean geometry. On this he changed his mind, if tradition has it right, conceding defeat to Einstein in 1911. (At least he should have. As he died before the appearance of general relativity, he could not respond to it; most physicists agree, however, it makes the rescue of Euclid both unnecessary and untenable).<sup>5</sup> This does not exhaust the important discussion of Poincaré, but let me leave him for now. Clearly, the present context raises at least one basic question: does economic theory possess such wonderful ideas as Euclidean geometry that would deserve rescue by *ad hoc* hypotheses if need be? This is a sixty-four dollar question. Since the idea of Poincaré that *ad hoc* hypotheses are at times welcome is questionable, we should decide if it deserves rescue from criticism – by *ad hoc* modifications if need be. I do not know. If Popper's theory attains what Poincaré wanted, then perhaps not. Does it? This too is a sixty-four dollar question.

Two *ad hoc* modifications are available of the view that economic theory does not/should not allow for *ad hoc* modifications; they are still on the right wing, but (unlike those of Lakatos) they still make good sense. One is the suggestion of Sir Edward Evans-Pritchard to view the social sciences as historical. It is terrific; yet applying it all the way is too expensive. For, we want repeatable social phenomena explained, and their explanations would be generalizing, not historical. The other suggestion is due to J. J. Klant, perhaps also to the young Lionel Robbins,<sup>6</sup> to reconcile ourselves to the absence of tests in economics and so of confirmations too, and make do with plausibility instead. This suggestion is obviously most plausible.<sup>7</sup>

What, however, makes for plausibility? Surely, whatever answer to this question that we may offer is question-begging, as it should be subject to critical examination.

Hence, said Polanyi, for plausibility to be operative we should view it as a matter not for a criterion but for expert judgment. There is a lot to this idea, but science is too important to leave it to experts. And so we are stuck. If economists find a theory plausible and are satisfied with it, then they will possibly ignore Popper's advice to try and criticize it. Yet, many economists are dissatisfied and they do not quite know what to do about the situation. They feel frustrated.

Popper had something to say about plausibility. He has left us with a few terrific slogans to mull over. One of them is, better our hypotheses die than we: they die instead of us. This is obvious: we all agree that clinging to error has all too often led to calamity. He also said, all life is problem solving. This is not literally true, but it hints at his formula, start with a problem, seek a theory that may solve it, test that theory and face the problem that the test gives rise to, thus moving in an open-ended spiral. This is the backbone of his theory of science.

## Popper's Methodology

Popper's starting point was his posing of two problems, Hume's problem of induction and Kant's problem of demarcation. The problem of induction he worded anew: how is theoretical learning from experience possible? To this, the most popular answer is what I would call the Popper–Hempel theory<sup>8</sup>: theoretical learning from experience is through corroboration. What is this learning and why? This is very problematic ever since the only two good answers to it were refuted. The first good answer is, corroboration is verification. The second is that corroboration is either due to the truth of the corroborated hypothesis or due to accident, which is increasingly improbable. Einstein refuted these arguments by explaining the most impressive corroborations to Newton's theory as a result of its being a good approximation to his. Hempel did not explain why positive evidence signifies. Popper's view is that theoretical learning from experience is due not to positive evidence but to negative evidence, to refutations. This does not agree with most philosophers. They agree with Bacon: the intellect cannot rest on the negative, he said; it must rest on the affirmative. Popper said, by contrast, the affirmative is not to rest on but to take as a challenge. When some intriguing theory appears on the horizon it raises the question, is it testable? Can it develop into a testable version? This holds for generalized relativity and for string theory and for the diverse versions of current economic theory, of the general equilibrium theory – of the Austrian school, the Chicago school, or of Kenneth Arrow and Gerard Debreu – or of the diverse versions of Keynesian economics, from that of Michael Kalecki onwards. This shows how useful Popper's theory is, whether acknowledged or not.

The second problem seems to me more problematic. It is, to repeat, Kant's problem of the demarcation of science, that Popper found the deeper of the two. This always puzzled me, and on two counts. First, Kant never had any doubt that a theory is scientific if and only if it is demonstrated, and if it is in any doubt then it should be banned as counterfeit and no one has the right to put it on display.

Second, science is an elite club, and membership in elite clubs is no matter for serious philosophical discourse (Agassi 2007). What has alleviated my puzzlement somehow was Popper's observation that this is the problem of the limit of knowledge, which problem is indeed deep and we may ascribe it to Kant. Yet the puzzlement is still there. How do the two problems (of the demarcation and of the limits of science) relate? I do not know. Let me leave this problem for a while, since it is exegetic and exegesis is better avoided if at all possible.

Popper's *magnum opus*, his *Logic of Scientific Discovery*, is problem oriented, but the theory that he advocates there is not: he says find a testable hypothesis and test it. Why? Better find a problem that the testable hypothesis comes to solve! Which problem?

Problems abound, and Popper has demarcated scientific problems, as opposed to scientific theories, but only in his classes. He only alluded to this in his latest writings. A scientific problem is, why is this or that general fact as it is? For example, why is the sky blue? Why are we mortal? Why are all Cretans liars? This requires that we go into specifics. Let us begin this with recording Popper's assertions on physics and move to his assertions on economics proper.

## Popper and Economics

Begin with the least contestable case: the part of physics that is the permanent paradigm of science: astronomy. It was always kept distinct from astrology and deemed scientific as the general facts that it studied were: for a statement of a general fact to be scientific, it should be repeatable and repeated (and thus corroborated). The tests of general facts were much less systematic even in physics and in chemistry. This is hard to envisage now in these fields, but it is still obvious in medicine and in economics: they are easily available, but it is hard to say which of them is corroborated. And as long as we do not have simple corroborated general facts, we have no science Popper-style. Econometrics is a good example: it is full of empirical information, but little of it is stated in the form of general facts, and those are not testable because the *ceteris paribus* clause in them is not specified in a testable manner.<sup>9</sup> This is well known. Lakatos used it to prove that Popper was in error: as econometrics is scientific and yet untestable, Popper's demarcation is erroneous.<sup>10</sup> The trouble is, we do not know if econometrics is testable. Milton Friedman cut things short and said, economic theory makes useful predictions and that is good enough. The trouble is, these predictions are not corroborations: they are not outcomes of controlled efforts to refute any theory. If this last sentence is an error, then the economist who will list the tested economic theories and their corroborations will advance the cause of learning. As is well known, even the following big question is open: was the Tennessee Valley Authority project a test for any economic theory? (Neuse 1983).

What problems does economic theory attend to? Here we see a great difference between the natural and the social sciences: Newton wanted to explain the

behavior of the solar system and was puzzled by facts, mainly the deviations of planets from their prescribed Keplerian ellipses and the tides that seemingly defy gravity. Darwin was puzzled by speciation and by such phenomena as preadaptation. The problems that Smith and Ricardo came to solve are different: they concern maximization of wealth. Anthropologists since Morgan were interested in the technological superiority of modern societies over traditional ones (Jarvie 1964, p. 10). The great refutations of great theories of the natural sciences fueled progress in them. What are the parallels of these in the social sciences? Assume that the prediction of Marx that trade cycles will become increasingly disastrous follows from his version of Ricardian economics and that this version is close enough to the original (Simkhovitch 1912). Then the refutation of this prediction is possibly the best, perhaps the only, outstanding example of a significant refutation in the social sciences.<sup>11</sup> This however is of merely historical significance. Historically, in any case, the marginalist revolution that came soon after Marx was not due to any empirical refutation – of Ricardian economics or of any other theory – but due to its inherent difficulties. And marginalism is still agenda. The most central question today is one that most economists evade: is any of its versions, say, the neoclassical theory, is it refutable? If so, was it ever tested? And if so, was it refuted or not?

Popper said much too little on economic policy and less on the scientific status of economic theory.<sup>12</sup> On policy he said, government intervention is a necessary evil to exercise only in great need. Intervention through legislation, he added, is preferable to *ad hoc* measures (Popper 1988; Eidlin 2005). This is commonsense and so it is too vague. But it raises good questions. When more exactly should governments implement economic measures? What is the best balance between free market and government intervention? Still better, what kinds of government-intervention are there other than the traditional fiscal and monetary measures? The neoconservative view that welfare is for NGO's to administer is wrong, since welfare is a right. Popper took insurance to be a better example, it being hardly government-administered but government-controlled all the same (Popper 1945, i, pp. 159, 285–286). This invites more good questions. On scientific status, he said, among the social sciences economics is best, as its theories are most testable. This is questionable. The starting point should be, what corroborated general factual statements are available and are there testable explanations for them?<sup>13</sup>

## Empirical Economics

Empirical research starts with efforts to explain generalized empirical observations. Classical economics began differently: it began with Smith and Ricardo who were concerned with questions of policy. The central idea of Smith, on which classical economic theory rests, is that economic activity is no zero-sum game, to put it anachronistically. This is not trivial, as is clear from the fact that economists still tend to forget it. Smith also offered a practically significant theory: the process of capital accumulation is self-regulatory and is optimal under no constraints, under

no government intervention. This is a refuted generalization, and, like all refuted general facts, it requires not rejection but qualification<sup>14</sup>: under what conditions does it hold, in a precise or approximate version. This still awaits study.

Soon after the rise of classical economic theory, the demand for economic generalizations appeared forcefully. Great thinkers declared it, including J. B. Say, Nassau William Senior, and even John Stuart Mill, although he qualified it by saying, economics is an inexact science, whatever this may mean. This was put aside as the marginalist revolution took place. This revolution rested not on any empirical discovery but on a brilliant solution of some theoretical difficulties. The culmination of this revolution was the system of dynamic equilibrium of Léon Walras whom Joseph Schumpeter declared the greatest if probably irrefutable. It is no accident that Milton Friedman's verdict is still popular: "the major work that needs now to be done is Marshallian rather than Walrasian in character ... A person is not likely to be a good economist who does not have a firm command of Walrasian economics; equally, he is not likely to be a good economist if he knows nothing else" (Friedman 1955).

Prior to efforts to explain general facts, a commonsensical preliminary step might be to test them. The vagueness of general facts rests on their being context-dependent. In other words, they are all meant with unspecified *ceteris paribus* clauses, and until these meet with proper specifications<sup>15</sup> things are in a mess. And proper specifications mean here corroborated ones. What makes this impossible is built-in vagueness.

Example. The neoclassical theory declares non-intervention best. No economist recommends this idea, no matter what they say. Rather, they debate the question, what intervention is best, monetary or fiscal. And many other kinds of interventions are available, legislating incentives being the best known among them. What modification should the theory undergo so that it allows for intervention in the first place?

This is questionable: all liberal economists, except for those who espoused strictly anarchist views, agreed that government should safeguard the freedom of the free market. As governments print money, they cannot overlook the risk to the market that this involves and so they must control the supply of money.<sup>16</sup> This argument is valid and shows that it is impossible to draw the line between intervention that maintains the market and one that regulates it, although, clearly, some regulations, as for example, the law of compulsory education and the law of minimum wages, are less in line with classical liberal economic theory than laws prohibiting theft and controlling the return of loans.

Keynes was a socialist, yet he valued traditional liberal economic theory enough to suggest that his hypothesis of liquidity preference only adds to the exogenous list of tastes so that his theory still is within tradition, although it makes the classical theory depend on investment and so when investment is in danger intervention is welcome. If monetary intervention will do, fine; if not, then he recommended a fiscal one, even if it is extravagant. So, what general fact renders liquidity preference a part of economic theory? I do not know the answer.<sup>17</sup> When is the market sufficiently on the verge of collapse to necessitate intervention? I do not know.<sup>18</sup>



Most economists who care about this matter say, theory rests on one central general fact: firms maximize profits.<sup>19</sup> This is certainly not the case: when one flaunts counterexamples, and Bill Baumol for example did that (Wong 1975; Winn and Shoenhair 1988, pp. 154–158), then the response shows that profit-maximization is not open to tests. The quantity theory of money is another example, and, on the face of it, it is a tautology. Much of neoclassical and Keynesian economics are open to similar readings of the equations of economic theory as implicit definitions. They may also be read as hypotheses (Agassi 1971). Before reading them this way, it may be preferable to seek clear-cut general facts for them to explain. These are very easy to find if vagueness is allowed in the normal way and not more. This shows that Popper's methodology may be a powerful research tool in the hands of the curious.

One more methodological point that has needlessly troubled students of economic method: idealization. There is no need to consider it in general terms: all theories idealize, and all idealizations are distortions. It is all too easy to refute a theory by reference to these distortions. This is why Popper said, the informative content of a theory is the set of its potential refutations: it behooves us to decide and declare, what virtual evidence we deem refuting and what not. We would then not fail to notice that the increase of precision of our equations is a reduction of the degree of idealization that is an increase of informative content.

The way we test an idealization is by saying in advance what virtual deviations from observation we deem the outcome of idealization and so we will not consider refuting and what deviations are serious. The paradigm case of idealization is the ideal gas law or the Boyle-Charles gas-law. It holds for ideal gases, and these do not exist. Hence, it is vacuously true. Nevertheless, it was refuted and replaced by the version of it due to van der Waals, which version is recognizably superior to the older one, yet van der Waals found it unsatisfactory all the same. Nor can one say *a priori* what has brought the improvement about: was it a refutation or dissatisfaction with the idealization? The basic idea of the change is so simple that it does not have to wait for empirical refutation. Thus, the initial formula says that the volume of a gas at absolute zero temperature is zero, and van der Waals said that the minimal volume has to be a positive quantity.

To return to economics, Friedman<sup>20</sup> expressed the permission to idealize as the awkward demand not to test assumptions. He would have done better had he also said, how else the theory is tested. He said it had been tested, but he mentioned no general fact that corroborates it. He did mention facts that refute the theory but that we should discount. For example, he admitted that the assumption of the neoclassical theory that there is free entry to the market is false. One cannot open today a cigarette factory. To take a simpler example, one cannot open an aircraft factory as the cost is prohibitive. Now this is due partly to the constraints that the economy presents partly due to the production functions so-called. For, clearly, were it easy to produce aircraft in one back yard, things would be better. So we could say, the idea is that if opening a certain enterprise is very expensive, then most people cannot do it. This sounds a tautology, but it is not, since it refers also to credit. There is no need to discuss this here. I mention it at this juncture to illustrate how hard it is to take a general fact and present it in a properly testable way.

The general ideas of classical economics still prevail. Keynes, whose views are deemed the greatest deviation from the classical canon, saw his own ideas within it. He said, he added to the list of preferences the liquidity preference that precludes purchase of stock despite reductions in prices. And, preferences are traditionally given, or, as they say, they are exogenous. This is a sleight of hand: the preference for liquid money over assets is different from the preference of oranges over apples: it is derivative of the profit motive, not of the exogenous list of tastes. What Keynes took as given is that when the value of stocks falls drastically it reaches a point where all trust in the market vanishes and so people cling to their cash. This is the crash of the stock exchange. Indeed, Keynes' cure for this ill is the creation of incentives to spend, and in three ways. First and most famous and most controversial is his recommendation for governments to spend on public utilities, better known among economists as fiscal intervention in the market or deficit budget. This was radically altered when the neocons moved toward the support of deficit funding yet without increasing the budget for public utilities. Second was his suggestion to advocate and encourage spending on luxuries. The last speech of President John F. Kennedy to the nation before he was assassinated was a lecture in Keynesian economics that ended with the proposal to spend on luxuries. This proposal no economist will oppose, except for the puritans among them. The third proposal of Keynes was to raise inheritance taxes. These would encourage spending and reduce the distance between the rich and the poor, which appealed to Keynes, as he was a socialist. Now socialism is also a motive, and it can become an economic force in diverse ways, and these are open to study as well, but outside the frame of neoclassical economics and within the frame of institutionalist political economy that is a rival school within economics, and one to which Popper's followers should pay more attention.

## Situational Logic

Popper's general scheme for the social sciences is his situational logic. This rests on the traditional idea of the Enlightenment that people want their own improvement, that inherently there is no conflict between diverse individuals' goals, and that therefore the evils that people perpetrate are the unintended consequences of their actions. In the context of economics this is particularly significant when resources are absolutely limited. This takes us to the current ecological crisis.

The major difference between Popper's situational logic and the classical rationality principle is that it places stress on institutions. I will not discuss this here, especially in the light of John Wettersten's recent *How do institutions Steer Events* (2007). Let me notice, however, that when the above-mentioned nineteenth-century economists asked for general facts they did so in an explicitly inductivist manner and so they were not clear whether their demand included reports on extant institutions and their structure. In political theory, incidentally, it is generally assumed that the study of the administrative structures of governments is a special study that differs from other empirical aspects of political life. Is that also the cases in

economic studies? Economic history is much about economic institutions and legal aspects of economic life, including customs and custom regulations, the poor law, the corn laws, and of course, the social welfare laws. All empirical studies of economic systems relate to such items as a matter of course. Situational logic relates to individual actions within their settings or the circumstances – natural and social – and their success or failure. Here, we have room for discussions of motives, profit motives as well as the desire for economic security and more, and the interactions between diverse circumstances that cause unforeseeable results of given actions. Much of the economic literature describes circumstances, and these are often given as universal. Actions are often recorded in many empirical studies, some governmental, some commercial, some academic, all historical and usually not easy to generalize. Most of the academic works are econometric, and these are seldom repeated with repeatable results. What their contribution to scientific knowledge is one can hardly tell. The best Popperian advice, perhaps, is to present the most pressing economic problems, to state clearly the generalizations that serve as their backgrounds, and to try to state them as repeatable ones and test them. This, if Popper's advice is of any use, should help break the current frustration in the field of economic policy, a field that is of increasing significance for global politics.

## Notes

1. The distortion was willful: he denied it and they never did him the courtesy of recognition of his denial. (They were not obliged to endorse his denial, of course, but they were obliged to acknowledge it). See Agassi ([1993] 2008, Appendix to Chap. 10).
2. Repeatedly arguments in favor of Popper's ideas are presented as if they are against them; critics present them as if they are so obvious that they want no explanation. This renders rebuttal very awkward. This achieves victory in public platform where sleight of hand is permitted; not in the rationalist camp.
3. This careful wording of Poincaré protects his view from my criticism that devastates the views of his latter-day copycats. He proposed to keep the meanings of the initial hypotheses vague so that the *ad hoc* amendments are corrections not of their wordings but of their meanings: *ad hoc* hypotheses change the meaning of terms. This philosophy is sophisticated; copycats have a vulgar version of it. The first of these was Wittgenstein, whose (1922, Sect. 6.341) advocates the conventionalism of Poincaré on top of demanding utter clarity.
4. It is a sad sign of the times that this point that Poincaré made in passing (Poincaré [1902] 1952, Chap. 4). Popper tried to explain in a clearer and much more elaborate version yet with little success.
5. In 1905 Einstein replaced Euclidean space with Minkowski space, which is much less radical a departure than that of his 1917 theory that presents variable, curved space. The rescue of Euclid is still possible, as was known well before Einstein overturned it, as the deviation of Mercury from its Newtonian path is amenable to a known, minor *ad hoc* correction. He said a few times he had never considered that option, as he was averse to the use of *ad hoc* hypotheses.
6. Robbins ([1932] 1935), takes economic theory to be no more than common sense.
7. Klant (1984, p. 184): "Popper's criterion should be applied by setting the requirement that empirical scientists should strive to achieve falsifiable theories ... in practice, however, it is not always possible."

8. Hempel has won this title when he distorted Popper's view, presenting it as the idea that we learn from experience by failing to refute theories. See Hempel (2000, p. 203).
9. Commentators often take for granted that the *ceteris paribus* clause renders a theory untestable. This is careless: listing the *cetera* and limiting the list to controllable ones, has the theory ready for a test.
10. Ironically, this renders Popper's theory empirically testable and by Lakatos' precept also irrefutable.
11. The hesitation on my part is due to the possibility that the prediction of Marx is the result of an error in reasoning rather than a corollary of a refutable theory. See Robbins (1932). See also previous note.
12. This is the complaint first launched by Popper's old friend, Colin Simkin, in his (1993, p. 126; see also p. 25). See my review of it Agassi (1994). See also Boland ([1982] 2003, final two chapters), Boland (1997, pp. 253–255), and Sassower (2006, final chapter).
13. The need to begin with generalizations to explain is central to the view of science that Robert Boyle espoused in the scientific revolution as well as to Popper's views, yet he did not apply it as much as it is advisable.
14. This is the rule that Newton suggested in the end of his *Opticks*. It is a part of Popper's methodology.
15. See the previous note.
16. This is best seen in the fact that Ludwig von Mises recommended the return to the gold standard in order to abolish this intervention in the first place.
17. Simkin (1993, p. 160), quotes Douglas Hands to say economic theory is untestable. My question is different: are there any general facts that economic theory explains? Perhaps those that declare supply and demand functions monotone. This is a good place to begin with. Of course, these generalizations are refuted by the Giffen effect. This may be a very good point to start with in efforts to assess the complaint of Hands.
18. This is not to deny that for practical purposes commonsense may suffice here. Whether this is so or not – and I do not know if it does – it is obviously not good enough for the seekers of (scientific) explanations.
19. The best-known text for this is Paul Samuelson's classic (1947).
20. Friedman (1953) is the most influential text on idealization in economics. It allows for idealizing distortions by reference to the friction that Galileo's equation for gravity ignores, in disregard for the fact that the equation was improved upon by considering friction by Galileo's followers, from Newton to George Stokes and beyond.

## References

- Agassi, Joseph. 1971. Tautology and Testability in Economics. *Philosophy of the Social Sciences* 1: 49–63.
- Agassi, Joseph. 1977. *Towards a Rational Philosophical Anthropology*. The Hague: Kluwer.
- Agassi, Joseph. 1993. *A Philosopher's Apprentice: In Karl Popper's Workshop*. Amsterdam: Rodopi. Second edition, 2008.
- Agassi, Joseph. 1994. Popper Systematized. *Philosophia* 23: 345–354.
- Agassi, Joseph. 1999. Let a Hundred Flowers Bloom: Popper's Popular Critics. *Anuar* 7: 5–25. Available on-line at: <http://www.tau.ac.il/~agass/joseph-papers/flowers.pdf>
- Agassi, Joseph. 2007. The Scientific Status of Economics. *Divinatio* 26: 1–26.
- Boland, Lawrence. 1997. *Critical Economic Methodology: A Personal Odyssey*. New York: Routledge.
- Boland, Lawrence. [1982] 2003. *The Foundations of Economic Method*. New York: Routledge.
- Eidlin, Fred.. 2005. Popper's Social-Democratic Politics and Free-Market Liberalism. *Critical Review* 17: 25–48.

