The Objectives of Science[1]

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Résumé : Contestant l'opinion commune selon laquelle le problème de la démarcation, contrairement au problème de l'induction, est relativement anecdotique, l'article soutient que le critère poppérien de falsifiabilité donne une réponse irrésistible à la question de savoir ce qui peut être appris d'une investigation empirique. Tout découle du rejet de la logique inductive, joint à la reconnaissance du fait que, avant d'être investiguée, une hypothèse doit être formulée et acceptée. Les hypothèses scientifiques n'émergent ni a posteriori comme les inductivistes le soutiennent, ni de quelque immaculée source a priori : elles sont des conjectures pures et simples. Les empiristes qui rejettent l'apriorisme ont donc rejoint trop rapidement les rangs non philosophiques du naturalisme épistémologique. L'article conclut par un résumé de l'objectivisme popperien et par de brèves réponses à certains arguments à la mode selon lesquels la vérité objective n'est pas un objectif atteignable.

Abstract: Contesting the common opinion that, unlike the problem of induction, the problem of demarcation is of little significance, the paper maintains that Popper's criterion of falsifiability gives an irresistible answer to the question of what can be learnt from an empirical investigation. Everything follows from the rejection of inductive logic, together with the recognition that, before it can be empirically investigated, a hypothesis has to be formulated and accepted. Scientific hypotheses emerge neither a posteriori, as inductivists

[1]This paper derives from a lecture given at the Popper Centennial Symposium held at the University of Virginia, Charlottesville VA on 22nd to 23rd November 2002, and at the Colloque Karl Popper : Philosophe du vingtième siècle held at the Sorbonne on 16th to 17th December 2002. It has been revised more than once since 2002, and an extended version has appeared as [Miller 2006a, Chapter 4]. Some changes of emphasis made here were stimulated by correspondence with Diego Rosende and with Darrell Rowbottom, whom I thank.

Philosophia Scientiæ, 11 (1), 2007, 21–43.

hold, nor from some immaculate a priori source, but from sheer guesswork. Empiricists who reject apriorism have therefore enlisted too zealously in the unphilosophical ranks of epistemological naturalism. The paper concludes with a summary of Popper's objectivism, and with brief responses to some fashionable arguments that objective truth is not an attainable objective.

1 Introduction

In this paper I should like to set out as clearly and unassumingly as I can the main points of Karl Popper's revolutionary theory of scientific method and of scientific knowledge, to explain what is revolutionary about it, and to suggest that it resolves a number of current intellectual controversies over which, to be quite candid, some people have got into something of a pickle. Many readers may regard such a paper as otiose. They may mention that Popper's distinctive views in the methodology of science have been so frequently discussed that there can be nothing of value to add, approvingly or disapprovingly. The subject, they may insist, has long since moved on to problems far removed from Popper's central preoccupations, the logical problem of induction and the problem of demarcation.

This censure might be appropriate if, in addition to being well known, Popper's falsificationism were always well understood. I think that its leading ideas have been repeatedly misrepresented, and that its lessons have been clumsily distorted. There is, in particular, no adequate recognition of how far beyond the philosophy of science Popper's developed epistemological views extend. A characteristic recent judgement, that 'Popper's epistemology is almost exclusively the epistemology of scientific knowledge' [Vickers 2006, § 4.2] is hardly at all supported by the supporting quotation from [Popper 1959, Preface] : 'most problems connected with the growth of our knowledge must necessarily transcend any study which is confined to common-sense knowledge as opposed to scientific knowledge'. Popper is routinely categorized as a philosopher of science and politics, who admittedly had interests in a multitude of other areas, and it is not appreciated that critical rationalism, the generalization of falsificationism first sketched in [Popper 1945, Chapter 24, §§ IIf.], coupled with the biological and objective approach sketched in [Popper 1972, Chapters 2–4], renders obsolete the traditional, but still obtusely prevalent, characterization of human knowledge as something along the lines of justified true belief [Miller 1994, Chapter 3.1]. This no doubt explains why Popper is still hardly known as an epistemologist, and his name does not appear in many anthologies on epistemology. (Compare the comments in [Popper 1972, Chapter 2.5] about Churchill's contributions to epistemology.)

Although I have no doubt that Popper made a contribution of lasting importance to the methodology of science, and to the theory of knowledge, I am not disposed here to tip the balance completely in the other direction. It is impossible that everything that he said about human knowledge was correct, if only because he contradicted himself on a number of central issues; not only, as we all do, through reconsideration, but because he sometimes approached the same problem from different angles, and unduly emphasized some of its features at the expense of contrasting ones. These inconsistencies need to smoothed out and rendered as harmless as possible. But the Spanish writer Unamuno was surely right when he remarked that '[i]f a man never contradicts himself this must be because he never says anything'.[2] Popper could scarcely be accused of such obmutescence.

Popper's first significant contributions to the theory of scientific method and to the philosophy of science are contained in his book *Die beiden Grundprobleme der Erkenntnistheorie* [Popper 1979] (*The Two Fundamental Problems of the Theory of Knowledge*), which was written at the beginning of the 1930s. An English translation is still awaited. Yet it was not until the appearance of *Logik der Forschung* [Popper 1934], a kind of half-brother of its predecessor, that the subversiveness of Popper's message was declared. Even then it was not fully recognized, although the book was much praised, both strongly and faintly. Popper set out to eradicate much that was traditionally accepted, and much that was contemporarily endorsed, in discussions of the procedures of science, and to replace the debris with a theory capable of surmounting all the problems at which earlier theories had stumbled. In one sense his proposal — that Plato was wrong to seek to distinguish knowledge from true opinion, and even more helplessly wrong to recommend knowledge, rather than simple truth, as the goal of inquiry — is simplicity itself. The fact remains that for 70 years most philosophers have had a hard time understanding its subtlety and strength, and few have welcomed its redemptive force.

