

Myths, Models and Paradigms:

A Comparative Study in Science and Religion

by Ian Barbour

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Chapter 6: Paradigms in Science

We must now carry further our analysis of the structure of science. Scientific models lead to theories which can be tested against observations. We must examine this process of assessment in science, and then compare it with the process of assessment in religion in the succeeding chapters.

Among criteria for assessing scientific theories are simplicity, coherence and agreement with experimental evidence. *Simplicity* is sought both as a practical advantage and as an intellectual ideal. This includes not only simplicity of mathematical form, conceptual simplicity, and a minimum of independent assumptions, but also an aesthetic element. It is not uncommon to hear scientists refer to the beauty or elegance or symmetry of a theory. *Coherence* with other accepted theories is also sought. The scientist aims at the comprehensive unification of separate laws, the systematic interrelation of theories, the portrayal of underlying similarities in apparently diverse phenomena. But the most important criterion is the number and variety of *supporting experimental observations*. A theory is valued if it accurately accounts for known observations and yields precise predictions of future measurements. The scientist is particularly impressed if it explains a variety of types of phenomena and, above all, if it leads to the discovery of novel phenomena not previously anticipated.

The empiricist accounts of science which were prevalent in the 1950's emphasized agreement with experiment as the main criterion for judging between rival theories. They defended the *objectivity* of science through three claims. (1) Science starts from publicly observable data which can be described in a pure observation-language independent of any theoretical assumptions. (2) Theories can then be verified or falsified by comparison

with this fixed experimental data. (3) The choice between rival theories is thus rational, objective, and in accordance with specifiable criteria.

These ideas came under increasing attack in the late 1950's and early 1960's, and three counter-claims were advanced. (1) *All data are theory-laden*; there is no neutral observation-language. (2) *Theories are not verified or falsified*; when data conflict with an accepted theory, they are usually set to one side as anomalies, or else auxiliary assumptions are modified. (3) *There are no criteria for choice between rival theories* of great generality, for the criteria are themselves theory-dependent.

The attack on empiricism was carried a step further in Thomas Kuhn's *The Structure of Scientific Revolutions* (1962). Kuhn held that the thought and action of a scientific community are dominated by its *paradigms*, defined as 'standard examples of scientific work which embody a set of conceptual, methodological and metaphysical assumptions'. He maintained that observational data and criteria for assessing theories are paradigm-dependent. Paradigms are therefore 'incommensurable'. A shift of paradigms during a scientific revolution is not a matter of logical argument but of 'conversion. Kuhn, according to his critics, portrayed scientific choice as irrational, subjective, and relative to particular scientific communities.

However, in the Postscript to the second edition (1970) of his book and in other recent essays, Kuhn has clarified and in some respects altered his earlier position; he now gives greater attention to the control of theory by experiment and the role of criteria independent of particular paradigms. On the other hand, some of the empiricists have qualified their assertions to take Kuhn's viewpoint into account. There is thus some evidence of convergence from the former 'objective' and 'subjective' extremes towards a middle position on each of the three points of disagreement. We will examine them in turn; it will be suggested later that each has significant implications for our understanding of religion.

1.The Influence of Theory on Observation

We shall first look briefly at new views of the relation of theory and observation. During the 1930's and 1940's there was wide acceptance of the positivist contention that science starts from indubitable data which can be described in a *neutral observation-language* independent of all theories. It was held that all theoretical terms must be translatable into pure

observational terms by means of operational definitions. What does the scientist do? He collects objective data and then forms inductive generalizations, according to the early positivists. Here was an emphasis on *observation* and its independence from theory.

It is well known that this positivist position was criticized by both scientists and philosophers. For one thing, it left out the place of creative imagination in the formation of theories. A theory is not given to us ready-made by the data, or by inferences from the data; it is a mental construct, a human invention. Often an important advance has come, not from new data, but from a new way of looking at old data. Furthermore, a theoretical term cannot be translated into equivalent observation terms, for it may be related to new types of observation which cannot be foreseen or specified. Theoretical entities are often only very indirectly related to observations -- especially in modern physics.

Now the versions of empiricism which were current in the 1950's took these criticisms into account. The importance of theoretical terms and non-observable entities in science was recognized. But it was still assumed that there are *fixed observational data* free from any theoretical interpretation. Nagel, Hempel, Braithwaite, Popper, and others¹ pictured two distinct levels in science: an unproblematic lower level of unchanging, objective data, describable in a pure observation language on which all observers can agree; and a separate upper level of theoretical constructs, acknowledged as products of man's creative imagination. In this scheme, the experimental data provide a neutral and impartial court of appeal for testing predictions deduced from alternative theories. The firm foundations of the scientific edifice are the solid data common to all observers. Here was an emphasis on both *theory* and *observation*, with a sharp distinction between them.

