The Realism of Popper and Russell

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The first volume of Karl Popper's *Postscript*, entitled *Realism and the Aim of Science*, was published last. It is extremely well written; on a central topic in philosophy; and even those in doubt whether it is in fashion will want to read it in seclusion, if they have an interest in science or in philosophy.

The reader will wonder why it is that fifty years after publishing *Logik der Forschung* (or *L.d.F.*) and twenty-five years after publishing its translation, *Logic of Scientific Discovery* (or *L.S.C.D.*) Popper has put before us a book which, though quite new, is in the nature of an afterthought. Thereby hangs a tale. The original publication of *L.d.F.* occurred at a time when Logical Positivism was very much the mode, and Popper's work was widely regarded as part of the movement. Popper complained of being misunderstood over the years, and in the 1950s decided to publish an English translation of the work. Most philosophers find themselves misunderstood, and some complain bitterly about it. It is difficult to judge whether Popper was more misunderstood than most, or whether he would have been better advised to accept the reception of his book with equanimity. It is a fact, however, that the English translation of his first published work had quite a large impact on the world of ideas, not only in England and America, where his thought was often followed at second hand, but also in Germany where one presumes that German is read. It appears from this slender evidence that by repeating himself in English Popper did himself a favour.

It was during the translation that a version of the *Postscript* began to emerge at first as appendices to the translation, and only later, on the advice of students and younger colleagues (who idolized him), as a separate work. W. W. Bartley, III, the eventual editor, was closely associated with its production between 1960 and 1962, and describes the entire background well in his Foreword, recounting how it grew eventually to a size that, on his recommendation, is published as a three-volume work.

The *Postscript*, from which this first volume is taken, is thus a child of misunderstanding, and of Popper's attempt to set things straight, but it also includes many improvements in the form as well as content of the ideas expressed in the original work. As a piece of writing, it had already been cited by Popper and a select group of students and confidants who were privileged to read it before publication. Many pieces were abstracted or even lifted, and published, from this work. Having become a tradition of sorts within the special social world of 'Popperians', it is perhaps unavoidable that it is published in its present


form, as the Postscript, and W. W. Bartley III must be thanked for bringing it before the public in its final state.

The first volume consists of two unequal parts. The first part, entitled ‘The Critical Approach’, consists of four unequal chapters of which the very first chapter, the largest, comprises nearly two-fifths of the entire volume. It is an extended treatment of the problem of induction, and Popper’s solution to it without recourse to an inductive method. This is followed by a chapter on the demarcation of science and metaphysics. A chapter criticizing Carnap’s attempt to demarcate metaphysics by meaning and logical analysis, is followed by Popper’s conception of ‘corroboration’ which is proposed to contrast with various attempts to show how the problem of induction can be solved by ‘confirmation’, particularly by Carnap. The second part of the book is entitled ‘The Propensity Interpretation of Probability’, and is his most complete statement on that subject to date.

Before we take up each of these major topics in turn, we must take up the question of misunderstanding at some length, because it appears to influence Popper’s writing considerably, and this in turn influences its reception by others.

The book begins with a charming and plaintive story from 1935. Bertrand Russell, ‘the greatest philosopher since Kant’, as he is described (and how can one disagree?), read a paper on ‘Limits of Empiricism’, upon which Popper commented. The comments were, he reports, ‘I did not believe in induction at all, although I did believe in empiricism...’ This statement was apparently taken as a joke by the audience, which laughed and clapped. Of course, Popper must have spoken with that great earnestness which all those who have met him can imagine. At any rate, he did not share the joke. All these years later, it still rankles, and he explains that his statement was taken to be an attack on science when it was never intended to be so. Popper does not think that the reaction was that of an English audience to the halting speech of a Viennese philosopher. He suggests in the very next section that he is very widely misunderstood. (In an ‘Introduction, 1982’ he also complains of misunderstanding even by some of his ardent supporters.) He also goes on to explain why he is not understood, an explanation I believe to be mistaken, and which I shall examine in what follows. It is worth noting, in parenthesis, that Popper’s first volume of the Postscript can be looked upon as an extended conversation with Russell. Russell never seems to have understood Popper, who appears to hope that Russell, or philosophers influenced by Russell, will understand him better if only he repeats his message more clearly, more often and more emphatically.

Popper’s explanation for others’ misunderstanding of him is this. ‘Most of us, especially most philosophers, hold a great number of theories consciously, and after critical examination; and we may be prepared to defend these by argument and to give them up when good arguments are brought against them. But we also hold theories which we take for granted more or less unconsciously and therefore uncritically; and these uncritically held theories often contain the strongest reasons for holding those other theories’. There is the suggestion made that Popper is misunderstood because he challenges those unconsciously held theories of most philosophers. It is for this reason he says he speaks so freely in terms of ‘isms’, because by so doing he draws attention to the framework he is attacking. ‘... [W]henever a framework is attacked, its defenders will as a rule interpret, and attempt to refute, the attack within their own adopted framework. But in trying to translate critical arguments directed against the framework into a language appropriate to that framework, they are liable to produce distortions

3 Popper, Realism and the Aim of Science, p. 14.
and misunderstandings. A discussion of ‘isms’ may diminish this to some extent by constantly stressing the fact that the framework itself is under fire.  

It is only the very last sentence which is implausible in this quotation. In practice, I believe that attacking an ‘ism’ only allows the philosopher concerned to say he is not one of those who believe it, or that the attack is not specific enough to meet his particular version. There is also another reason for doubting the efficacy of Popper’s strategy, namely that his distinction between the framework and the theory may need modification, but this issue might be postponed until later.

Popper suspects that his ‘own approach to the theory of knowledge was more revolutionary, and for that reason more difficult to grasp than’ he had thought. The revolutionary character of his thought was suggested to him by W. W. Bartley III, who is happily, the editor of the Postscript. What Popper learnt from Bartley is that a certain set of important problems of knowledge are solved by Popper by changing some of them. The central problem of the philosophy of knowledge since the Reformation, Popper writes, has been that of evaluating the claims of competing beliefs. It has led to a second problem: How do we justify our theories or beliefs? Popper thinks that an apparently innocuous assumption is often made in this connection, namely, that one adjudicates between competing solutions by determining which can be justified. Bartley tells Popper that all philosophers before Popper in the Western World have been ‘justificationists’, and hence liable to misunderstand Popper’s ‘non-justificationist solution’. Popper acquiesces to Bartley’s suggestion. Elsewhere, Popper is described by Bartley as entirely outside the realm of contexts to be found in Western Philosophy to date.

The description of ‘the central problem of philosophy since the Reformation’ as the task of evaluating the claims of competing beliefs seems to me to be inaccurate. That this is how a fallibilist would interpret the task is perhaps true. I do not believe a non-fallibilist would see it thus. But to describe it thus without qualification hardly helps to clarify matters. The description of all earlier philosophers as ‘justificationist’ also seems to me to be an egocentric distortion. To a fallibilist, all earlier philosophers will appear as ‘justificationists’. Any philosopher with a distinctive point of view can thus define all earlier philosophers as different in some respect, and claim to be entirely outside the Tradition of Western Philosophy. There can therefore be no such Tradition, because all those who might fall within it will fall without. Then the case of the fallibilist is quite normal, and it cannot explain why he especially is misunderstood.

These explanations for misunderstanding are only the expression of Bartley’s romantic picture of Popper as a ‘genius’. One wishes Popper had resisted the temptation of endorsing this interpretation. Such interpretations presuppose the Romantic picture of the philosophical tradition as a coherent and self-subsisting whole. The Romantics had reasons for believing this which, in the context in which they saw things, may be described as good reasons. They believed that the tradition constructed reality, or defined truth, logic and rationality, as some sociologists with a Romantic hangover still persist in claiming. Consequently, traditions define reality and circumscribe the thought of those who dwell in them, except for the genius, the path breaking individual who changes the tradition and is therefore misunderstood (until history validates the individual’s reality by changing the tradition).

5 Ibid., p. 16.
6 Ibid., p. 19 and section 2 generally for the rest of the quotations in this paragraph.
Where, you might ask, can we find criticisms of this Romantic perspective? Well, one of the most original and devastating attacks on 'historicism', as he called it, was Popper's in his *The Poverty of Historicism*. And where can we find a rationalist's tentative theory of traditions? In none other than Popper's 'Towards a Rational Theory of Traditions'. In fact, if we recognize in Romanticism the death throes of a misguided form of idealism, then it is clear why a realist is in a position to accept tradition as consisting of different non-coherent and non-consistent strands. Since reality exists independently of tradition, there is no need to define reality or tradition to fit the Procrustean bed provided us by the Romantic. Popper, whose first volume of the *Postscript* is entitled *Realism and the Aim of Science*, is a staunch realist. Indeed, he has taken realism as a supposition and delved far and deep to find out how our intellectual traditions can be improved upon to suit the doctrine. I would therefore suggest that we should overlook this momentary aberration on his part in acquiescing to this Romantic theory of why he is misunderstood.

Far from being outside the tradition of philosophy, Popper's work is very much within it. I suggest that it is, in fact, only a variant of Russell's realism, worked out along a surprising line. I believe, moreover, that Popper's views have not found as willing an audience among philosophers partly because few have seen that this is Popper's contribution to recent intellectual development, though there are more important reasons which are to be examined below. Popper himself is as much to blame for this as anyone else, for even in the preface to his very first published work he distinguishes between science, which proceeds very much from a given current state, and philosophy, which, he says, must be worked out anew. Here I suspect he has made a serious error, and one which pervades his attitude to metaphysics. Philosophy also takes up where others have left off. Popper takes up where Russell left off (in 1912, or thereabout). There is nothing strange about this. If we were to go back in philosophy to begin where Adam did, we should progress no further than Adam (to paraphrase Popper on another occasion).