[2]'Si un hombre nunca se contradice, será porque nunca dice nada.' Unamuno's words ('from conversation') are used as the motto to Chapter 7 of [Schrödinger 1944]. I have simplified his translation. An even freer translation is given by [Moore 1989, 359]. The *bon mot* may be compared with a remark made by Edward John Phelps in a speech to the Mansion House in London on 24th January 1899 (quoted in *The Oxford Dictionary of Quotations*, 3rd edition, 1979, 373 : 7) : 'The man who makes no mistakes does not usually make anything.'

The two fundamental problems of the theory of knowledge in Popper's title are the problem of induction and the problem of the demarcation of empirical science from what is not science : metaphysics, logic and mathematics, pseudoscience. In the next section I shall explain Popper's solutions to these problems. My remarks thereafter will concentrate on two aspects of his philosophy of science : the first is the fact that it is indeed a philosophy of science, not a natural history of science ; the second is its objectivism. On the way I shall say something about its rationalism too.

2 Induction and Demarcation

The *problem of induction* is identified these days, especially by Bayesian writers such as [Rosenkrantz 1977, 48], with the problem of *how we can learn from experience.* The *problem of demarcation* may be stated roughly as the problem of *what we can know through experience.* The significance that he attached to these problems reveals the origins of Popper's work in traditional empiricism, the doctrine that it is through experience that we learn about the world. But he was never a wholehearted devotee of empiricism, and was from the start, or even earlier, critical of the tradition. In *Logik der Forschung* [Popper 1934, § 10] he wrote that 'the main problem of philosophy is the critical analysis of the appeal to the authority of experience', which amounts to a succinct joint statement of the problems of induction and demarcation, a statement whose context shows that he was quite dissatisfied with the traditional empiricist response to these problems. I shall later say more about this passage. For the moment I wish to make it clear that, despite many passages in Popper's writings that may suggest the contrary, both problems concern not only scientific knowledge, but all knowledge that may be characterized as empirical.

In the opening section of *Die beiden Grundprobleme der Erkenntnistheorie* [Popper 1979] Popper stated that the problem of demarcation, the problem of what we can know, which he attributed to Immanuel Kant, is more fundamental than the problem of induction, the problem of how we can learn, which Kant had attributed to Hume. He wrote : 'The problem of demarcation deserves our primary interest. . . . it proves to be the central problem to which probably all other questions of the theory of knowledge, including the problem of induction, can be reduced.' Popper held this view for many years ; for example in a 1953 lecture (published in 1957), shortly after saying that 'the two problems — of demarcation and of induction — [are] ... in a sense one', he suggested that 'the problem of induction is only an instance or facet of the problem of demarcation' [Popper 1957, §§ VIIIf.]. Yet in the Preface to [Popper 1959] (the English translation of [Popper, 1934]) he made the announcement — it is much more than a suggestion — that '[t]he central problem of epistemology has always been and still is the problem of the growth of knowledge', a formulation that appears to emphasize the dynamic process of learning at the expense of the static state of knowledge. We may well wonder which poses the deeper difficulty : induction, which is recognized by all genuine philosophers as a genuine problem, or demarcation, which has been dismissed as verbal (by [McGinn 2002, 50] and others), and as 'a distraction' (by [Haack 2005, S69]).

Hume's problem of induction is a genuine problem because it identifies a contradiction at the heart of the empiricist tradition. What, Hume asked, can we legitimately learn about the unknown from what we have experienced ; what can we learn, for example, about the future or about what happens in general ? Nothing, he said. In [Hume 1738, Book I, Part III, § XII] he wrote (in a passage quoted in part in [Popper 1972, Chapter 2.26]) :

> Let men be once fully persuaded of these two principles, *that there is nothing in any object, considered in itself, which can afford us a reason for drawing a conclusion beyond it* ; and, *that even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience* ; I say, let men be once fully convinced of these two principles, and this will throw them so loose from all common systems, that they will make no difficulty of receiving any, which may appear the most extraordinary.

'These principles' Hume continued 'we have found to be sufficiently convincing'. In short, however plentiful and varied our experience, there is no reason to predict or to infer any one thing rather than another concerning what we have not experienced. Hume demonstrated that the method of induction, the supposed method of extrapolating or generalizing from experience, is rationally indefensible.

The problem of demarcation, on the other hand, arises only indirectly from a contradiction. Kant had argued in his Antinomies that, outside the empirical realm, cogent reasoning risks plunging us into uncovenanted and unforeseeable error. To protect ourselves from again committing such errors, it is urgent that we map out the realm of the empirical, and not step beyond it.

The problem of demarcation, though not often addressed explicitly by empiricists, was traditionally solved by the obvious expedient of identifying the empirical with the deliverances of sense experience together with what can be obtained from them by induction. Once Hume had exposed the fatal weaknesses of the empiricist theory of induction, therefore, the empiricist theory of demarcation became irrevocably implicated. They were in the same jam, and only together could they be rescued. Popper was quite right to say that, at least for empiricists, the two problems — of demarcation and of induction — [are] ... in a sense one', and that 'the problem of induction is only an instance or facet of the problem of demarcation' (quoted above). Within traditional empiricism, it is only when the problem of induction has been solved that the problem of demarcation can be solved.

Matters look different when we come to Popper's own solution of these problems, for with the rejection of inductive logic, 'the problem of demarcation gains in importance' [Popper 1959, § 4]. The problem of demarcation has indeed to be solved first, and only subsequently can the problem of induction be solved. The explanation of this change of emphasis is rather simple : whereas traditional empiricism held that knowledge consists of what is learnt (from experience), Popper decisively separated the categories of knowledge and learning. What we know is one thing, he suggested ; what we learn is some other thing. For empiricists, especially those who take seriously the possibility of learning by induction, learning precedes knowledge, both psychologically and logically ; that is why a solution to the problem of induction is a prerequisite to a solution of the problem of demarcation. According to Popper, on the other hand, knowledge must come first, and it is knowledge that is both the psychological and logical antecedent of learning. We cannot begin to learn anything if we do not already know something.