- Friedman, Milton. 1953. The methodology of positive economics. In *Essays in Positive Economics*. Chicago, IL: University of Chicago Press, 3–43.
- Friedman, Milton. 1955. Leon Walras and His Economic System. *The American Economic Review* 45: 900–909.
- Hempel, Carl Gustav. 2000. *Selected Philosophical Essays*. Cambridge: Cambridge University Press.
- Jarvie, Ian Charles. 1964. *The Revolution in Anthropology*. Chicago, IL: H. Regnery.
- Klant, Johannes Jacobus. 1984. *The Rules of the Game*. Cambridge: Cambridge University Press.
- Neuse, Steven. 1983. TVA at Age Fifty Reflections and Retrospect. *Public Administration Review* 43: 491–499.
- Poincaré, Jules-Henri. [1902] 1952. *Science and Hypothesis*. New York: Dover.
- Popper, Karl Raimund. 1945. *The Open Society and Its Enemies*. London: Routledge.
- Popper, Karl Raimund. 1988. *Popper on Democracy: The Open Society and Its Enemies Revisited*. *The Economist* April 23.
- Robbins, Lionel. 1932. Consumption and the Trade Cycle. *Economica* 38: 413–430.
- Robbins, Lionel. [1932] 1935. *An Essay on the Nature and Significance of Economic Science*. London: Macmillan.
- Samuelson, Paul. 1947. *Foundations of Economic Analysis*. Cambridge, MA: Harvard University Press.
- Sassower, Raphael. 2006. *Popper's Legacy: Rethinking Politics, Economics and Science*. London: Acumen.
- Simkhovitch, Vladimir. 1912. Marxism Versus Socialism. *P. S. Q.* 27: 73–91.
- Simkin, Colin. 1993. *Popper's Views on Natural and Social Science*. New York: E. J. Brill.
- Wettersten, John. 2006. *How Do Institutions Steer Events?* Aldershot: Ashgate.
- Winn, Daryl N., John D. Shoenhair. 1988. Compensation-Based (Dis)Incentives for Revenue-Maximizing Behavior: A Test of the 'Revised' Baumol Hypothesis. *Review of Economics and Statistics* 70: 154–158.
- Wittgenstein, Ludwig. [1921] 1922. *Tractatus Logico-Philosophicus*. London: Kegan Paul, Trench, Trubner.
- Wong, Robert. 1975. Profit Maximization and Alternative Theories: A Dynamic Reconciliation. *The American Economic Review* 65: 689–694.

# Popper and Sen on Rationality and Economics: Two (Independent) Wrong Turns Can Be Remedied with the Same Program

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**Abstract** Karl Popper and Amartya Sen have developed social theories, which are very close to each other, though neither has taken notice of the other. The independent programs they propose for the development of their theories go astray, because they build on standard economic methods, albeit in different ways. A better approach for the development of each program can be found by using Popper's important, but in the methodology of the social sciences hitherto ignored discovery, that rationality is social. Important contributions of Sen to economic theory may then be developed in ways which also contribute to Popper's social theory.

## An Alternative to Popper's Approach to the Methodology of the Social Sciences Is Needed

Although Popper said that economics was the best social science, the rationality principles which economists use are quite unrealistic: they presume (1) that individuals act in ways they cannot act at all, (2) a very narrow view of the aims of actions, and (3) that the sum of individual rational actions in a freemarket is a well-functioning system. Popper rejected these assumptions, but he nevertheless tried to save a version of the rationality principle, which was close to versions of it used in economics.

Popper took a clear position against (1) all functionalist versions of social scientific research, that is, all theories which presumed that societies were entities which could be treated as functional wholes which obeyed their own laws, against (2) all theories which sought historical laws of social development, above all, Marxist theories, and against (3) all those theories which claimed that social scientific theories had to be constructed with interpretative methods designed to look at events from "within".

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In *The Open Society and Its Enemies*, Popper developed a social philosophy which grew out of his studies of knowledge. But, he remained comparatively silent on individualist social science, especially economics, such as that practiced by his friend and supporter Hayek. He set the prime task for social science to be the discovery of the unintended consequences of rational action and he defended methodological individualism as the proper method for the social sciences. As Jarvie has pointed out, he portrayed his theory of science as social (Jarvie 2001). Although, Popper later praised economics as the best social science that we have, he did not explain himself. The closest he came to doing so was his defense of the use of the rationality principle in social science, a defense which raised difficulties, since he argued that this principle must be dogmatically assumed even though it did not correctly describe much of human behavior (Popper 1985). His argument is not only convoluted; it diluted his fallibilism and his realism (Wettersten 2006, p. 45ff).

A different approach to this nest of difficulties is to use new normative theories of rationality developed by Popper's followers to develop new descriptive theories of rational behavior. In the light of these theories, we may ask, can rational behavior be explained in some better and more hopeful way than the established rationality principles allow? (Wettersten 2006, 2007b).

On this approach the prime task of economic theory is the study of the consequences of rational action in institutional contexts, that is, the study of how specific institutions steer events by shaping the problems individuals pose, the solutions they select, and their critical methods for appraising both. This proposal fits far better not only with Popper's thesis of the limits of rationality and the need for the social sciences to discover unintended consequences of social policies, but also with his important thesis that rationality is social. It also fits quite nicely with Sen's studies of the needs of individuals to control their own fates and to take their institutional context into account. It extends the range of events which may be explained as rational in a realistic way, as Sen also wants to do.<sup>1</sup>

## **Popper's Defence of the Rationality Principle Ignores His Most Important Contribution to the Theory of Rationality**

Popper maintained that rational thinking is a social process of making conjectures and criticizing them so as to improve them. This process enables individuals working together to improve the knowledge of all. Without this no science is possible. But in his philosophy of the social sciences, he left aside his social theory of conjectures and criticism. Other than its appearance as a warning that even rational action can go astray and as a recommendation that social scientists should investigate unintended consequences of actions, it is not treated as relevant to social scientific explanation. He restricted social science to trace the consequences of individuals pursuing aims and following plans in accordance with their beliefs. This stance brought him very close to economists, where he wanted to be, but it failed to



integrate his fundamental discovery that we learn by criticism in interaction with others, into his methodology of the social sciences. Why should we ignore this crucial fact about learning and society in developing social scientific theories?

On the face of it, it seems that Popper desired to offer a methodological approach which would be simultaneously consistent with his own philosophy of the natural sciences, on the one hand, and with neoclassical economic research on the other. The result is curious. It does not merely ignore his significant discovery that rationality is social and critical, but it also is quite convoluted. He asserts that the rationality principle is “almost empty”. But it is hardly clear what that means. It sounds very positivistic where “empty” can mean “nonempirical”. But Popper does not accept any such theory. Social scientists should never deem the rationality principle to be refuted; they should presume it is true, when constructing models of social situations. The models, he says, should describe reality. For this reason, he claims to be offering a realistic theory of the social scientific research. But, he apparently views model construction itself as a rather ad hoc procedure: one seeks in various situations to build models. He gives no theory about whether models should be connected with each other, thereby building more comprehensive social theories, or how one chooses which social situations should be modeled.

Though at first glance Popper’s theory may appear to be internally inconsistent and/or inconsistent with his theory of the natural science, it is neither. But, it is very complex and says little that is useful about how to do social science. It is above all a defensive effort to reconcile his philosophy of the natural sciences with established economic methodology. Far more progress can be expected if we look for conflict as well, and then ask what will have to give.

## Sen’s Two-Sided View of Research in Economics

Before correcting Popper’s proposal for methodology in the social science by incorporating his view that rationality is social, on the one hand, and an explanation of how this correction may be used to develop Popper’s own social theory, on the other hand, I turn to the economic theories of Amartya Sen: He has developed virtually the same social theory as Popper and has also provided a poor approach to developing his theory because he clings to established approaches in economic theory. The social theories of Sen as well as those of Popper can be developed with the same methodological approach, because their perspectives are so close to each other, as I explain below.

Sen has observed that there are two kinds of research in economics today. On the one hand economists use rationality principles to construct elegant mathematical models. These models may presume the existence of systems in, or moving toward, equilibrium or they may attempt to describe a society with a proper distribution of goods – a so-called welfare function. On the other hand economists take account of social contexts which are not so easily put into the Procrustean bed of neoclassical economic theory. Sen’s view of progressive economic societies lies

on this side of the divide and is virtually identical to Popper's theory of the open society. His major interest lies in showing the limits of the former traditional side in order to make room for the latter progressive side. The latter is needed in order to understand economic development and to find some acceptable measure of social welfare. He hopes to preserve the ideal of the elegant, formal side of economics by extending it to take account of a wider range of rational behavior. Although he finds standard views of rationality limited, he not only does not reject them, he seeks to save them by extending the standard approach.

One of the crucial limitations of standard economic theory Sen finds is its ethical theory, that is, utilitarianism. For example, in *Ethics and Economics*, he gives an explanation of why the assumptions made in ethical theory are too narrow. The standard approach to economics requires that all rational behavior consists solely of attempts by individuals to maximize utilities. The utilitarian approach is thus needed in order to develop models of economic systems. Sen stresses that only a wider view of the rationality of human action can take into account the appreciation of values which individuals exhibit as well as their desire and ability to act autonomously by choosing their own actions. Actions are often pursued even at the expense of those sorts of well-being which are easily expressed as utilities. He finds that individuals have commitments, which are quite different from self-interest and these commitments in addition to the pursuit of self-interest guide their planning and choice of actions.

But, after convincingly arguing for this point of view, he adds that he hopes to extend the ethical theories of economics rather than to replace them. People do seek to maximize utilities, but that is not all they do. Sen does not say how the neglected aspects of moral, rational human behavior should be integrated with those that are taken into account in standard theories. But he emphasizes that he and others are working on the project of developing a more comprehensive and coherent view.

When discussing how we can tell whether individuals are rational, he offers no extension of standard views. He simply proposes that individuals are rational, if they have subjected their views to critical scrutiny. And when discussing justice, he does not hope to have a precise theory of the just society. But he says, it is sufficient if we can say that some conditions are quite unjust. A society which tolerates famine is unjust.

If we look at the contents which Sen places on the two sides of the division he describes between an imprecise description of economic behavior on the one hand and the formal apparatus used to develop economic models on the other, we can see that a new framework, not merely an extension of the existing framework is needed. The theory of rationality on the mathematical side is too narrow to take into account of the description of actual economic behavior. On the informal side of Sen's divide, we find such factors as the interest of individuals in controlling their own fate, their interest in both the process by which decisions are made and their autonomy in setting the direction they choose, and the need to take into account how real economic systems are regularly mixed, how, for example, family-based economic conditions interact with markets in specific societies. In his discussions of the elegant side of economics, Sen discusses above all how theories are limited,

because they do not take such factors as these into account. The theory of individual decision making does not take into account the importance of individuals in the process of decision making and attempts to find a social welfare function do not take into account the value which individuals place on the process by which decisions are made. The former should be explained and then incorporated into some proper welfare function, according to his program. When he comes to discussing what should be done, however, he says we need to take the facts more comprehensively into account. He does not say we need a new theory, though he does offer his own theory of the role of freedom in development as an alternative program.

## **Sen's Progressive Program in Economics Parallels Popper's Ideals**

Sen emphasizes autonomy, rationality as critical scrutiny, social evaluation as the identification of unbearable conditions, the importance of effective institutions for economic activity, the importance of taking unintended consequences into account, and the importance of democracy as a learning process which contributes to economic development. Both Popper's social theory and his theory of rationality fit extremely well with all of these points.

Sen contrasts rational behavior as postulated by standard theories of rationality with the behavior of individuals seeking autonomy. He sees this latter behavior as rational, but only in an intuitive sense. He offers no alternative theory of rationality which explains how and why such behavior is rational. He observes that it is not described by the standard principles of rationality which describe individuals as setting priorities and choosing those which have some desirable combination of the satisfaction of personal utilities and some probability of success. This theory is too narrow because individuals pursue aims which are not merely personal utilities. They attempt to solve problems in institutional settings which are defined not merely in personal terms but also in moral, family, or other social terms. Individuals have commitments which they use in making their plans.

Sen describes those problems individuals face in attempting to come to terms with their institutional contexts. He describes their desires and hopes to choose direction, rather than to simply have economic alternatives open to them in the sense of having various paths to financial well-being as measured in the amount of goods they have at their disposal. He also takes into account their desire for achievement and autonomy, their desire to solve problems.

The activities he describes are examples of the exercise of rationality as fallibilist theories envision it, that is, it is a problem-solving activity which involves learning from mistakes and setting new goals which would solve problems. Sen takes no notice of either this or the rationale these theories offer for viewing rationality as he does. Indeed, even though his numerous publications contain an unbelievable number of references, he avoids any mention of Popper or fallibilism. He never considers revising the rationality assumptions of established economic theory in

order to improve his research program. He merely notes that his contributions do not fit standard theories very well and expresses conviction that seeking to reconcile the differences will lead to progress.

From a moral point of view, the activities Sen describes are those called for in a fallibilist moral theory. They are autonomous activities which require that individuals take responsibility for their actions and learn from their mistakes. This moral view goes well beyond the utilitarianism to which economic theory is tied. Sen realizes this and hopes to extend utilitarianism, but this is not possible: the activities he describes are not merely extensions, but conflict with the moral judgment of the utilitarians. The normal theory can be extended in some easy ways. But, it cannot be treated as a catch-all for all moral perspectives. Sen claims that Nozick and Rawls each takes account of some important moral facts, but each ignores those facts which the other takes into account. But these so-called facts cannot simply be added together and then accounted for in some comprehensive theory. They are statements of competing moral perspectives.

### **Sen's Methods Cannot Reconcile Old Principles with New Results**

Sen hopes to reconcile standard approaches to rationality used by economists with his own innovations in economic theory by extending the former to include the latter. In doing so, he uses an inductivist method, pursues the theoretical ideal of a complete system, and presumes a functionalist social theory. In the end, he rejects all three as unrealizable: he knows that his inductivism cannot produce a holistic theory of a functionalist society. He suggests that we should approach as near as possible to the ideal until we find Arrowian inconsistencies, a procedure he calls "brinkmanship". We may then not have a perfect system, but we will have the best possible system.

Sen's inductivism is evident, above all, in his critical method. This method is to show that current systems do not take specific facts into account; they must be extended to remedy the deficiency. Although, he never explicitly states the inductivist assumptions that facts can be gathered without theoretical guidance and that all facts gathered should be incorporated into some ideal system, he offers no standards by which to judge whether some theory which does not take some facts into account should be deemed incomplete. Rather he adds facts he takes to be important. Although he uses his theory to gather and choose facts – when talking about freedom, for example, he argues that it is important for economic development – his criticisms of various alternatives are treated as mere observations that some criticized theory is incomplete. Nozick accounts for rights, and Rawls accounts for distribution of goods, so neither is wrong, but both are incomplete. Becker accounts for human capital as part of the market, but neglects the value of freedom itself. His view is not wrong, but incomplete. Sen does not explain why he dismisses the normal view that these are simply contradictory theories.

By demanding that a true theory should account for as close to all the facts as possible, Sen adds to his implicit inductivism the theory that the true theory will be an all-encompassing coherent system. A theory which accounts for all the facts will, we may presume, describe societies in comprehensive ways. He does not single out which aspects of some particular societies or of all societies he intends to account for. Having no standard to select those facts which should be explained as part of economic systems, and contending that theories are inadequate for not taking some facts into account, the only plausible interpretation of his critical approach is that it presumes that any social theory which fails to take some social facts into account is to that degree inadequate. This approach precludes the construction of adequate theories of aspects of societies. Each proposal should be subjected to the systematic analysis offered by the methods of the elegant side of economics. (He does this in essays in *Rationality and Freedom*.) If it fails to meet these standards, it must be extended to avoid inconsistencies.

Any holistic aim in the social sciences presumes a functionalist view of societies. Only societies which are functional, or functional under certain conditions, could be truly described by some comprehensive and systematic social theory. Functionalist social theory has been effectively criticized from both within and without the social sciences, but it is not surprising to find an economist taking it for granted. Neoclassical economic theory presumes that under certain assumptions societies can be described as well-functioning systems. Sen is not, of course, satisfied with a merely economic description, because he realizes that no economic description can be adequate which does not take into account the moral dimensions of human beings. Only then can their rationality be properly understood. But this does not lead him to question the functionalist assumptions of neoclassical theory, but rather to call for their further development. In doing so, he lands pretty much back where sociology under Parson's leadership was. A complete social system should be constructed, which can be applied to describing how each society functions. The only caveat is that no such system is possible, so we try to find out how close we can come to it.

The difficulty facing Sen's program for finding a social welfare function which his moral theory is intended to serve becomes clear at the end of his essay, "Social Choice and Individual Behavior" in *Freedom and Development*. He suggests that there are three reasons why one should not view the construction of a social choice function to be impossible. He wishes to answer each. The first reason for deeming such a function impossible is that Arrow's results show the impossibility of rational social choice. He suggests Arrow's negative result merely shows that not enough information has been incorporated. The remaining problem is merely one of incorporating enough information. He assures us that this is possible, but no test of this hypothesis is suggested. The second reason for deeming a social choice function impossible is that rational social choice cannot take account of unintended consequences. He suggests that the problem can be resolved, if one takes into account the unintended and predictable consequences of social action. He seems to assume that there is no serious problem in identifying these results, and no position is taken about the possibility of unpredictable unintended consequences or what the

consequence for theory construction should be if there are such. The third reason for rejecting the possibility of social choice function is that rational social choice does not take account of human motivation. He suggests that rational choice need not be so narrow as to be restricted to the pursuit of individual interests, that ethics plays important roles in all economic systems. But he does not explain how to extend the standard approach to take that into account.

## **Fallibilism Can Further the Programs of Popper and Sen**

Problems which Sen's studies raise include those of how to improve the opportunity and capacity of individuals to think better, of how institutions impact their capabilities to pose and solve problems, of how critical appraisals about what is to be done can be made, and of how one can cope with unintended consequences of social planning. All of these problems can be handled nicely in the context of a fallibilist theory which recognizes that all judgments are provisional, are made in social contexts which set parameters for them, and are subject to criticism in institutional contexts. These problems grow quite naturally out of the studies of both Sen and Popper. But, neither have developed methods for dealing with them within the most promising framework, perhaps the only framework, for dealing with them. This is a framework which builds on fallibilist theories of rationality. Unfortunately both Popper and Sen have concentrated on reconciling their own views with traditional views of rationality and economic methods. Popper has emphasized methodological individualism at the expense of his social theory of rationality. Sen turns far too much to attempts to put them in the Procrustean bed of standard economic theory.

An alternative program may avoid both the problems faced by Popper in his convoluted theory of the use of the rationality principle in the social sciences and those faced by Sen in trying to reconcile his progressive ideas with standard economic theory. This program takes account of (1) how institutions lead individuals to pose problems and to select solutions, of (2) how learning is hindered or furthered by institutions, thereby providing social accounts of rational practice in differing contexts, of (3) how institutions interact either by complementing each other, that is, by improving the ability of individuals to pose problems and solve them, or by hindering them (Wettersten 2006a, 2007b).

This program can be carried out if individual problem solving is viewed from the perspectives of the varying institutions in which problems are posed, solutions sought, and criticism of alternatives developed. Instead of using individual problem solving to explain institutions, we may use institutions to explain how individuals pose problems, how they solve them and how they critically evaluate alternative solutions to them. Institutions are not merely blocks in the road which have to be overcome after problems are posed or roads which make solving problems easy. They determine which problems are posed, which solutions are selected, and how individuals learn. The social theories of Popper and of Sen may be developed by the construction of empirical theories of how various institutions do these things.

## Note

1. Popper's theory that science is social grew out of the need to add methodological rules to a study of the logic of research. He discovered this need after he wrote his first philosophy of science. In his first view – *Die beiden Grundprobleme* without Chap. V—he presumed that basic statements were veridical and he ignored the possibility of ad hoc defenses of theories—as Reichenbach pointed out to him (Reichenbach 1930–1931). He then added methodological rules to remove difficulties which arose for his first view (Wettersten 1985, 1992, 2005). As Jarvie has recently emphasized, Popper developed his view of science as social in *The Open Society and Its Enemies* (Jarvie 2001; Wettersten 2006b). Joseph Agassi introduced the idea that rationality is partial (Agassi 1981) and Jarvie and Agassi together have developed this view by explaining the rationality of magic, dogmatism and irrationalism (Agassi and Jarvie 1987). But they do not make the study of varying rational practices in various institutional settings a task for the social sciences. The best description of Popper's theory of social scientific research in the sense of being the most sympathetic to him is Agassi's (Agassi and Jarvie 1987, pp. 119–150). But, he also ignores too much Popper's thesis that rationality is social. On my view in contrast to Popper, who said we should put as much as we can into the heads of individuals to construct social scientific explanations, we should put as much as we can into institutions to explain how they steer events. This should be done by, on the one hand, studying how institutions shape the problems individuals seek to solve, how they influence the content of solutions individuals choose, and how they enable or hinder the critical appraisal of both and, on the other hand, using results of such studies to explain consequences of institutional arrangements. Agassi's study of medical diagnosis carried out with Nathaniel Laor is an example of the research which the program here suggested advocates: They study social rules of diagnosis, the problems they pose for individuals, and the consequences of adhering to them (Agassi and Laor 1990). A further example of such a study is Michael Segré's portrayal of the decline of science in Italy after Galileo as a consequence of the institutions of the time (Segré 1991). For further discussion, see (Wettersten 1996, 2006a, c, 2007a, b).