Russell turned against the idealism of Kant and Hegel in about 1898, he writes in his *My Philosophical Development*. The particular form of his revolt was against monism, because amongst other things it made arithmetic impossible. He adopted pluralism, and the existence of real external relations between things, as his stance. Based on this and on the work of Peano which he soon came across, Russell was able to suggest how all of mathematics may be derived from logic. This achievement had already been independently attained by Frege. It is one of the most remarkable developments of modern thought, but it is outside the scope of this essay to review. It is worth noting that it created a great difficulty for Kant's view, which takes its inspiration from the analysis that in geometry and arithmetic (mathematics, that is) one must start from the pure intuition of space and time, respectively. Once Russell derives mathematics from logic, the importance of spatial and temporal intuition is considerably diminished. Russell candidly admitted that his attempt to provide a certain foundation for mathematics was a failure, but Russell never went back to monism or to idealism because, on the negative side, his arguments are quite successful.

Many philosophers not acquainted with the development of mathematics think that because Russell's foundations led to the insuperable difficulties of the

logical paradoxes, that a version of Kant’s point of view must be correct. This, however, is an error; it is, moreover, an error which Russell was fully in a position to recognize.

His very first publication regarding mathematics was his *Foundations of Geometry*, in which he defended a Kantian interpretation of mathematics even though non-Euclidean geometries were known. He proposed that though Kant was wrong on a matter of detail—there is more than one geometry—he was right in principle, because a more abstract form of geometry which preserves only some of the axioms of Euclidean geometry, restricts the number of possible geometries to three. It turned out that this is not the case, ‘The geometry in Einstein’s General Theory of Relativity is such that I had declared to be impossible. . . . Apart from details, I do not think that there is anything valid in this early book’.

In fact, Poincaré, a stern critic of Russell’s later views who chose to develop a Kantian approach to mathematics, proposed a most abstract form of geometry called ‘topology’, which investigated the properties that all spaces must have. This subject can be studied in a variety of ways, and it was the great hypothesis of Poincaré that all these ways of doing topology are equivalent. In 1960, however, John Milnor showed with his famous counterexample of the Exotic Seven-sphere, that they are not equivalent, thereby vindicating Russell’s position. At any rate, Russell had convinced himself that Kant was in error regarding the foundations of mathematics by the turn of this century, and showed how mathematics can be regarded as a discursive science, rather than one based on intuition. This provided an entirely changed set of problems of philosophy, which he took up in 1912 in a book published under that name. Having rejected Kant’s philosophy, Russell was obliged to begin with a new approach to the problems as Kant had found them in Berkeley and Hume.

This is also the background for Popper’s philosophy. For both realists and pluralists, the sum total of philosophy of the idealists and monists is misguided and erroneous, providing at best occasionally inspiring models to be modified and incorporated in a different overall philosophy. Nevertheless, it is a mistake to think that Popper’s philosophy begins simply from the situation as Hume had left it. For an important part of Popper’s approach is the new logic of Russell and this places Popper’s problem squarely with Russell’s exposition of it in 1912. All this might seem too banal to state, and would not have been stated, were it not for the fact that many different ideas have been proposed regarding Popper’s place in (or out of) the philosophical tradition, and this one has to be placed beside them for comparison.

Russell struggled all his life with the problems he set out in his *Problems of Philosophy*. He proposed many different solutions in rapid succession, prompting Whitehead to call him a Platonic dialogue in himself. Russell candidly admitted the failure of all his solutions at any time, except perhaps for the latest one which he had proposed. ‘I had when I was younger—perhaps I still have—an almost unbelievable optimism as to the finality of my own theories’. The problem that Russell faced, and Popper takes up, which Kant also faced, is this: How is empirical knowledge (science) possible? For our experience is too meagre and weak to encompass all that science has taught us. How can we have scientific knowledge which is empirical?

For Russell, this problem is closely bound up with the interpretation of mathematics. The reason for this is straightforward. The rise of modern science is marked by the development of mathematical physics—of mechanics. The ancient world view regarded the sublunar world as one where only qualitative descriptions are possible. Plato thought even the region of the stars is amorphous, though less so than the region of the earth, but the ancient tradition certainly was dominated by the view that the terrestrial world is quite amorphous.

With the discovery of mathematical laws in the terrestrial realm—laws such as that of falling bodies, of barometric pressure, of pendula and of the tides—there came the realization that the ancient cosmology that divides the universe into an amorphous terrestrial part and a mathematical (perfect) celestial part must be replaced by a new conception of the universe divided into an amorphous subjective part (the mind) and a mathematical objective part (bodies). The primary qualities inhere in the latter, and the amorphousness that we have in our perceptions of the latter are explained in terms of secondary qualities which we harbour in our minds.

‘How then can minds apprehend the real world as described by science?’ is a question that is closely bound up with ‘How is a mind able to understand the mathematical universe?’, which is in turn closely related to ‘How is mathematics possible as a form of knowledge?’

The last question must be addressed first. Both Russell and Kant do in fact address it first, and come to different conclusions—Kant finds mathematics to be based on synthetic a priori intuition, whereas Russell finds that it is empty, analytic, and derived from logic.

Popper never raises this problem, or discusses it at any great length. I suggest that he does not do so because in fact he has accepted Russell’s solution, and has no need to repeat it. In fact, this lack of attention to the foundations of mathematics which distinguishes the two philosophers masks a remarkable similarity between Russell and Popper, which the reader will not fail to notice. Both of them suffer from a failure of understanding, though it manifests itself in different ways in their two cases.

Russell felt quite out of place with the philosophy practised from the middle of this century. ‘It is not altogether pleasant to find oneself regarded as antiquated after having been, for a time, in the fashion.’ Speaking of the school of philosophy in vogue in the 1950s, he said, ‘In reading the works of this school I have a curious feeling, such as Descartes might have had if he had been miraculously restored to life in the time of Leibniz and Locke’. And again, he says of this school of language philosophy that it ‘remains to me completely unintelligible. Its positive doctrines seem to me trivial and its negative doctrines unfounded. I have not found in Wittgenstein’s *Philosophical Investigations* anything that seems to be interesting and I do not understand why a whole school finds important wisdom in its pages’. All this fits in well with Popper’s own sentiments. The following passage, from Russell’s review of Ryle’s *Concept of Mind* might be found in Popper’s writing just as easily:

One very general conclusion to which I have been led ... is that philosophy cannot be fruitful if divorced from empirical science. And by this I do not mean that a philosopher should “get up” some science as a holiday task. I mean something much more intimate: that his imagination should be impregnated with the scientific outlook and that he should

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15 Ibid., p. 214.
16 Ibid., p. 216.
17 Ibid., p. 216.
feel that science has presented us with a new world, new concepts and new methods, not known in earlier times, but proved by experience to be fruitful where the older concepts and methods proved barren'.

Russell's failure to find anything of interest in 'ordinary language philosophy' is not a sudden failure, but had its roots in his failure fully to appreciate Wittgenstein's very first book. 'I do not feel sure that, either then or later, the views which I believed to have derived from him were in fact his views. He always vehemently repudiated expositions of his doctrine by others, even when those others were ardent disciples'. In fact, Russell failed to see a strong Kantian streak in Wittgenstein, from the *Tractatus Logico-Philosophicus* onwards. It is not surprising that he misses this, because they seem to agree on so many issues and even on this fundamental issue Wittgenstein's view was only very subtly different.

Russell believed that mathematics is analytic, that logical truth is tautological, that logic considers all possible cases where empirical science concerns itself with what is in fact the case. With all this Wittgenstein agreed, and yet he gave a certain Kantian twist to these views. Wittgenstein held that the world *to the extent that we can speak of it* is captured by logical possibility. Thus language circumscribed the 'logical world' just as for Kant the world *to the extent that we could perceive it* was phenomenal and had its presuppositions. The difference between Kant and Wittgenstein, however, is this: for Kant, the world beyond what can be perceived (the 'thing-in-itself') cannot be shown (or seen) but we can *say* of it that it exists. He even thought that he could prove that it exists. But for Wittgenstein, what lies beyond language and logic (something structural) can be *shown*, but cannot be put in words, for obvious reasons. 'In emphasizing the importance of structure, I still think he was right, but as to the doctrine that a true proposition must reproduce the structure of the facts concerned, I now feel very doubtful, although at the time I accepted it', says Russell. 'For Wittgenstein, however, it was fundamental. He made it the basis of a curious kind of logical mysticism'. It is this mysticism which Russell never quite understood because he could never work up any sympathy for the new conception of the world as a *linguistic entity* which Wittgenstein had proposed. This was Wittgenstein's understanding of a *fact*, that it has a linguistic (or logical) structure. If the world is a collection of facts, not of things (the second sentence of his *Tractatus*) it is difficult to miss the significant point that the world has a logical structure.

Russell never fully understood how this could be so. 'There is a curious suggestion, already to be found among some *Logical Positivists*, that the world of language can be quite divorced from the world of fact. If you mention that a spoken sentence is a physical occurrence consisting of certain movements of matter and that a written sentence consists of marks of one colour on a background of another colour, you will be thought vulgar'. It is curious that Russell here describes the world of language as divorced from 'the world of fact' according to the *Logical Positivists*, because it is the analysis of 'facts' that leads Wittgenstein to a linguistic cosmology.