This is not intended to be a revival of the Platonic doctrine of *anamnēsis*, the doctrine that learning consists of recollecting what we know already. Popper was enough of an empiricist to demand that experience must be relevant not only to the learning process but also to the content of what we learn. What then can we learn through experience ? Not what we already know, since we already know it, but — most simply — the opposite of what we know ! Or, if you like to put it this way : the principal thing that we learn is that we do not know. To take advantage of the striking vocabulary that Popper later proposed [Popper 1963] : our knowledge consists of conjectures, but the most important part of what we learn consists of refutations ; that is, refutations of those conjectures.

The problem of demarcation can be solved in a satisfactory manner once we concede that what we call empirical knowledge cannot be knowledge in the traditional empiricist sense; that is, it cannot be derived by induction, or by any other method, from experience, but consists largely of unsupported conjectures or guesses. As Kant recognized, knowledge precedes experience. But that is not to say, as Kant came near to saying, that such knowledge cannot be modified in the light of experience. This indeed, Popper insisted, is the principal purpose of empirical investigation, and it is only knowledge that is susceptible to modification in the light of experience that is genuinely open to empirical investigation. For what could such an investigation be expected to achieve? An empirical investigation turns up (or in the case of an experiment, may bring into existence) singular facts. Either these facts — or more precisely, the empirical reports that describe them — contradict the hypothesis being investigated — these are refutations — or they are irrelevant to it — these are sideshows — or they tell us something that we already know — they confirm the conjecture. It should be evident that the second and third types of fact do not make any difference — either they ignore the hypothesis being investigated, or they repeat it (or some part of it). The only useful purpose that can be served by investigating a hypothesis empirically is, it seems, the acquisition of empirical evidence that refutes it. But this implies that we cannot subject to any useful empirical investigation a hypothesis that cannot be refuted by any empirical evidence. This is Popper's criterion of falsifiability, the criterion of demarcation between science and non-science. Only those hypotheses that are empirically refutable, or empirically falsifiable, can count as scientific.

Note two caveats. The criterion of demarcation as I have formulated it here is a negative one, telling us what is not scientific. Given the Kantian origin of the problem of demarcation, this is appropriate (though I am not suggesting that Kant would have been satisfied with Popper's proposal). But as Agassi and others have remarked, the converse of the criterion does not look right. A hypothesis may be empirically falsifiable but not be something that we are interested in investigating empirically (though we could of course make it our business to investigate it); and therefore not a part of what we usually call science. It is unhelpful too to call scientific such activities as technology (see [Miller 2002, § 2]), which is incontrovertibly empirical, and history, which is also empirical. If falsifiability is to provide necessary and sufficient conditions for anything, a criterion of demarcation in the strict sense, it should rather be between the empirical and the non-empirical. That that was from the outset the distinction Popper was trying to capture is, I think, plain from the declaration quoted above that 'the main problem of philosophy is the critical analysis of the appeal to the authority of experience' [Popper 1934, § 10]. It is admitted however that, no doubt for historical reasons, Popper at times treated as identical the categories of the scientific and the empirical. We must accordingly be careful, once we have separated them, to place the criterion of demarcation in its proper home. It has to be admitted too that, again for historical reasons, some of his most prominent presentations of the problem of demarcation, such as [Popper 1957, §§ If.], [Popper 1974, §§ 5–7], stress the demarcation between science and pseudoscience. But the difference between the scientific and the empirical is not of great significance if, as in the present formulation of the criterion of demarcation, we attend to the methods employed rather than to the hypotheses proposed. This is surely what Popper set out to do : 'The theory of knowledge, whose task is the analysis of the method or procedure peculiar to empirical science, may accordingly be described as a theory of the empirical method' [Popper 1934, § 5].

A second caveat is that falsifiable hypotheses may have unfalsifiable consequences, and indeed all of them do ; logical truths inevitably, and weak metaphysical statements in most cases (see [Popper 1934, § 78, note ∗4], [Miller 1994, Chapter 1.2]). This no more compromises their empirical character than does the presence of dead cells within its body inevitably deprive a healthy organism of life.

The solution to the problem of induction is now a straightforward task. We do indeed learn from experience, but what we learn is that our hypotheses are false, not that they are true ; if an experimental or observational result is in accordance with the hypothesis being investigated, then we learn nothing from the investigation. This does not mean that the investigation was a waste of time, any more than travel insurance taken out in advance of an accident-free trip is a waste of money. There is no need for any inductive inference from empirical evidence to empirical generalization, and certainly no need to justify any such inference, since a generalization may be added to our knowledge by being freely conjectured. Our knowledge consists of unsupported conjectures, our learning consists of refutations. Hume was right to suggest that induction cannot be rationally justified, but wrong to say that what we learn from experience is learnt irrationally.

These ideas, which form the backbone of Popper's philosophy of science, have been severely criticized. In [Miller 2006a, Chapter 4.4] I have discussed some of the most popular criticisms. In this place I wish rather to emphasize again the significance of the change of perspective that Popper offered us, embodied in 'the view that a hypothesis can only be empirically *tested* — and only *after* it has been advanced', as he put it at the beginning of [Popper 1959, § 1] (note that in the original German edition [Popper 1934], and in later German editions, there is nothing corresponding to these words). In the next section he went on : 'In order that a statement may be logically examined in this way, it must already have been presented to us. Someone must have formulated it, and submitted it to logical examination.' That statements, theories, hypotheses, conjectures, call them what you like, must be formulated before they can be rationally evaluated may be a platitude. It is decisive for much else besides.

