CHAPTER 4

Ants, Spiders and Bees: a third way?

Later in the *First Book of Aphorisms* Bacon tempers his claim that a gradual and unbroken ascent from the senses and particulars is 'the true way':

> Those who have handled sciences have been either men of experiment or men of dogmas. The men of experiment are like the ant, they only collect and use: the reasoners resemble spiders, who make cobwebs out of their own substance. But the bee takes a middle course: it gathers its material from the flowers of the garden and of the field, but transforms and digests it by a power of its own. Not unlike this is the true business of philosophy: for it neither relies solely or chiefly on the powers of the mind, nor does it take the matter which it gathers from natural history and mechanical experiments and lay it up in the memory whole, as it finds it, but lays it up in the understanding altered and digested. Therefore from a closer and purer league between these two faculties, the experimental and the rational (such as has never yet been made), much may be hoped.

These graphic similes highlight our previous two chapters. Formal systems and abstract theories are too like cobwebs to serve rationalist hopes that they correspond to the real, necessary order of the world. Pure empiricists who merely collect and use cannot do justice to the role of theory in guiding our steps. Admittedly this last point might not matter, if the process of discovery can be cleanly separated from the process of validation. But we shall be raising doubts about that in a moment. Meanwhile Bacon suggests that 'a closer and purer league between these two faculties, the experimental and the rational' will do what is needed. We are to be like the bee which 'takes a middle course: it gathers its material from the flowers of the garden and of

the field, but transforms and digests it by a power of its own'. This attractive thought certainly captures a general belief that knowledge is, somehow, a blend of theory and experience, to which each contributes something beyond the scope of the other.

Here lie a host of puzzles, which are the stuff of current debate. Bacon's idea of 'a closer and purer league' is somehow to combine 'axioms' derived from the senses with 'axioms' revealed to the intellect. That sounds like a shrewd move. Traditionally there are two firm constraints on what we can rationally come to believe about the world. One is that our beliefs must be consistent with any facts known to us by observation. The other is that they must be logically coherent. Within these bounds, beliefs will be more or less probable, depending on degrees of proof or evidence. The bounds themselves, however, seem to be given without further proof or evidence. Thus Bacon calls them axioms and many philosophers have held that, without some kind of 'foundations' there could be no knowledge.

The chapter will start by propounding this view; and will then challenge it by suggesting that there can be no facts prior to all interpretation. This will raise further questions about discovery and validation, to be tackled with the help of Karl Popper. But radical thoughts will have been stirred and we shall next consider Quine's Pragmatist image of science as a web of belief. When that reminds us of the spiders who make cobwebs out of their own substance, we shall turn to Thomas Kuhn's thesis that science depends on 'paradigms'. Having then caught up with the present, untidy state of the philosophy of science, we shall be ready for two chapters of dispute between holists and individualists.

FOUNDATIONS OF KNOWLEDGE?

The idea that knowledge needs to rest on 'foundations' is crucial for understanding much of modern philosophy. The claim is that nothing can be known by proof or evidence, unless something can be known without either. Relatedly, nothing can be probable, unless something is certain. We need to see why this thesis is intensely plausible, before turning to recent developments which nevertheless deny it.

The case for 'foundations' is most easily made by reflecting that much of our knowledge depends on inference. Suppose I were to list all the propositions which I fancy that I know to be true, and then set myself the task of overhauling the list in order to winnow out any which, on reflection, I cannot warrantably claim to know. Many of the entries depend on inferences. For example, my knowledge that there were once dodos on the island of Mauritius is inferential. An inferred entry is conditional, in the sense that it is warranted only if other entries are warranted. So I shall put a star against all conditional entries on the list. Am I justified in retaining a starred entry? That depends on whether premises from which it can be inferred are also on the list and on whether at least some of them are unstarred. For, however sound an inference is, its conclusion is not a known truth, unless its premises are known truths. This applies as much to a conclusion that there *probably* were once dodos on Mauritius as to a stronger conclusion that there were once dodos there.

Now suppose I find a subset of entries which is self-contained, in that all are starred and each depends solely on the others. A subset dealing with the existence and habits of fairies might be an example. To anticipate a later chapter, so might a conceptual scheme spinning together witchcraft, oracles and magic; or a set of religious beliefs, complete with a theology. I would have to conclude that I knew the truth of none of these entries. For, if I know the truth of P only if I know the truth of Q, and if, moreover, my warrant for claiming to know Q is P itself (or else R, S etc., whose warrant is P), then I know the truth of neither P nor Q. The diameter of the circle is irrelevant and, if my whole list turns out to form such a complete subset of itself, then I know nothing at all. Hence there will have to be some unstarred entries. For knowledge to be possible, there must be some propositions which can be known without proof or evidence. These are my foundations of knowledge.

This neat and powerful argument has traditionally impressed rationalists and empiricists alike, ever since Descartes used a version of it in his *Meditations*. It covers both the experimental and the rational faculties, and holds as well for basics of logic or mathematics as for givens in perception. When empiricists

rejected the pretensions of rationalism, they did not reject the argument. The data supplied to and by the senses were claimed to be 'self-evident', in the sense of known without proof or evidence, and so suited to serve as foundations. The Logical Positivists were as committed to the need for foundations as any rationalist or any other empiricist before them. All justification has to end with truths which need no further justification.