'But the adherents of [ordinary language philosophy] go further', says Russell. 'There had been two views about empirical statements: one, that they were justified by some relation to facts; the other that they were justified by some
conformity to syntactic rules. But the adherents of [ordinary language philosophy] do not bother with any kind of justification, and thus secure for language an untrammelled freedom which it has never hitherto enjoyed. It is clear that Russell never appreciated how the world could be linguistic, or how science could be just another linguistic exercise. ‘The desire to understand the world is, they think, an outdated folly. This is my most fundamental point of disagreement with them’. But the linguistic philosopher, and the early or late Wittgenstein would not want to deny the value of science—just its immediate relevance to what the philosopher does. This is one thing that Russell could never understand, because for him there was only one inquiry, be it science or philosophy.

Popper is not ignored today, by any means. Outside the narrow world of the specialist philosopher, Popper is widely and warmly acclaimed, both within and without intellectual institutions. It is curious that among philosophers he is least appreciated. This lack of appreciation shows itself sometimes in a supercilious attitude towards him, sometimes in a hostile attitude which is at its worst when he is pointedly ignored. The reason that he is ignored is the same one that made Russell feel out of place. Neither of them have appreciated the strength of the neo-Kantian point of view.

Popper pays great tribute to Tarski, not once but many times over in many different places, for providing a satisfactory theory of truth. Biographically speaking, one may concede that Tarski had a great and liberating influence upon him, which is quite a revealing fact about Popper. Tarski studies certain biconditionals of the form, ‘The statement “Russell is a realist” is true if, and only if, Russell is a realist’, which are used in a preliminary way to characterize the predicate ‘true’, before proceeding to study the difficulties of the semantic paradoxes which beset this formulation. (‘The statement “This statement is false” is true iff the statement is false’.)

Tarski provided an ingenious way of avoiding a commitment to a world of facts in his analysis of the concept of truth. He defined a notion of ‘satisfaction’, in which a sentence with a variable in it can be satisfied by sequence of objects. In many respects, this is just like the notion of ‘reference’ that we commonly understand, where ‘x is red’ is satisfied by those things which are red. Tarski then defines truth in terms of satisfaction so that just as a predicate is true of certain sequences of objects, and not of others, it turns out that sentences (with no free variables in them) will be satisfied by all sequences, if they are true, or by none, if they are not. In this way the concept of truth can be analyzed as some sort of a correspondence with reality without subscribing to a world of facts, not things, as Wittgenstein had done.

Popper, like Russell, believes in the world as described in physics and biology, or psychology and history. He therefore found Tarski’s answer to the question ‘How are things grouped together (arranged, structured . . .) to reveal a fact?’ appealing. Russell was also much concerned with this question and did not accept the world of linguistic entities as a substitute for the world of the scientist. ‘Some Logical Positivists—notably Neurath and Hempel, and Carnap at one time—maintained explicitly that sentences must not be confronted with fact. They maintain that assertions are compared with assertions, not with experiences and that we can never compare reality with propositions’. Hempel maintains that the system which we call true ‘may only be characterized by the

23 Ibid., p. 218.
24 Ibid., p. 219.
25 For example, chap. 10 of Popper, Conjectures and Refutations.
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historical fact, that it is the system which is actually adopted by mankind, and especially by the scientists of our cultural circle. Russell found this quite unacceptable.

Even though the problem of truth which Wittgenstein faced in the Tractatus was solved by Tarski without recourse to a world of facts, the conception of language that pervades post-Russellian philosophy continues to be Wittgensteinian in this respect: like Wittgenstein, philosophers have come to believe that certain abstract features of language reflect certain abstract features of the world. The study of the logic of language gives us an insight into the way things must be—something above and beyond what empirical science does, or can do.

Russell proposed a philosophy of language which is widely regarded as inadequate. Popper who offers no general perspective on language at all is therefore ignored all the more. Because Russell never appreciated the force of the neo-Kantian perspective in language, he tended to ignore recent philosophy. Because Popper seems to have nothing to say about language, the neo-Kantians neglect him.

In fact, Popper's point of view is realistic, like Russell's. Popper cannot hope to find a more sympathetic hearing until the problems of the philosophy of language have been seen to be better resolved from a realist's perspective.

INDUCTION

In The Problems of Philosophy, Russell summarizes his problems as follows:

...if we take any common object of the sort that is supposed to be known by the senses, what the senses immediately tell us is not the truth about the object as it is apart from us, but only the truth about certain sense-data which, so far as we can see, depends upon the relations between us and the object. Thus what we see and feel is merely 'appearance', which we believe to be a sign of some 'reality' behind. But if the reality is not what appears, have we any means of knowing whether there is any reality at all? And if so, have we any means of finding out what it is like?37

Russell struggled all his life with this set of questions, believing as he did that there is such a reality, and that science does provide us some means to finding out what it is like. Popper, like Russell, is a realist and, moreover, a scientific realist (though not in Popper's technical sense of that phrase). He, like Russell, thinks that the truth lies 'hidden in the deep', but is somehow accessible to us through science. Russell's many and different attempts to answer these questions were never totally satisfactory, either for us, or for himself—and so candid was he that he would acknowledge his failures. In 1912 he had already stated what might have been a summary of what he could accomplish on the subject. 'Philosophy, if it cannot answer so many questions as we could wish, has at least the power of asking questions which increase the interest of the world, and show the strangeness and wonder lying just below the surface even in the commonest things in daily life'.28

Popper would not accept the problem as Russell states it, particularly the reference to 'sense-date'. Our perceptions are much too complex to be understood in that way. But he, like Russell, would think that one cannot in perception find a direct way of apprehending the reality (or the natures) of things around us. To that extent Russell's problem may be put before Popper as the problem he has to solve, with differences of a verbal nature to suit Popper's philosophy.29

28 Ibid., pp. 24-25.
29 This relates to the 'fourth stage' of the problem of induction, reviewed below.
Russell also stated his problem in the following manner, as restated by Popper.

If Hume is right that we cannot draw any valid inference from observation to theory, then our belief in science is no longer reasonable. For any allegedly scientific theory, however arbitrary, becomes as good—or as justifiable—as any other, because none is justifiable: the phrase “My guess is as good as yours” would rule scientific method as the only principle. Thus, if Hume were right there would be “no difference between sanity and insanity”, and the obsessions and delusions of the insane would be as reasonable as the theories and discoveries of a great scientist.30

Popper responds to this problem as follows:

... we may defend the claim that the scientist’s theory is better—better even in the somewhat narrow sense of being better supported by observations. For they may contradict one while being compatible with the other. Hume’s argument does not establish that we may not draw any inference from observation to theory: it merely establishes that we may not draw verifying inferences from observations to theories, leaving open the possibility that we may draw falsifying inferences: an inference from the truth of an observation statement (“this is a black swan”) to the falsity of a theory (“All swans are white”) can be deductively perfectly valid.31

This is the fundamental innovation that Popper suggests to improve upon Russell’s point of view. The question is, of course, whether this difference is enough to distinguish science from insanity, though Popper does show that some difference can be found. If we turn to Russell’s problem as cited from the Problems of Philosophy we find that Popper’s otherwise distinctive point of view is not easily distinguishable from that of Russell. His response to ‘have we any means for knowing that there is any reality at all?’ is ‘No, if by “know” we mean, know for certain’32 and, ‘have we any means of finding out what it is like?’, his answer is that we have only the means of finding out that we are mistaken in thinking that it is thus.33 Because these answers sound somewhat weak, many philosophers regard Popper’s philosophy as too sceptical. For all of Popper’s love of science, philosophers who are not satisfied with these answers believe that Popper’s solutions, if accepted, would only undermine science, realism and rationality.

The question that we face, then, is this: is it possible to show how science is based on the principle that we may sometimes find out where a theory might be wrong? On this point Popper’s formulation is very carefully stated, when he says that observations ‘may contradict one while being compatible with the other’, suggesting that the falsifying power of observations is of greatest importance when they discriminate between theories (more on this later.)34 Popper anticipates the worry of the philosopher who thinks that the possibility of falsifying instances is not enough to show how science, as we know it, is possible. Popper tackles this as a second stage of the problem.

There is more to a good theory than that it has escaped falsification so far: even if we admit that we are always fallible and very prone to make mistakes, and that all scientific theories are conjectural, it is unreasonable to deny that there is a tremendous amount of positive knowledge in science. But how can we admit the reasonableness of this position... and, at the same time, admit that Hume is right?35

30 Popper, Realism and the Aim of Science, p. 54, quoting Russell, History of Western Philosophy.
31 Popper, Realism and the Aim of Science, p. 54.
32 Cf. sec. 7, ibid.
33 Ibid., p. 34.
34 See the review of corroboration, below.
35 Popper, Realism and the Aim of Science, p. 56. It is this, I suspect, which Russell
If science is better than insanity according to the weak criterion of falsification, is it better in the sense in which we value science as a repository of positive knowledge? Popper’s response is that this problem needs no new ideas to meet it. It may be answered as follows: the better theory is better not in the sense that it is true, where the other is false, but that it is more truthlike. Although he suggests that there is nothing new in this idea, it is in fact quite independent of the first solution, and, I believe, in error. ‘What we believe... is not that Newton’s theory or Einstein’s theory is true, but that they are good approximations to the truth, though capable of being superseded by better ones’. Popper’s idea of ‘good approximations to the truth’ or ‘truthlikeness’ or ‘verisimilitude’ seems at first blush a plausible notion. Had we known the truth in the sense of ‘the whole truth’ as Popper describes it elsewhere, we may then have asked of Einstein’s and Newton’s theory which of them better approximates that truth. We might then have been quite satisfied to find out that Einstein’s theory is a better approximation to it.