Even though my wish in this paper is to explain Popper's philosophy of science rather than to undertake the criticism of alternatives, let me conclude this section with a few words about how science is usually supposed to be constituted. The orthodox view vigorously repudiates the claim made above that facts that are predicted by a hypothesis do not make any difference to it, since they do no more than repeat some part of it ; it therefore repudiates the suggestion that the only point of submitting a hypothesis to empirical investigation is to refute it. The orthodox view amongst philosophers of science is that admission to science is in reality a two-stage process, the first stage offering merely tentative or provisional membership, while the second stage bestows full membership. First tenure-track, then tenure. The idea seems to be that any testable or falsifiable hypothesis qualifies at the first stage ; but only hypotheses that survive rigorous testing qualify at the second. The first stage, which inevitably involves some variant of Popper's falsifiability criterion, plays only a minor role, since what counts as genuine science is only what gets through the qualifying rounds by undergoing and surviving sufficiently austere scrutiny. Science, in other words, consists not just of any old falsifiable hypotheses, but of falsifiable hypotheses that have passed tests, or passed tests better than their rivals, so as to have earned the title of being confirmed or justified or, in current jargon, warranted.

If a hypothesis could be conclusively verified by means of the testing process, or through any other appeal to experience, then that would be valuable information, for it would mean that no further investigation of it would ever be necessary. But conclusive verification is generally accepted to be impossible ; though not by [McGinn 2002, 49], who advises us that '[i]t is not a tentative conjecture that water consists of H_2O molecules' (yet samples of water often exhibit ionization of a rather complex kind, and are not correctly described as collections of molecules of H_2O). What is put in its place, namely confirmation, or partial justification, or partial warrant, is something very different. In Popper's view, and also in my own, the second stage of the supposed two-stage process plays no significant role (and is better thought of as not happening at all). For once admitted to science a hypothesis is allowed to remain there unless it is falsified (or, more subtly, it is replaced by something more general). This applies as much to tenured hypotheses as to apprentices. The survival of rigorous tests does no more than maintain the status quo ante. The award of tenure or of a warrant is therefore an utterly empty gesture. It does nothing to protect a hypothesis against falsification later, and in no respect makes it any less tentative. I do wish someone would tell me why it is useful to know that a hypothesis has been confirmed, or partially justified, or partially warranted, and in what way this implies more than that it has not been refuted and has not been eliminated.

So strong is the general prejudice that if a statement is to be rationally accepted, or rationally held, then it must have been obtained by induction from the available evidence, that some authors have concluded that those who reject induction must prefer hypotheses that are contradicted by the evidence to those that are not, or at least be indifferent between them. An article in *The New Scientist* some twenty years ago is illustrated with a drawing in which the table at which a man is reading *The Logic of Scientific Discovery* 'in the instant start[s] to behave in a very un-table-like fashion', losing its solidity [O'Hear 1985, 44]. The suggestion is that someone who takes Popper's philosophy of science seriously will expect all manner of unexpected things to happen ; indeed, suffers all manner of unexpected things. In a similarly mocking spirit [Okasha 2002, 26] contains four cartoons captioned 'What happens to people who don't trust induction'. Two of the drawings are of men behaving rather timidly in everyday circumstances, one showing fear as he turns on his computer, the other wearing a gas mask as he opens his front door. In the other drawings two men are shown behaving decidedly imprudently : one preparing to fly from the roof of a building, the other drinking poison. The message here is apparently that those who do not take induction seriously will be both under-adventurous and over-adventurous, neurotic and quixotic. They will not endorse commonsense generalizations but will endorse generalizations that our current scientific (and even commonsense) knowledge indicates to be foolish. I need hardly say that these are crude misunderstandings. That a hypothesis is accepted unsupported does not mean that it is accepted irrationally, and it certainly does not mean that it is not accepted at all.

3 First Philosophy

It is a central doctrine, indeed the central doctrine, of empiricism that our knowledge of the world cannot be derived from first principles, or a priori, but must be obtained from experience, or a posteriori. The hopes of intellectualists (or rationalists, as they are often misleadingly called) such as Descartes and Leibniz that we may be able to construct a substantive theory of the world on purely abstract and philosophical foundations, must be rejected (on logical grounds, if on no others). With the stirring remark that '[t]here is . . . no first philosophy' [Quine 1969, p.127] signalled his extension of this doctrine from science to philosophy itself. Philosophy, including philosophy of science and the theory of knowledge, is continuous with natural science, Quine suggested, and should therefore be subject to the standards and the methods of investigation that are current in natural science. This programme he announced under the title of 'naturalized epistemology'. It is clear from many of his later writings that the science to which Quine wished to assimilate philosophy most closely is cognitive psychology, and in this project he has been joined, and, I need hardly add, anticipated, by a wide variety of thinkers. A similar desire to diminish and even to erase the peculiar character of philosophy, especially the philosophy of science, is evident in the myriad attempts in the last 40 years or so to combine the philosophy of science with the history of science, or with the sociology of science.

Popper would have none of it. A true philosopher, a true admirer of natural science, and even an adherent to the idea that 'there is no first philosophy', he rejected the inference that there is no genuine philosophy, and that philosophy is at best a branch of science. He wrote in a passage [Popper 1934, § 10] from which I quoted above several words :

> The controversial question whether philosophy exists, or has any right to exist, is almost as old as philosophy itself. Time and again an entirely new philosophical movement arises which finally unmasks the old philosophical problems as pseudo-problems, and which confronts the wicked nonsense of philosophy with the good sense of meaningful, positive, empirical, science. And time and again do the despised defenders of 'traditional philosophy' try to explain to the leaders of the latest positivistic assault that the main problem of philosophy is the critical analysis of the appeal to the authority of experience — precisely that 'experience' which every latest discoverer of positivism is, as ever, artlessly taking for granted. To such objections, however, the positivist only replies

> with a shrug : they mean nothing to him, since they do not belong to empirical science, which alone is meaningful. 'Experience' for him is a programme, not a problem (unless it is studied by empirical psychology).