## Bibliography

- Agassi, Joseph. 1981. Rationality and tu quoque Argument. *Science and Society*, pp. 465–476. Dordrecht: D. Reidel.
- Agassi, Joseph and Jarvie, Ian (eds.). 1987. *Rationality: The Critical View*. Dordrecht: Martinus Nijhoff.
- Agassi, Joseph and Laor, Nathaniel. 1990. *Diagnosis: Philosophical and Medical Perspectives, Episteme*. Dordrecht: Kluwer.
- Jarvie, Ian. 2001. *The Republic of Science*. Amsterdam/Atlanta: Ropopi.
- Popper, Karl Raimund. 1945. *The Open Society and Its Enemies*. London: Routledge (first published 1945).
- Popper, Karl Raimund. 1957. *The Poverty of Historicism*, London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1985. The Rationality Principle. In *Popper Selections*, ed. David Miller. Princeton, NJ: Princeton University Press.
- Reichenbach, Hans. 1930–1931. Comment on Popper's 'Ein Kriterium des empirischen Charakters theoretischer Systeme'. *Erkenntnis*, I: 427–428.
- Segré, Michael. 1991. *In the Wake of Galileo*. New Brunswick, NJ: Rutgers University Press.
- Sen, Amartya. 1960. *Choice of Techniques*. Oxford: Basil Blackwell.
- Sen, Amartya. 1986. *Food, Economics and Entitlements*. Helsinki: Wider.
- Sen, Amartya. 1987. *On Ethics and Economics*. Oxford: Basil Blackwell.
- Sen, Amartya. 1992. *Inequality Reexamined*. Oxford: Clarendon.



- Sen, Amartya. 2000. *Development as Freedom*. New York: Anchor.
- Sen, Amartya. 2004. *Rationality and Freedom*. Cambridge: Belknap.
- Sen, Amartya and Williams, Bernard (eds.). 1982. *Utilitarianism and Beyond*. Cambridge: Cambridge University Press.
- Wettersten, John. 1985. The Road Through Würzburg, Vienna and Gottingen. *Philosophy of the Social Sciences*, 15: 487–506.
- Wettersten, John. 1987. Selz, Popper and Agassi: On the Unification of Psychology, Methodology and Pedagogy. *Interchange* 18: 1–13.
- Wettersten, John. 1988. Külpe, Bühler, Popper. In *Karl Bühler's Theory of Language*, ed. Achim Eschbach, pp. 327–347. Amsterdam/Philadelphia: John Benjamins/Republic in the Rivivalist: [http://www.the-rathouse.com/Wettersten\\_on\\_Kulpe\\_Buhler\\_and\\_Popper.htm](http://www.the-rathouse.com/Wettersten_on_Kulpe_Buhler_and_Popper.htm).
- Wettersten, John. 1990. How Can Psychology and Methodology Be Integrated? *Zeitschrift für allgemeine Wissenschaftstheorie* 21: 293–308.
- Wettersten, John, *The Roots of Critical Rationalism*, Schriftenreihe zur Philosophie Karl R. Poppers und des kritischen Rationalismus, Kurt Salamun (Ed.) (Amsterdam und Atlanta: Rodopi, 1992).
- Wettersten, John, 'Eine aktuelle Aufgabe für den kritischen Rationalismus und die Soziologie', in Hans-Jürgen Wendel und Volker Gadenne (eds) *Kritik und Rationalität* (Tübingen: J.C.B. Mohr(Paul Siebeck) 1996), pp. 183–212.
- Wettersten, John, 'New Insights on Young Popper', *Journal for The History of Ideas*, 66, 4, Oct. 2005, pp. 603–631.
- Wettersten, John, *How Do Institutions Steer Events? An Inquiry into the Limits and Possibilities of Rational Thought and Action* (Aldershot: Ashgate Publishing Co., 2006a)
- Wettersten, John, review-essay of I.C. Jarvie, *The Republic of Science*, *Philosophy of Science*, 73, 1, Jan., 2006b, 108–121.
- Wettersten, John, 'Towards a New Theory of the Closed Society', in eds. Ian Jarvie, Karl Milford, and David Miller, *Karl Popper, A Centenary Assessment*, Vol. I, *Life and Times, and Values in a World of Facts* (Ashgate, 2006c): pp. 251–262.
- Wettersten, John, 'Popper's Theory of the Closed Society Conflicts with his Theory of Research', *Philosophy of the Social Sciences*, 37, 2, June 2007a, pp. 185–209.
- Wettersten, John, 'Philosophical Anthropology Can Help Social Scientists Learn from Empirical Tests', *Journal for the Theory of Social Behavior*, 37, 3, Sept. 2007b, pp. 295–318.

# Popperian Selectionism and Its Implications for Education, or ‘What To Do About the Myth of Learning by Instruction from Without?’

Joanna Swann

**Abstract** Few educationists are prepared to countenance that in learning *all* new expectations and other ideas are created wholly from within the individual – that is, by the learner. It is generally assumed there is some transference of information to the learner from the social or physical environment, and the processes of interpretation and construction take place after this basic information has been passively received. Karl Popper’s evolutionary epistemology challenges this assumption. Most academics refuse to take Popperian selectionism seriously. Even among those who are familiar with Popper’s epistemology, his characterization of learning as ‘imaginative criticism’ goes unheeded; the critical dimension of learning is, of course, acknowledged, but not the centrality of imagination. The case for Popperian selectionism and against the idea of learning by instruction from without needs, therefore, to be restated. This is the first of two tasks addressed in the paper. The second is that of challenging most of what takes place under the banner of formal education.

## Introduction<sup>1</sup>

I am very grateful to Zuzana Parusnikova for inviting me to give a presentation on Rethinking Popper. As an educationist, I have used Karl Popper’s evolutionary epistemology in the pursuit of the improvement of educational practice. I’ve drawn on his epistemology to challenge widespread assumptions about learning, particularly those embedded in the organisation and conduct of teaching. In short, I have been more concerned to exploit and disseminate Popper’s ideas than to criticise them.

As Popper did so much in his lifetime that is of value to humankind, it seems churlish to suggest that he didn’t quite do enough. Nevertheless, I propose that there is a crucially important aspect of learning – the creative aspect – which Popper did not explore in any depth (at least not in his published works). Popper clearly acknowledged that learning is a creative process, and a theory of creativity in

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learning can be drawn out of, and developed from, his evolutionary epistemology. But the function of *criticism* in the growth of knowledge was his primary concern; criticism, rather than creativity, is at the heart of his non-inductivist and anti-justificationist philosophy. Popperians too have largely focused on the significance of criticism in the development of objectified knowledge, and they, by and large, have neglected to explore creativity as a necessary feature of any situation in which learning can be said to have taken place.

This conference provides an opportunity for a collaborative exploration of Popper's selectionist theory of learning, and its implications both for the organisation of education and our practice as teachers. We are all teachers in that there are occasions when we speak to and show things to others with the intention that they may learn; to be human is to engage in (and receive) teaching of this kind. Many of us are also teachers in the formal sense; that is, we have a professional role as teachers in an educational institution.

In this presentation I will not assume that everyone at this session accepts Popper's published ideas about learning, nor even that everyone has a good understanding of them. So I will begin by outlining Popper's selectionist theory, or, rather, my interpretation of it, including my analysis of creativity and criticism in learning. I'll then summarise the principal arguments in defence of Popper's selectionist theory and against what seems to be the most common alternative view – which embraces what I disparagingly refer to as the myth of learning by instruction from without (Swann 2007b). Finally, I will outline the educational implications of my discussion and I'll address the question of what is to be done about the myth.

## Learning by Instruction from Within

The following quotations summarise Popper's thesis with regard to learning:

We do not discover new facts or new effects by copying them, or by inferring them inductively from observation, or by any other method of instruction by the environment. (Popper 1994, p. 9)

[W]e learn *only* through trial and error. Our trials ... are *always* our hypotheses. They stem from us, not from the external world. *All* we learn from the external world is that some of our efforts are mistaken. (Popper 1999, p. 47)

The process of learning, of the growth of subjective knowledge, is always fundamentally the same. It is *imaginative criticism*. This is how we transcend our local and temporal environment by trying to think of circumstances *beyond* our experience: by criticizing the universality, or the structural necessity, of what may, to us, appear ... as the 'given' or as 'habit'; by trying to find, construct, invent, new situations – that is, *test* situations, *critical* situations; and by trying to locate, detect, and challenge our prejudices and habitual assumptions. (Popper 1979 [1972], p. 148)

Although Popper was vehemently opposed to the discussion of words and their meaning (Popper 1992 [1974], Sect. 7), my experience in talking about learning with educationists has led me to accompany any exposition of a Popperian view of learning with what I term an evolutionary definition. I propose that learning

is best defined as what takes place when an organism develops, in the context of experience, new expectations, specifically expectations that are not purely an outcome of genetic inheritance or random mutation (Swann 2003b, 2008). Perhaps I should stress that the word 'expectation' is used here both for conscious and unconscious phenomena; and it is not confined to ideas which the organism – the individual – would itself be capable of articulating.

To clarify what is not learning: An amoeba may respond evasively when it encounters a toxic fluid; that is, its movements are not entirely random. Its response to this kind of situation can become more efficient, in that the period of time it takes to run through its repertoire of genetically programmed behaviours can become shorter. But aside from this efficiency gain, the repertoire remains unchanged (Petersen 1988, pp. 34–35). An amoeba solves problems, but it is not a learning organism because it does not expand its repertoire of expectations and responses beyond those that are genetically programmed.

You might wish to suggest that there is sleight of hand in what I have said so far, namely that I am defining learning in terms of newness, so it is hardly surprising that I argue that creativity is central to learning. But it seems to me that some kind of newness is inherent in what people are mostly talking about when they talk about learning. The issue about which there is the most widespread disagreement concerns the process by which the newness comes about, namely what happens when learning takes place.

Significantly, changes in expectation are accompanied by changes in what the organism is capable of experiencing and in its potential responses to its environment – what it is capable of doing and is inclined to do. An ability to learn is a specific form of adaptability, and the ability to adapt confers, at least potentially, an evolutionary advantage. Although learning is dependent on maturation, it involves more than the fruition of inborn characteristics. A learning organism develops new expectations and capabilities that may enable it to function more effectively in the situations in which it finds itself.

According to Popper's selectionism, the process by which expectations develop *always* involves trial and error-elimination, a critical and creative process of explicit or, more often, implicit problem solving. All our knowledge is either inborn or created by us through a process of trial and error. A learning organism is one that, so to speak, instructs itself from within – in the face of challenges from without. The effect of the social and physical environment is purely one of *selection*, the environment serving merely to challenge the learner's expectations (implicit and explicit) and potentially eliminate them.

The response of many people to this idea is incredulity: surely, they say, we do not engage in trial and error-elimination when listening to a set of instructions, chatting to our friends, and so on. I would argue that we do. Despite the many occasions when we are aware of our learning, most moment-by-moment learning takes place without our being aware of what has been learned, or even that learning has taken place. In order to understand what someone is saying we must solve a variety of problems; this we do rapidly and mostly at an unconscious level. We do not passively absorb what is said and there is no direct transfer of information. Whether or not we are aware of it, we are critical and creative listeners. In general, when learning

is seen to take place *in response to instruction* (which is distinguishable from learning *by instruction* – Swann and Burgess 2005), the learner will have made and tested informed guesses with regard both to the nature of the situation and what the teacher was trying to convey.

You may be disinclined to acknowledge the imagination required to engage with what I'm saying, not least because such an activity is commonplace and unremarkable. The process itself is not entirely conscious, so you will not be aware of more than a few aspects of it. But I put it to you that the process of listening and making sense of what is being said is hugely creative.

## Creativity and Criticism in Learning

It seems likely that most participants at this conference will have some familiarity with Popper's simplified schema of conjecture and refutation (Popper 1979 [1972], p. 243), but I need to re-present it here in order to highlight the way in which the process it describes is both creative and critical:

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

In this schema *P* represents a problem, *TS* a trial solution applied to the problem, and *EE* stands for error-elimination, the means by which some trials are eliminated. The elimination of an error gives rise to a new situation and, potentially, a new problem.

For an organism to have a problem, it must be dissatisfied with, and thus implicitly or explicitly critical of, the state of affairs in which it finds itself – 'hungry', for example – and it must wish, by implication at least, to achieve a state of affairs it deems to be better, such as 'not hungry'. But, contrary to common understanding, problems have to be created rather than merely 'identified'. It should be noted that many mismatches between expectation and experience remain unproblematised, and any one mismatch may be turned into a number of different problems (Swann 1999, 2003b).

Creativity may also occur at *TS*, when the organism responds to a problem with a trial solution: the learning individual may invent a course of action, a trial solution, which is entirely new to it, or it may adapt (rather than – as in the case of the amoeba – merely 'contract') a course of action that has already been developed.

Error-elimination, *EE*, though clearly a critical aspect of problem solving, may also – in the case of some human learning – be the result of a creative act. While most animals are largely constrained to discover errors and limitations by happenstance, humans are well equipped to search for error, that is, to create test situations. This is one of the principal means by which we accelerate our learning. In general, by discovering error and specific limitation, we are put under pressure to create new problems, and with them new solutions.

Criticism is often construed in terms of explicit statements of disapproval or rejection, such as 'That's wrong' and 'I've made a mistake'. But this view is too

narrow. Criticism is also embedded in the thought that ‘I could try something else’ or the question ‘What else might I do?’ And it is present in any unstated and even unconscious degree of dissatisfaction that leads an individual to decide to do something rather than nothing, or to do one thing rather than another. In general, the value of criticism is to be judged by its effect in a particular circumstance. A criticism, even if valid, may be inappropriate if ultimately it serves to stifle creativity and inhibit further trial and error-elimination (Swann 1983).

## **The Myth of Learning by Instruction from Without**

Many educationists adhere to the idea that learning is an active process requiring of the learner a personal interpretation of experience and the construction of her or his own knowledge. Yet few educationists, it seems, are prepared to countenance that in learning *all* new expectations and other ideas are created wholly from within the individual – that is, by the learner. It is generally assumed there is some transference of information to the learner from the social or physical environment, and the processes of interpretation and construction take place after this basic information has been passively received. With regard to hearing what someone says, the common assumption seems to be that some basic elements of what is being said are transferred, and what the effective listener then does is process these elements in such a way as to construct meaning.

What is at issue here is a choice between two competing theories. One proposes that ‘No learning takes place by instruction from without’, the other that ‘Some learning takes place by instruction from without’. Although both theories are about events in the world, neither has the potential to be refuted by reference to empirical evidence. The competing theories are, in the Popperian sense, metaphysical rather than scientific, though they may nonetheless lead to the development of testable, that is, refutable, hypotheses (Swann 2003a).

Perhaps you are thinking: ‘If the competing theories which support, respectively, “No learning takes place by instruction from without” and “Some learning takes place by instruction from without” are not susceptible to refutation by reference to empirical evidence, why should we care to argue about them?’ To which I would respond that the competing theories, regardless of their metaphysical status, have implications for what we do, and that the practice of education assumes to a large extent that some learning takes place by instruction from without. I further propose that if education were to be reconceived and redesigned in light of Popperian selectionism and the idea that no learning takes place by instruction from without, then the outcomes would be both significantly different and arguably better.

Before looking at the implications of Popperian selectionism for educational practice, there are three important philosophical points to be made against the idea that some learning takes place by instruction from without.

First, whereas Popperian selectionism presents a biological description of what happens when learning takes place, the idea that we receive informational elements

(sense data) from the environment relies heavily on analogy and metaphor. The learner is conceived as functioning like a sponge into which ideas are absorbed, a bucket into which ideas are poured through the senses, a receiver to which ideas are transmitted, or a computer into which data is transferred and stored. But, quite clearly, we are not sponges, buckets, radio receivers or computers; nor do we have a brain or any other organ that functions in such a manner that the use of these analogies would imply. Our memories, for example, are an outcome of learning and they are used in processes where there is learning, but there is no memory organ. The function of the brain is to select and create; it has no means of taking in information (Edelman 1992). It is surely incumbent on anyone who wishes to support the idea that there is learning by instruction from without the learner to provide an account of the process by which the informational elements are received, one which goes beyond analogy and metaphor to address the nature of learners as biological phenomena. As yet, no such account exists of learning by instruction from without.

It might be argued that associationism constitutes a refutation of my claim, on the grounds that it is an account of learning by instruction from without. There are, of course, numerous critiques of associationism (see, for example, Popper 1979 [1972], Chap. 2), but what is important to the present discussion is that associationism is a theory about how basic informational elements come to be combined in the 'mind'. What associationism does not provide is any kind of biological account of how these various irreducible elements – the elements that are combined – 'enter the mind' in the first place.

I am aware that Popper's selectionist account of learning has itself been construed as a metaphor because, for example, he proffers the idea of the mind as a searchlight as opposed to the mind as a bucket (Popper 1979 [1972], Appendix I). But although Popper uses the searchlight metaphor, he does so within the context of a much broader theory, a 'descriptive epistemology' (Campbell 1974, p. 413) which explains that the *same* process, not merely a similar process, applies to the growth of objective knowledge, individual learning, and the evolution of species. This process was summarised by Popper in his simplified schema of conjecture and refutation (as mentioned earlier). Evolutionary epistemology goes far beyond simple metaphor, and its nature was well described some 34 years ago by Donald Campbell (*ibid.*):

An evolutionary epistemology [is] at minimum an epistemology taking cognizance of and compatible with man's status as a product of biological and social evolution. ... evolution – even in its biological aspects – is a knowledge process, and ... the natural-selection paradigm for such knowledge increments can be generalized to other epistemic activities, such as learning, thought, and science.

Second, I invite anyone who wishes to defend the idea that there is learning by instruction from without the learner to make a case for the existence of basic informational elements (or sense data). Note also that when basic informational elements are assumed to exist, they are generally also assumed to provide true or certain information. So the challenge is to identify basic informational elements that are in some way true or certain. Given what we currently know, I put it to you that, in Popper's words, 'these data or elements do not exist at all. They are the inventions of hopeful philosophers' (Popper 1979 [1972], p. 63).



Third, if you would wish to argue that learners receive basic informational elements from the environment, I would ask you to explain why in any particular situation individuals receive some informational elements and not others. I anticipate that one type of explanation would refer to flaws in the 'receiving mechanisms' of would-be learners. Explanations of this kind, however, usually beg the question of why people with sensory disabilities or specific limitations of brain function are often highly successful learners; that is, they compensate for what they cannot do by developing what they can. This propensity is one we all possess. Also, an explanation is required to account for the fact that even when there are no assumed flaws in the learner's receiving mechanisms, not everything that could be absorbed is taken in. Selection seems to be a fundamental part of the explanation.

## What If This Is Correct?

It would be naïve of me to think that any persons present who have hitherto assumed that, at least to some degree, learning involves the direct transference of informational elements from the social or physical environment will be persuaded to change their mind as a result of what I have said. I hope, however, that even if you do not agree with the general thrust of my argument, you will be willing to consider its implications.

If learning by instruction from without is a myth, and if it is better to view learning as invariably a critical and creative process of trial and error-elimination, then the advancement of learning through education and teaching is not a matter of presenting people with basic information and helping them to interpret and utilise it. Rather, it is a matter of encouraging people to create ideas and test them. To return to one of the earlier quotations from Popper (1979 [1972], p. 148), imaginative criticism is the means by which we 'transcend our local and temporal environment'. I posit that formal systems of education generally do very little to bring about the transcendence that Popper describes. Of course, students learn when they are taught – they could scarcely do otherwise – but what they learn is often not what their teachers intend. And, more importantly, we need to entertain the idea that students' learning may be significantly less than what it might be. We should not be complacent about the success of our education institutions; we have, I believe, the potential to 'do education' much better.

Traditionally, educationists have, in general, vastly underestimated the human potential for imaginative criticism – because they have not recognised the extent to which it lies at the heart of what we, including the youngest children, do in order to succeed at even the most basic tasks. When educationists act on the assumption that there is some direct transference of information from the environment to the learner, they tend to focus their efforts on what they think they can control – on their own input as policymakers and/or teachers. The natural inclination of children to engage in exploratory activity – spontaneous trial and error-elimination – is then curtailed. What cannot be controlled is viewed with suspicion rather than treated as the primary resource.

It follows that teachers initiate most of the planned learning activities, and students are expected to rely on teachers for fundamental decisions about what to do and when to do it. Mostly, too, students are not encouraged to question or criticise the material with which they are presented. The task of the student is to learn the syllabus, not question it. For much of the time, students are expected to replicate the arguments of others rather than develop arguments of their own. Creativity is a concept largely confined to 'the arts', and outside of those fields of activity (and to some degree even within them) it is not usually encouraged. Not only are opportunities to engage in trial and error-elimination limited, many schools are also unsafe places in which to discover errors and inadequacies because revealing them tends to occasion a penalty of some kind. Teachers are put under pressure to produce individuals who are able to perform a limited range of tasks according to narrowly conceived standards. In such circumstances, the tendency is to penalise the student for failing to understand, failing to give the prescribed answer, failing to agree, failing to conform.

Formal education worldwide is largely controlled and organised by people who wish, perhaps for all the right reasons, to instruct, people who are preoccupied with a desire that children, adolescents and older students, learn specific things, things that they, the controllers and organisers, deem it important to teach. Thus a major obstacle to the exploitation of Popperian selectionism in educational practice is that those who wield the most power within formal systems are largely disinclined to favour the open-ended pursuit of learning and they are not predisposed to encourage either imagination or free-ranging criticality.

## So What Is To Be Done?

One fairly common response to the arguments I've outlined in this presentation is to say that Popper was wrong and that the substance of the discussion is without 'justification'. There are, of course, critiques of Popper's work, but the rejection of his ideas is a more complex matter than is often implied when the citation of a critique is a precursor to the hasty dismissal of his arguments (see Burgess and Swann 2003 for extended discussion of this topic). In contrast, a fairly common reaction on the part of Popperian thinkers is to suggest that Popper's thesis with regard to learning is 'old hat', that it is widely known and does not need restating. *If only this were true!*

With regard to the educational implications of Popper's selectionist account of learning, there seem to be at least two distinct camps among Popperians. Members of one camp, the one to which I belong, support a learner-centred approach to teaching. In my own work, following that of Tyrrell Burgess (1975, 1977, 1979), I argue for the development and adoption of student-initiated curricula, that is, curricula conceived and formulated by the students themselves. I have argued that student-initiated curricula are crucial in fostering learner autonomy, in challenging students to develop the assumptions that influence their everyday lives, and as a means of helping students to deal with their learning problems (that is, what they want to learn but are having difficulty learning) (for example, in Swann 1983, 2006, 2007a).

Members of the other camp, seemingly the larger of the two, comprise those who favour what Richard Bailey has called the ‘criticalist curriculum’ (Bailey 2000, pp. 192–197, 199–206). The criticalist curriculum is a means by which ‘children are introduced to the common culture and “the best that has been known and thought in the world”’ (p. 194). It differs from conventional school curricula in that critical discussion of public knowledge is a crucial element. By being initiated into the practice of critical discussion, students become able to ‘re-assess’ and facilitate the development of their cultural heritage – but, I stress, only on the basis of someone else’s decision about the canon.