It is worth stressing that some principles of inference need to be included in the foundations. Otherwise nothing can be built on the foundations. To infer that Q is true, we need to know not only that P is true but also that if P then Q. Inferences can be challenged, and the challenge can often be met by showing that a conditional statement like 'if P then Q' is true. But this cannot be done in every case, because every demonstration itself relies on inference. Hence some basic principles have to be self-evident. The point emerges readily, when we pose the riddle of induction from the previous chapter (p. 45) by asking what reason there is to accept that, if x per cent of known As are B, there is an x per cent probability that the next A will be B. If, for every reason, one had to offer a reason why it was a reason, the regress would make the riddle unanswerable. Perhaps it does! Similarly, there is also a riddle of deduction, because any proof of a principle of logic would require a principle of logic to certify it. So, if one asks what justifies the basic principles of logic, the only answer which might avoid begging the question seems to be that they are self-evident. Any embarrassment about self-evidence is peculiar neither to empiricists nor to rationalists. All foundational systems need ultimately to assume the soundness of their 'axioms' and method of construction.

Bacon presents the bee as simply combining the 'axioms' known to our rational and experimental faculties and using whatever proof and evidence is available to either. But his remark about the mind transforming and digesting its given material 'by a power of its own' raises a deep problem about objectivity. The aim of the first way was, in de Fontenelle's image, to go behind the scenes at the opera and see how reality causes appearances. The aim of the second way was to identify the regularities in phenomena (appearances) without having to speculate on

hidden causes. For both ways the mind was active in seeking truth but could be finally self-effacing, since the truths discovered were objective and untainted by any peculiarity of human involvement in the search. For both, one might say, the mind is finally a camera which records things as they are, however ingenious its operations. The current fashion is to deny that the mind can ever be a neutral recorder. Perhaps, indeed, not even a photograph is a neutral representation, because we interpret photographs, just as we interpret scenes photographed. Traditionally both ants and spiders have maintained that there are indeed moments of pure observation or intuition, when truth is revealed without interpretation. But the bee, Bacon suggests, lays nothing up in the understanding until it has been altered and digested. If he is right, then a closer and purer league between the experimental and rational faculties will not do the trick, and we shall need to find a third way.

INTERPRETATION

Empiricists are especially vulnerable to the idea that truth is never prior to all interpretation. The traditional core of their case against rationalists has always been that the 'experimental faculty' is pure, whereas the 'rational faculty' depends on the construction of the mind. Perception alone gives us unvarnished news, in the form of brute, uninterpreted facts, and, by the previous argument, without this foundation, we could know nothing of the world. The mind itself contributes nothing of substance. It is a *tabula rasa* or blank paper, on which experience writes the first knowledge. This doctrine remains crucial. For instance the hopes of a pure, Positive science discussed in the previous chapter depend squarely on it. The ingenious separation of the process of discovery from the process of validation was partly designed to preserve it. By letting the scientist import unobservables into theories and models for purposes of discovery, it took pressure off the moment of pure, neutral truth when prediction meets experience.

Yet the doctrine has been under fire from the start. A basic trouble, hinted at on p. 44, is that the idea of 'experience' is

ambiguous. It is used to refer both to what is presented to us, and to the experiencing; and it leaves the relation of subjective to objective elements unclear even for a 'given' like a perceived patch of colour. Although this is not the place for a tour of the philosophy of perception, it is easy to see that the ambiguity may be endemic. To describe what we experience we must apply concepts and the suggestion is that concepts are never merely dictated by phenomena, since they are involved in classifying even phenomena. In that case there is nothing more basic than an experiencing, where concept and object are inextricable. One sees the point of Immanuel Kant's famous remark in *The Critique of Pure Reason* (1781) that 'concepts without percepts are empty; percepts without concepts are blind'. To observe is not merely to register but to judge what concepts apply. Concepts are, somehow, supplied by the mind and, since they govern what we make of the world, are not the mere servants of experience.

Empiricists do not take this lying down, of course, and I shall not try to prove that they must concede it. But I can show what difficulties it causes for the idea of a Positive science. What follows is a brief outline of three recent contributions to the philosophy of science, each exploiting the idea that experience cannot play the role in scientific knowledge which was suggested in the previous chapter. Their authors are Karl Popper, W.v.O. Quine and Thomas Kuhn.

SCIENCE AS CONJECTURES AND REFUTATIONS

Popper has influenced social thinking directly with two books in particular. *The Open Society and its Enemies* (1945) reviews the history of political thought, condemns those, like Plato, Hegel and Marx, who have sought to entrench the power of the state and commends the openness to critical enquiry enshrined in a tolerant, liberal society. *The Poverty of Historicism* (1960) denies Marxist and Hegelian claims that there are laws of history and dialectical processes peculiar to the social world and hence to social science. It upholds the naturalist view that the natural and social worlds are all of a piece and are amenable to the same scientific method. The method is one of 'conjectures and refutations', an

enormously influential idea in the philosophy of science at large and one whose impact on social scientists has been no less for entering social science by that route.

His best known essay is probably 'Science: Conjectures and Refutations' (in Popper (1969)), the text of a 1953 lecture in which he reflected on his work in the philosophy of science since 1919. The question addressed is 'When should a theory be ranked as scientific?' Its most widely accepted answer, Popper remarks, is 'that science is distinguished from pseudo-science – or from metaphysics – by its *empirical method*, which is essentially *inductive*, proceeding from observation and experiment. This did not satisfy me' (p.33). For, if what counted were the amount of evidence confirming a theory, then many pseudo-scientific theories would have to be deemed scientific. Examples which had long troubled and infuriated him were Marx's theory of history, Freud's psychoanalytic theories and Adler's psychology. These theories were awash with confirming evidence but for the unsatisfactory reason that their adherents could square them with whatever happened. 'A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history' (p.35). They were, in a word, irrefutable. But, since this was because they ran no risk of refutation, it was no virtue. Hence '*the criterion of the scientific status of a theory is its falsifiability, or refutability or testability*' (p.37, his italics).