Here in 1934 Popper was alluding to the anti-philosophical philosophy promulgated by some members of the Vienna Circle. Although often partisan supporters of the psychology and the sociology of science, most members of the Circle had even less time for the history of science than they had for its philosophy, which, after all, they practised despite themselves. But thirty years later a depressingly similar 'entirely new philosophical movement' arose, fed this time by a newly-found fervour for the illumination promised by the history of science. Popper had this to say in a commentary [Popper, 1970, pp. 57f.] on the anti-philosophical movement associated with [Kuhn 1962] :

> ... to me the idea of turning for enlightenment concerning the aims of science, and its possible progress, to sociology or to psychology (or ... to the history of science) is surprising and disappointing.
>
> In fact, compared with physics, sociology and psychology are riddled with fashions and with uncontrolled dogmas. [...] Besides, how can the regress to these often spurious sciences help us in this particular difficulty ? Is it not sociological (or psychological, or historical) *science* to which you want to appeal in order to decide what amounts to the question 'What is *science* ?'... For clearly you do not want to appeal to the sociological (or psychological, or historical) lunatic fringe ...

The problem of demarcation, Popper insisted, cannot be settled by empirical or scientific means, since there is no fact of the matter to be settled. What is needed, he said is a proposal or (the words he used) a convention or methodological rule concerning what falls within the scope of scientific or empirical investigation. His first proposal was that we should admit any hypothesis that is open in some way or other to empirical falsification. A better one, soon spelt out by Popper himself, inserts the proviso that any hypothesis admitted to science should have been proposed in an attempt to solve some genuine problem or at least to answer some genuine query, so that science will not be asked to make room for testable hypotheses so idle and piffling that no one will ever be concerned to put them to the test. This second proposal strengthens the resolve, only implicit in the first one, that hypotheses that cannot survive empirical testing ought to be rejected. An alternative suggestion might be that we restrict admission to hypotheses thought up by people with established reputations in respectable scientific disciplines, a suggestion that seems to be rather common amongst scientists anxious to distinguish their own activities from those that they classify as pseudoscience, yet is unenlightening (and would exclude many hypotheses of interest and value). What clearly cannot be countenanced as a criterion for admission to science is any rule that requires a hypothesis seeking admission already to have been empirically investigated.

For the moment it suffices to observe that considerations such as these cannot realistically be regarded as belonging to empirical science. They are logical considerations or, if you like, a priori considerations. Nonetheless, as Quine said, '[t]here is ... no first philosophy'. The solution to the problem of demarcation does not consist in deriving from first principles the true character of science. Quine and other empiricists are quite right to remind us that there is no a priori fount of knowledge, quite right to remind us that, in philosophy as much as science, nothing substantive can be generated by a priori considerations alone. But these truisms, which are doubtless themselves fragments of a 'first philosophy', do not imply that our knowledge of the world is obtained a posteriori, from experience, nor that a priori considerations play no role. In fairness to Quine, I should add that what he actually said was '[t]here is no external vantage point, no first philosophy', which could well be understood naturalistically, to mean that no thinking takes place in the absence of experience. This is a doctrine that, I imagine, Kant would have happily consented to. But Quine's context makes it evident that he advocates the treatment of questions of the theory of knowledge as if they were questions of natural science.

Popper, as we have seen, proposed that within science, and in other empirical disciplines, our knowledge — that is, our hypotheses or conjectures — are indeed produced prior to experience, that experience is then brought to bear on them, and that what are genuinely a priori considerations — namely that contradictions indicate the presence somewhere of error — then lead us to abandon those conjectures that are contradicted by the reports of experience (or in some cases, to scrutinize again those reports). Much the same applies within philosophy itself, except that it is not to experience that we look for counterexamples or refutations, but (if anywhere) to the philosophical problems that provoked them [Popper 1958, § 2]. In the case of the problem of demarcation, we may consider and evaluate, in a largely a priori manner, various proposals about how best to conduct scientific inquiry. Let me be more explicit : it is a logical or a priori matter whether any credit accrues to a hypothesis that survives a severe battery of tests, though it is of course a psychological matter whether such survival may have a tendency to increase the confidence that a believer in the hypothesis places in it. Like any other human activity in the world, science can be investigated by psychological and sociological methods. We all agree about that. What partisans of naturalized epistemology have failed to make clear is why there can exist no further logicalcal considerations.

It can be admitted that pure logic is not informative about the world, and that logical truths presented to us a priori tell us nothing of interest. But once we appreciate that a conjecture must always comes first, that '[i]n order that a statement may be logically examined . . . , it must already have been presented to us. Someone must have formulated it, and submitted it to logical examination', we can see that the correct principle that '[t]here is . . . no first philosophy' is by no means equivalent to the fallacious principle 'there is no genuine philosophy'. In the presence of an interesting conjecture, the identification of a contradiction may be a genuinely informative contribution — for it may inform us that the conjecture is incorrect — even though the contradiction itself, considered abstractly, is of no real interest. Where Quine and others have gone astray, I think, is in implicitly supposing that the purpose of rational thought is fundamentally constructive or productive ; that is to say, that philosophical doctrines must either be derived from experience or be derived from first principles ; and since first principles yield nothing of interest, only experience is left. But there is a third option : philosophical doctrines, like scientific hypotheses, are nor derived at all. They are invented. The philosophical work — or, in the case of scientific hypotheses, the scientific work — comes later, in evaluating them as solutions to the problems that they attempt to solve. Problems of the theory of knowledge can be tackled by conjectural means. There is indeed a special difficulty in the theory of knowledge or methodology, which is much less pressing in the case of metaphysical speculation, if it is maintained that methodological rules are not factual, and have the status of conventions. But it can be only the absurd doctrine that all conventions are arbitrary conventions that can lead to the idea that the best that methodologists can do is to describe what scientists do.

