In some respects Popper's work is itself postpositivist in character. His criticisms, therefore, were generated within the positivist camp, or at least at its periphery, rather than without.

In Chapter 6, I gave a general account of a broad sweep of early to mid-twentieth-century philosophy of science, and we looked at some of the manifestations of the union of interests in logic and empiricist epistemology which gave rise to logical empiricism and the contributions of the so-called Vienna Circle. A general feature of the work of this school was its program of giving a formal account of how scientific knowledge might be built up logically from observational experiences of phenomena. We have seen that this program ran into considerable difficulties, and what might have appeared, at first blush, a fairly straightforward enterprise turned out to be something of a philosophical nightmare.

It was, I suppose, almost inevitable that there should have been a reaction against the efforts of philosophers such as Carnap, trying to formulate a coherent blend of logic and empiricism. But the particular form that the reaction would take could hardly have been foretold, a priori. And the reaction, when it came in the work of Sir Karl Popper and some of his 'disciples', was in a sense something of a palace revolution. For many of the general features of the logical-empiricist approach to metascience were retained in Popper's work, particularly the strong emphasis on logic, as the means whereby bona fide philosophy of science should be pursued. On the other hand, through his criticism of induction, Popper (hearkening back to Hume) broke decisively with the general program of logical empiricism. In doing so, however, he was, in a sense, only drawing once again upon the age-old tradition of hypothetico-deductionism, elements of which, as we have seen through the course of this book, can be traced right back to antiquity.

In this chapter, then, we shall seek to give an account of some of the broad features of Popper's work; and this will be the only chapter that is devoted to the work of one man. Such an emphasis will no doubt seem idiosyncratic to one nurtured in any philosophical tradition other than that of the Anglo-Saxon — if I may call it that. But there can be little doubt that such an expansive account is warranted by virtue of Popper's very considerable influence within the English-language philosophy of the twentieth century. Whether such emphasis will seem warranted a hundred years hence, is of course impossible to say. But under the present circumstances Popper does seem to deserve a chapter of his own. Also, by examining his philosophy of science and the subsequent
difficulties and controversies in which it has become engaged, we may conveniently trace the pathway from logical empiricism to present-day interests in the social aspects of scientific knowledge and the sociological dimension of epistemology. Popper also merits our attention for the respect accorded to his views by numerous practising scientists, most of whom have found the writings of logical empiricists unintelligible, patently absurd, or totally irrelevant to their interests.

Karl Popper was born in Vienna in 1902, son of a distinguished lawyer, and is still with us at the time of writing. As a young man, he studied at the University of Vienna, later becoming a school teacher, and in 1937 a professional philosopher. In his youth, Popper was attracted to Marxist ideas, to the extent that for a time he chose employment as a manual worker, but he fairly soon renounced his far left-wing views in favour of something akin to Fabian socialism, and then to what one might loosely call small-L liberal views. In his various political writings he has placed great emphasis on the importance of democratic principles. He is strongly opposed to all forms of 'revolutionary' political theory and practice, and urges instead what he calls 'piecemeal social engineering' — that is, the suggestion and implementation of relatively small social changes, followed in each case by consideration of whether the changes have or have not been beneficial, and adjustment of the plans accordingly.

Popper left Austria at the time of the rise of Nazism, and during the Second World War he held a lectureship at Canterbury College, Christchurch, New Zealand, a country where his views have for long been particularly influential. In this wartime interlude in the southern hemisphere, Popper wrote his well-known Open Society and its Enemies (his 'war effort') — a polemic chiefly directed against the philosophical systems of Hegel and Marx, whose thinking he characterised as 'historicism' in that (according to Popper) they purported to be able to make statements about general trends or patterns in history, seeking thereby to influence people's beliefs and behaviour. After the war, Popper made his home in England, where he occupied a chair in the logic and methodology of science at the London School of Economics. It was at the L.S.E. that his work achieved international recognition and renown — to the extent that he was knighted in 1965.

Popper's chief writings have been:

Logik der Forschung (1935) — English translation Logic of Scientific Discovery (1959);
The Open Society and its Enemies (1945);
The Poverty of Historicism (1957);
Conjectures and Refutations (1963);
Objective Knowledge (1972);
The Open Universe (1982);
Quantum Theory and the Schism in Physics (1982);

His first book, written during his early years in Vienna, is still to appear.

Though living in Vienna in his early days, and greatly interested in metascientific issues, Popper was never a direct participant in the deliberations of the Vienna Circle. He was, however, in communication with some of the members of the Circle, and there was interaction between their ideas. As we have seen, the members of the Circle were concerned to establish a satisfactory criterion that would, they hoped, enable them to distinguish between meaningful and meaningless propositions. Thereby, they sought to eliminate metaphysics from philosophical discourse, dismissing it as meaningless. The young Popper was concerned with somewhat similar issues, though they were by no means identical. He was interested in establishing a clear line of demarcation between science and pseudo-science. It appears that this led to some confusion at the time, for the logical positivists understood Popper to be concerned with the problem of meaning — which was not the case. However, this misunderstanding was soon cleared up.

It was in 1919 that Popper attended a lecture by Einstein in Vienna and was dazzled by the introduction to the new physics that was presented by the great iconoclast of modern science. Also, it was in 1919 that Eddington's observational data, which seemed to support Einstein's predictions concerning the gravitational deflection of light, were announced. If light is gravitationally attracted by massive bodies such as the Sun, then the effect may be detected at times of total solar eclipse by examining the patterns of appropriate stellar configurations in the sky, as Figure 45 should make clear.

Figure 45

Suppose two stars normally subtend the angle $\alpha$ at the surface of the Earth. The angle will be modified to $\alpha'$ on occasions when the light has to pass the Sun to reach the Earth — if it is the case that light is subject to slight deviations owing to the gravitational attraction of massive bodies such as the Sun. (If there were no such gravitational pull on light, when the Sun is in the position shown in Figure 45(b) the two stars would be invisible, being obscured by the Sun.) Under normal circumstances, it is hopeless to try to measure $\alpha'$, for the starlight is negligible compared with the light emitted by the Sun. But on the rare occasions of solar
eclipse, e may be measured. This is what Eddington did. And his observations seemed to support the 'risks' predictions made by Einstein, as deductions from the general theory of relativity — namely that light is subject to minute gravitational effects.