I don’t propose to end this presentation with an argument in support of student-initiated curricula, although I will certainly be inclined to defend the concept if required to do so during the open discussion. But in preparing the presentation it seemed that in order to advance the debate, Popperian educationists perhaps must first focus on a more basic educational problem, that of how to foster imaginative criticism – which I interpret as how to foster creativity and criticality not only in the development and testing of solutions to existing problems but also in problem formulation. This is a practical problem, hence its solution requires us to act, not merely talk about the issues or about what others might do (Swann 2003a).

Towards a solution:

1. I urge Popperians to promote, among educationists, discussion of Popper’s key ideas about learning, as summarised in the set of three quotations presented earlier in this discussion (at the beginning of the section “Learning by Instruction from Within”).
2. I urge all those who take the view that *everything the student thinks and does that is not inborn must have been created by the student (largely as a reaction to the challenges posed by the environment)* to consider what this means for their own practice as teachers and, if appropriate, as participants in an education system.
3. I urge those who hold this view not only to think but to act, to engage in trial and error-elimination (a) with regard to their interactions with others – by adopting and responding to the following personal practical problem: ‘In my educational relationships, how can I support the creativity and criticality of others?’, and, if appropriate, (b) in their engagement with social structures – by adopting and responding to: ‘How can I promote the development of educational structures that foster creativity and criticality?’ In respect of point (b), it seems to me essential that we counteract the propensity to corral children and older students in environments that inhibit autonomous activity, that discourage creativity and criticality and generally limit opportunities for trial and error-elimination.

## Note

1. In addition to the references provided in the main text of the chapter, sections of the argument draw on Swann (2007a, b, 2008 and, with Burgess, 2005).

## References

- Bailey, Richard. 2000. *Education in the Open Society – Karl Popper and Schooling*. Aldershot: Ashgate.
- Burgess, Tyrrell. 1975. Choice is not enough – go for responsibility. *Where (the Education Magazine for Parents)* 100: 5–7.
- Burgess, Tyrrell. 1977. *Education After School*. London: Victor Gollancz.
- Burgess, Tyrrell. 1979. New ways to learn. *The Royal Society of Arts Journal* CXXVII(5271): 143–157.
- Burgess, Tyrrell and Swann, Joanna. 2003. The rejectability of Karl Popper: Why Popper's ideas have had so little influence on social practice. *Higher Education Review* 35(2): 57–65.
- Campbell, Donald. 1974. Evolutionary epistemology. In *The Philosophy of Karl Popper, Book 1*, ed. Paul Arthur Schilpp, pp. 413–463. La Salle, IL: Open Court.
- Edelman, Gerald Maurice. 1992. *Bright Air, Brilliant Fire: On the Matter of the Mind*. New York: Basic Books.
- Petersen, Arne Friemuth. 1988. Why children and young animals play: A new theory of play and its role in problem solving. Monograph of The Royal Danish Academy of Sciences and Letters, Copenhagen. *Historisk-filosofiske Meddelelser* 54: 1–57.
- Popper, Karl Raimund. 1979. *Objective Knowledge: An Evolutionary Approach*. Oxford: Oxford University Press (first edition 1972).
- Popper, Karl Raimund. 1992. *Unended Quest: An Intellectual Autobiography*. London: Routledge (first published as Autobiography of Karl Popper. In *The Philosophy of Karl Popper, Book 1*, 1974, ed. Paul Arthur Schilpp, pp. 3–181. La Salle, IL: Open Court).
- Popper, Karl Raimund. 1994. *The Myth of the Framework: In Defence of Science and Rationality*, ed. Mark Amadeus Notturmo. London: Routledge.
- Popper, Karl Raimund. 1999. *All Life Is Problem Solving*. London: Routledge (some chapters translated by P. Camiller).
- Swann, Joanna. 1983. Teaching and the logic of learning. *Higher Education Review*, 15(2): 31–57.
- Swann, Joanna. 1999. What happens when learning takes place? *Interchange*, 30(3): 257–282.
- Swann, Joanna. 2003a. How science can contribute to the improvement of educational practice. *Oxford Review of Education* 29(2): 253–268.
- Swann, Joanna. 2003b. A Popperian approach to research on learning and method. In *Educational Research in Practice: Making Sense of Methodology*, eds. Joanna Swann and John Pratt, pp. 11–34. London: Continuum.
- Swann, Joanna. 2006. How to avoid giving unwanted answers to unasked questions: Realizing Karl Popper's educational dream. In *Karl Popper: A Centenary Assessment, Volume 3 – Science*, eds. Ian Charles Jarvie, Karl Milford, and David Miller, pp. 261–271. Aldershot: Ashgate.
- Swann, Joanna. 2007a. Teaching for a better world: The why and how of student-initiated curricula. In *Values Education and Lifelong Learning: Philosophy, Policies, Programmes*, eds. David Aspin and Judith Chapman, pp. 279–294. Dordrecht, The Netherlands: Springer.
- Swann, Joanna. 2007b. The myth of learning by instruction from without. *Higher Education Review* 40(1): 37–51.
- Swann, Joanna. 2008. Learning: An evolutionary analysis. *Educational Philosophy and Theory*, forthcoming in hard copy (and from 23-01-08 as Online. Early Article doi: 10.1111/j.1469-5812.2007.00410.x).
- Swann, Joanna and Burgess, Tyrrell. 2005. The usefulness of Karl Popper's selectionist theory of learning for educational practice. *Learning for Democracy* 1(3): 7–22.

# Applying Popperian Didactics

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**Abstract** University students often suffer unnecessarily during their studies – mainly due to traditional impositions, which have little to do with intellectual and professional growth. Encouraging judicious critical thinking may help alleviate this. Training students in rational critical thinking before they begin the prescribed curriculum brings astonishing results, leading to quick, rational and enjoyable studies. Students quickly grasp that knowledge evolves and textbooks may be questioned; they develop independent critical thinking that results in very good final results. The challenge, therefore, is to convince teachers to apply critical rationalism.

## Introduction

Rather than reviewing Karl Popper's philosophy, I would like to highlight one relatively neglected aspect: the application of critical rationalism to didactic, more specifically to academic didactic, as an alternative to traditional, unidirectional, dogmatic, authoritarian and often unpleasant teaching.<sup>1</sup> What follows is a mixture of history, philosophy and personal experience: I shall outline some historical roots of our university teaching, Popper's philosophy-related answer, and exemplify how it can work in the classroom, rendering studies more enjoyable and efficient.

Popper was interested in pedagogy mainly at the beginning of his career, as a high school teacher in Vienna. He wrote a couple of articles, but soon turned his attention to philosophy, regarding pedagogy, rightly or wrongly, as subject to logic and epistemology. In his 1974 intellectual autobiography, however, reminiscing about his university studies, he made a well-known statement:

I dreamt of one day founding a school in which young people could learn without boredom, and would be stimulated to pose problems and discuss them; a school in which no unwanted answers to unasked questions would have to be listened to; in which one did not study for the sake of passing examinations. (Popper 1976, p. 40)

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The statement is short, but the wisdom behind it is endless, and I imagine everybody who has been a student agrees with it. Why is it, then, so difficult, if not impossible, to apply it to higher education?

## Unidirectional Transmission of Knowledge

The extent to which a university such as that dreamt of by Popper remains a utopia became vividly clear to me when, after many years overseas, I joined the University of Chieti, in central Italy. Most Italian universities are state universities, and even those called “private universities” join them to form a system of mass higher education that is strictly regulated by law and bureaucracy. Within this context, the state University of Chieti – named after Gabriele d’Annunzio, the superb modern Italian writer – makes a praiseworthy effort to render, whenever possible, some of the shortcomings more bearable. It offers a green campus, elegant buildings, modern teaching facilities and much good will from dedicated faculty and tutors. I nevertheless soon became aware that the exchange of knowledge between my students and myself barely had anything to do with the things I was trying to teach: the students seemed almost exclusively interested in passing exams. It is really only during exams, which in Italian universities are oral, that I have the opportunity for some intellectual exchange with my students. If a student looks for me during the teaching term, it is usually to ask for details concerning the exams. Students who come to lectures do so above all to learn what will be asked in the exam. This calls to mind another passage of Popper’s, in *Open Society*:

Instead of encouraging the student to devote himself to his studies for the sake of studying, instead of encouraging in him a real love for his subject for inquiry, he is encouraged to study for the sake of his personal career; he is led to acquire only such knowledge as is serviceable in getting him over the hurdles which he must clear for the sake of his advancement. In other words, even in the field of science, our methods of selection are based upon an appeal to personal ambition of a somewhat crude form (Popper 1966, p. 135).

Italian universities made me realize to what extent all this can be fostered by a centralized system. Curiously enough, a semi-official expression current in universities is “*portare un testo*” (“to carry a textbook”). It denotes the practice of carrying a textbook to the exam both physically and metaphorically. Physically, because students are implicitly advised to show the examiner that they own the textbook, i.e. that they materially own a piece of knowledge. Metaphorically, to show that they carry the contents of the textbook in their head, i.e. they know it by heart and are ready to recite or pour it for the examiner. It is as if they carried a bucket and poured its contents into the examiner’s bucket; the extent to which they manage to fill the examiner’s bucket determines their grade. This calls to mind Popper’s metaphor of the mind as a bucket, portraying the mind as a passive receptor in his *Theory of Objective Knowledge*. In formulating it, Popper was influenced by the Würzburg School of Psychology led by Oswald Külpe that attempted to refute sensationalism and undermine associationism, all features of the traditional empirical learning

theory of Locke and Hume.<sup>2</sup> Amazingly, the jargon term “to carry” (“*portare*”) has developed independently of Popper. Popper’s teachings have, as a matter of fact, been unofficially banned for decades from Italian universities which – through a mechanism which is beyond my understanding – have been ruled for many years since World War II by an overwhelming majority of communists, cornering a minority of Catholics. Neither are very fond of Popper. Today, fortunately, Italian universities are no longer politicized, Popper’s philosophy is welcome, and the general standard has considerably improved. This points out some of their many positive aspects which include, among others, intellectual flexibility.

However, the majority of students, all over the world, continue to suffer during their studies, and the university establishment does not do much to alleviate this suffering. What is most excruciating of all is to walk through the university libraries and corridors during the exam period and see exhausted students, with red eyes and desperate expressions, highlighting paragraphs of textbooks to learn by heart. No wonder exams can turn into a tragicomic recitation with many unwanted answers to unasked questions. Not being a drama teacher, I get the mortifying feeling that I am mentoring parrots rather than young intellectuals, with the difference that parrots do not forget the stuff immediately afterward.

Must intellectual growth necessarily become a torment?

From my own experience, both as a student and as a teacher, if one applies rational criticism, studies can become enjoyable. Why, then, is it so difficult to change such an obviously anti-intellectual system? Popper succeeded in pinpointing just how deep the historical roots of the problem are – reaching back to the dawn of Western civilization.

## The Historical Background

The roots of our modern university system can be traced back to pre-Socratic time. In the second half of the fifth century B.C., Athens and other parts of Greece saw the appearance of the Sophists, itinerant teachers who were paid to teach rhetoric as a game and a persuasive art. At the end of that same century, Socrates, who belonged to the same tradition but is said to have disapproved of its practices for ethical reasons, introduced his method of inquiry and growth based on dialectic. It is essentially a dialogue wherein two (or even more) interlocutors discuss an issue and attempt, through questions and answers, to lead their counterpart into a contradiction.

Socrates endeavored to develop virtues rather than teach truths and the way he suggested it be put into practice is an exciting experience. Despite this fact, his method was not very successful. It was soon flanked by his best-known pupil, Plato, the founder of what can be considered the earliest Western academic institution: the Academy.

We know little concerning Plato’s early Academy. In the *Republic*, however, Plato presents a detailed educational program aimed at training leaders.



This is done more by unidirectional teaching of “truth” than through dialectic. Dialectic is formally given utmost importance but, as far as the process of learning is concerned, it is implicitly relegated more to the secondary, preparatory role of refuting deceptions. In the seventh book of the *Republic*, Plato portrays his best-known piece, the allegory of the cave. He depicts prisoners chained in a cave, who can only see shadows on the walls and thus, do not realize how wrong their existence is.

What would occur if one of the chained individuals were released from his bondage, left the cave and were enlightened by truth? He would realize, says Plato, how miserable his previous existence had been and would want to return to the cave and free his former fellow prisoners from their bondage.

As I was rereading the Platonic text, I saw both myself as a student and my present students metaphorically chained inside the cave. I, too, suffered as much as they did without really knowing why, not being able to imagine a world of studies without suffering. When I registered at university, I considered myself, just as so many other students do, an ignorant caveman acquainted only with shadows, and I was convinced that at the university some enlightened academics would grace me with true knowledge. I was ready to do anything they said, even if this meant sacrifice and suffering. I would also never dare to contradict them, even on those occasions when what they were saying seemed absurd to me. Whenever I failed to understand what my teachers were saying, I automatically blamed what I believed to be inexperience or foolishness on my part.

Plato’s pupil, Aristotle, the most famous collector of knowledge in history and the founder of the celebrated Lyceum, added another milestone to the basis of the modern university. According to Aristotle, one can discuss many issues except some evident truths. A similar approach was adopted by another schooling tradition at the basis of universities, the monotheistic one, beginning with ancient Jewish institutions of learning, in which one could discuss everything except the word of God as written in the Bible. As the Spanish, fifteenth-century Jewish scholar Hasdai Crescas pointed out in the earliest devastating critique of Aristotle, there is a difference between divine truth and philosophical truth. Yet either way, Aristotelian philosophy and theology, whether Jewish, Christian or Muslim, are dogmatic and therefore not always the best allies of science. Questioning them may even occasionally lead one to the stake.

Dogmatism, however, is the line of thinking adopted by universities. Moreover, universities were founded as corporations of teachers and students in medieval cities. Corporations can be vital in encouraging a certain type of activity, but once they have reached their goal they tend to become closed societies that hamper progress, as beautifully depicted in another allegorical work, Orwell’s *Animal Farm*. In fact, despite the arguments of some nostalgic historians, science developed primarily outside the universities.

The French revolution, in the wake of the Enlightenment, secularized the university – but not its way of thinking. Textbooks replaced the Bible but the sacredness remained and the related authoritarianism went on causing suffering.

To me, such tension is unbearable, although I was always astonished to see how some of my fellow students made their way through the process.

They diligently learned every single detail of a textbook by heart; they embellished what they learned with expressions taken from trendy publications in lectures at scholarly meetings, to the delight of the who's who in the field, and swiftly climbed the academic ladder. Popper's pupil, Joseph Agassi, uses a variation on an expression by Thomas Kuhn to label them "super-normal" students. A super-normal student is a student on the way to becoming a normal scientist. For Popper, the normal scientist "is a person one ought to be sorry for"...he "has been taught badly." (Popper 1970, p. 53)<sup>3</sup>

## Applying Popperian Didactics

In his *Open Society*, Popper went to the root of the problem by giving an implicitly revolutionary interpretation of Plato's cave allegory. Before Popper, surviving in the cave was regarded as erroneous, and the light shining outside was regarded as right. Are we certain, he asks, that that "light" is genuine? Is the world outside the cave the real good and are the shadows so evil? Who gives "light" a certificate of authenticity? Can one perhaps get precious information from a shadow, too?

Popper's challenge is remarkable; with it in mind, I attempted an experiment. To teach students to think and work critically and rationally, i.e. instead of teaching them the stuff, teaching them how to learn and evaluate the stuff. The analogy is that of the telephone book. One does not need to learn a telephone book by heart to find a number. It is enough to know how the telephone book is arranged and the alphabetical order.

I established a series of introductory lectures dealing with open questions such as what is science, how knowledge grows, what is a university and how one should study rationally.<sup>4</sup> This meant encouraging the students to speak up during the lectures and assuring them that nothing they said, as long as it was said respectfully, would be used against them. The university, I claim, unlike the real world, should be the incubator in which one can make mistakes without being punished. On the contrary, making a mistake offers a golden opportunity for teachers and students to take advantage of criticism and grow.

I teach students, *inter alia*, elementary procedures such as reading and evaluating a book or a textbook. Academics, who are used to writing book reviews, are able to get to know the contents of a book in a short time, but do not teach their students this secret art. I also teach them how to overcome the fear of writing a paper.<sup>5</sup> The idea is that they become independent, intellectually honest and responsible.

In the beginning, I was fully aware that I was committing a heresy, and in fact I got mixed reactions. But the students were enthusiastic, they crowded my lectures and seemed to begin to enjoy studying and learning how to criticize. Instead of unidirectional transmission of the curriculum, I tried to focus on major problems and their context and encouraged students to attempt an answer. This raised their interest, led to open debates, drew further questions from the students themselves and encouraged them to seek for answers in the literature, thereby increasing the

course efficiency. They mastered the curriculum relatively quickly and, at the end, obtained remarkable results as they were examined by commissions that were by no means Popperian. To my surprise, I was encouraged to carry on.

Since then, I have been holding lectures all over the country, trying to teach students how to avoid suffering. The repeated complaint is: university professors, in general, are not cooperative. Indeed, it is not an easy task for former super-normal students, even for those who sincerely declare themselves as Popperian, to encourage students to be active and independent. Yet, at the beginning of the twenty-first century and in an age of technology and science, it is the duty of the philosophical leadership to make an effort to open the university. Popper argues convincingly that the open society is the most appropriate one for science. The university is still a rather closed society and this is the basis for tension and suffering. Whether we like it or not, today we live in a globalized world which is a product of the open society; it offers terrific challenges and the university must cope with them. At the beginning of the third millennium we are flooded with data, and one of the main tasks of studies, academic in particular, should be to learn how to filter it critically and reasonably, rather than to collect it.

I suggest applying Popper's idea of gradual social engineering, i.e. trying to improve the university in small steps. The main avenue is not to excel, but to try to minimize the damage. I do not suggest turning upside down an institution that, through many centuries, has, after all, developed many praiseworthy aspects. Rather, I hope that ongoing independent initiatives in different countries join efforts. I also take the liberty of suggesting, in general, that university colleagues begin by trying to teach the use of critical rationalism. The advantages are invaluable for all of us.

## Notes

1. There are some excellent works dealing with critical rational in teaching. Among them: Bartley's *Unfathomed Knowledge* (1984) relates, rightly or wrongly, knowledge to material wealth, arguing how badly universities administer it. Perkinson's *Teacher's without Goals* (1993) is a short, lucid manifesto in favour of rational critical teaching of evolving knowledge, criticizing the "modern" teaching of allegedly accumulative knowledge. Agassi's delightful "Dissertation without Tears" ridicules the traditional perfectionist practice of writing university dissertations and advocates a more rational and enjoyable way of writing. Finally, Swann's article in this book offers a concise theoretical basis. These and other welcome agendas should be put into practice.
2. As Berkson and Wettersten argue, drawing attention to the contribution of twentieth century psychological debate in Central Europe involving psychologists such as Ostwald Külpe and Otto Seltz (Berkson and Wettersten 1984).
3. This comment, by the way, was made directly to Kuhn in an International Colloquium in the Philosophy of Science that was held on July 11-17, 1965 at Bedford College, London. Popper was replying to Kuhn's criticism for having undervalued the importance of "normal science."
4. As Henry Perkinson pointed out to me, these are what Popper, in chapter 11 of his *Open Society*, calls "essentialist questions" and should be of no concern to science. One should, rather, ask what science should look like, what the function of science or of a university should

be, or what universities are for. Didactically, however, proposing these questions may play a helpful role for students, since they are familiar with these types of questions and presenting them as open avoids the main danger of giving “essentialist” answers.

5. Much in the wake of (Agassi 1999).

## Bibliography

- Agassi, Joseph. 1999. Dissertation Without Tears. In *Critical Rationalism and Educational discourse*, ed. Gerhard Zecha, pp. 59–89. Amsterdam: Rodopi.
- Berkson, William and Wettersten, John. 1984. *Learning from Error: Karl Popper's Psychology of Learning*. La Salle, IL: Open Court.
- Lakatos, Imre and Musgrave, Alan (eds.). 1999. *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.
- Perkinson, Henry J. 1993. *Teachers Without Goals, Students Without Purposes*. New York: McGraw-Hill.
- Popper, Karl Raimund. 1966 (first edition in 1945). *The Open Society and Its Enemies. Vol. 1: Plato*. London: Routledge & Kegan Paul.
- Popper, Karl Raimund. 1972. *Objective Knowledge: An Evolutionary Approach*. Oxford: Clarendon.
- Popper, Karl Raimund. 1976 (first edition in 1974). *Unended Quest. An Intellectual Autobiography*. Glasgow: Fontana/Collins.
- Popper, Karl Raimund 1999. Normal Science and Its Dangers. In *Criticism and the Growth of Knowledge*, eds. Imre Lakatos and Alan Musgrave, pp. 51–58. Cambridge: Cambridge University Press.
- Segre, Michael. 2002. Popper e l'educazione. *Nuova Civiltà delle Macchine*, 20(2): 82–88.
- Segre, Michael. 2004. *Accademia e società: conversazioni con Joseph Agassi*. Soveria Mannelli: Rubbettino.
- Segre, Michael. 2005. Studiare senza soffrire. *Il Monitore* 39/4, pp. 5–7. [filosofia.nextteam.it/download/studiare\\_senza\\_soffrire.pdf](http://filosofia.nextteam.it/download/studiare_senza_soffrire.pdf). Accessed 19 June, 2008.
- Wolfson, Harry Austryn. 1929. Crescas' Critique of Aristotle. *Problems of Aristotle's Physics in Jewish and Arabic Philosophy*. Cambridge, MA: Harvard University Press.
- Zecha, Gerhard (ed.). 1999. *Critical Rationalism and Educational Discourse*. Amsterdam: Rodopi.