We may have two minor difficulties which makes us reluctant to accept this account without some pause for reflection. First of all, we do not know the truth. But this is a matter easily dealt with by Popper who invokes this idea of ‘better approximations’ also as a guess, not as ‘knowledge for certain’. More serious is the point that there is no criterion of ‘good’ or ‘better’ approximation to the truth which stands up to logical scrutiny. Doubts on this count are dismissed as ‘incompetent’ because he can use the notion as a ‘primitive’ or undefined concept. The trouble with this dismissal is that it does not address the possibility that the undefined concept may be empty—there may be nothing to correspond with ‘better approximation to the truth’—or, equivalently there may be several such measures, each as good as the other, which give different ordinal preferences between the same theories. Popper has not met an elegant argument in the literature which claims that this is in fact the case.

Much more important than that is the question whether Popper’s attempted solution is adequate to solve the problem for which it is proposed. To help us evaluate this, let us conduct a thought experiment, namely, to account for the positive knowledge contained in science. Let us imagine that our world is quite different in general from the little corner in which we live. The general principles of this world are modified in our little corner by the particular conditions prevalent here which makes our little corner appear to exemplify principles quite at variance with the general principles true in the entire universe. This is not so far from the case, in one or two respects, as we know it. Aristotle’s theory of motion suggests that any motion will continue only for a finite time unless there is a cause to perpetuate it. In the universe as a whole motion needs no cause for its perpetuation, according to Newtonian physics, but it needs a cause to slow it down (or speed it up), which is provided on earth by the presence of surrounding media which exert friction. Thus the physics of the earth appears to show how right Aristotle is, because of the normal existence of frictional forces, even though it truly exemplifies modern physics.

regarded as ‘the somewhat despairing theory of Professor Popper’ in his preface to Jean Nicod, *Le Problème Logique de L’Induction* (Paris 1961). See also J. Wettersten’s recent discussion of this passage in his ‘Russell and Rationality Today’, *Methodology and Science*, 18, 1985, 155. Wettersten regards Russell’s philosophy, as I do, to be the background against which Logical Positivists, Polanyi and more recent writings in philosophy and history of science must be read regarding the question of rationality.

36 Popper, *Realism and the Aim of Science*, p. 57.
37 Ibid., p. xxxv.
In an analogous vein let us imagine what is quite likely to be true, namely, that in some unsuspected but important way, our physics is locally but not globally accurate. Let us also imagine, for the sake of argument that for a long time we do not find out our peculiar situation. Then, unknown to us, it may well be true that the progression of scientific theories, though better and better approximations to local truth, are worse and worse approximations to the global truth—let us say the truth of the universe beyond the range that light can reach us. Now we imagine that one day the truth is discovered, say a thousand years hence. Would we then see the progression of progressively 'less truth-like' scientific theories for a thousand years as containing less and less 'positive knowledge' as it develops? It is clear that this is far from what we would say. What we would say is that science did provide better and better theories, and more and more 'positive knowledge': though with a limited scope of application. This shows, I believe, that one cannot equate what positive knowledge one gets from science with its verisimilitude. It seems that there must be two different ways, at least, in which a theory can approximate the truth—in the faithfulness to the world as it impinges on us in multifarious ways, or in its similarity on matters of abstract principle to the laws operating in the universe as a whole. It would be a scientific hypothesis, and likely a false one in the light of the history of physics, which would assert that the more accurate theory on matters of detail is also the more 'truth-like' theory in its abstract or overall form.

'Verisimilitude' does not, I conclude, give any account of the positive knowledge that science provides us. To account for this, it seems to me, one has to use entirely different principles than the one invoked by Popper in his theory of 'good approximations', one of which is this. Any hypothesis, whether true or false, has an infinite number of true consequences, according to the logic of Bertrand Russell. Accordingly, every theory has much positive knowledge. Only false theories also have false consequences. This answer, though it accounts in a way for the positive knowledge in a scientific theory may not satisfy the philosopher who is looking for still more from science than from insanity. For an insane man's theory also has many true consequences, as anyone who studies the power of delusions on the deluded will testify.

There is a sense in which the knowledge that is contained in science gives us 'power over nature' which those who have no such knowledge can have only to a lesser extent. This still needs to be captured from a realist's point of view. The fallibilist's approach, while it offers some improvements upon Russell's point of view, does not give a satisfactory answer to the problem of induction till it shows us how a society which has a scientific tradition has greater power over nature than one that does not. It is the question of the 'power over nature' that science gives to us that appears to be addressed in what is called the 'third phase' of the problem of induction.

This phase of the problem, according to Popper, is not at all important. It is, he says, 'a typical philosophical muddle' if one has accepted his solution to the first and second phase of the problem, as reviewed above. 'Admittedly, an inductivist like Hume or Russell may think that it is indistinguishable from what I have called the problem of induction, and he may even think that it is a superior formulation of the same problem', when it is posed as, 'How do we know that the future will be like the past?' or, 'How do we know that the laws of nature will continue to hold tomorrow?' He replies: 'I do not know that the future will be like the past; on the contrary, I have good reason to expect that it will be different

39 Ibid., p. 62.
in many ways'. He does think that we have good reason to believe in well-tested scientific hypothesis.

The language philosopher who is giving Popper a chance up to this point now gives up hope that he can provide a viable answer. 'I have found, however', says Popper himself, 'that inductivists are not satisfied by these answers, and that they do not feel that the third phase of the problem, the problem of tomorrow, is solved'. With this candid assessment Popper proceeds to clarify some other views of his which might be misconstrued to suggest that he gives an answer of the 'inductivist' sort for which these philosophers are looking. Popper's remark that it is the 'inductivist' who is not satisfied with Popper's reply is apposite but nevertheless misleading: he who is not satisfied by Popper's reply is thereby called 'inductivist'. The prior question is, of course: should one be satisfied with Popper's reply?

The very first thing to notice is that the third phase of the problem does not necessarily concern science, and yet it is not noticed that there is a difference worth remarking. Most contemporary 'common sense' philosophers would say that we know that the sun will rise tomorrow, that the bread we receive will nourish us, and so on and so forth. Popper's remarks that we do not know this for certain would be dismissed as irrelevant, for the usual sense of the word 'to know'. For such a philosopher, there is a sense of the word 'to know' in which I do know that the sun will rise tomorrow, but do not know that the taxes will rise next year. This idea is not found in Popper's solution to the problem of induction. Popper, who finds little use for ordinary language or common sense philosophy, might sympathize with Russell's remark that he cannot see why we should be too concerned with 'the different ways in which silly people can say silly things'.

But I confess I find Popper's lack of sympathy for the rejoinder by the common sense philosopher to be the main reason for his poor reception amongst philosophers generally. Let us distinguish three different positions that may be held regarding the status of our everyday knowledge.

(a) We just happen to know that the sun will rise tomorrow even though we cannot validly infer this from past experience.
(b) We know that the sun will rise tomorrow because we can (somehow!) infer that from our past experience.
(c) We do not know in the sense of 'know for certain' that the sun will rise tomorrow because it cannot be inferred from experience.

Both the ordinary language philosopher (a) and Russell (b) would dismiss Popper's solution (c) as too sceptical.

The first type of philosopher may or may not agree that there is a problem of induction for science, but he would contend that this problem does not affect common sense or everyday knowledge. Everyday knowledge is often factual, and may depend upon our understanding of the world, but it does not affect and is not affected by science. In this respect, Popper and Russell agree in regarding the common-sense philosopher as in error. When Popper defends his point of view, he argues against the inductivist, who holds (b), but not against the ordinary language philosopher who holds (a). The two appear to be similar only in that they find Popper's answer too sceptical. While a further analysis of the first point of view will have to wait until we review Popper's discussion of the demarcation of science from metaphysics, it is worth noting that Popper's viewpoint does agree with that of the common-sense philosopher up to a point. He does believe that in a sense of the word 'to know' (which does not include

40 Ibid., p. 63.
41 See in particular, G. E. Moore's essays Philosophical Studies, London 1922.
certainty), we do know that the sun will rise tomorrow. The difference between Popper and the ordinary language philosopher appears to be Popper's conviction that while everyday beliefs are often largely independent of science (and our belief in the regularity of the solar day predates the astronomer's explanation of it), science may nevertheless improve upon or modify our common-sense knowledge, because all knowledge is of a piece.

But for the commonsense philosopher this only makes matters worse. How can science, if it is as tenuous as Popper's account seems to him to leave it, improve upon the solid common sense on which our life is based? Either Popper must believe that science is more solidly (i.e. inductively) founded, or Popper must hold that our everyday knowledge is highly dubious. Popper stoutly denies the first, and he could not seriously believe the second (so the ordinary language philosopher would contend) if he says that in the ordinary sense of the word 'to know', we do know the common-sense things that we ordinarily say we know.