Popper compared the very 'risky' and precise predictions made by the new physics with the situation that obtained in three putative sciences: the Marxist theory of history, Freud's psychoanalysis, and Adler's individual psychology. These would-be sciences signally failed to make precisely formulated predictions that could be subjected to direct (or indirect) empirical test. Indeed, they seemed to be compatible with any states-of-affairs that might obtain, or any events that might occur! So in 1919–20 Popper came to the following conclusions:

1. It is easy to obtain confirmations, or verifications, for nearly every theory — if we look for confirmations.
2. Confirmations should count only if they are the result of risky predictions...
3. Every 'good' scientific theory is a prohibition: it forbids certain things to happen. The more a theory forbids, the better it is.
4. A theory which is not refutable by any conceivable event is non-scientific. Irrefutability is not a virtue of a theory, but a vice.
5. Every genuine test of a theory is an attempt to falsify it, or to refute it...
6. Confirming evidence should not count except when it is the result of a genuine test of the theory; and this means that it can be presented as a serious but unsuccessful attempt to falsify the theory.
7. Some genuinely testable theories, when found to be false, are still upheld by their admirers — for example by introducing ad hoc some auxiliary assumption, or by re-interpreting the theory ad hoc in such a way that it escapes refutation. Such a procedure is always possible, but it rescues the theory from refutation only at the price of destroying, or at least lowering, its scientific status. 11

So in summary, said Popper:

'The criterion of the scientific status of a theory is its falsifiability, or refutability, or testability — not its confirmability.' 11

Thus, early in his life, Popper came to establish his famous criterion of demarcation which (so he supposed) could be used to distinguish science from pseudo-science. But, as indicated above, contrary to the logical positivists, Popper was concerned with the falsifiability of theories as a criterion of their scientificity. He was not seeking to deal with the actual meaning or meaningfulness of propositions.

In 1923, Popper tells us, 14 he became interested in the problem of induction, which like so many others before he approached through the writings of Hume. Popper accepted Hume's refutation of inductive inferences as being 'clear and conclusive'. But he was dissatisfied with the 'psychological' explanation that Hume offered to account for our claimed propensity to make and accept inductive inferences. 15 Popper contended that the philosophy of science of his day (for example latter-day Millian empiricism, or the products of the logical positivist/empiricist movement) was based upon the supposed legitimacy of inductive inferences. So he concluded that philosophy of science within the empiricist tradition had reached an impasse. The only way to put matters right was to turn the whole system upside down, so to speak, and place the main emphasis on scientists' efforts to test their theories — to show

in what respects they were wrong — rather than to show in what manner they were verified, confirmed, supported or whatever, by the empirical evidence. So according to Popper's new way of thinking, metascience should place emphasis on the procedures of falsification, rather than verification — the showing of a theory to be false rather than correct. 'I thought', he wrote:

that scientific theories were not the digest of observations, but that they were inventions — conjectures boldly put forward for trial, to be eliminated if they clashed with observations, with observations which were rarely accidental but as a rule undertaken with the definite intention of testing a theory by obtaining, if possible, a decisive refutation. 16

Thus did Popper come to bis version of the hypothetico-deductive methodology of science — the method, as he subsequently called it, of 'trial and error', or of 'conjecture and refutation'. Moreover, he claimed that by adopting this approach to scientific investigation the problem of induction could be circumvented. In fact, in 1972 he claimed directly that he had 'solved a major philosophical problem: the problem of induction. 17 Unfortunately, however, it is not the case that the adoption of a particular methodology in scientific inquiry 'solves' the problem of induction. To adopt a particular method does not show inductive arguments can be rendered deductive, or otherwise certain. At best, to invoke a new methodology might show how science can get along without inductive inference — although as we shall shortly see, falsificationism doesn't even achieve that satisfactorily. Popper's own approach was, in effect, to turn away from the problem rather than solve it.

Putting the matter in what is, admittedly, a somewhat oversimplified way, the Popperian methodology can be construed as a straightforward instance of hypothetico-deductive thinking. Thinking back to the 'arch of knowledge' once more, it is illustrated yet again by Popper's description of science. One starts with a problem, and conjectures are made — in the light of known information and data that may be collected relevant to the problem — as to ways in which the problem may be solved. This involves formulating (or conjecturing) an hypothesis (or theory) that may account satisfactorily, or deal with, the problem or difficulty. The hypothesis is then rigorously tested. This is done by drawing the logical consequences from the hypothesis and testing these consequences experimentally. Thereby, the hypothesis may be shown to be false (using the logic of the modus tollens); 18 but it cannot be shown to be true by reason of the fallacy of affirming the consequent. 19 The data collected in the course of the experimental testing of the hypothesis may in turn lead to new problems, and so science may continue on its merry way, with an unlimited succession of 'conjectures and refutations'. In terms of our arch model, then, Popper's attention is directed almost exclusively towards the 'downward,' 'deductive' limb. Indeed, he has claimed that the philosopher of science can say little that is of interest about the mysterious processes of hypothesis formation. Such creative acts, if they are to be examined at all, lie in the domain of the psychologist rather than
be moving with respect to the aether. For it could have been in such a position in its orbit that the motion of the Earth round the Sun and the motion of the Solar System as a whole exactly cancelled out. However, the experiment has been repeated at other times of the year, and always with a null result. The possibility that the Earth was not moving round the Sun at all was not, so far as I am aware, even given consideration. The optical theory seemed satisfactory. There was, at the time, no alternative on offer to the assumption of motion with respect to the aether (the physicists of the late nineteenth century mostly still living in a Newtonian world of absolute space and time, so far as their theories were concerned).

Faced with such a situation, the physicists were in a considerable quandary, and the logic of the *modus tollens* was of scant assistance. They could not be sure whether their experiments were conducted adequately or inadequately, whether the optical theory was mistaken, or whether the Earth was not in fact moving with respect to the aether. None of these alternatives seemed very attractive, and as we have seen in Chapter 7, what happened was that the so-called Lorentz–Fitzgerald contraction hypothesis was proposed, whereby the physical apparatus itself was supposed to contract when in motion with respect to the aether! Further, it was suggested that it contracted by just such an amount as would account for the null result of the experiment. The thought of shattering the whole edifice of Newtonian physics, without being able to offer anything else to put in its place, was just too awful to contemplate, thought it must be admitted that the 'uncaused' contraction of physical objects was probably as unpalatable as the thought of there possibly being no absolute frame of physical reference.