# The Difficulties with Popper's Nontraditional Conception of Metaphysics

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**Abstract** Popper, trying to demarcate empirical science and nonscience, developed the falsifiability thesis as the criterion for demarcation, a criterion used for demarcating empirical science from metaphysics too. Popper, in opposition to Logical Positivists' meaningfulness criterion, claimed that metaphysics may be meaningful, while it is not a science. Calling a set of currently untestable ideas as metaphysical, he claimed that they may be testable in future, so that we may regard a set of metaphysical ideas as protoscience. He afterwards propounded "metaphysical research programme" as historical development of a science out of a metaphysical idea. Applying the criterion of testability to demarcating science and metaphysics allows some currently metaphysical ideas to become "testable" and, therefore, "scientific". Thus, Popper's conception of "metaphysics" is different from the original conception of the term. In spite of the importance of regarding metaphysical statements as meaningful by Popper, his particular conception of metaphysics leads to a confusion of metaphysics with protoscience, and gives rise to serious difficulties in philosophy, science, history of science, and philosophy of science.

## Introduction

What is metaphysics? Is it a science? What is the meaning of science here? Has there been any change in the meaning of metaphysics throughout the history of philosophical thinking? Has there been a tradition of metaphysics in the history of philosophy? How some philosophers, particularly the Logical Positivists, have rejected metaphysics? How has Popper defended metaphysics? What conception of metaphysics has Popper had in mind? Is there any correspondence between Popper's conception and traditional meaning of metaphysics?

These are the important questions most of which are dealt with in this chapter. The author has found no remarkable writing treating of the subject. Accordingly, the author tries to address the subject and give proper answers to appropriate

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questions. Popper has argued that metaphysics is meaningful. While agreeing with Popper in this respect, the chapter comes to the conclusion that in Popper's conception of metaphysics there is a serious deviation from traditional meaning of the discipline. Besides an inaccurate usage of the term, that is a significant shortcoming in itself, such a deviation leads to some misinterpretation of both history of science and scientific theories.

## “Metaphysics”: Terminological Discussion

Following the efforts of ancient (pre-Socratic) Greek philosophers to explain the evolution of the physical world as a whole, a science was formed as a speculative discipline to treat of principles and notions applying to Being *qua* Being. This discipline, under the title of any name other than “metaphysics”, came to be the central part of academic theoretical inquiry and scholarly education up to the times of Plato and Aristotle. The term “metaphysics”, from the Greek *meta ta physika*, is a term used by Andronicus of Rhodes (c. AD 70) as the title of a particular collection of Aristotelian treatises.

Aristotle had used three terms to refer to that kind of philosophy presented in that collection:

1. The “wisdom” (*Sophia*), dealing with the first causes (*aitia*) and the principles (*archai*) of things (as the foundation of all other inquiries).
2. The “theological science” or simply “theology” (*theologikê*), referring to discussions concerning God (including arguments for His existence, study of His nature and attributes, the nature of the world, questions about creation).
3. The “first philosophy” (*prôtê philosophia*), referring to discussions concerning the first causes of things. Aristotle made use of “first science” too; its subjects are those of “wisdom”. This science deals with the fundamentals (such as causality and substance) that are basic and common to all special sciences.

Andronicus of Rhodes overlooked these titles, and, because he believed that that part of the Aristotle's *corpus* came naturally after the physical treatises (called *Physics*), placed those books right after *Physics*, and called them *τά μετά τά φυσικά βιβλία* (*ta meta ta physika biblia*) or, “the books that come after the [book on] physics.”

The above title was read by Latin scholiasts and Islamic philosophers as meaning “the science of what is beyond the physical”. The word for this “science” (or study, or discipline, or *episteme*, or *scientia*) in modern European languages are ‘metaphysics’, ‘la métaphysique’, ‘die Metaphysik’, ‘la metafísica’, ... as adaptations of the Latin word to the phonetic and orthographic requirements.

The book was translated into Arabic under the title “mā ba'd-a ttabī'a” meaning “what is beyond the nature” or, simply, “beyond the nature”. This, of course, connotes “the science of what is beyond the nature” or, simply, “the science of beyond the nature”. In all expressions the word “beyond” denotes not an entity but the principles. Thus, Islamic philosophers such as Ibn Sina [Avicenna] considered it as the

science that must be before physics in both the rank and learning; accordingly, Ibn Sina [Avicenna], regarding it as a science having a rank higher than that of physics, made use of the term “mā ba'd-a ttabī'a” meaning “[the science of] what is before the nature” or, “[the science that is] before the [the science of] nature”, using the word beyond as denoting not an entity but the principles (see Ibn Sina (1960)).

## The Main Definitions and Domains of Metaphysics

For our purpose in this article, as it has been seen throughout the history of metaphysics, at least before modern philosophy, we may be entitled to make use of the word ‘metaphysics’ as a name for the “science” (or discipline, or science, or *episteme*, or *scientia*) whose subject-matter is one and the same subject-matter of Aristotle's *Metaphysics*. Accordingly, accepting the method of defining a science according to its subject matter(s) throughout its history, we may say that metaphysics has been (and, probably, is) the science of “the first (or/and highest) causes of things” or “first principles” or “immaterial being(s)” or “the most abstract and universal concepts” or “unchanging things” or, finally “being as such” (or Being *qua* Being). (It must be noted that, in the last case, the subject matter is not “Being *qua* Being”, rather “being”, using a manner in which “being”, as the subject matter, is studied ‘in so far as’ it is being.)

Looking at the history of what has been called metaphysics, we see a science that has been searching for a description and/or explanation so simple, so fundamental and so all-inclusive that could apply to everything having some kind of beingness and being entitled to be called entity. It tries to investigate into notions, topics, and problems so diverse that all entities and events, either divine or human, are included under one or the other of them, so that the domain of metaphysics would cover all things entitled to be regarded as enjoying some kind of beingness. In enumerating the different meanings of metaphysics and its subject matter(s) from Aristotle's *Metaphysics*, we may both cite directly his own expressions and infer indirectly his conceptions.

Aristotle, regarding the discipline that had in his mind as a science of reality, tried to give some definitions of it within the framework of above three terms that are found in his *Metaphysics*. Generally surveying the book, one may find several definitions of metaphysics covering all notions, topics, and problems as well as all beings. These definitions, based mainly on Aristotle's own definitions may overlap or even mean one and the same thing. It is evident that we may propose a purview corresponding to each definition.

The historians of philosophy have had some problems with “a unitary conception of metaphysics in the Aristotelian treatises” (Owens 1951, p. xvi). Owens puts forth the problem as follows:

Instead of dealing with a philosopher who had one science of a proposed contradictory object, or who at successive stages of his career developed different conceptions of the science that upon close scrutiny turn out to be contradictory, one has now to meet also an Aristotle who himself developed and held simultaneously two distinct metaphysical sciences, each having a different Object. (*ibid.*)



Owens cites both the “science of supersensible Being *only*” that is “described as Primary Philosophy or theology” and the “science of Being *qua* Being” that “is described as metaphysics or [...] ontology”. He calls attention to the Book K (Aristotle: I, 1026a29–32) in which the author (most probably, Aristotle himself) writes “[I]f there is an immobile Entity, the science of this must be prior and must be primary philosophy, and universal in this way, [...] and it will belong to this to consider Being *qua* Being – both what it is and what pertains to it *qua* Being.” The author, in Book Γ (Aristotle: III, 1005a33–b1) says that the philosopher “whose inquiry is universal and deals with primary Entity” deals with the axioms applying universally to all Beings. Regarding all citations and their connotations and implications, one may come to the conclusion that

[T]he science of Being *qua* Being, universally, is the science that has as its subject the Entity prior to sensible Entity. Against such a background there is no room for two distinct sciences, one of which would deal with supersensible Entity and the other with Being *qua* Being. Both formulae denote the same scientific subject. (Owens 1951, pp. xviii–xix)

While the leading mediaeval metaphysicians had agreed over Being *qua* Being as the subject matter of their discipline, there have been some controversies among them in relation to giving a Platonic or Aristotelian interpretation to “Being *qua* Being” as, respectively, *ens perfectissimum* or *ens commune*. How was it possible for Christian metaphysicians to accept that there is a one to one correspondence between subject matter of metaphysics and the God of the religious faith? Ibn Sina and some Latin thinkers had tried to combine all conceptions of metaphysics into one all-inclusive science that its subject has various manifestations. According to Aristotle “there is a science which investigates being as being, and the attributes which belong to this in virtue of its own nature” (*ta toutô huparchonta kath hautô*) (Aristotle: IV 1003a21). This science considers only Being and its highest determinations, being called transcendental. These transcendental determinations are unity, truth, goodness, and beauty, all of which being coextensive with Being itself. The object of metaphysics is Being, each different part of which is the subject matter of a particular science. If Being is the source of any particular reality, and if metaphysics abstracts its object from all things, of reality as a whole, then metaphysics must be the most real of all sciences.

All sciences, from physical to metaphysical sciences, to be scientific knowledge, ought to begin with abstraction: abstraction of universals from individuals or particular individuating characteristics. Each science has a degree of abstraction; or, we may say, there is a spectrum of abstraction: from abstraction of physical properties, concepts, and laws to abstraction of metaphysical concepts and principles as the most abstract universal concepts and the most comprehensive principles. Metaphysics, carrying the process of abstraction farther than the stage reached by physics and even mathematics, is the science of the highest abstraction. Metaphysics deals with the concepts and principles applying to all entities, so that one does not find any entity that may not be included under metaphysical concepts and principles in relation to some qualities that are common to all entities as such.

Aristotle has said that the first science deals with the things that are unmovable and separate from matter (Aristotle: VI, 1026a16). According to scholastic tradition

and Wolffian metaphysics these things may be divided into those existing without matter (i.e. immaterial things such as human soul and God) and those being comprehended without entering any matter (i.e. (immaterial) concepts such as cause), studied, respectively, in special metaphysics and general metaphysics (i.e. ontology). Special metaphysics itself is divided into rational psychology, rational cosmology, and rational theology. Accordingly, one may define metaphysics from different viewpoints, all inferred from Aristotle's *Metaphysics*, as follows: (1) metaphysics as the science of the most abstract and universal concepts, (2) metaphysics as the science of the first principles, (3) metaphysics as the science of Being *qua* Being, (4) metaphysics as the science of immaterial being, (5) metaphysics as the science of the first and the highest causes, (6) metaphysics as the science of the primary entity (or first ousia/entity/being/substance), and (7) metaphysics as the science of the God.

It may be possible to show that all definitions are more or less equivalent. But, ignoring all controversies among the commentators of Aristotle, among Platonists and Aristotelians, and among Muslim or Christian philosophers, mystics and theologians, we may agree that there has been a relative consensus over Being *qua* Being as the subject matter of a discipline called metaphysics, regardless any agreement over authenticity and capabilities of such a discipline in attaining genuine and true knowledge of its subject matter. In this science, different topics are studied among which we may mention Being (*on, onta*), God, Particulars (*en merei, epimeros*) and Universals (*katholou*), Unity, Change and Identity (or Continuity), Causality, Genesis and Corruption, Space and Time, Necessity and Possibility, Free Will, Categories (*katêgoria*), Substance and Accidents, Matter (*hulê*), Motion (*kinêsis*), Actuality (*energeia*) and Potentiality (*dunamis*). In contemporary philosophy some new branches have been generated within which some traditional problems of metaphysics are being studied. We may mention philosophy of science, philosophy of mind, philosophy of language, philosophy of religion as these new branches. On the other hand, after some destructive attacks on metaphysics by both Anglo-Saxon and Continental strands in Western philosophy, now we observe some relatively new topics and/or perspectives. Traditional metaphysics has not dealt with some problems which are dealt in new metaphysics, amongst which we may mention those of epistemology or theories of knowledge.

## Post-Cartesian Critiques of Metaphysics

Metaphysics, having great claims concerning being as being and being as a whole, has been attacked by different scholars from various fields such as religious theologies, mysticism, as well as philosophy, particularly in its post-Cartesian era. It has been regarded as being excessively vague, futile, worthless, and even misleading. Some philosophical schools have rejected metaphysics as claiming doctrines straightly opposed to their own claimed doctrines. Materialism, for example, has not found any justification for an alleged science claiming the knowledge of the immaterial while, according to it, there is nothing in the world except

matter. It pays no attention to the metaphysicians' claim that materialism itself is a metaphysical theory of reality, contributing to the very science that is trying to refute! Throughout the post-Cartesian period, the traditional metaphysics has had its most important criticisms by David Hume, Immanuel Kant and the Logical Positivists. Hume, accepting only two kinds of statements (i.e. empirical ones of natural-empirical sciences and analytic ones of logic and mathematics) went so far as to write:

If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask, does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion. (Hume 1748, p. 107)

Immanuel Kant, restricting knowledge to the world of actual sense experience as produced through cooperation of sensibility and *a priori* pure concepts of understanding, established the doctrine of the unknowableness of noumenal reality (*Ding an sich*), on the one hand, and unknowability of soul, world as a whole, and God as three ideas of pure reason, on the other hand. He, rejecting any knowledge of the realm beyond experience, thus dooms to failure special metaphysics in three aspects of rational psychology, rational cosmology, and rational theology as well as in an aspect claiming the knowledge of noumenal reality, all of them being different aspects of transcendent, ultra-empirical knowledge. We are not justified to claim any knowledge concerning a world beyond our sense experience (except the knowledge that such a world exists):

Metaphysics – a wholly isolated speculative cognition of reason that elevates itself entirely above all instruction from experience, and that through mere concepts [...], where reason thus is supposed to be its own pupil – has up to now been so favored by fate as to have been able to enter upon the secure course of a science, even though it is older than all other sciences, and would remain even if all the others were swallowed up by an all-consuming barbarism. (Kant 1781, p. Bxiv)

Kant, accepting *a priori* synthetic statements as a third kind of statements in addition to two Humean ones, argued that not only all statements of pure mathematics and pure natural sciences are of this kind, but also all statements of (general) metaphysics as a science must be of such a kind (for a comprehensive discussion and understanding of Kant's analysis of metaphysics, one may see Musa Akrami (1384/2005) as well as Kant's own *Critique of Pure Reason* (Transcendental Dialectic) (1781/1997) and *Prolegomena* (1783/1996); Michelle Grier (2004) gives a good overview of "Kant's Critique of Metaphysics").

## Logical Positivism and Metaphysics

The Logical Positivists through about 3 decades of their active presence on the stage of philosophizing, and in their more or less different strands, put forth criterions for recognizing statements of science and distinguishing them from nonscientific ones of fields such as metaphysics. We may insist on the criterion of meaningfulness

as a result of which the statements of metaphysics are rejected as meaningless, as was emphasized by A. J. Ayer in his *Language, Truth and Logic*. According to the Logical Positivists, therefore, many so-called philosophical problems, in particular those of metaphysics, were termed pseudo-problems, so that science is the supreme judge and intermediary in getting to truth. Popper writes on Wittgenstein's views in his *Tractatus Logico-Philosophicus* (1921/22):

Wittgenstein is an anti-metaphysician. 'The book', he writes in the preface, 'deals with the problems of philosophy and shows, I believe, that the method of formulating these problems rests on the misunderstanding of the logic of our language.' He tries to show that metaphysics is 'simply nonsense' and tries to draw a limit, in our language, between sense and nonsense: 'The limit can ... be drawn in languages and what lies on the other side of the limit will be simply nonsense.' According to Wittgenstein's book, propositions have sense. They are true or false. Philosophical propositions do not exist; they only look like propositions, but are, in fact, nonsensical. The limit between sense and nonsense coincides with that between natural science and philosophy: 'The totality of true propositions is the total natural science (or the totality of the natural sciences).—Philosophy is not one of the natural sciences.' (Popper 1945, p. 614)

Using naturalistic philosophy of language as the basis for defining meaning, the criterion of meaningfulness was accepted by the Logical Positivists such as Carnap in his *Der Logische Aufbau der Welt* following Wittgenstein's *Tractatus Logico-Philosophicus* (see Popper 1963, p. 349). Verifiability was put forth as the criterion of meaningfulness.

The Logical Positivists, in their more or less dominating approach,<sup>1</sup> put forth the verifiability criterion of meaning to distinguish the non-analytic meaningful statements from non-analytic meaningless ones. A statement would be meaningful if one could find a procedure, in practice or in principle, to show conclusively its truth or falsity. According to such a criterion, scientific statements are empirically verifiable and, therefore, meaningful, while the statements such as metaphysical statements, falling short of the verifiability criterion of meaning, are considered as meaningless.

But some difficulties arose with verifiability and meaningfulness: (1) verifiability does not exclude clear metaphysical statements, (2) science, as regarded by logical positivists, could not make use of non-empirical arguments, and (3) metaphysics, as logical positivists call it, could not draw on empirical premises.

The Logical Positivists, thinking that meaningfulness is a problem in natural science or in psychology, interpreted it in a naturalistic way. Such an interpretation prevented them to define meaningfulness.

## Popper and Metaphysics

Popper, confronting both the tradition of metaphysics and the logical-positivist effort to reject metaphysics, has had his own attitude towards metaphysics. We discuss his position briefly in three sections: his critique of the positivistic approach, his criterion for demarcation, and his own conception of metaphysics and the metaphysical.

## ***Popper's Critique of Vienna Circle and the Positivistic Approach***

Popper recognized that the logical positivist criterion of meaningfulness was not capable to exclude metaphysical statements while did exclude some statements of science. On the other hand, he argued that some alleged epistemic activities (such as astrology, Marxism and Freudian psychology) were not truly scientific. He, regarding them as pseudoscience, tried to demarcate empirical science and such apparently scientific activities. Trying to analyze and solve the problem of demarcation, Popper came to the conclusion that classical empiricism and logical positivism, particularly as manifested in observationalist-inductivist account of science, must be criticized from a viewpoint that came to be called "critical rationalism", a term that was used to describe his own philosophy.

The repeated attempts made by Rudolf Carnap to show [that] the demarcation between science and metaphysics coincides with that between sense and nonsense have failed. The reason is that the positivistic concept of "meaning" or "sense" (or of verifiability, or of inductive conformability, etc.) is inappropriate for achieving this demarcation—simply because metaphysics need not be meaningless even though it is not science. In all its variants demarcation by meaninglessness has tended to be *at the same time too narrow and too wide*: as against all intentions and all claims, it has tended to exclude scientific theories as meaningless, while failing to exclude even that part of metaphysics which is known as "rational theology". (Popper 1963, pp. 341–342)

Popper highlights the difference between science and pseudoscience from the viewpoint based on the position taken by figures such as Bacon:

The most widely accepted view was that science was characterized by its *observational basis*, or by its *inductive method*, while pseudo-science and metaphysics were characterized by their *speculative method* or, as Bacon said, by the fact that they operated with "*mental anticipations*"—something very similar to hypotheses. (Popper 1963, p. 344)

Popper holds that the positivists associated to Vienna Circle have never succeeded to distinguish science from metaphysics because of failure to define meaningfulness. He argued that despite the fact that metaphysics is not science (in modern sense or as the Positivists call it), it needs not to be regarded as meaningless. According to Popper, Logical Positivism, applying its criterion of meaningfulness to theories, excludes some scientific theories as meaningless, on the one hand, and fails to exclude metaphysics as meaningless, on the other hand. In Popper's view, the Positivists, adopting a psychologistic philosophy of knowledge, have confused the psychology of knowledge with the logic of knowledge.

How is it possible for meaningfulness to exclude metaphysical statements and yet include scientific ones? Popper holds that it is "strange to call metaphysical statements meaningless, or to exclude them from our language" (Popper 1963, p. 348).

Popper himself had regarded "the problem of meaninglessness as a pseudo-problem" from the beginning of his career. He had opposed to the idea that meaninglessness "may be identified with the problem of demarcation" (*ibid.*).<sup>2</sup>

## ***Popper's Criterion for Demarcation***

Popper, arguing that many statements of alleged epistemic fields other than science are meaningful, accepted such criteria as testability or provability and fallibility, instead of the criterion of meaningfulness, to distinguish science from nonscience.

The title of the 11th chapter of Popper's *Conjectures and Refutations* is 'The Demarcation between Science and Metaphysics' in which he calls to mind his first facing the problem of demarcation:

It was in 1919 that I first faced the problem of *drawing a line of demarcation* between those statements and systems of statements which could be properly described as belonging to empirical science, and others which might, perhaps, be described as "pseudo-scientific" or (in certain contexts) as "metaphysical", or which belonged, perhaps, to pure logic or to pure mathematics. (Popper 1963, p. 344)

Popper argued that metaphysical statements are not necessarily meaningless. They may have their own specific meanings. We can say that they are generally not fallible, testable or provable statements in the sense that we cannot have a valid set of empirical observations as well as a valid set of logical arguments which could absolutely prove the truth or falsity of metaphysical statements.

Popper presented (empirical) falsifiability, in place of verifiability, as the criterion of demarcation. This criterion apparently proved to be fruitful to questions of demarcation.

I proposed [...] that the refutability or falsifiability of a theoretical system should be taken as the criterion of its demarcation. According to this view, which I still uphold, a system is to be considered as scientific only if it makes assertions which may clash with observations; and a system is, in fact, tested by attempts to produce such clashes, that is to say by attempts to refute it. Thus testability is the same as refutability, and can therefore likewise be taken as a criterion of demarcation. (Popper 1963, p. 345)

Popper makes use of an exemplum to explain his conception of the line of demarcation:

Take a square to represent the class of all statements of a language in which we intend to formulate a science; draw a broad horizontal line, dividing it into an upper and lower half; write 'science' and 'testable' into the upper half, and 'metaphysics' and 'non-testable' into the lower: then, I hope, you will realize that I do not propose to draw the line of demarcation in such a way that it coincides with the limits of a language, leaving science inside, and banning metaphysics by excluding it from the class of meaningful statements. On the contrary: [...] I stressed the fact that it would be inadequate to draw the line of demarcation between science and metaphysics so as to exclude metaphysics as nonsensical from a meaningful language. (Popper 1963, pp. 346–347)

## ***Popper's Conception of Metaphysics and the Metaphysical***

Popper tries to state his conception of the metaphysical: "Is the world ruled by strict laws or not? This question I regard as metaphysical" (Popper 1934, p. 244).

He proposes a conception of the metaphysical through his belief in the metaphysical status of causality:

The belief in causality is metaphysical. It is nothing but a typical metaphysical hypostatization of a well justified methodological rule—the scientist's decision never to abandon his search for laws. The metaphysical belief in causality seems thus more fertile in its various manifestations than any indeterminist metaphysics of the kind advocated by Heisenberg. (Popper 1934, p. 245)

It is possible to give a criterion for metaphysical theories versus scientific ones. Popper insists on testability as the demarcation between science and metaphysics. One may regard a metaphysical theory as one that cannot be tested empirically through confrontation with basic statements which describe observable events of definite space-time coordinates. Of course, showing the possibility of combining some individually untestable metaphysical statements to yield a testable theory (Popper 1982b, Chap. III), Popper has argued that we cannot simply say that metaphysical statements are specified by the criterion of untestability.