I estimate therefore that this longer and more carefully worked out analysis of the problem of induction will not do enough to improve its reception amongst those philosophers who are not already convinced that he is right. Even Popper's response in another volume, 'The Two Faces of Common Sense', which contrasts common-sense realism and the common-sense theory of knowledge fails to meet the common-sense conviction that they are both right. Of course they are not both right, in the light of modern science. How then do we give an account which shows that they 'approximate the truth'? The task of finding a fallibilistic answer to this question, perhaps using neo-Darwinian considerations, remains an important avenue for further research along these lines.

The everyday knowledge of everyday things, upon which we rely for our daily existence does not rest upon a priori scientific investigation of its reliability. On the contrary, any tests in science depend upon many assumptions of an everyday variety. But the relation between science and everyday knowledge is not an antithetical one, as appeared to be the case in the aftermath of the scientific revolution. In the seventeenth century the claims of science and of reason served to override the claims of 'solid common sense', which appeared riddled with superstition.

It is a curious turn of events that today the advance of science itself gives us an understanding of why our everyday knowledge of our environment is reliable, and it is this which Popper can depend upon to respond to the difficulties raised by ordinary language philosophers. The scientific theory which has taught us how empiricism works is Darwin's conception of the evolution of species. It tells us that any species of organisms which has survived long periods of time will have behavioural skills that allow the organisms to manoeuvre in and take advantage of this environment. The skills of such organisms allow them to function effectively. And this is precisely the 'common sense' philosophers' perception of how we human beings understand our surroundings and each other—namely, as a way of doing things.

Popper's judgement that the third problem of induction is not important has led him to neglect the interesting relationship between normal everyday skills and our 'common sense' knowledge of the world and of each other. Every skill of an organism presupposes that the environment in which that skill is appropriate has certain special features. We may say that the skill presupposes the proposition 'the environment is... where the dots are replaced by appropriate descriptions which make the skill applicable there. The presupposed propositions would be 'common-sense' knowledge of the most general and basic sort. They

would often be left unarticulated, of course. One of these might occasionally be erroneous (our skills do sometimes fail us). But we are nevertheless sufficiently often correct in our estimates of the environment, when we are in our natural habitat, that we have a great feeling of confidence. It is this that the 'ordinary language' philosopher and Russell have noticed and which they value so much about everyday knowledge.

Elaborating along these lines, it seems to me that Popper can make a reasonable response to the question that worried Russell so much, namely how it is that we can know so much about the world if induction fails—as Hume had indicated it did? It is not necessary to regard Popper's ideas as scepticism (or, contrarily, 'inductivism') in disguise. Popper's point of view opens the fascinating prospect of scientific solutions to our philosophical problems. This prospect is not as bleak as many contemporary philosophers might think who believe in a separation of science from philosophy. We need only remember that almost all of our science has been invented to solve old philosophical problems in a way that took them out of philosophy. Nothing could be more in the spirit of Russell's scientific realism than to find in our scientific conception of organisms the explanation of how we can understand the world at all. For the word 'philosophy' stands for something neither above nor below the natural sciences, but for something beside it.

The fourth and final stage of the problem of induction is called the 'metaphysical' stage, and is expressed as a challenge to show that there are true natural laws. Though preliminary, this formulation is preferred to the more common ones, like 'Will the future be like the past?', which presupposes a 'highly suspect theory of time', from the standpoint of the theory of relativity. This stage is 'metaphysical' because science is distinguished from metaphysics by the criterion (to be reviewed next) that metaphysical statements cannot be falsified, where scientific statements can, and a statement of the form 'there is at least one true law of nature' is unfalsifiable as a purely existential statement. The reason why this is the fourth stage of the problem of induction is because it is an attempt to answer Hume's scepticism, which is paraphrased as follows. 'I believe that we live in a real world, and in one exhibiting some kind of structural order. Can you show that this belief is reasonable?' In fact, the discussion of this fourth stage turns out to be a discussion of two things: (a) can we know that there is a real world? (b) what can we know about its structural form? And this is remarkably similar to the question with which Russell begins his life-long quest in Problems of Philosophy. Popper describes his arguments for metaphysical realism as 'inconclusive'. Coming from a philosopher who believes that one finds no conclusive proof outside, perhaps, of formal logic and mathematics, this gratuitous admission that his arguments are inconclusive suggests somewhat more than it says. This section includes a spirited attack on solipsism, which takes Popper no further than Russell; and an attack on instrumentalism as a solution to the problem of induction, which, though more successful, must be familiar to the readers from his other writings.

The discriminating reader will be pleased to see 'The Aim of Science', which was extracted and published in Ratio in 1957, appearing here as a section in its original setting. This paper, like some others extracted from the Postscript, have exerted considerable influence long before this work appeared before the public.

'The Aim of Science' explores the relation between realism and the aim of science, and is important because it attempts to set out systematically Popper's new ideas on methodology since publishing L.d.F. He claims here to derive most

43 Ibid., p. 80.
of what he has to say on methodology from the statement that the aim of science is to find

*satisfactory explanations* of whatever strikes us as being in need of explanation. By an *explanation* (or a causal explanation) is meant a set of statements one of which describes the state of affairs to be explained (or the *(explicandum)*), while the others, the explanatory statements, form the ‘explanation’ in the narrower sense of the word (the *(explicans)* of the *(explicandum)*).

The deductive model of explanation, here invoked, had played only a secondary role in the *L.d.F.*, being suggested as an explication of the notion of ‘cause’ (though Popper would no doubt not like this way of describing it, given his general dislike of meaning analysis). Yet this explication, or *meaning-reduction*, is here offered as a basis from which his entire methodology is to be derived. The importance of this paper, however, lies not in Popper’s capitulation to language philosophy, which has not taken place. Nor is it that here he joins hands with Hempel (which he does), who had been advocating a deductivenomological model of scientific explanation since the 1940s, and which was already somewhat influential, though it was later to become dominant in philosophy of science. The importance of this paper is that it is so rich in its analyses that it inspires the eventual rejection of the explanatory model for science which is advocated in it.

In this section is studied a curious relationship between general laws, such as the laws of motion proposed by Newton, and ‘derived’ laws, such as those proposed by Galileo and Kepler. The fact addressed was first noticed by Duhem at the turn of the century, that one could not derive the more general laws from the less general because, among other things, *The two are incompatible!* It is therefore concluded, quite correctly, that the process of induction which we ordinarily assume proceeds from observation through laws of a low level of generality through a middle level of generality, to a higher level of generality is impossible because, in the majority of cases middle level laws are logically incompatible with the general laws.

Thus, to give a simple example, Galileo’s law of falling bodies which states that freely falling bodies accelerate *uniformly* is not compatible with a Newtonian expression of the gravitational law which says a body falls proportionately to a force which is in turn proportionate to $m^1 \times m^2/d^2$, where $m^1$ and $m^2$ are the mass of the body and of the earth, respectively, and $d$ is the distance of the body from the centre of the earth. It is straightforward that while $m^1$ and $m^2$ are constants, $d$ changes as the body falls, and therefore the quantity $m^1 \times m^2/d^2$ increases as the distance $d$ decreases, so the acceleration increases with time, contrary to Galileo’s law.

Galileo’s law is not grossly in error because the difference to $d$ when a body falls in the vicinity of the earth’s surface is such a small proportion of the total distance $d$ from the centre of the body to the centre of the earth that the effect of the change in $d$ is negligible. But as the distance a body falls increases, the effect becomes less and less negligible. From a logical point of view, however, a difference, though negligible, makes the two laws incompatible. But from Newton’s laws we can derive a weaker law in which distance is given ‘approximately’ which would be compatible with Galileo’s laws. In other words, Galileo’s laws are incompatible with, but good approximations to Newton’s laws near the earth’s surface.

This logical state of affairs may also be shown to hold between Newton’s laws and each of Kepler’s laws of planetary motion. In fact, Einstein’s famous paper on ‘The Electrodynamics of Moving Bodies’ (The Special Theory of Relativity)
also contains a demonstration of Newtonian mechanics as an approximation to relativistic laws for slow moving bodies. It seems to be a very general phenomenon in physics (and science generally) that new and more general laws which are proposed are incompatible with, but in some way explain the plausibility of the earlier less general laws. Popper uses this fact of incompatibility to knock down the last defences of the ‘inductivist’ historian of science; and he is inspired by the notion of ‘good approximations’, as we have seen, to propose his idea of ‘truth likeness’ or ‘versisimilitude’.

Popper’s analyses, however, have inspired a new romanticism in the philosophy of science, quite inadvertently, and hardly to his liking. Inspired by this paper, Feyerabend wrote a critique of explanation and reduction in 1962. Popper’s well argued study of Newton and Kepler (or Galileo) shows not only that Newton’s theory cannot be inductively obtained from Kepler’s incompatible laws, but also that Newton’s theory does not explain Kepler’s laws, in precisely Popper’s sense of deductive explanation. What is deductively explained is something like Kepler’s laws which is not logically the same as Kepler’s laws. Thus the inventor of a general system of thought invents not only the explicans but the explicandum, too. A significant theory invents its own facts! Feyerabend was inspired by this, in the years to follow, to find in science a role for the romantic genius, in the form of the scientist who transcends rationality, who is opportunistic, who is a creator rather than an analyst. If Feyerabend influenced his friend and colleague Thomas Kuhn who was also at that time at Berkeley, California when they were both ‘in ferment’ it was to inspire another kind of extraordinary romantic model of science that has found a prominent place in the world of ideas. Kuhn suggests a temporal model (‘historicist’ on one understanding of the word) in which an epoch in science defines both the tasks to be performed (in the deductive model, the explicandum) and the manner of performing the tasks (the explicans).