For a theory to be falsifiable there must be possible conditions in which it would be shown to be false. These conditions need to be specified in advance of testing and stood by, if the test goes against the theory. There must be no 'conventionalist stratagem' of conjuring up special reasons in the form of *ad hoc* extra assumptions or reinterpretations of results to save the theory. Scientific theories take genuine risks: pseudo-scientific or metaphysical theories do not. Correspondingly, this is the difference between critical thinking and dogmatic thinking (and, one might add, between open and closed societies). Critical thinking adapts to refutation by experience; dogmatic thinking rejects the counter-examples.

Popper presents his account of falsifiability in science as a rejection of Hume's analysis of knowledge and of the ideas

about Positive science which stem from it, especially those propounded by the Logical Positivists. It is not immediately obvious why. The difference between empiricists and others seems to be precisely that empiricists seek humbly to respect the findings of experience, whereas Bacon's 'men of dogmas' spin cobwebs out of their own substance. The diagram of scientific method on p.63 taken from Lipsey and the sketch of a Positive economics given by Friedman appear to embody a clear sense that falsifiability is crucial. What, then, is novel and disconcerting about Popper?

Popper himself makes it partly a matter of the psychology of science and partly of the logic of science, with Hume as a target for both. Hume, as we saw, took the relation of cause and effect as central to knowledge of the world, because it was the only one which went beyond mere impressions and ideas, but then reduced causation to regularities or 'constant conjunctions' in nature, coupled with a psychological expectation that they would continue. In effect this made science an exercise in induction, as on Bacon's second way, but with the sharp proviso that 'all our reasonings concerning matters of fact rest in the end on custom'. Custom here refers to 'the association of ideas', the standard eighteenth-century account of how we come by concepts and learn language. Ideas are prompted in us by 'impressions' or simple experiences. Frequent impressions give rise to concepts, and regular conjunctions of impressions lead us to associate ideas, thus producing a conceptual scheme which reflects the world as we find it, provided that we attend to experience. The crucial relation in the forming of concepts is 'resemblance': we simply recognise that two red patches resemble one another in both being red. The edifice of knowledge thus depends on regularities in nature obtruding themselves on the mind.

Let me say at once that a careful reading of Hume finds that imagination is involved in the association of ideas and the expectations aroused by constant conjunctions. That makes for a less passive mind than I have just suggested. But it is not altogether easy to integrate these active elements with the rest of Hume's science of mind, where associations occur passively in the main, and Popper certainly takes Hume to rest everything on the givenness of resemblances. His criticism of Hume is therefore radical

and uncompromising: there is no process of merely registering impressions and patterns of impressions, and hence no psychological process of induction. Hence 'the belief that we can start with pure observations alone, without anything in the nature of a theory is absurd' (p.35).

Popper contends that 'we are born with expectations: with knowledge which, although not valid *a priori*, is psychologically or genetically *a priori* i.e. prior to all observational experience' and that 'one of the most important of these expectations is that of finding a regularity' (p.47). We might usefully regard this as a gloss on Bacon's idea that the mind alters and digests experience with a power of its own and so as one reason why the scientist cannot merely collect and use observations. It thus encourages the separation between the process of discovery and the process of validation which we have already made on behalf of Positive science. Whereas Wallace's wheel (p.60) pictured a single process of mechanical generalisation for both purposes, Lipsey's apparatus (p.63) was more complex – more like a percolator – and had a box where conjectures could be introduced, provided that the work of validation was done by empirical testing. Since Lipsey admires Popper, this is no surprise; but it leaves us still asking why Popper denounces Positivism.

The answer lies in the implications for the logic of science. In inductive reasoning, broadly speaking, the more *A*s are found to be *B*s the better confirmed is the hypothesis that all *A*s are *B*s. Popper has no patience with a scientific method which relies on this logic. He flatly denies that if a hypothesis (*H*) implies an observation statement (*O*) and if *O* is true, then *H* is thereby confirmed. In formal logic there is no valid inference:

(1) $H \rightarrow O$
(2) O
therefore (3) H

Nor is there merit in making the conclusion expressly probabilistic:

therefore (3) *H* is more probable

That is merely the same inference in thin disguise. On the other hand there is no similar objection to a logic of falsification. There is a valid inference:

(1) $H \rightarrow O$
(2) *not-O*
therefore (3) *not-H*

and this is precisely the inference which dogmatists try to shrug off. That is the crucial difference between confirmation and falsification and the final reason why

> Induction, i.e. inference based on many observations, is a myth. It is neither a psychological fact, nor a fact of ordinary life, nor one of scientific procedure. (1969, p.53)

In upshot, science is always open-ended, offering no certainties and no rest for the enquiring mind. The process of testing does not tend to eliminate all hypotheses other than the true one. Whenever a genuinely risky conjecture survives refutation, there are always plenty of other conflicting conjectures which would have survived too. The logic of validation establishes only that some theories are false. Although science eliminates theories and passes on only survivors, it passes them on 'not as dogmas but rather with the challenge to discuss them and improve on them' (p.50). The image of science which emerges is no longer one of making a uniquely correct map of a partially known landscape. Nor is it one of erecting a building on sure foundations in the traditional way. As Popper declares in *The Logic of Scientific Discovery* (1959):

> The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, as least for the time being. (p.111)

Popper's ideas are exciting and he writes splendidly. But they are not as radical as he makes out. He presents refutations as decisive moments when a theory falls foul of observation and is eliminated. These are meant to be unmistakable moments of truth, even if the truth is the negative one that a theory is false. Yet there cannot possibly be such decisive moments, unless we are sure that the same would always occur if the test were repeated. But that depends on an inductive inference from the present occasion to the next. Otherwise why not simply try it again? Deny the soundness of induction, and we have no reason to eliminate a theory just because its predictions have not been upheld on particular occasions. If Popper has indeed shown that induction is a myth, we cannot rest content with the logic of falsification. For, if he means just what he says, there will be no reason to prefer unfalsified theories and we have been led into a general scepticism. Yet, as soon as falsification is seen to rely on induction for its claim to be decisive, the riddle of induction resurfaces and Popper can no longer declare, 'Thus the problem of induction is solved' (1969, p.55).