Popper recognized that a metaphysical statement may imply an idea concerning the universe which may seem reasonable while it is not possible to verify it empirically. According to Popper, such an idea might be varied in a non-arbitrary way, on the basis of argument or empirical evidence, without there existing any argument or empirical evidence so strong that could absolutely prove the falsity of that idea.

He argued that metaphysics is meaningful in general and may be useful for science in some cases.

We may highlight two important points concerning metaphysics and the metaphysical: (1) many metaphysical statements are meaningful, and (2) verifiability and falsifiability are not sufficiently exhaustive to be accepted as criterion of meaningfulness, so that one may find many meaningful statements which are not verifiable and falsifiable. Moreover, there are degrees of testability:

This indicates that the criterion of demarcation cannot be an absolutely sharp one but will itself have degrees. There will be well-testable theories, hardly testable theories, and non-testable theories. Those which are non-testable are of no interest to empirical scientists. They may be described as metaphysical. (Popper 1963, p. 346)

Popper, regarding some myths, or metaphysical theories, as the origin of some scientific theories, disagrees with regarding them as nonsensical:

It would hardly contribute to clarity if we were to say that these theories are nonsensical gibberish in one stage of their development, and then suddenly become good sense in another. (Popper 1963, p. 347)

Examples of metaphysical theory, according to Popper, are atomism (Popper 1959, 1963, 1983), corpuscular theory of light (Popper 1959, 1963), "the theory of terrestrial motion (opposed by Bacon as fictitious)" (Popper 1959, p. 278), "determinism, the irreversibility of time, and recently the idea of locality in physics".

According to the exemplum used, Popper describes the process leading to a "metaphysical system":

Those theories which are on too high a level of universality, as it were (that is, too far removed from the level reached by the testable science of the day) give rise, perhaps, to a



'metaphysical system'. If, on the other hand, a crucial experiment can be designed for it, then the system will contain, as a first approximation, some well corroborated theory, and at the same time also something new-and something that can be tested. Thus the system will not, of course, be 'metaphysical'. In this case, the system in question may be looked upon as a new advance in the quasi-inductive evolution of science. (Popper 1959, p. 277)

## The Metaphysical Research Programme: From Metaphysics to Science

Popper makes use of his exemplum to show the process of transition from metaphysics to science:

To obtain a picture or model of this quasi-inductive evolution of science, the various ideas and hypotheses might be visualized as particles suspended in a fluid. Testable science is the precipitation of these particles at the bottom of the vessel: they settle down in layers (of universality). The thickness of the deposit grows with the number of these layers, every new layer corresponding to a theory more universal than those beneath it. As the result of this process, ideas previously floating in higher metaphysical regions may sometimes be reached by the growth of science, and thus make contact with it, and settle. (Popper 1959, p. 277)

Popper's *Quantum Theory and the Schism in Physics*<sup>3</sup> (1982b) contains a 'Metaphysical Epilogue' that is remarkable not only as the inspiration for Lakatos' theory of scientific research programmes, but also as a key to understand Popper's sense of metaphysics and the metaphysical.

In the 'Metaphysical Epilogue', Popper tries to develop his theory of "metaphysical research programme" on the basis of the history of a subject, and its current status.

He presents an account of the program:

In science, problem situations are the result, as a rule, of three factors. One is the discovery of an inconsistency within the ruling theory. A second is the discovery of an inconsistency between theory and experiment - the experimental falsification of the theory. The third, and perhaps the most important one, is the relation between the theory and what may be called the "metaphysical research programme". In using this term I wish to draw attention to the fact that in almost every phase of the development of science we are under the sway of metaphysical - that is, untestable - ideas; ideas which not only determine what problems of explanation we shall choose to attack, but also what kinds of answers we shall consider as fitting or satisfactory or acceptable, and as improvements of, or advances on, earlier answers. (Popper 1982b, p. 161)

Popper explains the change in a research programme or replacement of a research programme by another one:

By raising the problems of explanation, which the theory is designed to solve, the metaphysical research programme makes it possible to judge the success of the theory as an explanation. On the other hand, the critical discussion of the theory and its results may lead to a change in the research programme (usually an unconscious change, as the programme is often held unconsciously, and taken for granted), or to its replacement by another programme. These programmes are only occasionally discussed as such: more often, they are implicit in the theories and in the attitudes and judgements of the scientists. (*ibid.*)

The change or replacement takes place when some parts of the programme come to be testable and included into science. This is the reason that metaphysics has

a residual status relative to science. Popper then sets forth another reason for the name:

I call these research programmes ‘metaphysical’ also because they result from general views of the structure of the world and, at the same time, from general views of the problem situation in physical cosmology. I call them ‘research programmes’ because they incorporate, together with a view of what the most pressing problems are, a general idea of what a satisfactory solution of these problems would look like (*ibid.*).

Popper, intending to show the role of metaphysical research programme in development of science, argues that such programmes can help to determine problem situations of science because of their role as a view about the general structure of the world which allows one to have a new way of looking at the world and the things.

Metaphysical research programme is untestable at the point in time, and therefore the “metaphysical” in Popper’s sense is not testable throughout its history unless it would become changed into the scientific. Popper recognizes two kinds of metaphysical systems:

In the concluding paragraph of the concluding section of the concluding volume of the *Postscript*, Popper states that there may be a criterion of demarcation within metaphysics between what he calls “rationally worthless” metaphysical systems on the one hand, and metaphysical systems that are worthy of discussion and thought on the other hand. (Hickey 1995/2005)

Some writers noted the influence of Kuhn on Popper in “recognizing the existence of protoscientific metaphysics”. Hickey believes that Popper “does not characterize the basis for [...] a demarcation within metaphysics” between “rationally worthless metaphysical systems” and worthy ones,

but his motivation for recognizing the existence of protoscientific metaphysics within residual metaphysics seems clearly to have been the result of the influence of Kuhn. In the 1982 “Introductory Comments” in *Quantum Theory and the Schism in Physics*, Popper compares metaphysical research programmes to Kuhn’s concept of paradigm, while stressing that metaphysical research programmes must be seen in terms of a situation that can be rationally reconsidered, and that scientific revolutions viewed as changes of paradigms are due to rational criticism. (*ibid.*)

One may consider Popper’s own work as a program in accordance with his metaphysical research program. This program is a combination of some epistemological, metaphysical and methodological theories such as objectivism (versus subjectivism), indeterminism (versus determinism), and realism (versus instrumentalism). Popper speaks of “the guiding role of metaphysics in constructing scientific hypotheses” (Popper 1983, pp. 192–193).

## Science, Protoscience, Pseudoscience and Metaphysics

It seems necessary to pay attention to meanings and differences of such important terms as “science”, “protoscience”, “pseudoscience”, and “metaphysics”. We saw the traditional meaning of metaphysics, though it has had variations from ancient times to our days.

One may choose some more or less appropriate definitions of “science”, “protoscience”, and “pseudoscience” from one of the dictionaries:

- Science is “knowledge or a system of knowledge covering general truths or the operation of general laws especially as obtained and tested through the scientific method and concerned with the physical world and its phenomena” (*Merriam-Webster's Medical Dictionary* 2002).
- Protoscience is “a set of beliefs or theories that have not yet been tested adequately by the scientific method but which are otherwise consistent with existing science; a new science working to establish itself as legitimate science” (Kipfer 2002–2006).
- Pseudoscience is a “system of theories or assertions about the natural world that claim or appear to be scientific but that, in fact, are not. For example, astronomy is a science, but astrology is generally viewed as a pseudoscience” (*The American Heritage New Dictionary of Cultural Literacy* 2005).

It seems that there has been a confusion between protoscience and pseudoscience in Kuhn's reflections just 3 years after *The Structure of Scientific Revolutions* (1962) at a famous international colloquium with Popper in the chair<sup>4</sup>:

In any case, there are many fields — I shall call them proto-sciences — in which practice does generate testable conclusions but which nevertheless resemble philosophy and the arts rather than the established sciences in their developmental patterns. I think, for example, of fields like chemistry and electricity before the mid-eighteenth century, of the study of heredity and phylogeny before the mid-nineteenth, or of many of the social sciences today. In these fields, too, though they satisfy Sir Karl [Popper]'s demarcation criterion, incessant criticism and continual striving for a fresh start are primary forces, and need to be. No more than in philosophy and the arts, however, do they result in clear-cut progress. I conclude, in short, that the proto-sciences, like the arts and philosophy, lack some element which, in the mature sciences, permits the more obvious forms of progress. It is not, however, anything that a methodological prescription can provide. Unlike my present critics, Lakatos at this point included, I claim no therapy to assist the transformation of a proto-science to a science, nor do I suppose anything of this sort is to be had. (Kuhn 1970, pp. 244–245)

It is reasonable to expect one to pay attention to a criterion such as testability, in practice or in principle, for distinguishing protoscience from pseudoscience. But in such a symposium, in the presence of Popper, there is no remembering of “pseudoscience”. Of course, Popper himself had insisted that “the criterion of demarcation cannot be an absolutely sharp one” (Popper 1963, p. 346). There are several parameters responsible to make difficult any sharp distinction between (real) science, protoscience, and pseudo science. We may agree with Feyerabend's observations in concluding chapter of his *Against Method*:

The idea that science can, and should, be run according to fixed and universal rules, is both unrealistic and pernicious. It is *unrealistic*, for it takes too simple a view of the talents of man and of the circumstances which encourage, or cause, their development. And it is *pernicious*, for the attempt to enforce the rules is bound to increase our professional qualifications at the expense of our humanity. In addition, the idea is *detrimental to science*, for it neglects the complex physical and historical conditions which influence scientific change. It makes our science less adaptable and more dogmatic: every methodological rule is associated with cosmological assumptions, so that using the rule we take it for granted that the assumptions are correct. (Feyerabend 1975, p. 295)

I classify science, protoscience, and pseudoscience in one family with their “family resemblances”, while I see no resemblance between this family and metaphysics except the existence of some (more or less) metaphysical and extra-scientific elements in bodies of knowledge that are classified as science, protoscience, and pseudoscience. I agree with Popper’s and Feyerabend’s emphases on not so much sharp distinction between science and nonscience. But, in spite of such emphases, and even the proclamation of “The demise of the demarcation problem” by Larry Laudan (1983), I emphasize on necessary and enough attention to both resemblances and differences.

## Natural Philosophy

Traditionally, up to the development of modern science, “natural philosophy”, has been understood as the objective study of nature. All Greek great Pre-Socratic philosophers, from Thales to Democritus, wished to explain the evolutions of nature and the physical universe in the framework of their own natural philosophy mixed with their particular metaphysics. Plato and Aristotle flourished in such an atmosphere of naturalistic explanation of the universe and nature. It is not amazing that a great metaphysician such as Plato (or Socrates (?)), in *Charmides*, as one of his earliest dialogues, takes the position of a natural philosopher or philosopher of science, and tries to draw a distinction between the sciences which produce theoretical physical results, and the bodies of knowledge having no such results. One may consider Plato’s *Timaeus* as a metaphysical dialogue. It may be more plausible to consider Aristotle’s *Physics* as a good example of natural philosophy.

Following Aristotle’s division of philosophy into a theoretical and a practical part, the theoretical one has been divided into three disciplines in both Islamic and Western Peripatetic traditions: “First Philosophy”, “Mathematics” (arithmetic, geometry, astronomy and music), and “Natural Philosophy”. “Natural Philosophy” has been a combination of some “scientific researches” on nature and natural objects on the one hand, and philosophical reflections leading to some metaphysical fundamentals on the other hand. Referring to Aristotle’s works on natural philosophy, one may have a survey of main topics of the discipline: cause and effect, motion and change, matter and elements, divisibility of matter, physical entities and the relations between them, natural qualities, space, time, celestial bodies, and macro-structure of the universe or cosmos. What is interesting is Popper’s admiring approach towards Plato’s theory of Ideas, according to Aristotle’s reports and “Plato’s own arguments proffered in the *Timaeus*”. Popper has an interpretation of Plato’s view which, though lengthy, is worth to be quoted: Plato’s account

shows that Plato’s fundamental problem was to find a *scientific method*<sup>5</sup> of dealing with sensible things. He wanted to obtain purely rational knowledge, and not merely opinion; and since pure knowledge of sensible things could not be obtained, he insisted, as mentioned before, on obtaining at least such pure knowledge as was in some way related, and applicable, to sensible things. Knowledge of the Forms or Ideas fulfilled this demand, [...]<sup>6</sup> According to our analysis, the theory of Forms or Ideas has at least three different functions

in Plato's philosophy, (1) It is a most important *methodological device*<sup>7</sup>, for it makes possible *pure scientific knowledge*<sup>8</sup>, and even knowledge which could be applied to the world of changing things of which we cannot immediately obtain any knowledge, but only opinion. [...] (2) It provides the clue to the urgently needed *theory of change*, and of decay, to a theory of generation and degeneration, and especially, the clue to history. (3) It opens a way, in the social realm, towards some kind of social engineering; (Popper 1954, pp. 38–39)

No doubt, Plato's theory of Ideas has some scientific echo, both theoretical and practical, in Popper's interpretation. Moreover, we are reasonably entitled to consider the traditional natural philosophy as the precursor of our modern natural science, especially physics, which historically have developed out of philosophy. So it cannot be denied that the history of natural philosophy has been continued through the history of natural sciences such as physics, chemistry, biology and geology. On the other hand, one must find the historical background of such sciences in the history of natural philosophy.

## Atomism: Metaphysical or Scientific

According to atomism, as a theory in natural philosophy, all the natural objects are composed of small, indivisible and indestructible building blocks. Atomism has had both Greek and Indian versions. Greek atomism, developed by Later Ionians, has preserved some of its epistemic characteristics through the development of physics and chemistry, though the contemporary indivisible and indestructible building blocks of physical nature are not ancient atoms but some other elementary entities such as, for example, quarks and/or strings.

Popper, in different places, cites atomism as an example of a metaphysical theory (Popper 1983, p. 192, see also Popper 1959, and 1963). Popper holds that a theory must expose itself "to attempted empirical refutations whose failure" means "the success of the theory". He regards this procedure as a process during which the theory in question is confirmed or corroborated (to be scientific): "only if a theory successfully withstands the pressure of these attempted refutations can we claim that it is confirmed or corroborated by experience" (Popper 1963, pp. 345–346).

He speaks of the increase in confirmability with testability:

[S]ome theories expose themselves to possible refutations more boldly than others. [...] A theory which is more precise and more easily refutable than another will also be the more interesting one. Since it is the more daring one, it will be the one which is less probable. But it is better testable, for we can make our tests more precise and more severe. And if it stands up to severe tests it will be better confirmed, or better attested, by these tests. Thus confirmability (or attestability or corroborability) must increase with testability. (Popper 1963, p. 346)

As we saw, Popper describes as metaphysical those theories which are non-testable and, therefore, of no interest to empirical scientists.

This indicates that the criterion of demarcation cannot be an absolutely sharp one but will itself have degrees. There will be well-testable theories, hardly testable theories, and non-testable theories. (*ibid.*)

Popper argues that the line of demarcation must not be drawn too sharply. He has regarded atomism as a myth in both his *Logic of Scientific Discovery* and *Conjectures and Refutations*.

This becomes clear if we remember that most of our scientific theories originate in myths. The Copernican system, for example, was inspired by a Neo-Platonic worship of the light of the Sun who had to occupy the 'centre' because of his nobility. This indicates how myths may develop testable components. They may, in the course of discussion, become fruitful and important for science. In my *Logic of Scientific Discovery* I gave several examples of myths which have become most important for science, among them atomism and the corpuscular theory of light. (Popper 1963, p. 347)

The subject referred to *Logic of Scientific Discovery* is from the page 277 as continuation of the exemplum cited above in which Popper mentions the idea "atomism" as one of the examples – "the idea of a single physical 'principle' or ultimate element (from which the others derive)". Popper continues to speak of atomism "as a metaphysical theory" and its impact on science (Popper 1983, p. 192).

But, as history of science shows, ancient atomism is rather a scientific theory than a metaphysical one, though it is inoculated with metaphysical elements in the sense of untestable elements. Even though one cannot regard it as science, one is entitled to regard it as "protoscience" and of the same genre as science.

Popper has regarded this protoscience as metaphysics. This conception of metaphysics is both incorrect and lowers the status of metaphysics. Protoscience, of course, has some empirically untestable elements without being called "metaphysical".

Popper, criticizing Whitehead's views in his *Process and Reality*, tries to show an important difference between physics (as an exemplary science or scientific discipline) and metaphysics:

[W]hile physics progresses, metaphysics does not. In physics, there is a 'proper test of progress', namely the test of experiment, of practice. We can say why modern physics is better than the physics of the seventeenth century. Modern physics stands up to a great number of practical tests which utterly defeat the older systems. And the obvious objection against speculative metaphysical systems is that the progress they claim seems to be just as imaginary as anything else about them. This objection is very old; it dates back to Bacon, Hume, and Kant. (Popper 1945, p. 443)

Why is it not allowed to consider the ancient atomism as a stage of a scientific theory that has had its own progresses? It is plausible to make use of 'proper test of progress' for explaining a more or less continuous history of a scientific theory of the ultimate structure of matter. One should not deny the continuity of the development of the theory throughout the history of thought, so that one might say that the theory has had its own (more or less continuous) history. All scientific efforts to find elementary indivisible constituents of matter, be atom, quark, electron, or any other smaller fundamental particle, are more or less in line with ancient atomism of natural philosophy.

No doubt, atomism in all of its displays, from Democritus up to our era, has been meaningful. There is no segment in the chain of the development of atomic theory that one can regard as meaningless. Each segment is linked with its previous and subsequent segments.

All segments of the chain have been both meaningful and of the same genre as science mixed with some empirically untestable (or even irrefutable) elements. These elements do not make the segments metaphysical. All segments are, therefore, scientific/protoscientific in character not metaphysical.

## The Difficulties with Popper's Conception of Metaphysics

It is evident that there is a serious opposition between Popper's conception of metaphysics and the traditional meaning of the term. We do not deem it necessary to try further to show such an opposition. But it seems that this opposition implies some difficulties in explanation of the development of some scientific theories throughout their histories.

There is a problem in relation to the temporal character of Popper's idea of metaphysical research programmes. The residual status of metaphysics in Popper's philosophy brings about some complication.

The metaphysical research programme is not atemporal and eternal like the ontological foundations of the essentialists and of the Positivists. It is part of the historical problem situation at a particular juncture in the history of a science, and it is also untestable at the point in time, and therefore "metaphysical" in Popper's sense. Most notably in Popper's view, at the given point in the history of the science the metaphysical research program functions as an ontological criterion for what constitutes a satisfactory explanation. This complication arises from Popper's way of demarcating between science and metaphysics. (Hickey 1995/2005)

Popper, in 1958, in "On the Status of Science and of Metaphysics" which has been reprinted in *Conjectures and Refutations*,

says that one can discuss irrefutable metaphysical theories rationally in the sense that one can discuss their ability to solve the problems that they purport to solve, that is, in relation to their problem situation.

This complication has its origin in the residual status of metaphysics in Popper's philosophy. Metaphysics for him contains a great heterogeneity of types of knowledge, which need have nothing in common, but their irrefutable character and therefore their nonscientific status. Historically philosophers have not treated metaphysics in so residual a manner, but instead have offered positive characterizations of metaphysics, which have sometimes been called "transcendental metaphysics", and which are not typically viewed merely as protoscience. (*ibid.*)

In analyzing and criticizing Popper's conception of metaphysics, it seems necessary to pay attention to some relatively critical points concerning the scientific and the metaphysical:

- To regard some theory as scientific does not mean to regard it as correct.
- To regard some theory as scientific does not mean to regard it as composed of elements that all of them are to be empirically testable.
- It is not justified to regard any theory mixed with empirically untestable elements as metaphysical.



- It is not justified to regard the falsifiable as one and the same as the empirically testable; we may say that there is no one-to-one correspondence between falsifiability and empirical testability.

To show one of the difficulties occurred through Popper's special conception of metaphysics, we may construct an inference:

1. Scientific theory (i.e. science) is falsifiable (as Popper thinks).
2. Protoscience is merged into metaphysical statements (i.e. metaphysics) so that we may speak of protoscientific metaphysics, or metaphysical protoscience which is essentially metaphysical (as Popper thinks).
3. Protoscience is of the same genre as science (mixed with some empirically untestable (or even irrefutable) elements (as I think)).

Therefore, metaphysics must be falsifiable!

To explain the claim, suppose that a meaningful scientific theory **S** has been originated from a metaphysical "theory" **M** through subsequent scientific theories **S**<sub>1</sub>, **S**<sub>2</sub>, **S**<sub>3</sub>, ....

Firstly, if **S** is meaningful, then **S**<sub>1</sub>, **S**<sub>2</sub>, **S**<sub>3</sub>, ... and even **M** must have been meaningful.

Secondly, if **S** is scientific, then **S**<sub>1</sub>, **S**<sub>2</sub>, **S**<sub>3</sub>, ... and even **M** must have been scientific, although we accept that some elements and/or statements might have been entered from other fields.

Now, we may put forth two important questions, and reply to them:

1. Is it possible to combine **I** meaningless statements with **J** meaningful ones leading to a final meaningful conclusion?

No!

All statements must be meaningful in order to have the final statement as meaningful.

2. Is it possible to combine **K** nonscientific statements with **L** scientific ones leading to a final scientific conclusion?

Yes!

But the first statement, **M**, and principal/core statements **S**<sub>1</sub>, **S**<sub>2</sub>, **S**<sub>3</sub>, ... must have been scientific.

There are extrascientific metaphysical elements unifying scientific statements. Those **K** nonscientific statements are metaphysical ones, some extrascientific elements, some colorless links connecting **L** scientific statements (**M**, **S**<sub>1</sub>, **S**<sub>2</sub>, **S**<sub>3</sub>, ...) to each other to make the final scientific theory.

One may agree with Popper that such theories or ideas will get scientific character only if they are presented in a falsifiable form, and if it is possible to decide empirically between them and competing ones.