My own view of this matter is that the romanticism of both Feyerabend and Kuhn is excessive, each in its own way, and not borne out by what we know of the history of science. I suggest, instead, that the idea of interpreting science as a deductive system must be abandoned altogether, and we should seek a different way of integrating scientific method. The development of such a theme, however, goes well beyond the scope of a review.44

DEMARCATION, MEANING, CORRIBORATION

Reading the section on metaphysics is like going into a time-machine today, and emerging, with the heroine of a science-fiction novel, over fifty years ago when L.d.F. was being published. In those days, the distinction between science and metaphysics was widely discussed. The logical positivists had proposed an empiricist theory of meaning, which would allow for science, and rule out metaphysics as meaningless. Now, fifty and more years later, philosophers are as little concerned about that issue as they are about the issues that exercised Bosanquet and Green, Bradley, McTaggart and Taylor in a yet earlier age. The cynic might conclude that philosophical fashion has changed. I believe, myself, that we have made considerable progress since then, though, no doubt one could find examples among philosophers of science who assume that the distinction

made in the second quarter of this century between science and metaphysics by logical positivists is basically correct, though not worth discussing.

Popper has a very good apparent reason for studying the demarcation problem anew, and for defending his old solution, though the reason collapses upon examination. As one who has given up induction, he says, he is faced with the question of how are we to distinguish the theories of empirical science from pseudo-scientific or non-scientific or metaphysical speculation? This, he says, is the problem of demarcation. But this seems to me to be a mere duplication of effort. When studying induction we saw that we must take up the question raised by Bertrand Russell, namely, how does science differ from insanity? Popper's response is that science is distinguished by the critical discussion which its theories endure, as opposed to the theories of the insane. But if it is critical debate which separates scientific speculation from the empirical theories of the insane, so to speak, why not let that stand as the solution to the further problem? There must be another reason for proposing the thesis that science consists of falsifiable theories.

In an 'Introduction, 1982' he takes critics to task who understand 'falsifiable' to mean 'may be conclusively falsified', rather than the technical meaning given to it in L.d.F., which is that a falsifiable theory is contradicted by a 'basic statement' which may or may not be true. A basic statement is existential in logical form, and is restricted spatio-temporally, as in 'There exists a black swan at the Victoria Gardens zoo this year', to make 'All swans are white' falsifiable.

But Popper is not oblivious to the question whether this issue is worth addressing at all. 'For what's in a name, or in a distinction, or in a classification, or in a demarcation?'

Further on, '... subject matters and other divisions of learning are fictitious and badly misleading, convenient though they may be as administrative units'. He contends, furthermore, 'as far as science and metaphysics are concerned, I certainly do not believe in anything like a sharp demarcation. Science has at all times been profoundly influenced by metaphysical ideas; certain metaphysical ideas and problems... have dominated the development of science for centuries, as regulative 'ideas', while others... have by degree turned into scientific theories'. One would think that the attempt to demarcate is close to being abandoned as worthless. 'As these examples show, there cannot be any sharp demarcation between science and metaphysics; and the significance of the demarcation, if any, should not be overrated. In spite of this, I contend that the problem of demarcation is highly significant'. This is puzzling. If the demarcation is not significant, how can the problem demanding demarcation be highly significant? 'It is so', he continues 'not because there is any intrinsic merit in classifying theories, but because a number of genuine and important problems are linked with it; in fact, all the main problems of the logic of science'. This reason seems to me to be quite flimsy. I confess I do not understand the theory of problem links, having never come across an explicit statement on it anywhere. But, whatever the theory, it seems to me that if a problem A is not important, but it is linked to problems B, C and D which are sublimely important, why not just discuss B, C, and D and mention A in a footnote, if at all? Why resist the force of the original and considerable arguments sketched above?

45 Popper, Realism and the Aim of Science, p. 159.
46 Ibid., p. 159.
47 Ibid., pp. 159-60.
48 Ibid., p. 161.
'But the problem of demarcation is also of practical importance. I stumbled upon this problem, and upon its solution, several years before I had become interested in the problem of induction, and before I had perceived the links between the problem of induction and demarcation, to which I have just referred'.\textsuperscript{49} This story is well worth retelling.

It was in 1919 when I became suspicious of various psychological and political theories which claimed the status of empirical sciences, especially Freud’s ‘psychoanalysis’, Adler’s ‘individual psychology’, and Marx’s ‘materialist interpretation of history’. All these theories were argued in an uncritical manner, it appeared to me. A great number of arguments were marshalled in their support. But criticisms and counter arguments were regarded as hostile, as symptoms of a wilful refusal to admit the manifest truth; and they were therefore met with hostility rather than with arguments.

What I found so striking about these theories, and so dangerous, was the claim that they were ‘verified’ or ‘confirmed’ by an incessant stream of observational evidence. And indeed, once your eyes were opened, you could see verifying instances everywhere'.

In summary, ‘It was precisely this fact—that they always fitted, that they were always “verified”—which impressed their adherents. It began to dawn on me that this apparent strength was in fact a weakness, and that all these “verifications” were too cheap to count as arguments’.

The motto ‘irrefutability is not a virtue but a vice’, will have to be included as an important element of any historical of the social or psychological sciences in this century. It throws light, however, on certain pseudo-scientific practices, rather than on science. And even if it does throw light on science to the extent that we know that it is not like that, how can it tell us about the demarcation of science from metaphysics? Is pseudo-science the same thing as metaphysics? I believe the connection between the difference between science and pseudo-science, which struck Popper in 1919, and the demarcation between science and metaphysics cannot be appreciated until we remember that in the context in which he was working out his ideas, in the late 1920s, in Vienna, ‘metaphysics’ was a term of abuse, frequently associated with pseudo-science and pseudo-statement. It seems that the real importance of the problem of demarcating science and metaphysics is that it allowed a means of attacking the attractive and powerful ideas of the logical positivists. Its value was a critical one. Moreover, what it showed is that a reasonable demarcation does not allow one to distinguish science by the language it uses, because of the way science and metaphysics are linked together, rather than how they are distinguished.

Now, fifty years later, Popper still defends the distinction as he once drew it which appears to me to be both incorrect and not worth making, though it is still valid as a criticism of the position that he was trying to combat in \textit{L.d.F}. I shall therefore take the liberty of reviewing the chapter criticizing logical positivism first, and then return to the task of reviewing the demarcation problem.

Wittgenstein is said to have proposed two programmes in his \textit{Tractatus Logico-Philosophicus} which were later on taken up by Carnap, which were mistaken (1) that non-verifiable sentences are meaningless because they violate the grammatical rules of language (2) that they are meaningless because they employ words or expressions although a meaning has not been given to them’.\textsuperscript{50} I shall leave the question of the correct interpretation of Wittgenstein’s words, and address the issue with the following assumptions—(a) that Carnap so interpreted the programme (b) that he, and other logical positivists tried to carry out the programme so understood.\textsuperscript{51}

\textsuperscript{49} Ibid., pp. 162-63 also for following passages.
\textsuperscript{50} Ibid., p. 195, referring to Wittgenstein’s \textit{Tractatus}, op. cit., 5.473 to 5.4733 and 6.53.
\textsuperscript{51} Popper, \textit{Realism and the Aim of Science}, p. 195, n. 2.
What Popper shows in his critique of Carnap is twofold (a) that two sentences of arithmetic which are identical in their logical syntax but different in only the use of a plus-minus sign in one place, are nevertheless such that one is scientific and the other is metaphysical; (b) that within a language constructed by Carnap, in which many concepts of science would be indefinable, we could define concepts which allowed us to say 'God exists', in the form 'there exists an omnipotent, omnipresent and omniscient personal spirit', which is described as the 'archmetaphysical statement'. Such an argument is not convincing unless one is demarcating science from metaphysics by the falsifiability criterion. The logical positivist who rejects such a proposed demarcation has a ready response: all these statements are scientific and meaningful. If one defines 'there exists an omnipotent, omnipresent, omniscient, personal spirit' in the language that a logical positivist (i.e., Carnap) constructs to eliminate metaphysics, the reply is made that this statement is empirical but false.

Popper claims that the logical positivists of today have not paid attention to what was originally to be ruled out as 'metaphysical', which is precisely what is shown to be allowed in Carnap's language system. Few of Carnap's students are convinced that this is in bad faith. It is clear from reading Carnap, and Popper, that in their polemic each has understood by 'metaphysics' what he has defined it to mean. In order to see this matter in perspective, we must ask what it is that was understood by 'metaphysics' antecedently by both. Why is eliminating it a programme for Carnap? How is it related to science, so that the demarcation between the two is a reasonable aim for a philosopher to follow?

The contrast between the method of philosophy and the method of science goes back to the rise of modern science. In that context it was clear that the empty deductive method of scholastic philosophers led to nothing, whereas the powerful new 'inductive method' of science gave us all the great modern discoveries. There was, on this view, only one real method to understand the world, and that was the scientific method (whatever that is), while the scholastic 'method' was no method at all.