Although Popper may well be right that the 'pronouncements of a *theory* of method . . . are . . . for the most part conventions of a fairly obvious kind . . . [and] [p]rofound truths are not to be expected of methodology' [Popper 1959, 54], it hardly follows that methodology consists of a system of logical truths or empty recommendations. What we consider a good method is at least in part affected by what we regard as the aims of scientific activity. Nonetheless, I am inclined to agree that in methodology we have learnt rather more than we know. We have learnt through criticism that many proposals are unsatisfactory, and not much remains in our stock of methodological knowledge except the nearly truistic statement that since we do not know what to look for when we investigate the unknown, we should be ready to try out every conjecture that we can formulate sufficiently precisely to make criticism possible [Miller 2006b]. We cannot do better than this, since it does not prohibit us from trying anything, but of course we might do worse. For example, we might stand and wait for a message from Heaven. This is not prohibited either, but it is not 'try[ing] out every conjecture that we can formulate sufficiently precisely'.

Naturalized epistemology has sources other than Quine, the Vienna Circle, and Kuhn, of course, but it is largely Quine's variant that has interested me here. My point is that Quine is quite right in 'asking us to set aside the entire framework of justification-centered epistemology' [Kim 1988, § 3], but quite wrong therefore to dismiss the possibility of 'normative epistemology'. Nor need we, like Kim himself, an opponent of Quine's on this point, make the concession that 'epistemological supervenience is what underlies our belief in the possibility of normative epistemology' or admit that methodology has to be subject to 'naturalistic criteria' [Kim 1988, § 7]. What makes a theory about the natural world a good theory, namely its truth or closeness to the truth, is no doubt reducible to features of the world. But what makes a method a good method is not a natural property, but a logical property, and deserves to be recognized as such.

4 Objectivism

My first point about objectivism is solely a disclaimer. Nothing said here should be taken as relating to the philosophy of Ayn Rand, which is also known as objectivism. It would not be surprising if there were similarities between some of Popper's views and some of Rand's. It is a simple theorem of logic that however much two thinkers may appear to disagree, they agree on exactly half the points on which they both have opinions.[3]

[3] Suppose that Mr A's opinions are summed up in the theory A, and Mrs C's opinions are summed up in C. Then the theory $\mathsf{A} \vee \mathsf{C}$ sums up the points of agreement. Let b be some proposition on which Mr A and Mrs C disagree, and y be any proposition on which they both have opinions. If y is a point of agreement, then the biconditional $b \leftrightarrow y$ is another point of disagreement. But if y is a point of disagreement, then $b \leftrightarrow y$ is a point of agreement (since $\neg X \leftrightarrow \neg Z$ has the same truth value

Let us begin with objective knowledge. Nobody denies that scientists are individual people, with the usual facility of being in individual ways in a variety of psychological states, in particular the psychological states of belief, doubt, and understanding. These psychological states come in degrees : one can believe more or else tenaciously, doubt more or less intensely, understand more or less completely ; perhaps also believe more or less confidently, doubt more or less hesitantly, understand more or less deeply. One may also be more or less consciously aware of the presence of the belief, doubt, or understanding. Like other more obviously physical and better-regulated dispositions, such as colour-blindness, perfect pitch, and the ability to distinguish Château Lafite from Château Latour, an individual's psychological states inevitably have some effect on the way that he or she behaves, on what he or she attends to, on what he or she hears or sees. But in the normal run of things, these subjective phenomena are not of much public interest. Although they can be expected to influence a discussion or an investigation, usually in unexpected ways, they are not what constitute the subject matter of most public discussions and investigations. When we discuss the world we live in, or our involvement in it, we are not interested in the beliefs and doubts of our fellow discussants in the sense that these beliefs and doubts are subjective states. We are interested only in things that can be shared. Now what is called the content of a belief, in contrast to its presence, what gives it its individuality and its point, may also be idiosyncratic ; but if it is, then we shall not be interested in the contents of others' beliefs either, but in something more abstract. These more abstract objects are for the most part (but not invariably) formulated in the sentences of a shared language. That the sentences themselves are then understood in idiosyncratic ways by those who hear or read them does not in the least detract from the fact that they are objective in the ways that beliefs, and the individual contents of beliefs, are not. It may be something of a miracle that we manage to understand each other, that we can command intersubjective understanding, and sometimes even agreement ; but we

as $X \leftrightarrow Z$). In addition, $b \leftrightarrow (b \leftrightarrow y)$ is logically equivalent to y. In other words the function that associates y with $b \leftrightarrow y$ is a bijection from $\mathbf{A} \vee \mathbf{C}$, the set of propositions on which Mr A and Mrs C agree, to the set of propositions on which they disagree. Points of agreement should therefore be expected all over the place, and we are well advised not to take them too seriously.

This is a simple variant of the proof presented in [Miller 1994, Chapter 10.4*a*] of the theorem of Keuth and Vetter that the truth content and falsity content of a theory are equipollent. It has as a simple consequence the main theorem of [Harris 1974], to the effect that Popper's theory of closeness to the truth does not apply to false theories even if 'the truth' is represented by an arbitrary, not necessarily maximal, theory $\mathbf{C}$. For a comment what Harris did and did not discover, see [Miller 2005, § 1].

do, and we do understand each other sufficiently well at times to disagree with each other.

Scientists, being human, are no different, even in scientific contexts. What scientists observe is equally at the mercy of subjective factors, some of them idiosyncratic, some of them widespread but nonetheless subjective. What scientists (except for human psychologists) are interested in are not the private psychological processes of their colleagues, but those parts of the contents of these processes that can be formulated in language. Science concerns itself, as we have already repeatedly noted, with linguistically formulated statements, hypotheses and theories. Scientific knowledge consists of such linguistic entities. When entertained by some mind, the statements we consider may be prey to any number of subjective embellishments. Nonetheless as statements they are objective public items and they belong to no one in particular.