Popper's view on metaphysics leads us to a "falsifiable metaphysics"! Margaret Masterman has spoken of such seemingly self-contradictory terminology in the presence of Popper (Masterman 1970, pp. 67 and 71). It is a "science emerging out of metaphysics"; it is a metaphysics out of which a science emerges. Is it plausible to think that there is really such a thing as science-metaphysics or metaphysics that is falsifiable?

## Conclusion

We saw that there is a serious deviation from traditional connotation of the term metaphysics in Popper's conception of the term. The traditional conception has had and continues to have a brilliant spectrum of advocates throughout the history of philosophy, but there is no correspondence between such a conception and Popper's one. Popper's attitude towards metaphysics as protoscience leads to some critical problems in explanation of history of science, progress of science, the nature of scientific theory, and the structure of science. How is it possible to regard the development of a scientific theory as a "metaphysical research programme" and, at the same time, to expect the origination of some empirically testable theory from something that it is not empirically testable?

I insist on the fact that science, protoscience, and pseudoscience belong to a big family with close or far (inborn) resemblances in qualities between its members, while there is no family relation between these theoretical-practical activities and metaphysics except the existence of some metaphysical and/or extra-scientific elements that may seem necessary or unnecessary. In this regard, we must modify Feyerabend's and Laudan's views on the one hand and Popper's account on the other hand: it is true that

- There are certain differences between science, protoscience, pseudoscience as the members of the same family.
- There is no family relationship and resemblance between these activities and metaphysics (modification of Feyerabend and Laudan's views).

It seems that Popper's position is only half of the project of giving back meaningfulness to metaphysics. It is OK to do such a thing! Metaphysics does have meaning! It is recovering the place of metaphysics in philosophy particularly through the analytic philosophical efforts. But metaphysics is neither protoscience nor a member of science family. One may wish to see metaphysics as a "science" in a Kantian project; but the difference of this "science" and science as manifested in natural science is too obvious to be mentioned!

## Notes

1. One may find a review of various versions of the criterion of meaning in Carl Hempel (1950).
  2. Such an attitude, as he himself says, is apparent from his "first publication on this subject ('Ein Kriterium des empirischen Charakters theoretischer Systeme', *Erkenntnis*, 3, 1933, pp. 426 ff., now in *The Logic of Scientific Discovery*, pp. 312–314, see also Sections 4 and 10)" (Popper 1963, p. 348).
  3. Popper's Postscript to the *Logic of Scientific Discovery*, edited by William W. Bartley, appeared in three volumes in 1982 and 1983: *Realism and the Aim of Science* as the 1st volume, *Open Universe: An Argument for Indeterminism* as the 2nd volume, and *Quantum Theory and the Schism in Physics* as the 3rd volume.
  4. The international colloquium held in London in 1965, the papers presented to which were edited by Lakatos and Musgrave (1970).
- 5,6,7,8,9. My italics.

## References

- Akrami, Musa. 1384/2005. *Kant va Mā ba' d-a ttabi' a (Kant and Metaphysics)*. Tehran: Gām-e Now.
- Aristotle. 2001. *Metaphysics*. Translated by Joe Sachs. Santa Fe, NM: Green Lion Press. 2nd edition.
- Aristotle. 2007. *Metaphysics*. Translated by W. D. Ross. eBooks @ Adelaide. <http://ebooks.adelaide.edu.au/a/aristotle/metaphysics/>. Accessed 20 September 2007.
- Ayer, Alfred Jules. 1936/1952. *Language, Truth and Logic*. New York: Dover Publications.
- Feyerabend, Paul. 1975. *Against Method*. New York: Humanities Press.
- Grier, Michelle. 2004. *Kant's Critique of Metaphysics*. Stanford Encyclopedia of Philosophy. <http://plato.stanford.edu/entries/kant-metaphysics/>. Accessed 22 September 2007.
- Hempel, Carl. 1950. Problems and Changes in the Empiricist Criterion of Meaning. *Revue Internationale de Philosophie* 41: 41–63.
- Hickey, Thomas J. 1995/2005. *History of Twentieth-Century Philosophy of Science*. Book V: Karl Popper and Falsificationist Criticism. Page 5. <http://www.philsci.com/book5-5.html/>. Accessed 20 September 2007.
- Hume, David. 1748/2004. *An Enquiry Concerning Human Understanding*. Courier Dover Publications.
- Ibn Sina. 1960. al-Isharat wa-'l-tanbihat (*Remarks and Admonitions*). Part three. Cairo: S. Dunya.
- Kant, Immanuel. 1781/1997. *Critique of Pure Reason*. Translated by Paul Guyer and Allen W. Wood. Cambridge: Cambridge University Press.
- Kant, Immanuel. 1783/1996. *Prolegomena to any Future Metaphysics*. Translated by Paul Carus. Reprinted in Immanuel Kant's *Prolegomena to any Future Metaphysic in Focus*, ed. Beryl Logan. New York: Routledge.
- Kipfer, Barbara Ann, ed. 2002–2006. *Webster's New Millennium Dictionary of English*. Preview Edition. LLC Long Beach, CA: Copyright by Lexico Publishing Group. <http://dictionary.reference.com/help/wmde.html/>. Accessed 21 September 2007.
- Kuhn, Thomas Samuel. 1970. Reflections on My Critics. In *Criticism and the Growth of Knowledge*, eds. Imre Lakatos and Alan Musgrave, 231–277. Cambridge: Cambridge University Press.
- Laudan, Larry. 1983. The Demise of the Demarcation Problem. In *Physics, Philosophy and Psychoanalysis*, eds. Robert S. Cohen and Larry Laudan, 111–128. Dordrecht: Reidel.
- Masterman, Margaret. 1970. The Nature of a Paradigm. In *Criticism and the Growth of Knowledge*, eds. Imre Lakatos and Alan Musgrave, 59–90. Cambridge: Cambridge University Press.
- Merriam-Webster's Medical Desk Dictionary*. 2002. Revised Edition. Merriam-Webster. <http://dictionary.reference.com/help/mwmed.html/>. Accessed 21 September 2007.
- Owens, Joseph. 1951/1978. *The Doctrine of Being in the Aristotelian "Metaphysics"*. Toronto, ON: Pontifical Institute of Mediaeval Studies. Third Edition. Revised.
- Popper, Karl Raimund. 1945/1966. *The Open Society and Its Enemies*. Fifth edition (revised). London: Routledge.
- Popper, Karl Raimund. 1959/1980. *The Logic of Scientific Discovery*. Tenth impression (revised). London: Unwin Hyman.
- Popper, Karl Raimund. 1963/2002. *Conjectures and Refutations: The Growth of Scientific Knowledge*. London: Routledge.
- Popper, Karl Raimund. 1982a. *Open Universe: An Argument for Indeterminism* (Vol. 2 of the Postscript to *The Logic of Scientific Discovery*). London: Hutchinson.
- Popper, Karl Raimund. 1982b. *Quantum Theory and the Schism in Physics* (Vol. 3 of the Postscript to *The Logic of Scientific Discovery*). London: Hutchinson.
- Popper, Karl Raimund. 1983. *Realism and the Aim of Science* (Vol. 1 of the Postscript to *The Logic of Scientific Discovery*). London: Hutchinson.
- The American Heritage New Dictionary of Cultural Literacy*. 2005. New York: Houghton Mifflin. <http://dictionary.reference.com/help/ahcl.html/>. Accessed 21 September 2007.

# Review

*Out of Error: Further Essays on Critical Rationalism*

By David Miller

Ashgate, 2006, pp. 314, £55.

In his new book, Miller returns to his central philosophical interest – to critical rationalism. Readers who are familiar with his previous book *Critical Rationalism. A Restatement and Defence* (Open Court 1994) know that Miller there reaffirms and further develops Popper's falsificationism and considers it not just a methodological issue relevant to science but a philosophical issue of rationality. In what new directions does *Out of Error* take us, given the fact that *Critical Rationalism* presents a pretty comprehensive account of the most important problems of Popper's methodology, including a systematic enumeration of objections voiced by his critics over the years, followed by their elimination? In this review, I will argue that readers will not be disappointed; Miller both provides new insights to the problems he dealt with before and addresses new problems, especially problems concerning applied science, the demarcation criterion, the use of Popper's rationalism against the fashionable postmodern currents, and the employment of paraconsistent logic in falsificationism.

The book can be divided into three main parts: chapters 1, 14 were written in memoriam; in the second part (chapters 2–7) Miller carries out a philosophical investigation of critical rationalism; the third part (chapters 8–13) is more technical and deals with various logical aspects of critical rationalism. I will focus on and discuss mainly the problems of the first part of *Out of Error*.

The first chapter, *Karl Popper: A Scientific Memoir*, and the last chapter, *In Memoriam*, were written after Popper's death and Miller looks at Popper's intellectual development (1) and links to his life and personality (14). Since the (pre)-war period is well documented by Hacohe, readers will probably find more interesting Miller's description of the time that Popper spent in England. Miller, a graduate student at LSE, one of Popper's assistants, his colleague and life-long friend, guides us through Popper's academic and personal life in great detail and with moving sensitivity.

Chapter 2 defines three basic components of critical rationalism that, as Miller stresses, must *all* be accepted if we want to become truly critical rationalist. The *Three Stages of Critical Rationalism* are fallibilism (denial of certainty), negativism (exclusively negative method of testing) and scepticism (utter impossibility of ever justifying anything). There is not much trouble with the first (epistemological) stage, but it takes some courage to accept all the implications of the second

(methodological) and the third (epistemological), especially in Miller's version, which is even more radical than Popper's. Miller makes it clear that the negative method and the absolute scepticism are non-negotiable and must not and need not be tainted with any inductivist or justificationist flavour; at the same time, radical falsificationism should not be confused with irrationalism and relativism. This is the leading idea throughout Miller's book, only briefly touched upon in this chapter and developed in various directions in the following chapters. I appreciate the emphasis that Miller puts on the negative method as constituting *reason*; reason is thus defined by its negative *action*, not by *provisions* of (good) reasons. "Rationality ... is wholly a matter of method" (p. 50), he says, and the "disappearance of the epistemological dimension of rationality is what is least understood in Popper's philosophy" (50) and what Miller set out to rehabilitate.

In this chapter, Miller also makes a sharp distinction between falsificationism and justificationism (positivism) in their respective philosophical focus. Critical rationalists are in their empirical investigations concerned primarily about our relation to the world, justificationists are concerned about our relation to theories (about what can we *do to the theory*). Critical rationalism proposes that "theories are unchanged by their interaction with experience" (p. 54) and even falsification of a theory is a decision that "goes beyond the empirical investigation itself" (*ibid*). I take it that Miller wants to show a change of emphasis in critical rationalism from knowledge to learning, learning itself facilitated by critical investigation of the conclusions of the theory. It is out of error that new problems are born. This message, however, does not come out entirely clearly; perhaps, Miller could have given more space to explaining these issues instead of including in this chapter yet another portion of personal reminiscences.

Miller makes it up to us in Chapter 3 in which he elaborates on the negative, destructive methodology of critical rationalism. The problem in the title of this chapter, *What Do Arguments Achieve?*, is a serious one and Miller outlines it in the opening paragraph (p. 63): "Arguments are either conclusive or they are inconclusive. Conclusive arguments are deductive arguments, and are circular; inconclusive arguments are non-deductive and are invalid. Circularity and invalidity are bad things. Why, then, bother with arguments? What do arguments achieve?" His reply follows two main lines: 1) arguments achieve nothing if we expect from them some kind of justification of the investigated proposition, and 2) arguments do not matter; "... the total repudiation of all schemes of partial justification, the total repudiation of any attempt to use arguments to support the hypothesis that we adhere to ... amounts to saying that it is not the argument that matters, but only the conclusion" (p. 78) and, of course, our determination to show their falsity. Critical arguments criticize *and stop there*; this is enough to promote the search for truth - not (partially) justified truth.

Miller disqualifies three types of answers, namely persuasion (dogmatic), discovery and justification (cannot be done, for logical reason sketched in the opening). Miller then explores the "unnourishing pastures of inconclusive justification" (p. 75), (Lipton, Bunge, Musgrave, Pargetter & Bigelow and others) and concludes that the considerations of probability or 'degrees of rational belief' or reasonableness

make “little impact on the problem of justifying inductive inferences” (*ibid*). He rejects the criticism blaming critical rationalism for destroying any secure (rational) foundations for the acceptance or the elimination of hypotheses (after all, theories cannot be confirmed or disconfirmed by experience); “... if we are lucky we can sometimes be right (though we never know when)”, says Miller, and wonders why this should surprise justificationists who, too, do not think that the methods of justification are persistently successful.

Also worth noting is Miller’s stern warning against linking his denial of the relevance of arguments (that may sound relativistic) to postmodernism, the proponents of which happily revel in relativism and “dismiss objective truth as an uninteresting delusion” (p. 79). This warning taken up in Chapter 6 entitled *Back to the Frying Pan*. Miller is infuriated by the argument offered by Sokal & Bricmont in *Intellectual Impostures* (1998) that Popper – in his extremely negative falsificationism—contributes to the rise of irrationalism. Of course, Miller is sympathetic to Sokal’s and Bricmont’s undertaking to mock the fashionable postmodern trends and to expose their pretentious postmodern jargon as gibberish. But they commit, says Miller, exactly the mistake he himself warned against, and proceeds to list the ‘vices’ frequently attributed to Popper (and also cited by Sokal and Bricmont); these are the destructive and too complicated nature of falsification, the absence of good reason, the lack of grounds for reliable predictions. Miller then argues that the alleged vices are in fact virtues, enabling us to formulate “a smarter, leaner, tougher, more wholesome, and less dissolute rationalism than most rationalists are accustomed to” (p. 133).

As much as I agree with Miller in promoting critical (here “radical”) rationalism, I am not impressed with his depiction of postmodernism. He throws all postmodernists (naming just a few criticized by Sokal & Bricmontat, and omitting the most representative figures of postmodernism) into one huge sack, without giving any serious consideration either to their ideas or to their differences. Despite not having discussed any of their philosophies he does not hesitate to call them names: fad, ruffians characterized by insulting infantilism or meretricious desipience (pp. 64, 133). Such scorn for postmodernism does not help the case of critical rationalism. It would be more effective to consider that *Popper shares with postmodernists a relevant concern* about the dominance of the authoritarian rationalism in the Western philosophical tradition; in this tradition, philosophy is seen as a discipline having a privileged access to some indubitable foundations (metaphysical, epistemological, textual) that can legitimate certain discourses or, in Popper’s terminology, justify certain knowledge – and thus enable to make meaning of signs or the truth manifest. Whether we call these philosophical models classical rationalism, logocentrism or the metaphysics of presence does not change the basic fact that they all are united by the need for grounding, this being the primary target of criticism for both postmodernism and Popper. Taking postmodernism more seriously places Popper right in the middle of the contemporary philosophical disputes and strengthens the impact of Popper’s solution as an alternative to both the discredited, hollow authoritarian principles endorsed by the old oligarchs of ‘rationalism’ (p. 145) and postmodern relativism.

In *Falsifiability: More than a Convention?* (Chapter 4), Miller “treads over well-trodden ground” (p. 81) of the problem of demarcation between science and non-science (or, as he prefers it, between what has / does not have empirical content). He takes the view that falsifiability is more than a mere convention but an “irresistible proposal” enabling us to make “sense [of science] as an objective enterprise” (p. 81). He then ties the problem of demarcation to the problem of how we learn from experience and criticizes the inductive models in which “*what you know is what you learn* (WYKIWYL)” (p. 85). By contrast to the view that learning produces knowledge, Popper’s epistemology begins with another question, namely “*what we can know through experience*”? (p. 83). Miller’s answer is, as we might expect, that we learn from experience that our hypotheses are false. Therefore, knowledge (however poor) must come first and a hypothesis must be accepted before it is rejected (or tested). This is the crucial point of this chapter and Miller goes well beyond Popper who sometimes wrote about acceptance in an inductivist sense; for Miller, “testing cancels acceptance, but it does not create it” (p. 125). In other words, “*there is no such activity as accepting as true a hypothesis that has already been accepted*” (p. 91). If the hypothesis is not falsified nothing happens to it, “all unfuted hypotheses are epistemologically equivalent” (p. 86). Miller also deals with some objections against this deductive epistemological conception. He rejects the methods of so-called data mining (popular in the sciences), questions the objections typical for naturalism and naturalized epistemology, namely that the history of science does not conform to the conjectures-refutations pattern, discusses the relevance for critical rationalism of Duhem’s thesis, and also deals with the problem of existential statements in science. To me, the most interesting issue is Miller’s dispute with naturalism, which, as he shows, sets aside justification-centered epistemology but at the cost of resigning its normative role; a price that we should not and need not pay.

I can fully understand that Miller’s firm stand on the priority of acceptance to testing is necessary in order to remove any ‘whiff’ of induction from corroboration. But why is it necessary, in order to maintain the hypothetico-deductivism in methodology (and rationality), to deny that induction might play a role in learning? I am probably missing something important here but it is not clear to me why we could not simply declare that knowledge is logically antecedent to testing regardless of how it is produced; from the rational perspective, all knowledge is purely conjectural and only refutation-attempts are permitted in evaluating it. The imperative of falsification can be maintained as a methodological norm and, simultaneously, as a standard of reason. Once we rid reason of its justificationist role we have a logically consistent theory of rationality (*ratio negativa*) defining reason by its critical activity; the procedures of knowledge acquisition (or belief formation) should be investigated empirically, not forced to comply to a logical pattern, as Popper suggests in his principle of transference (“what holds in logic ... holds in psychology”, OK, p. 24).

In Chapter 5, *Induction: A Problem Solved*, Miller defends the purist deductive position against some objections that blame critical rationalism for not providing a rational criterion of acceptance of theories, for its lack of predictive import and for



the implicit inductive assumption of the uniformity of nature. Popper's notorious remarks – that it is rational to prefer the best-tested theory as a basis of action (since it has greater verisimilitude) – are often quoted and several objections are raised: Popper's advice assumes that the future will be like the past (inductivism) and if it does not then the rationality of our preference between theories and their use in the applied science are undermined; science ends up in irrationalism. Miller again rejects this 'clumsy insistence' on the need of grounding for rationality, as it was first declared by Lakatos and then further supported in various forms by Worrall, Zahar, Musgrave, O'Hear, Watkins, Shearmur, Salmon and others.

A critical rationalist, says Miller, should not give any advice on what theory we should prefer: "What must not be admitted is the suggestion that a proposal that has been subjected to critical scrutiny, and has survived it, thereby qualifies as a better proposal than one that has not been subjected to critical scrutiny. That would convict deductivism not only of inductivism but of clairvoyance ..." (p. 124). Miller recommends a more ascetic approach than Popper did and replaces Popper's positive rule of choice by a negative one: "*Refrain from any practical proposal that does not survive critical scrutiny as well as others do*" (*ibid*). This approach involves a radical change of perspective; in critical rationalism one accepts that the process of the growth of knowledge, as well as the process of living, is an open-ended and risky yet a rational business. Its rational core, however, lies purely in its critical method, not in justifying its own activities, achievements or decisions. In other words, the fact that "neither beliefs nor acts of belief, nor decisions, nor even preferences, are reasonable (p. 111) ...does not imply that people cannot think or make decisions reasonably or rationally" (p. 112). If an agent accepts spatio-temporally universal generalizations he also accepts that in some respect the future will resemble the past. But "no metaphysical principle of induction is needed to generate such predictions" (p. 115) since accepting a proposition involves accepting as true all its logical consequences – "universal laws do pass judgment on their own future performance, and if we classify them as true, so do we" (p. 116). Yet, Miller continues, "it seems not to be reason, but another decision, that leads us to adopt the best-tested theory as the basis for action" (p. 118). No need to add that this decision contains no warranty for the future: "if [the agent] desires to avoid ill success tomorrow, he can cross his fingers or he can make the effort to be rational today" (p. 119). Miller then discusses the problem of applied science and makes a sharp distinction between science and technology; the latter being concerned with generic plans and practical proposals how to 'tame' nature according to our designs. He provocatively says that "for most of their histories science and technology proceed independently of each other" (p. 122) and that "science has very little to contribute to the advance of technology except for criticism" (p. 123). This startling statement certainly needs a more thorough explanation than Miller provides here.

*Being an Absolute Sceptic* (Chapter 7) was originally published in *Science* and considers the relevance of critical rationalism to scientists. Miller lists the following ways in which critical rationalism could beneficially influence science: scientists should stop exaggerating the power of scientific rationality and likewise, the public should moderate their expectations of what can be accomplished in science.

Further, scientists should be less harsh on their colleagues who make interesting but false conjectures, and more ready to admit ignorance. If scientists (as well as philosophers of science) cease to “to attribute to well-tested hypotheses a security or reliability that they do not possess” – but stop short of a slide into relativism – it is possible to “rescue science as a ... rational enterprise *par excellence*” (p. 150).

The more technical part of *Out of Error* now follows in a brief overview. Chapter 8 can be characterized as an investigation within the inductivist tradition, an attempt to see whether good sense can be made of the idea that induction ‘presupposes’ the uniformity of nature, especially in the context of the practice known as ‘assumption spotting’ (p. 69). Chapter 9 deals with the problem that statements are language constructs that may not correspond to facts; it is an external relation. Miller imposes two requirements, namely Tarski’s requirement that the language in which truth is defined (the metalanguage) assumes more than the language for which it is defined (the object language)—this is why acceptance of a statement as true goes beyond ‘the statement itself’- and the dualism of facts and decisions – this is why acceptance of a statement as true goes ‘beyond its truth’. In Chapter 10 Miller subjects to rigorous criticism the somewhat unsophisticated proposal of McAllister, endorsed by Kuipers, that theoretical beauty is somehow indicative of increased truthlikeness. The main thesis of Chapter 11 is that the many attempts to defend language-dependent theories of verisimilitude all fail. Miller admits that we must reject all such theories and all theories that tie verisimilitude to syntactic structure, despite the fact that there is not much to put in their place. In Chapter 12 Miller discusses the suggestion that certain Central African peoples (the Azande and the Nuer) adhere to inconsistent systems of belief. He argues that it is not desirable to ascribe paraconsistent thinking to any groups who appear to hold contradictory beliefs, or do not endorse all the consequences of what that they believe, be it the Azande, or the Nuer, or white Westerners. In Chapter 13, Miller develops critical rationalism further beyond the Popperian framework and comes up with the suggestion that we should introduce into falsificationism some kind of paraconsistent logic, specifically a dual-intuitionistic system. To those who think that he lost his grip on philosophical reality by doing exactly what was dismissed by Popper in his criticism of dialectics, Miller replies that we can make fruitful use of paraconsistent logic in the task of understanding the permanent existence of problems (contradictions). He calls this alliance “a perfect marriage of convenience” (p. 250) and explains how the dual-intuitionistic logic arises naturally within a classical falsificationist context.