Furthermore the moment of truth is one where theory is tested against pure observation or brute fact. Or so Popper implies. Yet 'the belief that we can start with pure observations alone, without anything in the nature of a theory is absurd' (1969, p.35). In that case theory is involved in defining the test situation and in identifying what is observed in it. When refutation is deemed to occur, the tester must, in effect, be weighing the merits of the theory which yielded the prediction against the merits of the theory which yielded the description of what experience showed. Experiments are a complex business and there is always scope for contending that they are somehow defective or do not show exactly what is supposed. Interpretation, in short, is never absent, and there is no neutral standpoint when judging which theories it is rational to accept.

These are serious objections, which show, I think, that vintage Popper is closer to a classic empiricism than he supposed. That might sound good news for Bacon's ant-like 'men of experiment', since it leaves the process by which the bee transforms and digests its material finally subject to experience. But the trouble goes deeper, as witnessed by changes in Popper's own views, for example *Objective Knowledge* (1972). We are left trying to maintain both that there are moments of pure truth when facts test theories, and that observation is never innocent of theory. The bee is not just the ant turned ruminative. Pragmatism beckons.

SCIENCE AS A WEB OF BELIEF

Pragmatism insists that the mind is always active in deciding what counts as knowledge. Yet, although that makes all our concepts and beliefs revisable, revisions are to be made in the light of experience. To put it paradoxically, theory governs experience and experience governs theory. This interplay may cause trouble in the end but is immensely fertile in the meantime. The readiest introduction to recent developments is Quine's electrifying and prescient (1953) essay 'Two Dogmas of Empiricism', whose theme I shall now sketch. The two dogmas are the twin pillars of Logical Positivism, which we treated as a rationale for Positive science in the previous chapter. They are, firstly, the analytic–synthetic distinction and, secondly, the given, uninterpreted character of basic facts of observation. Quine's article seeks to demolish both, thus also subverting the broader empiricism which Logical Positivists had intended to render precise.

As noted in the previous chapter, the analytic–synthetic distinction is a neat device for keeping what Hume called 'relations of ideas' distinct from 'matters of fact and existence', thus heading off rationalist hopes that we can have *a priori* knowledge of reality. Analytic truths are 'true by convention' and so harmless to empiricism, once one realises that they result solely from how we decide to use words. Quine endorses the part played by human convention in giving some statements a privileged position in our knowledge. But he denies that even the truths of logic and mathematics are as utterly distinct from empirical statements as the analytic–synthetic distinction makes them. His argument defies compression; but the nub of it is that 'true by convention' cannot be construed as Logical Positivism hopes. What exactly is it that marks off analytic statements from others? The question can be answered only by appeal to notions like 'necessity', 'logical

equivalence' or 'sameness of meaning', which the notion of analyticity is alleged to account for. But to make it somehow ultimately self-evident that a truth like 'All bachelors are unmarried' is analytic is to license the kind of *a priori* intuition which empiricists must reject.

To avoid playing into the hands of the opposition, we need to think of analytic statements as held in place by conventions which experience can bring us to revise. They may be more deeply entrenched in our conceptual scheme or web of belief than are synthetic statements; but they cannot be immune to revision altogether. When we do revise them, it has to be for the same sort of reason too, namely that experience is resisting our attempts to describe and order it with their help. For example, astronomers long worked with a geometry derived from Euclidean axioms. But, when the pressure of experience led them to wonder whether space is best described in Euclidean terms, they revised Euclid. Some revisions are less far-reaching than others and we try the less radical revisions first. But priorities are a matter of degree of entrenchment in our thinking. No statement is finally immune to revision, not even the most elementary parts of logic and mathematics. The threads of our intellectual fabric are none of them pure black or white (purely analytic or purely synthetic) but various shades of grey.

By disputing the first dogma of empiricism, Quine gives scope for experience to influence all forms of theory. Conversely, by disputing the second, he involves theory in every moment of empirical truth. Synthetic or empirical statements are never directly at the mercy of experience. Even very particular ones, like 'the cat is on the mat', are connected to others as part of a web whose inner strands and nodes are remote from the experiential perimeter. A whole section of the web is thus at stake, when we look to see whether the cat is on the mat. The greater the stakes, the greater becomes our resistance to letting experience surprise us. The connections supply us with defences, since we can invoke them to show that we have misinterpreted experience. When experience seems to conflict with our beliefs, we always have a choice of what to revise; and, since we interpret whenever we describe, one choice is to reinterpret the experience. Our beliefs

do face the tribunal of experience; but they face it as a single body and there is always room for manoeuvre. In the web of belief no statement ever has to be given up, just as no statement is immune to revision.

This takes the idea that observation without theory is absurd far further than Popper does. Observation has become so bound up with interpretation and hence with theory that, in deciding what the facts of observation are, we may be deciding between rival theories. I shall not trace the theme deeper into pragmatism at large, since its history and ramifications are too complex for this book. To strengthen our feel for it, however, here are three glorious paragraphs from Quine's 'Two Dogmas', which bring pragmatism vividly to life. Notice how even physical objects would be unobservables, were they not treated as 'convenient intermediaries', and are assigned the same epistemological status as Homeric gods.

> The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth values have to be redistributed over some of our statements. Reevaluation of some statements entails reevaluation of others, because of their logical interconnections – the logical laws being in turn simply certain further statements of the system, certain further elements of the field. Having reevaluated one statement we must reevaluate some others, which may be statements logically connected with the first or may be the statements of logical connections themselves. But the total field is so under-determined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reevaluate in the light of any single contrary experience. No particular experiences are linked with any particular statements of the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole.
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> If this view is right, it is misleading to speak of the empirical content of an individual statement – especially if it is a statement at all remote from the experiential periphery of the field. Furthermore it becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements, which hold come

what may. Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle?