But the rise of science led to a new kind of philosophy also, and this modern philosophy led to its own sceptical conclusions. The new method of science was said to be empirical, and yet the sweeping claims it made regarding the mathematical principles underlying nature could not possibly be contained in, or derived from, our perceptions. The empiricist critique (particularly the arguments of Berkeley and Hume) was a serious challenge to the true new method of science. It is Kant who met the challenge by introducing the conception of two legitimate methods, a scientific method and a separate philosophical method in intellectual endeavour. According to Kant, we could establish the nature of space, time and dynamics by looking from above, or 'transcendently' at how objects must be perceived, and in physics we establish empirical laws by the inductive method—by reviewing cases. It is thus a fundamental tenet of the monism, and of the idealism which Russell and Popper deny, that there are two methods in intellectual matters, one a transcendental philosophical method, and the other a lesser scientific method. We have already noted the fact that a certain Kantian element in Wittgenstein's thought was missed consistently by Russell. In the preface to Wittgenstein's Tractatus Russell described the effort as one related to the logically perfect language, and never quite understood why Wittgenstein vehemently opposed this interpretation. Wittgenstein intended his treatise to investigate the very possibility of a language (any descriptive language), in much the way that Kant investigated the very possibility of science. Truth, in particular, was enigmatic, for explaining which Wittgenstein proposed
The Realism of Popper and Russell

that there is a structural similarity between the facts which constitute the world and the sentences which truly describe it.

Now the important thing to notice is that if Wittgenstein is right, then there are two methods which are legitimate in intellectual endeavour, a method of philosophy and a method of science. The prior study of language, and how it is at all possible is one method, while the study of empirical science follows another method. Whereas for Kant the thing which is in-itself, that is to say which is not perceived or conceived by an intelligent being, cannot be studied by us (it is beyond understanding), so too for Wittgenstein, those aspects of the world which lie beyond language are beyond discussion—they cannot be the topic of sensible speech. If we call what lies outside of language ‘metaphysics in the aspect of ontology’ as Kant might do, then as Wittgenstein understands it, it is meaningless or nonsense. The meaninglessness of metaphysics is thus a corollary of the thesis that there are only two methods available to us (logical analysis and induction).

Unlike Russell, who never fully appreciated this point of view, even though at times he appeared to endorse it, Popper has gone out of his way to point out that he does not accept it. In a famous talk in Cambridge, which Wittgenstein left early, Popper explicitly attacks this doctrine. This paper, published as ‘The Nature of Philosophical Problems and Their Roots in Science’, begins with the forthright thesis that there is no method characteristic of philosophy, and that the interesting problems of philosophy have arisen entirely from the sciences, a position which is quite close to that of Russell.

On this view, there is one world, and (at most) one true method, and there is no feature of the world, be it called ‘metaphysical’, or ‘ontological’ which necessarily lies beyond our ken, or beyond what we can express.

If the logical positivists had adopted Wittgenstein’s views to his satisfaction in their entirety, however, there would have been no programme for constructing a language for science. This task is more due to Russell’s influence, and presupposes a realistic point of view. It seems that Carnap’s point of view falls just into this category, and it is just this view which Popper’s demarcation between science and metaphysics is designed to undermine.

The logical positivists, we must remember, had looked for ‘criteria of meaningfulness’, and proposed various such criteria including the verification criterion, the verifiability criterion, the falsifiability criterion, (under Popper’s influence, appropriately reinterpreted) and so on. In all of these criteria, there is one feature which is most puzzling. If a statement is meaningful, how can its negation, its logical consequence, a statement out of a set of statements from which the statement is derivable, etc. be meaningless? The basic principle of logic that truth-functional (and quantified) combinations of meaningful statements are also meaningful statements undermines the attempts to find criteria of meaningfulness in terms of testability. Popper’s demarcation of science from metaphysics explicitly recognized the corollary that if a statement is ‘metaphysical’ (e.g., ‘There exists a perpetual motion machine’) its negation may be falsifiable and ‘scientific’ (e.g. ‘There exists no perpetual motion machine’). He concludes that empiricism and science must be studied independently of meaning. And we find as a consequence that Popper’s philosophy of science has been little affected by the momentous changes that have rocked theories of meaning and of language in this century.

53 Popper, Conjectures and Refutations, chap. 1 is based on a talk given in Cambridge in the 1940s.
The fallibilists’ demarcation between science and metaphysics shares an important assumption with logical positivism. I shall argue that this assumption is mistaken, and undermines the falsifiability criterion.

The assumption that it shares with many early logical positivists’ views is that corresponding to a statement of natural language there exists one and only one corresponding sentence form of logic. In general, how we translate from English into a canonical language depends on what we take to be predicates of the canonical language, or, in Quine’s manner of looking at it, over what we take our quantified variables to range. If there is a natural translation to a logical calculus, as many positivists have assumed, then we may conclude that statements of English have a canonical form, and also that ‘to be is to be the value of a bound variable’, a form of linguisticism. But logical form is not natural, it is just a matter for choice. In which form we choose to represent a statement depends entirely on which inferences we are interested in studying. A statement like Newton’s First Law of Motion: ‘A body which has no external forces acting on it will either remain in a state of rest or of motion in a uniform right line’, may also be read as ‘Inertial motion is conserved in the absence of forces’, or equally as ‘The Universe is Newtonian’ depending on which predicates we prefer and over which objects our quantifiers will range.

If this is so, then it seems that Popper’s demarcation criterion is untenable simply because it is not unequivocal. We note, however, that on any version of the statements of science, the negation of a meaningful statement is a meaningful statement. Thus the falsifiability criterion can still serve its function as a critique of theories of meaning in terms of testability.

When *L.d.F.* was being written, Russell’s approach to language, according to which the statements of a language have logical structure, was unquestioningly accepted. Given that assumption, it seems that taking falsifiability as a mark of science is perhaps the very best way of understanding it, as Carnap himself has acknowledged. If we do not accept such an assumption, and try to study methodology without paying attention to the logical form of statements being studied, the task of describing how science is done seems to be made more difficult. It is, however, not impossible, and a clue may be found in Popper’s brilliant notion of corroboration, which is dealt with in the last chapter of the first part of the volume under review.

The word ‘corroboration’ is proposed for contrast with the word ‘confirmation’ which has come to be closely allied with non-fallibilistic ideas. Following Keynes and Russell, there has been a great deal of effort spent to find a solution to the problem of induction using the idea of probability. While evidence accumulated to support a hypothesis in science may not be enough to validly imply that hypothesis as a matter of deduction, it has been hoped to show that the hypothesis is made more probable by the evidence.

Popper argues strongly against a solution to the problem of induction within probability theory. If we apply the mathematical theory of probability to statements, in a manner which is explained in *L.d.F.* and the Postscript, it turns out that it is a theorem that the probability of a universal hypothesis is zero or vanishingly close to zero (i.e., almost certainly false), and that any finite number of positive instances (in a world with infinite instances) will improve this so little.

54 ‘Logical form’ here refers to ‘prenex form’. But in *Realism and the Aim of Science*, pp. 198f., Popper actually gives several versions of the Goldbach conjecture, some with and some without an existential quantifier in prenex normal form. See n. 3.

that it will not be anything but zero or vanishingly close to zero after all the evidence is taken into account. Popper therefore discredits any attempt to find a confirmation function based on probability theory.

He proposes instead that a theory only ‘proves its mettle’ by passing severe tests. Thus, the idea of ‘corroboration’ is a measure of the severity of tests, i.e., it is an evaluation of evidence, rather than of theory. A test of a theory is severe if it is expected to go against the theory. If a theory predicts something, and this something is impossible or improbable given the ‘background knowledge’ at the time, then the test is severe. If, however, the difference in the probabilities assigned an event \( (a) \) based on a new theory and \( (b) \) based on background knowledge is the same, then the test is not severe.

To consider an example, let us suppose that a new theory of gravitation is proposed to counter those of Newton and Einstein, and it predicts that the sun will rise tomorrow. This would not impress any neutral person particularly, since, having read much philosophy in the Humean tradition, such a person would have been conditioned to accept “The sun will rise tomorrow” as true in any case. But if, per chance, a new theory of the heavens said that the sun will not rise for a day or two, let us say, in two years time on each and such days, and it came to pass, this would be a very ‘severe test’ that the new hypothesis would have passed, and would surprise the Humean, though perhaps it should not.

Popper’s elegant definition of ‘corroboration’ throws much light on the difference in the way he is suggesting we look at science, as compared with the way Russell himself looked at it, and as most philosophers look at it under Russell’s considerable influence. Russell suggests we start with what is clearly known to us by our senses, and proceed to see how, on the basis of those ‘given’ facts we may be able to infer the hypotheses of science. This leads, according to Popper, to the ‘subjectivism’ that Russell had to accept, in spite of himself, because he never seemed to be able to escape the knowing subject once he started there.

Popper’s alternative strategy is to take the proffered hypothesis as given and (against the ‘background knowledge’ which yields other expectations than the ones suggested by proffered hypotheses) to evaluate evidence accordingly. For Russell, data is vast, and a good hypothesis is an implication of that vast data according to rules which are not quite available to us to state. For Popper, there are no such rules possible. But according to him, we have an array of proffered hypotheses, which are, in some sense of the word, ‘given’ (i.e. they are somehow produced by scientists, by no ‘method’ that can be deciphered) and testing consists in finding those few appropriate empirical results which are unexpected. The vast majority of our experience which Russell thought to be ‘given’, is in fact irrelevant to science.

There is, perhaps, a subjectivity of a different sort in this scheme of evaluation. Who, we might ask, does the expecting when we talk about the expectations based on ‘background knowledge’? Or, alternatively, how is this background knowledge to be regarded? Is it possible that it is sometimes contentious? Is it possible for two different scientific communities to have two different sets of assumptions? Many such questions arise. In his writings on it, Popper is more concerned to show how corroboration differs from confirmation, as the latter is understood by his opponents, than to develop the idea any further.