*Out of Error* is a thought-provoking book written with clarity and punch. Inevitably, certain key arguments from *Critical Rationalism* are repeated – and they should be. The principles of radical falsificationism need to re-affirmed for several reasons – and here I speak as Miller’s sympathizer. First of all, they are frequently misunderstood and various alliances of critical rationalism with some elements of justificationism are sought and deemed necessary for rationality. Secondly, Popper’s ban on justification represents a radical denial of almost all traditional epistemology (perhaps except Socrates) and is hard to digest. As Miller remarks in *Critical Rationalism*, the majority of intellectuals are still justification junkies

to whom “cold turkey is recommended” (p. 49). Thirdly, Popper’s ambition was to propose more than just a methodology but a non-foundationalist theory of rationality. This philosophical challenge of critical rationalism has not been adequately appreciated in the anglophone world. On all these counts, *Out of Error* is a valuable contribution to contemporary debates on rationalism, a contribution that might help to revive Popper’s philosophical legacy.

Zuzana Parusniková

# Index\*

## A

Ach, N., 178  
 Ackerman, R., 300, 301  
 Adorno, T., 93, 95, 96, 266, 332  
 Adronicus,  
 Agassi, J., 47, 63, 68, 69, 128, 143, 144, 152,  
 218, 233, 233, 253, 258, 268, 269, 283,  
 292, 336, 357–367, 377, 393, 394, 395  
 Agrippa, 246  
 Aiton, E., 49, 59  
 Akrami, M., 397–415  
 Albert, H., 21, 22, 88, 91–99, 246, 279–281,  
 283, 332, 333  
 Alcibiades, 308, 316  
 Allen, N., 175, 178, 307  
 Alt, J.A., 270  
 Anaxagoras, 319  
 Andersson, G., 21–30, 199  
 Andreas-Salomé, L., 98  
 Anytus, 309  
 Appel, 98  
 Arancibia, 160  
 Aristotle, 54, 65, 69, 151, 173, 246, 392,  
 398–401, 410  
 Armstrong, D., 182  
 Armstrong, K., 21  
 Arrow, K., 360  
 Artigas, M., 346  
 Augustine, St., 151  
 Avicenna (IbnSina), 398, 399  
 Avineri, S., 277  
 Ayer, A.J., 8–9, 358, 403

## B

Bacon, F., 106, 117, 118, 128, 129, 132, 249,  
 251, 360, 404, 406, 412  
 Bailey, R., 387  
 Bar-Am, N., 63–70

Bartley, W.W., 27, 44, 82, 83, 85, 87, 88, 124,  
 185, 217, 228, 232, 233, 247, 346–347,  
 394, 415  
 Baruch, H., 305–319  
 Baumol, B., 364  
 Becker, 374  
 Bednar, M., 237–244  
 Bell, D., 274, 275, 300  
 Bellamy, R., 150, 242  
 Bellarmino, R., 48  
 Benhabib, S., 355  
 Benson, H., 307  
 Bergson, H., 252  
 Berkeley, L., 230, 354  
 Berthelot, R., 146  
 Bigelow, 418  
 Birner, J., 185–201  
 Black, M., 233  
 Blaug, M., 162  
 Bloor, D., 296–298, 300, 301  
 Boettke, P., 163  
 Bohendrieth,  
 Bohner, A.,  
 Bohr, N., 350  
 Boland, L., 367  
 Bolzano, B., 79  
 BonJour, L., 84  
 Borges, J.L., 40  
 Boyer, A., 245–254, 353  
 Boyle, L., 354, 364, 367  
 Brahe, T, 51, 52, 55  
 Brandwood, L., 318  
 Bratko, 106  
 Breuer, J., 120  
 Bricmont, A., 419  
 Brod, M, 59  
 Bronowski, J., 252  
 Brouwer, L.E.J., 186  
 Brown, M., 300, 301

---

\*The name of Karl Popper is not listed in the index.

Bubner, R., 98  
 Bucciantini, M., 59  
 Buchanan, A., 106, 275, 278  
 Bueno, O., 81–89  
 Bühler, K., 178, 181, 182, 188, 189, 200, 252,  
 257, 258, 261, 264–266, 268, 269, 270  
 Bunge, M., 230, 418  
 Burgess, T., 382, 386, 387  
 Burke, E., 211, 341  
 Burnet, 306, 307, 317, 318

## C

Caldwell, B., 186, 199  
 Campbell, D., 192, 200, 384  
 Carnap, R., 69, 95, 107, 108, 110, 138, 139,  
 140, 141, 151, 232, 252, 262, 269, 341,  
 354, 403, 404  
 Caspar, M., 48  
 Cassirer, E., 98  
 Castiglione, D., 242  
 Chaerephon, 308–310  
 Chalmers, A.F., 291, 300, 301  
 Charmides, 308, 410  
 Cheyne, C., 19  
 Cintora, A., 79  
 Clavius, C., 50  
 Coady, D., 232  
 Cohen, R.S., 248  
 Collingwood, R., 221  
 Colombe, A., 51  
 Columbus, C., 51, 84  
 Compton, K., 193, 225  
 Comte, A., 328  
 Conklin, K.R., 71  
 Copernicus, 49, 51–54, 60  
 Corcoran, J., 69  
 Corfield, D.M., 114  
 Craig, E., 284  
 Cranston, M., 300  
 Crescas, H., 392  
 Crick, B., 231  
 Critias, 308  
 Crito, 305–308, 315, 318, 319  
 Currie, G., 8, 9, 14–16

## D

d'Alembert, J., 136–138, 141  
 Dahrendorf, R., 96  
 Dale, J., 91  
 Darwin, C., 50, 196, 362  
 Davidson, D., 13, 14, 26, 355  
 de Groot, A., 175–178, 180, 183

de-Shalit, A., 277  
 Debreu, G., 360  
 Democritus, 410, 412  
 Dennett, 14  
 Derrida, J., 39–44  
 Descartes, R., 34, 48, 57, 136–138, 193  
 deVries, R., 199  
 Donahue, W., 49, 50, 60  
 Duhem, P., 11, 135, 136, 141–144, 146,  
 148–150, 220, 249, 420  
 Dworkin, R., 276

## E

Eccles, J., 182, 199, 200, 218  
 Eddington, A.S., 136  
 Edelman, G. M., 384  
 Eidlin, F., 155, 362  
 Einstein, A., 66, 93, 136, 152, 224, 233,  
 246, 250, 266, 327, 330, 350, 354,  
 359, 360, 366  
 Engels, F., 146  
 Erwin, E., 134  
 Euripides, 307  
 Evans-Pritchard, E., 359

## F

Fabricius, D., 55, 56, 60  
 Feigenbaum, J., 106  
 Feigl, H., 331  
 Ferrara, A., 277  
 Feyerabend, P.K., 95, 358, 409, 410, 415  
 Feys, R., 75  
 Fisher, S., 122  
 Forster, E.M., 6  
 Foucault, M., 40, 45, 221  
 Frank, P., 153  
 Frege, G., 186  
 Freud, S., 117–133, 136, 253, 404  
 Friedman, M., 361, 363, 364, 367  
 Fries, J.F., 25, 246, 247  
 Frijda, N., 176  
 Frisby, D., 332

## G

Gadamer, 98  
 Galileo, 47, 49–51, 54, 58, 59, 367, 377  
 Galston, W.A., 290, 300  
 Gattei, S., 47–60, 266, 270  
 Gavroglu, K., 19  
 Geiger, H., 152  
 Gentzen, G., 67, 68

Gerken, L., 242, 243  
 Geuss, R., 289, 291, 300  
 Gewirth, A., 277  
 Gillies, D., 59, 80, 103–115, 140, 150  
 Gingerich, O., 60  
 Glymour, C., 131  
 Gödel, K., 76, 251  
 Goldschmidt, R., 195  
 Goleman, D., 127  
 Gombrich, E., 354  
 Gonzalez, W., 156–158  
 Goodman, N., 226, 233, 248  
 Gorgias, 305, 307, 314, 318, 319  
 Gorton, W., 156, 158  
 Gottfried, P.E., 289  
 Goudaroulis, Y.,  
 Gould, S.J., 200  
 Gray, J.N., 287  
 Greenberg, R.P., 122  
 Grelling, K., 152  
 Grier, M., 402  
 Griere, R.,  
 Grossner, K., 94, 95  
 Grünbaum, A., 117–133, 227, 358  
 Gutman, A., 290

## H

Haack, S., 8–10, 17, 80  
 Habermas, J., 96, 98, 99, 120, 243, 266, 332, 333, 355  
 Hacıhamdioglu, C., 353  
 Hacohen, M.H., 32, 35, 44, 172, 185, 199, 219, 259, 268, 346, 417  
 Hamilton, L., 232  
 Hands, D., 367  
 Hanly, C., 120  
 Hansen, J.E., 162, 172, 219  
 Harré, R., 234  
 Havel, V., 1  
 Hayek, F.A., 157, 162, 173, 185–201, 211, 261, 262, 268  
 Hayes, C., 280, 281  
 Hebb, D., 201  
 Hegel, 34, 42, 89, 221, 223, 295, 300, 355  
 Heidegger, M., 31, 41, 91, 224, 252  
 Hempel, C.G., 138–140, 252, 357, 358, 360, 367, 415  
 Herf, J., 291  
 Hertel, R., 200  
 Herzog, R., 242, 243  
 Hesiod, 39, 45  
 Hickey, T.J., 408, 413  
 Hilbert, D., 69

Hippias, 311, 319  
 Hippocrates, 310–312, 316  
 Hitler, 210, 211  
 Hobbes, T., 230  
 Hofstadter, R., 221  
 Holt, R.R., 127  
 Holzman, P., 127  
 Hook, S., 120, 125, 131  
 Hoppe, H., 163  
 Höreth, M., 242  
 Horkheimer, M., 95  
 Hume, D., 6, 23, 31, 35–38, 43, 45, 109, 150, 162, 163, 185, 197–198, 201, 213, 230, 247, 248, 351, 360, 391, 402, 412  
 Husserl, E., 247

## I

Ibarlucía, 150  
 Ibn Sina, 398, 399, 400

## J

James, V.R., 50  
 Jarvie, I.C., 59, 69, 172, 198, 217–233, 267, 283, 333, 336, 362, 370, 377  
 Jennings, H.S., 176, 180  
 Johansson, J., 25  
 Jokic, A., 134

## K

Kahane, D., 300  
 Kalecki, M., 360  
 Kant, I., 14, 32, 34, 36, 38, 99, 136, 163, 173, 186, 206, 207, 213, 248, 250, 252, 253, 326, 327, 339, 342, 343, 346, 353, 360, 361, 402, 412, 415  
 Kautz, S., 276, 277  
 Kean, T.H., 232  
 Kepler, J., 47–59, 60  
 Keynes, J., 112, 232, 360, 363–365  
 Khomeini, A., 208  
 Kierkegaard, S., 346  
 Kipfer, B.A., 409  
 Kirk, G.S., 226  
 Kirzner, I., 173  
 Klant, J.J., 359, 366  
 Kneale, W., 140  
 Kochan, J., 287–301  
 Koertge, N., 300, 323–336  
 Koestler, A., 48, 49, 59  
 Köhler, W., 182  
 Koslow, A., 69

Koyré, A., 49, 59, 249  
 Krausz, M., 284  
 Kraut, R., 307, 318  
 Kuhn, F., 40, 150, 200, 227–228  
 Kuhn, T.S., 228–231, 233, 325, 358, 393, 394, 408, 409  
 Kuipers, T., 422  
 Külpe, O., 178, 390, 394  
 Küng, H., 98, 99  
 Kusch, M., 298, 300, 301  
 Kvasz, L., 71  
 Kymlicka, W., 275–277, 279, 290, 300

## L

Lakatos, I., 66, 104, 133, 141–144, 146, 148, 151, 218, 225, 227, 232, 250, 358, 359, 361, 407, 409, 421  
 Laor, N., 377  
 Larmarck,  
 Latour, B., 233  
 Laudan, L., 410, 415  
 Leeson, P., 163  
 Leifer, V.,  
 Lejewski, C., 63, 64, 67, 68, 68, 69  
 Lender, L.,  
 Lenk, H., 94, 95, 98  
 Lesniewski, S., 64, 69  
 Lessnoff, M., 287  
 Limnaeus, G., 54  
 Lipton, P., 418  
 List, C.P., 253  
 Locke, J., 230, 251, 339, 391  
 Lomesky,  
 Long, J., 233  
 Longomontanus, C.S., 55  
 Lukasiewicz, J.,  
 Lukes, S., 291  
 Lycan, W., 17  
 Lyotard, J., 42, 43

## M

MacIntyre, A., 120, 274, 283, 291, 294, 300  
 Mackie, J.L., 253, 352  
 Magee, B., 287, 288  
 Magellan, F., 51  
 Mander, G.,  
 Mannheim, K., 177, 178, 190, 297  
 Marbe, K., 178  
 Marcuse, H., 332  
 Marshall, 363  
 Martinich, A.P., 268, 269,

Marx, W., 42, 77, 78, 146, 206, 221, 258, 328, 332, 362, 367  
 Masterman, M., 414  
 Mästlin, M., 52–54, 59  
 Maxwell, G.  
 Maxwell, N., 139, 140  
 McCarthy, T., 283  
 McDivitt, H., 354  
 McDowell, 14  
 McGinn, C., 354  
 McNaughton, D., 351, 355  
 Meehl, P.E., 125, 128  
 Meinong, A.I., 179  
 Mejer, J., 269  
 Melancton, 53  
 Menger, K., 172, 173, 332  
 Meno, 246, 305–307, 312–317, 318  
 Michalski, 106  
 Milford, K., 172, 173, 201  
 Mill, J.S., 172, 248, 276, 287, 340, 341, 363  
 Miller, D., 2, 23, 24, 27–29, 33, 34, 69, 78, 88, 109, 115, 137, 138, 140, 150, 152, 172, 220, 246, 250, 251, 258, 259, 264, 267, 268, 270, 417–423  
 Miller, F.,  
 Minogue, K., 282  
 Mises, L., 161–173  
 Mohammed, 212  
 Monnet, J., 239–242  
 Morgan, T.H., 362  
 Muggleton, 105  
 Mulhall, S., 275, 277, 300  
 Muller-Lyer, F., 16  
 Münchhausen, I.,  
 Musgrave, A., 5–19, 24, 26, 27, 29, 80, 89, 149, 152, 295, 415, 418, 421

## N

Nagel, E., 95, 120, 248  
 Nagel, T., 351  
 Nails, D., 233  
 Naraniecki, A.J., 257–270  
 Neal, P., 289  
 Neurath, O., 11, 103, 112–114, 137, 249, 252  
 Neuse, S., 361  
 Newell, A., 175–178, 183  
 Newton, I., 23, 48, 49, 85, 87, 125, 136, 137, 141, 143, 144, 148, 149, 152, 250, 360, 361, 366, 367  
 Newton-Smith, W., 85



Nicolocopoulos, P.,  
 Niemann, H.J., 326, 336  
 Nietzsche, F., 214, 252  
 Nozick, R., 86, 87, 276, 374

## O

O'Hear, A., 109, 153, 205–215, 295, 421  
 Odgen, K.,  
 Ogden, R., 178  
 Orwell, G., 210, 213, 392  
 Owens, J., 399, 400

## P

Pargetter, R., 418  
 Paris, D., 289  
 Parsons, S.D., 173  
 Parusniková, Z., 31–45, 417–423  
 Pascal, B., 245  
 Pasteur, L., 146  
 Paul, E.K., 128  
 Peirce, C., 247, 251  
 Pencer, K.,  
 Penner, T., 318  
 Pericles, 307, 309, 314, 341, 354  
 Perkinson, H., 394  
 Petersen, A.F., 269, 270, 381  
 Pettit, P., 253, 288, 353  
 Pfeiffer,  
 Pheidias, 31  
 Phillips, D., 277, 279  
 Piaget, J., 177  
 Pilot, H., 96  
 Pinder, J., 239  
 Plato, 21, 206, 221, 223, 231, 247, 295, 305–308,  
     310, 315, 317, 391, 392, 398, 410  
 Poincaré, H., 148–150, 220, 359, 366  
 Polanyi, M., 150, 229–231, 358, 360  
 Policleitus, 311  
 Popkin, R. H., 37  
 Post, H., 246, 323, 401  
 Prätorius, N., 54, 59  
 Prior, W., 305, 307  
 Prodicus, 311  
 Protagoras, 305, 307, 309–312, 314, 316,  
     317, 318, 319  
 Ptolemy, 51  
 Putnam, H., 143

## Q

Quesada,  
 Quine, W., 136, 147–149, 152, 251

Quinlan, 105  
 Quinton, A., 287

## R

Radnitzky, G., 142  
 Rahnfeld, M.I.,  
 Rasmussen, D.,  
 Ratzimer, J.,  
 Rawls, J., 250, 276, 300, 301,  
     341, 374  
 Raz, J., 276  
 Reagan, R., 207  
 Reichenbach, H., 377  
 Reinhold, E., 53  
 Reiss, H., 353  
 Ricardo, D., 362  
 Ricoeur, P., 120  
 Rivadulla, A., 160  
 Robbins, L., 359, 366, 367  
 Robinson, P.I., 129, 226  
 Rodrig-Farhi, M., 300  
 Rorty, R., 96  
 Rosenblum, N., 278  
 Rosende, D.L., 135–153  
 Röslin, H., 54  
 Rothbard, M.N., 163  
 Rousseau, J.J., 228  
 Rowbottom, D.P., 23, 81–89  
 Rudner, R., 158, 160  
 Rushdie, S., 212  
 Russe, B.,  
 Russell, L.J., 36, 76, 77, 112, 139, 148, 151,  
     152, 186, 246, 247, 249, 250, 252, 345,  
     346, 354  
 Rutherford, E., 152  
 Ryan, A., 287, 339  
 Ryle, G., 232

## S

Salamun, K.,  
 Salmon, W., 109, 131, 227, 421  
 Samuelson, P., 367  
 Sandel, M., 274, 275, 300  
 Sarkar, S., 232  
 Sartre, J.P., 120  
 Sassower, R., 367  
 Saussure, F., 41  
 Say, J.B., 363  
 Schilpp, P.A., 68, 25, 140, 142, 148, 218, 227,  
     233, 250, 252, 354  
 Schlick, M., 182, 187  
 Schmidt, H., 98

Schrödinger, E., 224  
 Schroeter-Yeiste, P.,  
 Schumpeter, J., 254, 363  
 Scriven, M., 125  
 Segré, M., 377  
 Seiffert, H., 142  
 Sellars, W., 14  
 Selz,  
 Sen, A., 369–377  
 Senior, N. W., 363  
 Sextus, 246  
 Sharratt, 347, 354  
 Shaw, G.B., 343  
 Shea, W., 60  
 Shearmur, J., 288, 339–355, 421  
 Shellard, J., 353  
 Shoenhair, J.D., 364  
 Simkhovitch, V., 362  
 Simkin, C., 367  
 Simon, H., 175–178, 183  
 Singer, P., 343, 348, 354  
 Skinner, Q., 353  
 Smart, N., 340  
 Smith, A., 349  
 Smith, B., 173  
 Smith, M., 352, 355, 362  
 Socrates, 43, 224, 233, 305–319, 325, 327,  
 391, 410, 422  
 Sokal, A., 419  
 Sophocles, 307  
 Spengler, O., 221  
 Stäuble, E., 300  
 Stegmüller, W.,  
 Stelzer, H., 273–284, 300  
 Stephenson, B., 49–51  
 Steup, M.,  
 Stokes, G., 265, 266, 270, 287, 288, 367  
 Strachey, J., 120  
 Sunstein, C., 341  
 Swann, J., 379–387, 394  
 Swift, A., 275, 277

## T

Tam, H., 282  
 Tamburrini, G.I., 106, 107  
 Tarski, A., 64, 77, 137, 250, 251, 252,  
 257–270, 422  
 Taylor, C., 274, 300, 306  
 Teira, D., 114  
 Ter Hark, M., 175–183  
 Thales, 225, 410  
 Thatcher, M., 208  
 Themistocles,

Thucydides,  
 Timm, 115  
 Tokumaru, N., 161–173  
 Topitsch, E., 98  
 Toulmin, S., 119  
 Toynbee, A., 221  
 Trollope, A., 331  
 Turing, A., 76, 182

## U

Udehn, L., 292, 294  
 Udell, L., 300  
 Ursus, N.R., 54

## V

van der Waals, 364  
 van Fraassen, B., 89  
 van Kamp, F.G.T., 55  
 Verdugo, C., 155–160  
 Vlastos, G., 233, 305–307, 318  
 Voelkel, J.R., 50, 55, 59, 60  
 Voltaire, 326  
 von Laue, M., 60  
 von Neumann, J., 189

## W

Walras, L., 363  
 Walzer, M., 274, 275, 277, 282, 300  
 Warnock, M., 209  
 Wartofsky, M.,  
 Watkins, J., 44, 85, 89, 138, 201, 232, 421  
 Watson, 229, 233  
 Watt, H., 178  
 Weber, M., 94, 98  
 Weiss, R., 314  
 Wellmer, A., 323  
 Wells, H.G., 210  
 Westman, R.S., 60  
 Wettersten, J., 219, 270, 336, 365, 369–377, 394  
 Whewell, W., 220  
 Whitehead, A.N., 412  
 Whiteside, D.T., 59  
 Whyte, W.H., 233  
 Williams, B., 354  
 Williams, D., 153  
 Wilson, C., 49, 59  
 Wind, J., 270  
 Winn, D., 364  
 Wisdom, J., 232  
 Wittgenstein, L., 92, 120, 153, 223, 224, 233,  
 247, 252, 357, 366, 403

Wolenski, J., 259  
Wong, R., 364  
Woolgar, S., 233  
Worrall, J., 421

**X**

Xenophon, 318, 319

**Y**

Yoshida, K., 283

**Z**

Zahar, E., 421  
Zelenak, E., xii  
Zeman, V., 91–99