. . .

As an empiricist I continue to think of the conceptual scheme of science as a tool, ultimately, for predicting future experience in the light of past experience. Physical objects are conceptually imported into the situation as convenient intermediaries – not by definition in terms of experience, but simply as irreducible posits comparable, epistemologically, to the gods of Homer. For my part I do, qua lay physicist, believe in physical objects and not in Homer's gods; and I consider it a scientific error to believe otherwise. But in point of epistemological footing the physical objects and the gods differ only in degree and not in kind. Both sorts of entities enter our conception only as cultural posits. The myth of physical objects is epistemologically superior to most in that it has proved more efficacious than other myths as a device for working a manageable structure into the flux of experience. (1953, section 6, paras 1,2,4)

Cashing in Quine's riotous metaphors, we are being invited to recognise both that no single hypothesis can be tested in isolation and that every observation is linked theoretically to other observations. There is no longer a simple logic of falsification. Instead, there are always irreducible options:

(1) (H_1 and H_2 and H_3 . . . etc.)→(O_1 and O_2 and O_3 . . . etc.)
(2) *Not*-(O_1 and O_2 and O_3 . . . etc.)
therefore (3) *Not*-H_1 or *not*-H_2 or *not*-H_3 . . . etc.

Choice of where exactly to point the accusing finger of refutation is ours, not nature's, because there is nowhere for the mind to stand prior to all interpretation.

Thus prompted, we can now usefully ask whether Lipsey's percolator (p.63) truly embodies the idea of a Positive science after all. The percolator is meant to depict a definite process of adapting scientific hypotheses to independent facts of experience, thus serving to advance Positive economics. It is more sophisticated than Wallace's wheel (p. 60) on two counts. By recognising a distinction between processes of discovery and validation, it can allow for 'assumptions' which refer to unobservables; and, by saying that confirmation calls for 'no consequent action', it signals agreement with Popper's theme that confirmation does not raise the probability of a hypothesis. These refinements are consistent with a Positive economics. But it also takes a further step. On reflection, why does Lipsey mark the moment of truth with the word 'appears' ('the theory appears to be either inconsistent with the facts or consistent with the facts')? What determines choice at the fork where the theory is to be amended (how?) or discarded? Why is it to be discarded only if there is 'a superior competing theory'? Pragmatist thoughts are at work here. Lipsey has quietly credited the mind with more of a power of its own than straight empiricism can allow. When theory *appears* at odds with fact, we decide whether it *is* so, against a background of hitherto accepted theories. The process of testing thus slides into one of 'working a manageable structure into the flux of experience'.

Similarly, we can now resume the cryptic hints in the previous chapter that Friedman's (1953) essay gives theory more to do than a Positive science can countenance. The first half, with its celebrated account of Positive economics, leads on to a second half which could fairly be called pragmatist. 'Known facts cannot be set on one side; a theory to apply "closely to reality" on the other. A theory is the way we perceive "facts", and we cannot perceive "facts" without a theory' (p.34). This remark follows a discussion of how we should choose between theories, when several predict consistently with the facts. Friedman does not merely tell us to leave it to further experience. Instead, he recommends choosing theories which involve an abstraction or 'ideal type' marked by 'economy, clarity and precision' (1953, p.33). Theory is no longer simply a recording device or 'filing system' but has become a

source of selections from the mass of data. Data can even be discarded for the sake of economy and clarity. Why exactly are we to prefer these virtues? Friedman's answer is arresting.

> A fundamental hypothesis of science is that appearances are deceptive and that there is a way of looking at or interpreting or organising the evidence that will reveal superficially disconnected and diverse phenomena to be manifestations of a more fundamental and relatively simple structure. (1953, p.33)

If this 'fundamental hypothesis of science' is not to be a concession to the reality of fundamental structures in reality, it will have to state a principle for filing systems or languages. Even so, the second half of the essay threatens to destabilise its better known first half. Positive economics as an empirical science whose sole task is to predict phenomena with success yields to a Pragmatist economics, whose aim is the most simple and elegant theory consistent with those 'facts' which it leads us to perceive and deem significant. Theory is no longer merely the servant of experience.

Very well; but why exactly are we to prefer theories with the Pragmatist virtues of economy, clarity, precision, elegance, simplicity or suggestiveness? If disconnected and diverse phenomena are not manifestations of a simple, fundamental reality, such criteria are not necessarily guides to truth. Why are elegant theories more likely to survive the tribunal of experience? The answer will have to be internal to the web of belief. *Everything* which the bee lays up in the understanding has been altered and digested by the mind, operating with a power of its own. Claims about the structure of reality are no exception. It is one thing to show that theory is irreducibly involved in all understanding, and quite another to connect this activity to the search after truth.

Quine says that the tribunal of experience issues verdicts which we must accept, even if we choose how to adjust the web of belief in consequence. Whose tribunal is it? It seems to me that it can only be ours. Nature has become a myth or cultural posit, like the gods of Homer or the everyday physical objects which we bump into. In a system where no statement is immune to revision, how

could statements about nature be otherwise? Indeed even the claim that we can keep (or revise) any statement at a price is misleading. It suggests that prices are imposed by nature and are a fixed element in the bargains struck with experience. But prices too are finally internal to the mind's activity, even if they have to do with the most deeply entrenched features of the web of belief, the most elementary notions of consistency and coherence. No doubt it remains useful to think in terms of negotiation with nature but, epistemologically speaking, the tribunal of experience is another myth.