Of course, no one was particularly happy with the Lorentz–Fitzgerald hypothesis, and from the perspective of Popper's metascience it might seem a thoroughly bad thing. It was, in Popper's terminology, a typical *ad hoc* hypothesis, dragged in to shore up a theory that was getting into difficulties. As we have seen, Einstein subsequently restructured the most basic assumptions of physical science when he introduced his relativity principles. And then the *ad hoc* Lorentz–Fitzgerald contraction became unnecessary, or rather it was subsumed by the new relativistic mechanics. But at the time it was proposed, the contraction hypothesis was the best that could be offered, even though the authors could suggest no theoretical reason as to why such a contraction should occur.

Now, according to Popper's description of science, the introduction of such an *ad hoc* hypothesis was most unsatisfactory. It was reprehensible, for though the hypothesis might deal with the difficulty in hand it did not offer any wider 'empirical content' for the theory. The theory of absolute motion was not 'allowed' to be falsified. So, in Popper's language, with the introduction of the Lorentz–Fitzgerald hypothesis, the doctrine of absolute motion was being *conventionalised*. A 'conventionalist strategem' was being deployed, which would be a 'bad thing' according to the Popperian description of science. Scientists ought to expose their theories to the most rigorous test, and accept falsification if that is what the experimental results dictated.

However, as we have seen, the message dictated by the data may often

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**Chapter Eight**

the philosopher of science. All this is something of a caricature of the full Popperian position, in all its sophistication and complexity. Nevertheless, Popper's system can undoubtedly be accommodated to the traditional arch model. But as we have indicated, his emphasis is asymmetrical, yielding another of our ungainly and distorted structures, in this time with a strong deductive leg and a very insubstantial inductive side. We shall (later on) want to know whether Popper does in fact have some play with inductive reasoning. But for the moment we shall examine some other issues. In particular, we should consider whether the program of falsification allows a clear-cut, logical certainty to the Popperian scientist from which the inductivist is debarred.

We have already noticed, in our discussion of the work of Duhem, that the logic of the *modus tollens*, though valid in itself, does not allow *isolated* hypotheses to be tested. One always has to test hypotheses in clusters, so to speak. The situation is always such that (even at best) one is testing a system that consists of some hypothesis under investigation, together with a greater or lesser number of auxiliary hypotheses — that is to say, hypotheses drawn from other branches of science, or related branches of the science into which the investigation is being made. Thus, the situation (as we have previously noted) should be represented thus:

\[ H \text{(hypothesis under test)} + A \text{(auxiliary assumptions or hypotheses) \downarrow \text{Deduction}} \]

*Implications — which are to be tested empirically*

So, in the event of the tests proving unsatisfactory, they do not point unambiguously to the falsity of \( H \). It is the combination of \( H + A \) that is shown to be wrong. And the trouble may lie in \( A \), just as much as \( H \), so far as the logic of *modus tollens* can show.

For our present purposes, it will be helpful to illustrate this important point further by means of an historical example: In the Michelson–Morley experiment no shifts of the interference fringes were observed when the apparatus was rotated through 90 degrees. The hypothesis being tested was that the Earth was moving through 'the aether', which served as an absolute frame of reference; and an attempt was being made to determine the Earth's absolute rate of motion. The auxiliary hypothesis was that the Earth was actually in motion round the Sun, as the Copernican theory stated. And there were, besides, various assumptions drawn from optical theory needed in order to carry out the test. The experimental results provided no evidence of any motion of the Earth relative to an aether, contrary to what was expected. But from this alone, it was not possible, by application of the logic of *modus tollens*, to tell whether the difficulty lay in the optical theory, the assumption that the Earth was in motion with respect to the aether, or whether the Earth was not in fact in motion round the Sun, as Copernican theory required.

The first possibility considered was that the Earth might, by chance, *not*
be ambiguous. We do not know exactly where the arrow of the *modus tollens* should be directed. Hence, it is not surprising that scientists do not always accept the verdict of the data, and resort to conventionalist stratagems, or suspend judgment, awaiting new theoretical and experimental developments. And given the obvious difficulty in applying falsificationist directives in practice, it is scarcely surprising that for many years (say, until the 1960s) scientists tended to react adversely to Popper's urgings that they should engage in attempts at falsification rather than verification — though more recently some well-known scientists such as Sir Peter Medawar and Sir John Eccles have publicly tied their colours to the Popperian masthead.

One may suppose that the problem of auxiliary hypotheses — which is at the centre of Duhem's thesis — is such that a falsificationist approach to philosophy of science offers little advantage over an inductivist or verificationist position. The thesis indicates that one cannot hope for clear-cut falsification of any given hypothesis. Moreover, the empirical evidence against an hypothesis, or a system of hypotheses, will have to be built up by successive observations, and this process is itself subject to the problem of induction. For a single observation against an hypothesis would not in itself suffice and one would need to build up a strong empirical case, for a falsification, just as much as for a verification.15

Are we to suppose, then, that Popper was unaware of such problems, even in his earliest work? He may have been, in the 1920s, when he was first developing his philosophical position in some detail. But he did make considerable efforts to deal with some of the difficulties sketched above, even in the early *Logic of Scientific Discovery*, and in looking at some parts of this work we may usefully be carried somewhat more deeply into the Popperian system.

Popper, as we have seen, has been and is concerned that science should make highly falsifiable statements — that is, precise predictions that can be tested experimentally. In this way, *bona fide* science is supposedly distinguished from pseudo-science. The making of precise, highly falsifiable statements is, in fact, for Popper more important than the acquisition of the 'truth'. It may be true to say that it will rain some time next year. But this is not a particularly useful piece of information. To say, however, that it will rain this afternoon conveys much more. Such a statement has, in Popper's language, a greater 'empirical content'. On the other hand, it is much less probable than the imprecise statement that it will rain some time next year. And according to Popper, 'empirical content' in science is more important than truth. So science, on this view, should be concerned with statements of high content and low probability, which, however, by repeated testing may gradually be revised and brought closer to the truth.