This is what Popper calls our objective knowledge. Most of science is objective knowledge in this sense. It is neither psychological nor physical, though it invariably appears to us in a psychological or physical form : as a thought, as a speech, in a book. But the mode of physical or psychological presentation is not what is scientifically interesting. What we strive for, to be sure, are formulations that are capable of withstanding the excesses of idiosyncratic interpretation. That may be why scientific language is often dry and rather literal, avoiding figurative language that may (but of course may not) impede ready understanding. The form in which an idea is presented may be likened to the material in which cough mixture is packaged. We want a material that keeps it intact — asbestos is better than cardboard — and interacts with it as little as possible — glass is better than asbestos — but it is the cough mixture that is important. The bottle is not.

If the subjective influences on what we think, and the subjective effects that our thoughts have on their bearers (and on those to whom they are communicated) are not important, then what is important about the hypotheses that they embody ? In short, what are we investigating ? Popper hardly has any claim to originality in maintaining that it is the objective truth values (the truth or the falsity) of our hypotheses that are our principal concern. We test hypotheses in various ways because we think, rightly or wrongly, that the results that we obtain from properly conducted tests can sometimes indicate that our hypotheses are false, though (Hume taught us) never that they are true. Perhaps we are wrong to think this ; perhaps there are more things wrong with traditional empiricism than its mistaken doctrine that experience impresses ideas on our minds. It may be that few of the observational and experimental reports published in scientific journals are literally true. I can only say that this hypothesis is itself open to empirical test.

Some will no doubt protest that if science offers us only empirical methods for testing statements about the accuracy of empirical methods, then empiricism is self-reinforcing, and the tests are no more convincing than two dubious witnesses in court who support each other's testimony. This cannot be denied, but it is an elementary logical mistake to suppose that it is relevant to what I said. Of course empirical methods cannot show empirical methods to have a proclivity to generate objectively true observational and experimental reports. But it may be that they can show that some methods are rather bad at doing this, and that is what I was talking about. In this way we have been pressed to formulate hypotheses about colour blindness and about other perceptual idiosyncrasies, and to require double-blind testing in many parts of human (and even animal) psychology. There is no crasser logical error persistently committed by otherwise clear thinkers than the confusion of circular arguments, which assume what they want to establish, and therefore establish nothing, and critical (or reductio ad absurdum) arguments, which assume the negation of what they want to establish [Miller 1996, § 1]. I do not say that empirical methods can show empirical methods to have a proclivity to generate objectively true observational and experimental reports. I do say that they can show that empirical methods sometimes produce contradictory results.

Perhaps I should be cautious here. There is one other most regrettable logical mistake that is commonly committed, the mistake of supposing that because our language, and our thoughts, are imbued with unspoken theories and prejudices, because we are always looking at things from an unarticulated perspective, because all our language too embodies a point of view, it follows that it is not possible to make statements that are objectively true and false. This conclusion is false. It would be like arguing from the premise that, in order to describe points and events in space and time, we are obliged to fix, in a more or less arbitrary manner, a set of coordinate axes and an origin, to the conclusion that we cannot make statements that are independent of these coordinate systems. To describe the orbits of the planets Venus and Mars we may need to refer them to some set of axes. If the origin of coordinates is at the centre of the Sun, the orbits will be described as approximate ellipses; if the origin is the centre of Venus, the orbit of Venus will be described very differently. That much may be admitted, though the two descriptions are intertranslatable, and only superficially different. But however we describe the two orbits, whatever coordinate system we refer the motions to, these two orbits do not intersect. They may appear to intersect ; that is, their projections on to the surface of the celestial sphere may intersect. That is not the same thing.

There is a widespread view that the world itself, independently of the language that we use to describe it, has (or at least may have) a determinate structure. As a consequence, some languages are thought to be more appropriate than others. People talk of 'cutting reality at its joints'. I have no doubt that this takes realism too far. The world has any number of different structures, depending on how we choose to describe it. We can depict it as a group, or as a lattice, or in any number of different ways. Structural realism is a fallacious doctrine if it asserts the objectivity of structures. Yet the objectivity of truth is not compromised. Conceptual or expressive relativity is indeed perfectly compatible with alethic absoluteness. By all means talk, as many do, of mind-dependent reality, if you mean only that every descriptive language embodies a point of view. Do not suppose that truth is mind-dependent too.

From 1935 until the end of his life Popper championed the relevance to the theory of knowledge of the classical theory of truth as it was formulated by Tarski, who, he claimed, rehabilitated and demystified the classical idea of truth as correspondence to the facts, an idea that is both absolute and objective, properties that Popper usually identified. To take the stock example, the sentence 'Snow is white' is true if and only if snow is white. It may be, of course, that only from an incongruously specialized viewpoint could anyone be led to formulate such a sequence of words as 'Snow is white', yet the incongruously specialized viewpoint is not what the sentence is about ; and although it may be only from that viewpoint that it is possible to state the conditions under which the statement is true, that it meets those conditions is independent of the viewpoint. I do not want to defend everything that Popper said about Tarski's theory of truth, but on this point I think that he was absolutely correct [Miller 1999a].