The last paragraph could be accused of begging a large question, however. We have so far assumed a correspondence theory of truth, in which an empirical statement is true if and only if it corresponds to the facts. This accords nicely with the image of science as exploration and suits the case for demanding foundations of knowledge. But Pragmatism accepts none of this baggage. In internalising facts to the web of belief, it is happy to dispense with a correspondence theory of truth and to replace it with some equivalence between what is 'true' and what contributes to the simplest coherent web or what it is finally useful to believe. Since these moves take us deeper into the theory of knowledge than we can go now, I shall not try to press the previous paragraph home.

But one conclusion can safely be drawn. If Quine is right and *every* statement is open to revision, there must be more ways of ordering experience than we attempt. What limits our efforts? The traditional answer given at the start of the chapter is that reason and experience confine us to theories which accord with the known facts and the rules of logic. But Pragmatism has made both these kinds of constraint revisable. Why then do we subscribe to the myth of physical objects and the general theories of nature which go with it? Quine himself has conjectured that the answer might lie in the biology of the brain and our human constitution: we are, so to speak, hard-wired to construe experience very broadly as we do. But others have been more inclined to give cultural posits a cultural explanation.

PARADIGMS AND AFTER

The leading suggestion has been made by Thomas Kuhn in *The Structure of Scientific Revolutions* (2nd edition, 1970), where he introduces the notion of a 'paradigm'. Kuhn's researches into the history of science convinced him that the schoolroom or Enlightenment story of the smooth and steady progress of reason was simply a fiction. For instance the 'Copernican revolution', by which the earth was displaced as the fixed, central body in the heavens, involved no moment of truth when the old Ptolemaic astronomy was refuted and the new theory replaced it. On the contrary, the two astronomies coexisted uneasily for several centuries. Both could claim the evidence of observation and, although the balance shifted as telescopes improved, the Ptolemaic still had eminent defenders as late as the eighteenth century. Nor is this surprising, given that the relative motion of bodies can always be described in several ways by taking each in turn as the fixed reference point. Meanwhile the revolution which finally occurred was, at heart, a conceptual one, a growing willingness to think of the cosmos and its moral order in new ways, which presently coalesced into the modern world view. As concepts changed, historians started to view the old order through the eyes of the new and were thus led to construct the schoolroom story of reasoned discoveries.

Reflecting on this and other episodes, Kuhn was led to distinguish between normal and revolutionary science. 'Normal' science is the organised, progressive, everyday work of gathering evidence and testing hypotheses. It goes on within a framework of intellectual assumptions and established practices, which it takes for granted. This framework or 'paradigm' is not immutable, however. When normal science starts to throw up consistently unexpected results, it comes under strain. When a radically fresh way of viewing the wayward results emerges and is widely deemed to make convincing sense of them, it is overthrown. This is a 'scientific revolution' of the sort that occurred in the shift to a modern astronomy or when Einstein's theory of relativity replaced the creaking Newtonian paradigm which had served since the seventeenth century. As a paradigm shift makes its way through into a new way of conducting normal science, the scientist comes to work 'in a different world'.

The history of Kuhn's book illustrates its own thesis. The project was originally conceived in the 1940s as part of the *International Encyclopedia of Unified Science.* This was a series started by the Logical Positivists in the 1930s with the aim of overtaking and completing *L'Encyclopédie*, the project of charting all knowledge begun by 'Les Philosophes' at the acme of Enlightenment optimism. *The Structure of Scientific Revolutions* set out to convey some factual information about the history of science, thus filling a gap with, presumably, true synthetic statements. The first edition (1962) attracted little attention. But its thesis in fact threatened to put paid to the whole Positivist programme by showing that science depended on elements which had no possible place in the Logical Positivists' scheme. The second edition, with its new introduction, detonated this time-bomb and the book has become compulsory reading. To grant its thesis is to think of science, and indeed knowledge in general, in a new way, since the unavoidable paradigms which regulate normal science are not open to direct refutation, are not mere filing systems or tautologies and are too mutable to be attributed to universal and external Reason. The thesis is, in short, revolutionary.

A paradigm has two principal aspects, one intellectual and the other institutional. Intellectually it consists of a set of guiding 'axioms', to use Bacon's term, or basic tenets about the broad character of nature and how it is to be studied. Descartes' intellectual system is a good example with its bold, simple ideas about the unified system in nature, its new mathematical physics and its account of knowledge and how to achieve it. Whereas Descartes himself claimed to have discovered this new system by rational intuition, as we saw earlier, Kuhnians regard it as resting on presuppositions, for which there can be no warrant: since they comprise a framework within which all reasoning and interpretation then proceeds, they are beyond the reach of reason and experience. Yet they are not empty or idle, since they regulate the permissible uses of reason and interpretations of experience. In short they have the air of those irrefutable tenets which Popper condemned as pseudo-scientific.

On the other hand, presuppositions can shift, despite their apparent immunity to revision. The Cartesian system presently yielded to the rival Newtonian system. The Newtonian system was generalised by Immanuel Kant in *The Critique of Pure Reason* (1781) as the embodiment of the only complete and consistent set of categories capable of making sense of experience. But Kant spoke too soon, if Einstein can be credited with a better alternative – a matter which I shall not try to assess. Nor presumably will that be the end of the story in a hundred years' time. As Kuhn himself points out, these shifts emerge in the course of reasoned debate, even if each framework also sets the canon of reasoned debate. So we have a puzzle in accounting for the intellectual dynamics of systems which are equipped to rule out challenges to their stability. Perhaps some of the impetus comes from internal contradictions, which force intellectual choices when they work their way up to the surface. But Kuhn's clear message is that reason alone cannot account for everything done in the name of reason.