Here an analogy with gunnery target practice may be helpful. By continual repetition of shots, and adjustment of the aim after each firing, the aim of the gun may be altered so that eventually it exactly hits the bull's eye. The analogy is imperfect, however. Science, for Popper, certainly involves repeated conjectures and refutations — which may be compared with the directions of aim and the corresponding tests of those aims by examination of the marks on the target. But according to Popper, although such a method of conjecture and refutation will bring the scientist nearer to the truth, it will not bring him or her there exactly. Or if it does, the scientist has no means of knowing that he or she has arrived at the truth. There is, so to speak, no bull's eye in science — no spatial (or conceptual) region can be known to correspond with the 'truth' per se.

So Popper has maintained that the higher the 'empirical content' of a statement the greater is its degree of falsifiability. The more it rules out (that is, the more precise it is) the more it says in a useful scientific way; the more a statement rules out, the more is it saying about the world of experience. Thus, one can supposedly compare the 'empirical content' of two theories by comparing their falsifiabilities, the 'empirical content' being defined by Popper as the class of statements which constitute its potential falsifiers. (Popper's 'empirical content', by the way, should be distinguished carefully from Carnap's 'logical content'. For Carnap, the 'logical content' of a statement was equal to the class of non-tautological statements that could be derived from it.)

But how are the 'falsifiabilities' of two theories to be compared? One cannot merely compare the amounts of empirical data that they entail, since they will inevitably be infinite (in a universe infinite in space and time) in both cases. Popper's procedure, therefore, has been to seek to compare the sizes of the classes of basic empirical statements which might clash with rival theories, rather than in some manner seeking actually to count the number of potentially falsifying statements that may be possessed by a particular theory or hypothesis. Thus, if the class of potential falsifiers of theory T1 includes the class of potential falsifiers of T2 and is thereby larger than T2, then we may say that T1 is more falsifiable than T2 and has a higher 'empirical content'. To quote Popper's own illustration, the theory that planetary orbits are circular has more potential falsifiers than does the theory that they are elliptical — for the class of circles is a subclass of the class of ellipses. Consequently, if we suppose that the orbits of planets are circles we rule out more observational possibilities than if we propose the hypothesis that they are ellipses. There are more potentially falsifying observations for circles than there are for ellipses. That is, the class of potentially falsifying statements for Tcircle is greater than the class of potentially falsifying statements for Tellipse. So the 'empirical content' of Tellipse is greater than the 'empirical content' of Tellipse. The example is no doubt rather contrived; but it should display Popper's position well enough in a general way.

Where such subclass relationships of circle and ellipse cannot be used, Popper has suggested that the 'dimensionality' of two theories may be compared. If we compare the number of observational statements required in principle to eliminate (or test) two hypotheses, the one that requires less may be said to have lower dimension, and thereby it is regarded as more falsifiable or testable, or (as before) of greater 'empirical content'. But, as mentioned before, the higher the 'empirical content' of a theory, the higher its falsifiability.

It is important to note that it is within his doctrine of 'empirical content' that Popper sought to provide his answer, such as it is, to the challenge posed by the Duhem thesis. As we have noted, the arrow of the
modus tollens cannot be directed unambiguously to a single, isolated hypothesis. And, according to Quine any hypothesis can be 'saved' by the introduction of suitable ad hoc hypotheses (such as the Lorentz–Fitzgerald contraction hypothesis). Popper urged, therefore, that the new auxiliary assumptions brought into a theory ought to be such that they increase the theory's observational falsifiability. If they do not, they are of the reprehensibly ad hoc variety, and should be eschewed. Of course, in saying this, Popper moved further from the realm of strict logical analysis to one of moral exhortation. In keeping with this, we find that his system contains a considerable number of methodological rules (which we examine collectively below), which rather detract from its would-be logical purity.

It should also be recognised that Popper's system did not envisage scientists as being engaged solely in the formulation of hypotheses and their testing to destruction. For Popper, a theory is falsified by a combination of discordant facts and an alternative theory which explains those facts satisfactorily. So his falsificationist system is closely tied to the experimental comparison and critical discussion of competing theories. For such reasons, Popper does not regard the views of Duham and Quine as insurmountable objections to his system.

Another important term within the Popperian vocabulary is 'corroboration'. As we have seen, throughout his work Popper has placed emphasis on falsification rather than verification. He urges scientists to examine the weaknesses of their theories rather than their strengths. But it seems that experimentation does, in some manner, lead scientists towards the acceptance of theories. So Popper introduced the term 'corroboration', meaning 'failure to falsify'. A theory that has been well tested is said to be well corroborated, in that all attempts to prove it wrong have so far been unsuccessful. Also, a theory of high corroborability is one that is highly testable and of high content, but of low probability.

Popper acknowledged in Logic of Scientific Discovery that he could not quantify the notion of 'corroboration' precisely. Nevertheless, some formulae that were supposed to give a measure of the conception were proposed. These were:

1. 'Explanatory power' of theory \( x \) with respect to evidence \( y \):
   \[
   \text{Probability of } y \text{ with respect to } x = \frac{\text{Probability of } y \text{ with respect to } x}{1 + \text{Probability of } y}.
   \]

2. 'Degree of confirmation [or corroboration]' of theory \( x \) with respect to evidence \( y \):
   \[
   \text{Degree of confirmation of } x \text{ with respect to } y = \frac{(\text{Explanatory power of } x \text{ with respect to } y) \times \text{Probability of } x \text{ with respect to } y}{1 + \text{Probability of } x \text{ with respect to } y}.
   \]

Working with these equations, we may now proceed as follows. If the theory \( x \) is a universal generalisation, according to Popper its probability would be zero. Also, if the evidence \( y \) is logically deducible from the theory \( x \), then 'Probability of \( y \) with respect to \( x \)' will be 1. So, considering the test implications of a claimed universally valid theory, the equations reduce to:

Qualitatively, the significance of this formula within the Popperian system seems to be as follows. A highly probable theory is one of low corroborability. The repeated testing of a theory does not raise its corroborability. For high corroborability one requires low probability. That is, the theory must make 'risky' predictions that can readily be falsified — something like Einstein's relativity theory, as opposed to Freud's psychoanalysis. However, I suggest that Popper's equations here were really 'invented', so to speak, to give the appearance of quantitative expression to his qualitative appraisal of the relative merits of different theories. In any case, they don't really do the work that is required of them. If a theory becomes well supported by the accumulation of empirical evidence, presumably the probability of this evidence rises. But by Equation 3 above, this pushes down the corroboration of the theory. And the equation tells us that about the corroboration of the theory, rather than its corroborability. So we don't know, from some experimental results and the use of Equation 3, whether the theory's corroborability is improving or deteriorating as a result of the experimentation or what its standing is.