This idea of corroboration may be generalized very simply by making the ‘severity of tests’ relative to the opposition, rather than to ‘background knowledge’. A proffered hypothesis \( A \) and a contrary one \( B \) may rate a test passed by \( A \) as severe, whereas the same test may be rated as negligible from the point of

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56 Ibid., p. 235 and following.
view of another rival hypothesis C. On this generalization, the evidence sought by scientists is always a 'relativized severe test', or what may be called 'discriminating evidence' because it is not equally probable according to the two hypotheses. On this generalization, Popper's frequent remarks about a 'critical debate' take on a much more concrete realization than the one that the reader might find suggested in his descriptions, which always seem to show each hypothesis facing the facts expressed in 'basic statements' by itself. On this generalized model, it also becomes unnecessary to define 'basic statements' in terms of logical structure, because what is a severe test will determine this without falling back on logic, in a sort of dialectical way, as the word has come to be understood since Hegel. Popper's realism, needless to say, is not thereby compromised.

Finally, we must note that from this perspective 'testable' is relative, which changes with the change of debating contestants, as new hypotheses are produced. This makes Popper's distinction between science and metaphysics untenable because an untestable hypothesis may become testable as soon as a new alternative hypothesis comes along which makes possible a 'severe test' of the hypothesis in question. ('Testability' is not a two-term but a three-term relation.)

Two differences exist between this generalization of Popper's notion of corroboration and Popper's original ideas on scientific method in *L.d.F.* According to the generalized form of corroboration, the test which refutes both of two hypotheses A and B is not a 'severe test' of either hypothesis. (Of course, if there is a third hypothesis proffered (C) which is confirmed by the test, then it, C, has passed a severe test relative either to A or to B. But we assume that sometimes there is no such candidate C.) The idea of a refutation that does not count seems irrational at first, until we recognize that it is quite reasonable to work with hypotheses which have 'open problems'. Indeed, the central criticism of those who have claimed that the history of science refutes Popper has been that since the very best hypotheses in science have at times not been given up for long periods of time even though they were beset by falsifications (by using 'conventionalist strategems' which are decried by Popper as unscientific), Popper must be mistaken. But the generalized form of corroboration can actually describe the conditions under which refutation does not count, and why. A full treatment of these issues, under the rubric of 'problems' is too extensive for the purposes of a review.57

The other difference that a generalization of 'corroboration' makes for Popper's methodology is that it does not leave open a defence for Popper to claim that his own methodology is metaphysical, and therefore that we should neglect any attempt to falsify it by whatever means we have at our disposal. On the contrary, it seems to me that Popper's original description of science was false, but he has given the germ of an ingenious new way of looking at science which promises to be much more successful.

**THE PROPENSITY INTERPRETATION OF PROBABILITY**

The second part of the book under review deals with the most complete statement of Popper's views on the 'propensity' interpretation of the Calculus of Probability. While the central ideas of this interpretation have been published already, this is the original text on which Popper had based his early short pieces. The central aim of this volume in treating of probability is to rid it of any subjective element. In this respect, this part is entirely of a piece with the first. The difference, however, is that whereas the first part of the book deals with

57 J. N. Hattiangadi, 'The Structure of Problems I and II', *Philosophy of the Social*
issues that Russell himself struggled with all his life, the interpretation of the Calculus of Probability is a consequence of realism that Popper has pursued on his own, and with considerable courage when you consider the reputation of the physicists with whom he has taken up debate (Heisenberg, Schrödinger and Einstein in particular).

Quantum mechanics uses probabilities in very important and essential ways when describing the motion of very low energy particles. The use of probabilities in this way leaves a question or two regarding the interpretation of quantum mechanics, and of the probabilities used therein. A statement like ‘The probability that it will rain tomorrow is twenty percent’ may be understood, as it is quite often, as a description of our uncertainty, or of our incomplete knowledge. We might imagine a demon, for example, who could calculate the movements of each particle of the earth’s atmosphere and thus might be able to predict with a hundred percent accuracy whether and when it will rain here tomorrow.

Analogously, we might interpret the essential use made of statistics and probability in quantum mechanics as being simply an inadequate knowledge of underlying hidden variables of slow energy particles (Einstein’s point of view) or that we have in quantum mechanics reached the lower limit of human knowledge (Heisenberg’s view). Popper criticizes both points of view as fundamentally in error, an error arising out of the subjective interpretation of the notions of probability. In *L.d.F.* Popper had already attacked subjectivism in this very prestigious discipline. In *L.SC.D.* he had even reproduced and translated Einstein’s response on this subject. Popper’s basic point of view in *L.d.F.* was that probability may be interpreted as a *frequency*, in which case no subjective element need enter into our understanding of either probability or quantum mechanics. Thus ‘the probability that the die will come up 5 is one-sixth’ is simply a description of the frequency with which ‘5’ appears in a long run of throws. There is nothing subjective about such frequencies. The suggestion that probabilities be used in the sense of ‘frequencies’ in quantum mechanics, and not as ‘estimates of knowledge’ works well in most cases. The difficulty that remains is merely with single instance runs. How can we describe the frequency of 5 coming up in a sequence of throws if there exists only one throw? A brief look at a case in physics will make the difficulty more poignant.

Quantum mechanics describes the world in a somewhat ‘atomistic’ way. Electrons, for example, are treated in many ways like particles, though they are not entirely like Newtonian particles because they are ‘smeared’—i.e. they do not have a sharply defined spatial or temporal boundary. Light, for example, which was understood in terms of wave motion prior to quantum mechanics, is treated therein as ‘photons’, which are quite particle-like. There is a well known test (a ‘severe test’) which wave theory passed (as against earlier particle theories) known as the two-slit experiment, which is of interest. If we hold two safety razor blades tightly together and make a ‘double slit’ in any stiff piece of paper, we will see bands of colours, and of brightness and shadows, if we look through such slits. In a famous theory, Fresnel showed how this was due to the interference of waves of light as they pass through the double slits, with destructive interference of the waves, as they spread, causing dark bands and constructive interference among the waves causing bright bands. Newtonian particles would be expected to go straight through the two slits. From the perspective of quantum mechanics these multiple bands of light and dark must be described in terms of the statistical behaviour of particles when they ‘scatter’ through the slits. A bunch of electrons travelling together interfere with each other and behave ‘statistically’ (they would not each follow strict mechanical laws, as envisaged in Newton’s theory). In this case we can still treat the statistical
description of the particles which go through the two slits to create the familiar band as a frequency in Popper’s sense.

But there is a version of the two-slit experiment which is most astonishing. Suppose we take a screen with two slits appropriately made, and devise an experimental set up in which we send particles through the slits one at a time at, let us say, ten second intervals. Now it is impossible for the electrons to interact with each other. If we close one of the two slits, the electrons passing through do not show the interference pattern, but if you leave the two slits open, then eventually the pattern of the accumulated ‘hits’ of the bunch of particles sent across the two slits will resemble the ‘interference’ model. The very existence of the second slit seems to affect the statistical likelihood of how a particle hits the screen even though each particle can go through only one slit or the other. But each particle is affected in this way all by itself! Obviously, we cannot use the frequency interpretation of the probability calculus to describe how each particle moves!

This is only a poignant case which brings out a difficulty which is quite general in the ‘frequency’ interpretation of probability, which is that it cannot be applied to singular events.

Popper suggests that we interpret probability descriptions as descriptions of an objective propensity. The propensity then actualizes as a frequency when there is a sequence.

To sum up, the propensity interpretation may be presented as retaining the view that probabilities are conjectured or estimated statistical frequencies in long (actual or virtual) sequences. Yet by drawing attention to the fact that these sequences are defined by the manner in which the elements are generated—that is, by the generating conditions—we can show that we are bound to attribute our conditioned probabilities to those generating conditions. . . . This modification of the frequency interpretation leads almost inevitably to the conjecture that probabilities are dispositional properties of these conditions—that is to say, propensities. This allows us to interpret the probability of a singular event as a property of the singular event itself, to be measured by a conjectured potential or virtual statistical frequency rather than by an actual or by an observed frequency. 58

Part two of this volume deals exclusively with the propensity interpretation of probability as such, and leaves to the second and third volumes the further implications of these ideas for quantum mechanics. Although it is beyond the scope of this review, I shall mention a corollary of my earlier remarks which affect Popper’s views in the other two volumes also. In his discussion of quantum mechanics is proposed an interesting idea of a ‘propensity field’, which is then described as being metaphysical, thereby robbing it of empirical value. 59 It is not necessary that it should be so interpreted, and in fact, if I am right that one should abandon the two-termed notion of falsifiability, it is a mistake to treat any theory as ‘metaphysical’. All theories are on a par (though some are considerably less successful in facing or passing severe tests than others). A ‘propensity field’, if it is real, must have some other testable consequences. In the nineteenth century when Faraday proposed that there were lines and fields of force, each line was considered as merely imaginary by others, being the path showing how a test body would move if placed along it. It turned out to be real because it vibrates and then it gives out a signal. Maxwell (theoretically) and Hertz were able to demonstrate light itself is such a wave. What are the signals sent out by propensity fields when they are disturbed? How would one disturb them? How would we detect a disturbance?

58 Popper, Realism and the Aim of Science, pp. 358-59.