Accordingly, the other principal aspect of a paradigm is institutional. Normal science is also kept on track by social mechanisms. It is highly organised activity, usually with a hierarchic power structure. Young scientists serve apprenticeships, in which they learn to think and practise as required by the prevailing paradigm, and are promoted for learning the lesson well. The heroic saga of the isolated individual genius is purely a myth. Real scientists work in hierarchical communities, subject to a discipline which reinforces the paradigm. Also they need funds. Science is an industry with investors to satisfy as well as an exercise in curiosity. That usually means pleasing the government, whose aims are not disinterested. Those who pay the piper call the tune. Thus the knowledge industry is enmeshed in a wider social and political system, which helps further to explain why a particular paradigm persists and how it regulates the practice of science. Equally, a paradigm shift is likely to go with deep shifts in the distribution of power in the wider society. Even if epistemology remains important, the sociology of knowledge moves in to fill the gap which opens when paradigms are found to be beyond the epistemic reach of reason and experience.

The challenge to Enlightenment ideas of scientific knowledge is radical. I have stated it more starkly than Kuhn does, but a stark version helps to show why the notion of a paradigm has had such impact. There have been broadly two reactions, which I shall outline very briefly to show the sheer range of current discords.

Firstly, the invitation to make Reason the subject of a sociology of knowledge has been widely accepted and has encouraged many revealing studies in the sociology of science. The history of medicine, for instance, is illuminated by recognising that the acceptance of medical theories is related to the power of the church, the rise of a medical profession, the fact that doctors are mostly male and midwives female or the influence of giant pharmaceutical companies. Such applied sociology of knowledge is not subversive of Reason, if it presumes medicine to be a largely rational activity and seeks only to explain sociologically what is irrational or non-rational about it. But the very existence of paradigms suggests that what is regarded as rational activity is itself as much a social as an intellectual matter.

This suggestion invites a general and subversive relativism, where all beliefs are related to features of their social context, whatever their intellectual rationale. This is the line taken by the 'Strong Programme' in the sociology of knowledge, robustly articulated by Barry Barnes and David Bloor (1982), among others. Bloor has remarked that 'Knowledge for the sociologist is whatever men take to be knowledge. It consists of those beliefs which men confidently hold to and live by' (1976, p.2). If there is no more to knowledge than belief confidently adhered to, then even the internal connections within a web of belief depend on rules of reasoning whose local authority is a sociological matter. This is explicit in the Strong Programme and it applies equally to rules of scientific method.

Secondly, however, defenders of the Enlightenment project have not taken Kuhn lying down. Popperians especially have tried to uphold the idea that falsification is an objective process which advances knowledge, despite both the theory-dependence of observation and the apparent invulnerability of paradigms. Imre Lakatos (1978), in particular, has suggested that a scientific theory should be viewed as a core of key propositions crucial for

the theory, protected by a belt or penumbra of auxiliary hypotheses many of which could be rejected without having to abandon the core. When prediction conflicts with experience, the scientist has a choice of whether to let an auxiliary hypothesis fall prey to the counter-example or to suspend judgement in the face of the anomaly to see whether the theory gets into wider trouble. The choice is governed by the state of health of the larger research programme to which the theory belongs. Any theory can always be saved by patching on new auxiliary hypotheses to explain away the previous conflict with experience. But if the patches are many and *ad hoc*, meaning that they have no theoretical rationale and are merely sticking plaster after the event, then the programme is 'degenerating'. A 'progressive' research programme responds to trouble in ways which make for theoretical sense and new conjectures in a spirit which Popper would applaud.

A feature of this response has been a counter-attack on Kuhn's sharp distinction between normal and revolutionary science. Popperians retort that the difference is a matter of degree of entrenchment, with normal science more willing to question its core theories than Kuhn recognised and revolutionary science more continuous with what went before. A sort of rapprochement has been reached. Popperians have become more holistic, more ready to think in terms of whole sets of interrelated theories and hypotheses, including those which import theoretical interpretation into the process of experiment and observation. This may abandon the definitive moments of truth at the heart of 'Conjectures and Refutations' but it lets science progress towards 'verisimilitude'. Kuhnians, no doubt reflecting that the thesis about paradigms is supposed to be a piece of objective science, have shied away from the acute relativism apparently implied by *The Structure of Scientific Revolutions*. Once we have absorbed the lesson that reason cannot be the sole arbiter of which beliefs it is rational to accept, the road is open for an objective account of science which takes social and political contexts into account. Perhaps we can come to identify the sort of context, liberal and democratic presumably, in which science fares best.

But the dragon's teeth have been sown and there may be no honest way for the warriors to avoid destroying one another. The

distinction between 'progressive' and 'degenerating' research programmes makes no sense to me, unless it presupposes the traditional view that there is an objective truth about an independent natural world to find. Yet it seems to have been conceded that the penetration of fact by theory and the influence of paradigms on the criteria of sound theory destroy all traditional access by reason to a world independent of our concepts and theories about it. The hope that we can restore objectivity just by recognising the intellectual and institutional role of paradigms, as a prelude to making allowance for it, seems to presuppose the neutral scientific standpoint thus undermined. At any rate, such suspicions have been shrewdly fuelled by Paul Feyerabend's *Against Method* (1975). So late in a long chapter I shall not try to summarise his reasons for declaring that all attempts at universal rules of scientific method are not only misplaced but also pernicious. They are well worth reading and his theme is nicely captured by his remark that 'All methodologies have their limitations and the only "rule" that survives is "anything goes"' (p.296).

The current scene is thus turbulent, even if we confine ourselves to the descendants of Bacon's ants, spiders and bees. It is more turbulent still, if we include the debates going on under the banners of Deconstruction and Critical Theory. Since these require awareness of the hermeneutic tradition, however, I postpone even a token gesture, until we have explored the idea of Understanding. Meanwhile, the chapter can best end at a point further back on the Enlightenment trail, with *The Critique of Pure Reason*. Kant has had slight mention so far and I do not see how to do him justice in an introduction to the philosophy of social science. But the *Critique* remains the preeminent attempt to combine the experimental and rational faculties, as Bacon demanded, to recognise that all knowledge is mediated by interpretation and yet to retain the idea of foundations for knowledge.