It is further worth noting that Popper has stated that the efforts to refute the theory must be made severely and genuinely. Sincere attempts must be made to refute theories, rather than verify them, if the notion of corroborability is to be sound. This introduces an additional element of moral exhortation into the discussion, which can scarcely be captured by formal equations such as those given above. It is hardly surprising, therefore, that the theory of corroborability has been deemed unsatisfactory. It does not reconcile the kind of theory with which one is dealing (for example, 'risky', 'bold', 'timorous', 'cautious' or whatever — features that Popper seems to think highly relevant to the question) and the actual status of that theory at any given epoch, according to whether or not it has been well supported by experimental investigation.

It will be recalled that the logical positivists sought to provide a construction of physical theory grounded on observation of 'things'; or, in Carnap's Aufbau, an attempt was made to build up physical theory starting at the very lowest level with sensations and the relations between sensations. Popper's writings don't display quite so much interest in the 'thing level' as do the writings of Carnap and his colleagues, but Popper (despite his repudiation of inductivism) is an empiricist, just as were the original members of the Vienna Circle, against whose work he reacted so strongly.

In The Logic of Scientific Discovery, Popper dubbed the view that all statements must ultimately be justified by perceptual experience with the label 'psychologism'. He maintained that it was unsatisfactory, for it founndered on the rocks of induction and universals. Universals, he rightly maintained, could not be logically 'constituted' in the way that Carnap had attempted in his Aufbau from 'sense data' alone. Nevertheless, Popper's
falsificationism required the use of empirical elements in order that the test-implications of theories might be checked out. So he introduced the notion of ‘basic statements’. Their role was somewhat similar to the ‘protocol sentences’ of Neurath and Carnap.

‘Basic statements’, we read in The Logic of Scientific Discovery, ‘have the form of singular existential statements’.39 (For example: ‘There is an apple on the plate’; or ‘The pointer points to the reading 0.75 amp on the ammeter’.) Such statements must be testable, inter-subjectively, by observation. That is, different people must be able to check on the statements by observation and agree with one another as to what is observed. A single claimed sighting of Nessy in Loch Ness, which others could not confirm, would not count as a basic statement. So the notion of ‘observable event’ becomes, as it were, a primitive concept in Popper’s system: ‘Basic statements are ... statements asserting that an observable event is occurring in a certain individual region of space and time’.40 But there are, of course, an infinite number of events occurring all the time. It becomes a matter of decision, therefore, as to which particular event is selected for the purpose of making a corroboration or a falsification. This makes Popper’s ‘basic statements’ substantially different from the ‘protocol sentences’ of Neurath and Carnap. For the Popperian, it is not a question of opening up one’s senses, which may then serve as the first rungs for an ascent of the inductive ladder. Rather, Popper’s basic statements are accepted or rejected in the light of the application of a particular theory or group of theories. So certain methodological rules have to be provided for this purpose. The process of acceptance or rejection of a basic statement may, says Popper, be compared to the process whereby members of a jury formulate their verdict on a case.

We see, then, that in the discussion of basic statements, in the discussion of auxiliary hypotheses and of corroboration, and elsewhere in Popper’s writings, there is need for a set of methodological rules for the conduct of science. For despite Popper’s liking for strict logical thinking and the empirical aspects of science, he recognises that there must be more to science than this. A normative component is also present. And this he has been active in attempting to supply over the years.

Popper’s various methodological rules are to be found scattered through The Logic of Scientific Discovery, but they have been conveniently gathered together for us by Johansson, who gives them as follows:

1. Demarcation rule:
A theory is scientific if and only if it is falsifiable, i.e. if it is inconsistent [or incompatible] with at least one singular existential statement that can be tested by intersubjective observation.

2. Rules against conventionalist strategies:
(i) [Adopt a rule not to use undefined concepts as if they were defined [by the theory]]
(ii) [Only those [auxiliary hypotheses] are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question, but, on the contrary, increases it]
(iii) Surrogates and manipulations of usage are forbidden.

(iv) Inter-subjectively testable... [theories] are either to be accepted, or to be rejected in the light of counter-experiments.

(v) [Auxiliary hypotheses should be used as sparingly as possible.

3. Rules demanding a high degree of falsifiability:
(i) [Rule a natural law as synthetic and strictly universal statements.
(ii) [Try to avoid these hypotheses should be given preference which can be most severely tested.
(iii) [The number of... axioms — of... most fundamental hypotheses — should be kept down.

(iv) [Any new system of hypotheses should yield, or explain, the old, corroborated, regularities.

(v) [A new theory should proceed from some simple, new, and powerful, unifying idea about some connection or relation...between hitherto unconnected things... or facts... or new 'theoretical entities'.

(vi) [A new theory should be independently testable.

4. Acceptance rules for basic statements:

(i) [A theory is taken to be falsified only if we discover a reproducible effect which refutes the theory. In other words, we only accept the falsification if a low-level empirical hypothesis which describes such an effect is proposed and corroborated.

(ii) [One should not accept strange basic statements — i.e. logically disconnected ones — but... one should accept basic statements in the course of testing theories; of raising searching questions about these theories, to be answered by the acceptance of basic statements.

5. Acceptance rules for theories:
(i) [A theory is to be considered a positive degree of corroboration if it is compatible with the accepted basic statements and if, in addition, a non-empty sub-class of these basic statements is... accepted as the results of sincere attempts to refute the theory.

(ii) [It is not so much the number of corroborating instances which determines the degree of corroboration as the severity of the various tests to which the hypothesis in question can be, and has been, subjected.

(iii) [One must not] accord a positive degree of corroboration to a theory which has been falsified by an inter-subjectively testable experiment based upon a falsifying hypothesis.