5 Conclusion

It seems to have been a disregard for, or an obliviousness to, the difference between objective truth and warranted truth that has led to the doctrine that there is no objective or absolute truth. You still hear people assert that all truth is relative, meaning no more than that no truth can be established with certainty. I have indicated above that Popper's view, which I share and perhaps even go beyond, was that our knowledge not only is not certain, it does not receive, and does not need, a jot of objective warrant from the fragmentary empirical evidence that we have. This view is shared also by those who call themselves social constructivists, who have seen through the traditional epistemological protestations that, despite all that Hume said, there is a positive solution to the problem of induction. Critical rationalists and post-structuralists are at one in agreeing that this is logicall and epistemological wishful thinking at best, and in some cases simply bluster. But critical rationalists reject absolutely the untenable doctrine that because objective warrant is a prize not worth struggling for, so too is objective truth. We go further, and dismiss as an unworthy goal even that kind of local, relative, culture-specific, or subjective justification that many social constructivists appear to admire [Miller 1999b]. It is appropriate, I hope, to end in this way this tribute to the memory of Karl Popper by so accentuating what separates his philosophy from the various systems of his contemporaries, and to underline again the significance of the way in which his philosophy unites the best elements of both empiricist and intellectualist thought.

References

Haack, Susan

2005 Trial and Error : The Supreme Court's Philosophy of Science, *American Journal of Public Health* 95 (S1) S66–S73. http ://www.defendingscience.org/loader.cfm ?url=/commonspot /security/getfile.cfm&PageID=2408/.

Harris, John

1974 Popper's Definitions of 'Verisimilitude', *The British Journal for the Philosophy of Science*, 25 (2), 160–166.

Hume, David

1738 *A Treatise of Human Nature*. Book I, *Of the Understanding*, London : John Noon. References are to the edition of D. G. C. Macnabb, London & Glasgow : Collins, 1962.

Kim, Jaegwon

1988 What is Naturalized Epistemology ?, *Philosophical Perspectives 2 : Epistemology*, James Tomberlin, ed., Atascadero : Ridgeview Publishing Company, 381–405.

KUHN, THOMAS

1962 *The Structure of Scientific Revolutions*, Chicago : University of Chicago Press. 2nd edition 1970.

MCGINN, COLIN

2002 Looking for a Black Swan, *The New York Review of Books*, 21 November 2002, 46–50.

MILLER, DAVID

1994 *Critical Rationalism. A Restatement and Defence*, Chicago & La Salle : Open Court Publishing Company.

1996 What Use is Empirical Confirmation ?, *Economics and Philosophy* 12 (2), 197–206.

1999a Popper and Tarski, *Popper's Open Society After Fifty Years : the Continuing Relevance of Karl Popper*, Ian Jarvie & Sandra Pralong, eds, London : Routledge, 56-70. Reprinted as Chapter 9 of [Miller 2006a].

1999b Being an Absolute Skeptic, *Science* 284 (5420), 4 June 1999, 1625f. Reprinted as Chapter 7 of [Miller 2006a].

2005 Beauty, a Road to the Truth ?, *Confirmation, Empirical Progress, and Truth Approximation*, Roberto Festa, Atocha Aliseda, & Jeanne Peijnenburg, eds, Amsterdam/Atlanta : Rodopi B.V., 341-355. Reprinted as Chapter 10 of [Miller 2006a].

2006a *Out of Error. Further Essays on Critical Rationalism*, Aldershot : Ashgate.

2006b Darwinism is the Application of Situational Logic to the State of Ignorance, Ian Jarvie, Karl Milford, & David Miller, eds, *Karl Popper : A Centenary Assessment.* Volume III : *Science*, Aldershot : Ashgate, 155–162.

MOORE, WALTER

1989 *Schrödinger. Life and Thought*, Cambridge, New York, & Oakleigh : Cambridge University Press.

O'HEAR, ANTHONY

1985 Popper and the Philosophy of Science, *New Scientist*, 22 August 1985, 43–45.

OKASHA, SAMIR

2002 *Philosophy of Science : A Very Short Introduction*, Oxford : Oxford University Press.

Popper, Karl

1934 *Logik der Forschung*, Vienna : Julius Springer Verlag. 10th edition, Tübingen : J. C. B. Mohr (Paul Siebeck). Popper's own translation [Popper 1959] has been used in quotations.

1945 *The Open Society and Its Enemies*, London : George Routledge & Sons. 5th edition 1966, London : Routledge & Kegan Paul.

1957 Philosophy of Science : A Personal Report, *British Philosophy in the Mid-Century*, C. A. Mace, ed., London : Allen & Unwin, 155–191. Reprinted as Chapter 1 of [Popper 1963].

1958 On the Status of Science and Metaphysics *Ratio*, 1 (2), 97-115. Reprinted as Chapter 8 of [Popper 1963].

1959 *The Logic of Scientific Discovery*, London : Hutchinson. Expanded English translation of [Popper 1934].

1963 *Conjectures and Refutations*, London : Routledge & Kegan Paul. 5th edition, London : Routledge, 1989.

1970 Normal Science and Its Dangers, *Criticism and the Growth of Knowledge*, Imre Lakatos & Alan Musgrave, eds, Cambridge & elsewhere : Cambridge University Press, 51-58.

1972 *Objective Knowledge*, Oxford : Clarendon Press. 2nd edition 1979.

1979 *Die beiden Grundprobleme der Erkenntnistheorie*, Tübingen : J. C. B. Mohr (Paul Siebeck).

Quine, W. V. O.

1969 Epistemology Naturalized, In *Proceedings of the XIVth International Congress of Philosophy, Vienna, 2nd to 9th September 1968*, VI, Vienna : Herder, 1971, 87–103. References are to the preprint in W. V. Quine, *Ontological Relativity and Other Essays*, New York & London : Columbia University Press, 1969.

Rosenkrantz, Roger

1977 *Inference, Method and Decision*, Dordrecht : D. Reidel Publishing Company.

Schrödinger, Erwin

1944 What Is Life ? Reprinted in Erwin Schrödinger, *What Is Life ? & Mind and Matter*, Cambridge & elsewhere : Cambridge University Press, 1967.

VICKERS, JOHN

2006 The Problem of Induction, Edward N. Zalta, ed., *The Stanford Encyclopedia of Philosophy* (Winter 2006 Edition), http ://plato.stanford.edu/archives/win2006/entries/induction-problem/.