Kant was cited above for his remark that concepts without percepts are empty and percepts without concepts are blind. This interdependence of concepts and percepts is at the core of the troubling process by which empirical material is laid up in the understanding altered and digested. Our problem has been to see why recognising this process does not undermine claims to

objectivity. Kant's solution in the *Critique* was to identify the concepts fundamental to our understanding of experience as acquaintance with a world of physical objects, causally related and persisting in space and time. Since experience itself acquaints us only with phenomena, this categorial apparatus is imposed by the mind. But that does not make it merely subjective or inter-subjective. Kant argued that, if we ask what makes knowledge of the world possible, we can answer by stating unique preconditions for finding a rationally describable order in experience. Any rational understanding whatever therefore presupposes this single way of working a manageable structure into the flux of experience. The categories on which understanding relies transcend experience, thus assuring us not that reality itself conforms to them but that our thinking is objectively warranted in using them. This line anticipates Quine's Pragmatism, quoted earlier, but with steel necessities, rather than rubber ones, so to speak.

Not seeing how to say more without writing a different book, I turn now to the lessons of Chapters 2, 3 and 4.

CONCLUSION

'Reason is the pace, increase of science the way, and the benefit of mankind the end,' Hobbes declared (in *Leviathan*, 1651, Chapter 5). We set off down the Enlightenment trail in high spirits. But the rationalist and empiricist ways of discovering truth both seem to have petered out; and attempts to combine them have left us afraid that 'anything goes'. Although it is too soon to despair, it is certainly time to take stock.

The rationalist way began excitingly, with the world as a watch driven by hidden wheels and springs and Reason as a source of insight into such structures and forces. But it involved a dislocation between reality and appearance, which invited Bacon's scornful comparison with spiders who make cobwebs out of their own substance.

The empiricist way therefore looked more promising. The world was to consist of observable particulars; induction and prediction were to do the methodological work; and a tough-minded epistemology would dispense with necessities, causal

and logical alike, as components of empirical knowledge. But it proved impossible to confine interpretation and theory to the humble role proposed, even after separating the imaginative process of discovery from the patient process of validation.

Why not combine the two ways? Our attempt soon threatened to undermine the whole idea of knowledge as a temple built on self-evident foundations, *a priori* or empirical. Indeed it threatened any idea of objectivity. Even Popper's minimal moments of objective falsification proved vulnerable to Quine's insistence that no statement is immune to revision. Human knowledge emerged as a 'man-made fabric which impinges on experience only along the edges'; and, granted the pervasiveness of interpretation, even the edges were problematic. In making the mind incurably active as an interpreter of experience, we effectively blocked both of Bacon's ways to truth.

So the axe has been well and truly laid to Descartes' tree, and anything goes? Well, there are strong grounds for holding that our claims to knowledge include more than Reason, in any of its traditional definitions, can justify.

We are by now wary of the starting picture of nature as a realm independent of the enquiring mind, which science can explore with god-like objectivity. We have acquired reason to distrust the familiar distinction between human subject and external object. The general primacy traditionally given to epistemology has been shaken by challenges to the very idea that knowledge can have or needs to have foundations. This all spells trouble for the Enlightenment project. But how deep the trouble goes and what follows for the theory of knowledge are questions too large to pursue.

Enough has emerged to allow some cautious pointers to the following chapters. Here it may be helpful to reproduce Figure 1.2.

Firstly, we have found no single and commanding analysis of causal explanation in the philosophy of the natural sciences which social scientists are bound to accept. There are strong contenders inspired by Hume, which treat causal relations as statistical and think in terms of the success or failure of falsifiable predictions. But efforts to subject M. Rouget to the covering-law model and

	Explanation	Understanding
Holism	Systems	'Games'
Individualism	Agents	Actors

Figure 1.2

hypothetico-deductive method left us still wondering why he votes communist. In general, the underlying epistemologies, empiricist and pragmatist, have enough troubles of their own to leave richer analyses of causation in play.

Secondly, therefore, an ontological realism about mechanisms, forces, laws and structures also remains in play. Even if rationalism is not the friend it first appeared, empiricism and talk of webs of belief have failed to rule realism out. This is not to say that ontologies can be merely asserted, and we shall still need ways of avoiding dogmatism. But it releases an argument, which Positive science had hoped to side-track, about the merits of holism and, with it, about whether there is a distinctive sort of explanation, where individual behaviour is explained by its place in a system. That is the topic of the next chapter. Not all realists belong in the top left box of Figure 1.2, however, and, in any case, we have yet to consider the 'Agents' of the bottom left box. That will be done in Chapter 6. We broach both chapters with an unallayed curiosity about explanation and a readiness for the promised dispute between 'top down' and 'bottom up' in the left-hand column of Figure 1.2.

Thirdly, M. Rouget invites thoughts about Understanding, which will occupy Chapter 7. The original account in terms of probabilities was curiously silent about how M. Rouget viewed his vote, his world and himself. That may have been partly because Przeworski and Teune directed us to behavioural indicators like gender and occupation and we have yet to pursue J. S. Mill's call for psychological laws (or generalisations) in the opening chapter.

But it may also be because M. Rouget's own understanding does not take the form of psychological generalisations. His own 'web of belief' seems to be strung together from meanings, reasons and values. That may make us prefer Quine to Mill; but it also gives scope for disputing the naturalism so far assumed.

Fourthly, however, to explore the 'Understanding' column of Figure 1.2 is to activate a parallel dispute between 'top down' and 'bottom up'. Here the initial presumption favours individualism, in immediate reaction against the idea that history is to be written by identifying universal laws of behaviour. Yet, whatever may be in the minds of individual actors on historical occasions, they act in a context of shared meanings and rules, which permeate the options available and give scope to holism. That too will become plain in Chapter 7.