(iv) [The] theory should pass some new, and severe, tests.

6. Rule for falsifying probability statements:

(i) [Regard highly improbable events as ruled out... or prohibited.

7. Rules for the social sciences:

(i) [All theoretical or generalizing sciences should make use of the same method, whether they are natural sciences or social sciences.

(ii) [The task of science is... to describe how things behave... This is to be done by freely introducing new terms whenever necessary, or by re-defining old terms wherever convenient while cheerfully neglecting their original meaning [sic].

For... words are merely... useful instruments of description.

(iii) [A preconceived selective point of view should be introduced into one's history... Or write that history which interests you...]

(iv) [The task of social theory is to construct and analyse... sociological models carefully in descriptive or nominalist terms, that is to say, in terms of individuals...]

All this demonstrates that Popper's metascience involves more than logic. However, when I was a brash young student, I had the good fortune to attend a series of lectures given by Popper at University College, London, and in the discussion period after one of the lectures I asked Popper whether he considered the description of science he was giving was provisional, subject to correction and test, falsification, and so on. The answer I received was that 'There is nothing provisional, conjectural or falsifiable about logic'. No doubt, Popper was thinking that the force of the argument of modus tollens was such that falsificationism was the
correct metadescription of science, and that inductivism was wholly unsatisfactory, for the kinds of reasons that Hume had given in the eighteenth century. However, I take it that he did not think of the problems raised by Duhem and Quine on that occasion. And as can be seen from the foregoing list, when Popper's methodological rules are brought out into the open, what may at first sight have seemed to be a carefully wrought, logical system turns out to be rather fuzzy, and open to a number of different possible interpretations.

On the other hand, it must be acknowledged that Popper has never made the slightest attempt to conceal the 'conventional' character of his 'methodological rules', and he regards them as subject to change — to be criticised and improved if possible. Even so, the very presence of these 'methodological rules' within the Popperian system is an indication that it offers much more than a logical analysis of science. Popper is telling scientists how he thinks they ought to conduct their inquiries. To be fair to Popper, however, he would maintain that he can see from the history of science that the kind of rules that he advocates have, in fact, been effective for the prosecution of science. So he would want to argue that the rules that have been successful in sustaining the scientific enterprise are the ones that ought to be pursued. (This, I suppose, gives Popper's position a pragmatist flavour.) There is, it would seem, an element of paradox here, for one who claims to be deeply concerned about 'Hume's problem'; but presumably it would be answered that methodological rules can be followed without contravening any logical principles. Anyway, it is clear that the Popperian meta-level has a distinct normative aspect; and obviously there can be differences of opinion on matters of value — on questions of 'ought'. So Popper's enterprise cannot be represented as one that is wholly dispassionate and neutral, even if it is claimed that it is grounded in logic, about which (one may suppose) there is nothing that is 'provisional, conjectural or falsifiable'.

To illustrate some of the difficulties associated with Popper's methodological rules, consider for example the ones associated with 'basic statements'. We note that there is nothing said about the question of observability. So we have little guidance as to what happens (or should happen) when a theory is refuted (by a basic statement), for we are forced to rely on some intuitive notion of observability. Again, Rule 3(v) enjoins us to favour simple, new, powerful and unifying ideas; but Rule 3(ii) asks us to give preference to those theories that may be most severely tested. Now it is rather difficult to test Einstein's theory of general relativity, and it is certainly anything but simple. On the other hand, it is (relatively speaking) new and powerful and brings about a remarkable consistence or unification of ideas. So Popper's rules would not have given a clear indication as to what to do when considering whether or not to subscribe to the theory of general relativity. All this might not, perhaps, be of great importance were we not hoping for science to be a strictly rational and logical enterprise, based upon empirical sources of information. But Popper undoubtedly holds that science does have this character. Why else would he be so concerned about the problem of induction?

In considering Popper's work, it is most important to keep at the front of one's mind that he regards himself as a realist, both in his ontology and in his epistemology. He believes that there is a real, external world (he is no phenomenalist), and that by the formulation and rigorous testing of hypotheses, science enables human beings gradually to acquire an increasingly accurate knowledge of this real world. For this reason, Popper has been a vigorous critic of instrumentalism throughout his career. But as we have seen above, he does not claim that the never-ending formulation and testing of hypotheses will generate truth or certainty. One may shoot closer to the centre of the target with the arrow of the modus tollens, but one can never know when one has hit it. So Popper does not seek to mount his realism directly on a doctrine of truth — perhaps some correspondence theory of truth or a coherence theory. Rather, he has devised a new conception, named 'verisimilitude', which is intended as a measure of the 'truthlikeness' of hypotheses or theories. If we have an empirical statement, then the class of true statements that it entails is called its 'truth content', and the set of false statements it entails is called its 'falsity content'. The 'verisimilitude' is defined as 'truth content' minus 'falsity content'. The verisimilarities of two theories which offer competing explanations of the same phenomena may thus supposedly be compared if one of them explains or accounts for that which the other explains and also some additional phenomena; and the first theory has stood up to tests in this extra domain where the second theory is unsuccessful, or which it does not cover.

Unfortunately, however, it has been shown by Tichý that the verisimilarities of two theories cannot be satisfactorily compared if one of them is false, and this has led Popper to attempt a modified definition of verisimilitude. We shall not seek to trace the details of these contemporary controversies for the matter is still under active debate. I believe, however, that at the time of writing no generally acceptable doctrine of verisimilitude has been found, so that there is a distinct lacuna in the Popperian system. Yet this is scarcely surprising. If one had a satisfactory doctrine of verisimilitude, one would in fact have a kind of 'hook' (as I have previously called it) whereby theories about the world might be connected with the world. Or at least one would have a kind of vantage point from which the success or otherwise of the attempted hooking-together of ideas and states-of-affairs in the world might be appraised. Besides, if one is really worried to death about the problem of induction, one must also, I think, be dispossessed with any theory that purports to give measures of the truthlikeness of scientific statements. For such a theory can always be subjected to the kinds of criticisms that were directed against induction by Hume, since 'truth content' and 'falsity content' cannot be determined a priori, only by empirical investigation.

There are numerous other interesting aspects to Popper's work, but we only have space to touch on them briefly here. Popper has for many years mounted a strong campaign against the doctrines of essentialism and instrumentalism. It is interesting to note that his nominalism has led him to discount the twentieth-century philosophers' customary interest in the analysis of language, which was a major concern of some of the logical
posdintists. To look for exact definitions and formulations of meanings of terms is, in Popper's view, philosophically fruitless. Rather, one should be striving for theories that approach the truth (that is, theories that are of increasing verisimilitude). The distinction between Popper's views and those of analytical philosophers and logical positivists is easily seen from Figure 46, a table that was published in Popper's autobiography.4

**Figure 46**

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<td>UNDEFINED CONCEPTS</td>
<td>PRIMITIVE PROPOSITIONS</td>
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*The attempt to establish (rather than reduce) by these means their*

MEANING                        TRUTH

leads to an infinite regress

According to Popper, the left-hand side of the table is quite trivial; but the right-hand side is philosophically all-important. It will be seen that the... by analogy of language. Rather, it is the... inquiry, conjecture and (attempted) refutation, and critical discussion that provide an endless road of progressive inquiry.

Among other things, Popper has been particularly influenced by the Darwinian theory of evolution, as is specially evident in *Objective Knowledge*, subtitled *An Evolutionary Approach* (1963). But even in his earliest formulations of his doctrine, Popper believed that each person is equipped with certain inborn expectations, and gradually builds up a more truthful picture of the world by trial and error — by conjecture and refutation. There is, then, the notion of a kind of 'survival of the fittest' among ideas, and Popper's doctrine is made to mesh quite well with that of Darwinian evolutionism:

> All organisms are constantly, day and night, engaged in problem-solving; and so are all those evolutionary sequences of organisms — the phyla which begin with the most primitive forms and of which the new living organisms are the latest members.48

We have already encountered various uses of the analogy between the evolution of ideas and the evolution of living organisms in nineteenth-century philosophers of science such as Peirce and Mach. The analogy has continued to be used in the present century by various philosophers, perhaps in the most unguarded (or flagrant) way by Stephen Toulmin.49 Popper is only one of many who find the biological analogy useful in epistemology. But it is, after all, only an analogy, with many 'negative' aspects as well as 'positive'. Perhaps the most important 'negative' feature of the analogy is that ideas are put forward consciously, and with particular ends or purposes in view — to solve problems, as Popper would say. But the variants upon which natural selection acts among living organism are, according to the Darwinian theory, produced randomly, with no particular end or purpose in view.

Be this as it may, Popper sees the process of problem-solving — which he thinks has characterised the evolutionary history of the human race — as being involved with language (critical discussion) and also with ethical systems, law, religion, science, philosophy and various social institutions. These together form vitally important aspects of the environment in which man evolves, over and above the physical environment provided by the Earth. And such systems form part of what Popper calls 'World 3'. Let us look briefly what he means by this.

For Popper, there are three so-called 'worlds'. 'World 1' consists of material things such as sticks and stones, bodies, brains. 'World 2' is made up of all the various thoughts in people's minds. It is the world of mental phenomena, but not the world of brain cells which (according to the traditional dualist view of the mind-body problem) merely serve as a kind of 'substrate' for thoughts, which are non-material entities. 'World 3', according to Popper, consists of 'objective' structures that are the products of minds or living beings. It includes such things as information in libraries, works of art, language, music, scientific theories, and so on. It constitutes the cultural heritage that is preserved in 'World 1' objects such as books, gramophone records or brain cells. The contents of this third world are, on Popper's view, man-made, yet autonomous. They constitute what he calls 'objective knowledge'.50 It is through this 'objective knowledge' that it is possible for ideas to have histories. Ideas, therefore, can evolve without needing some 'historicism' (in Popper's sense of the word)51 explanation to account for their evolution. It is, says Popper,52 the process of criticism that provides the motor for the evolutionary development of 'World 3' objects.

The whole idea of a 'World 3' — a domain of 'objective knowledge' —
has proved unattractive to many modern critics, and it is clear that the doctrine has a decidedly Platonist aspect. However, whether or not we like his metaphysics at this point, we should take note of the way in which Popper has used the notion of 'objective knowledge' to mount an attack on orthodox theories of knowledge. For example, even an author who has just completed a book does not know (in a 'World 2' sense) every item of information that is contained in that book. Nevertheless, the knowledge is there (encoded in 'World 1' form), objectified in that book, and other persons may gain access to it if they so wish. As a result of such arguments, Popper regards traditional theories of knowledge such as those of Descartes, Locke or Kant as misconceived, for they quite overlook what he regards as a vitally important aspect of the way in which human knowledge is gained and deployed. Popper, then, seeks to rectify such 'mistaken' views, taking due regard of the 'World 3' features of human culture. His ideas on epistemology and the mind-body problem are further developed in his The Self and its Brain which adopts a very strong dualist approach to the relationship between mind and body.

The influence of Karl Popper on twentieth-century philosophy of science has been very considerable. Certainly, in his emphasis on the need for critical examination of ideas, he has had a beneficial influence amongst the scientific community and the plea for scientists to seek to falsify their ideas, rather than verify them, has not gone unnoticed. Also, the suggested line of demarcation between science and pseudo-science has had a salutary effect, even though, as we shall see in a moment, it no longer seems as satisfactory as once was the case. In his emphasis on critical discussion and by drawing attention to the fact (in stating his various methodological rules) that there is a definite normative dimension to science, Popper has been — perhaps unwittingly — preparing the ground for the present-day interest in the sociology of science, with epistemologies based on sociological principles, about which we shall say something in our next chapter.

On the other hand, I think it has to be said that falsificationism, and Popper's account of the 'logic' of scientific method, have to be accounted as failures. The incubus of the Duhem/Quine thesis has still not been exorcised successfully, so that there is really no more certainty in the process of falsification than in the processes of verification or induction. Indeed, as recent commentators such as David Stove have shown, the whole logic of falsificationism is something of a shambles. Also, as Anthony O'Hear has pointed out, in his efforts to counter the objections of his critics, and even in his original position, Popper has allowed inductivism into his system by the back door, so to speak. Following a kind of Kantian transcendental argument, O'Hear maintains that one cannot make sense of the external world at all unless one assumes some kind of stability therein. Certainly, one could not make a falsifying observation without some such assumption. Thus some (perhaps weak) inductive assumption of uniformity of nature is necessarily presupposed in scientific work, as in all human activity. O'Hear claims, in fact, that our customary distinction between the subjective and the objective — between us as persons and the external world as objects — would be impossible without some inductive presupposition. It is interesting, therefore, that in one of his later writings Popper has acknowledged that his realist assumption that the results of scientific inquiry lead one to greater verisimilitude is infected by a 'whiff' of inductivism. In saying this, Popper has in fact conceded a great deal, for the whole elaborate apparatus of falsificationism, empirical content, corroboration, verisimilitude, and so on, was created in order to circumvent 'Hume's problem' entirely. Now, it seems, the effort may not really have been worth it! Inductivism is creeping in again, even within Popper's own writings.

It must also be acknowledged that the much-vaunted distinction between science and pseudo-science is now beginning to look somewhat tatty. One can accept that a theory that is formally unfalsifiable cannot be scientific, but it is often the case that a falsifiable theory is not falsified because its proponents choose not to press home the falsifying evidence — Popper's methodological rules against conventionalist stratagems notwithstanding. They prefer to invoke ad hoc hypotheses, or to conventionalise the theory. But this has to do with aspects of human behaviour, rather than some logical feature of the structure of the theory — its inherent unfalsifiability. So whether or not theories are falsified on any given occasion is as much a social question as anything else. So, as I say, the distinction between science and pseudo-science should be made by examination of social practices just as much as by examination of the logical structures of theories.

In fact, Popper himself acknowledged this long ago when he wrote:

Only with reference to the methods applied to a theoretical system is it at all possible to ask whether we are dealing with a conventionalist or an empirical theory.

With hindsight, this can perhaps be seen to have been the thin end of the wedge of the sociology of knowledge cutting into Popper's theory. Thus, if we want to know whether a theory is or is not scientific, we should look and see how it is handled by people, rather than consider its logical structure.

The movement towards a widespread recognition of the social dimension of scientific knowledge will be treated in our next chapter. The chief historical roots of the sociological approach to scientific knowledge certainly do not lie in the work of Karl Popper. Nevertheless, we can proceed from Popper to our final discussions by considering the work of some of his followers and the controversies that have broken out in recent years between Popper and his critics. To these tasks we may now address ourselves.
NOTES
1 For details of Popper’s life beyond the very brief account given in this chapter, readers are referred to his most interesting autobiography. (See below, note 9.)
2 Popper’s use of the word historicism is generally regarded as somewhat idiosyncratic. Generally speaking, the word refers to the position which holds that the best way to understand something is to be achieved by examining its history. Historical determinism or historical inevitability are not usually thought to be essential components of the historicist position. But Popper seems to think otherwise — or rather he uses the word in a sense rather different from that which is customary.
3 K R Popper Logik der Forschung zur Erkenntnistheorie der modernen Naturwissenschaft Springer Vienna 1935
4 K R Popper The Logic of Scientific Discovery Hutchinson London 1959 (We may mention here that the first German edition of this work had rather little impact on the philosophical world. It was only with the publication of the English edition, following the success of The Open Society, that Popper’s views on the philosophy of science began to be really influential.)
5 K R Popper The Open Society and Its Enemies 2 vols Routledge London 1945
6 K R Popper The Poverty of Historicism Routledge & Kegan Paul London 1957
8 K R Popper Objective Knowledge An Evolutionary Approach Clarendon Press Oxford 1972
9 K R Popper Unended Quest An Intellectual Autobiography Fontana/Collins Glasgow 1976
10 K R Popper & J C Eccles The Self and Its Brain Springer Berlin & London 1977
12 Here Popper explained that he subsequently referred to such a ‘rescuing operation’ — involving the ad hoc Introduction of hypotheses to help keep an endangered theory afloat — as involving a ‘conventionalist twist’ or ‘conventionalist stratagem’. The point is, of course, that when ad hoc hypotheses are introduced in this way there is a refusal to allow a theory to submit to falsification. Nothing is allowed to count against it. Thus, in language reminiscent of that of Price, the theory may be said to be ‘conventionalised’.
13 Popper op cit (note 7) pp 36—7
14 Ibid p 82
15 See above, p 114
16 Popper op cit (note 7) p 46
17 Popper op cit (note 8) p 1
18 See above, p 45
19 Ibid
20 Popper op cit (note 4) p 31
21 See above, p 201
22 For detailed discussions of this important issue, see S G Harding ed Can Theories be Refuted? Essays on the Duhem-Quine Thesis Reidel Dordrecht & Boston 1980
23 See above, p 266
24 For discussion of the concept of ad hoc hypotheses, see J Leplin ‘The Concept of an Ad Hoc Hypothesis’ Studies in History and Philosophy of Science 1975 Vol 5 pp 309–45 (It may be noted in passing that the simple contraction hypothesis was tested independently in 1952 by the so-called Kennedy–Thomson experiment.)
25 This argument against Popper was adduced by Reichenbach long ago. See his review ‘Induction and Probability: Remarks on Karl Popper’s The Logic of Scientific Discovery’ in M Reichenbach & R S Cohen eds Hans Reichenbach Selected Writings 1929–1953 Reidel Dordrecht 1978 Vol 2 pp 372–87 (at p 574) (first published in German in Vol 5 of Erkenntnis 1955)
26 Popper op cit (note 4) p 120 & passim
27 Ibid p 113
28 Ibid p 120
29 For Popper’s doctrine of ‘basic statements’, see p 338
30 Popper op cit (note 4) p 122
31 Ibid pp 126–30
32 In these present strange philosophical days even this apparently innocuous statement is by no means universally acceptable. See, for example, the work of F K Feyerabend discussed below, pp 354–42
33 Popper op cit (note 4) pp 269–70 & Appendix IX
34 Ibid p 400
35 This is so, in Popper’s view, because of the lack of an acceptable principle of induction. Personally, I should be happy to suppose that some scientific statements of universal scope have a probability greater than zero.
36 Popper op cit (note 4) p 414
37 See above, p 239
38 Ibid p 94

THE REACTION TO INDUCTIVISM
By the same author

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THE ARCH OF KNOWLEDGE

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