But during the 1960's even these modified versions of empiricism came under attack. Kuhn, Hanson, Polanyi, Feyerabend, Toulmin and others² concluded from their work in the history of science that the philosophers and logicians who set forth the empiricist position had not looked carefully enough at the real work of scientists. There are *no bare uninterpreted data*. Expectations and conceptual commitments influence perceptions, both in everyday life and in science. Man supplies the categories of interpretation, right from the start. The very language in which observations are reported is influenced by prior theories. The predicates we use in describing the world and the categories with which we classify events depend on the kinds of

regularities we anticipate. The presuppositions which the scientist brings to his enquiry are reflected in the way he formulates a problem, the kind of apparatus he builds, and the type of variable he considers important. Here the emphasis is on *theory* and the way it permeates observation.

In N. R. Hanson's oft-quoted words, '*All data are theory-laden.*' The procedures of measurement and the interpretation of the resulting numerical values depend on implicit theoretical assumptions. Most of the time, of course, scientists work within a framework of thought which they have inherited. Most scientists in their day-to-day work presuppose the concepts and background theories of their day; in testing theories of limited scope they can therefore obtain unambiguous data which can be described in a commonly accepted observation-language. But, says Feyerabend, when the background theory itself is at issue, when the fundamental assumptions and basic concepts are under attack, then the dependence of measurement on theoretical assumptions is crucial. 'Every theory has its own observation language.' Consequently, comprehensive theories are 'incommensurable'³

Feyerabend maintains that in the switch from Newtonian physics to relativity there was *a change in the meaning* of all the basic terms. Time, length, mass, velocity, even the notion of simultaneity, were redefined in the new system. In classical physics, mass was an inherent and unchanging property of a body. In relativity, however, mass is a property of the relationship between a body and a frame of reference, i.e., the mass of an object increases with an increase in its velocity relative to the observer. The equivalence of mass and energy -- totally unexpected by the Newtonian.- follows directly. Similarly, the distance and the time interval between two given events will be different for observers in different frames of reference, i.e., moving with respect to each other. Of course the Newtonian equations for the motion of an object can all be obtained from Einstein's equations as limiting cases for velocities which are small compared to the velocity of light. But even identical formulas are not equivalent if their terms have different meanings, according to Feyerabend.⁴

In response to this thesis that theories are incommensurable, several recent authors have acknowledged that all data are indeed theory-laden, but have insisted that there is a very wide variation in the degree to which *any given observation* is dependent on *any given theory*. In most experiments the data are not affected by the differences between the immediate hypotheses being compared; therefore the observations do exert some control over the choice

of hypothesis Moreover, expectations influence but do not completely determine what we see; unexpected events may make us revise our expectations. Israel Scheffler, replying to Feyerabend, writes:

Our expectations strongly structure what we see, but do not wholly eliminate unexpected sights... Our categorizations and expectations guide by orienting us selectively towards the future; they set us, in particular, to perceive in certain ways and not in others. Yet they do not blind us to the unforeseen.⁵

When two theories conflict, their protagonists can withdraw, not to a supposedly pure observation language, but to an observation language whose theoretical assumptions are not immediately at issue. There will usually be enough overlap between the assumptions of the two parties that a *common core of observations-statements* can be accepted by both -- even, I would argue, in a change as far-reaching as that from classical physics to relativity. Proponents of these two theories could agree as to how to measure the observed angle between two stars, even though they disagreed concerning the geometry of space. When the two theories yielded different views of the simultaneity of distant events, both parties could retreat to observations on which they concurred, namely the simultaneity of two signals reaching a single point. From the equivalence of mass and energy in relativity theory, together with theories about the fission of heavy nuclei, it was predicted that if a certain mass of uranium was brought together, an explosion would occur; surely all observers in the New Mexico desert on that day in 1943 could agree as to whether an explosion occurred.

But note that the shared *observational core*, against which competing theories may be tested, is not in general free from *theoretical interpretation*. The overlapping assumptions common to two theories will not be the same in all periods of history; they carry no guarantee of infallibility. Moreover, the categories of classification employed in an observational description may themselves need to be revised in the light of subsequent developments in the theory. Scheffler acknowledges that though observation exerts a control over theory, any given observation statement may find itself overridden in the end and subject to modification (here he significantly departs from earlier empiricist assumptions). Theory is revisable in the light of observation, but observation may also sometimes need to be reconsidered in the light of theory.⁶

Thus the line between *observation* and *theory* is not sharp or fixed. The decision to look on a given statement as primarily theoretical or primarily observational is relative, pragmatic, and context-dependent, as Mary Hesse contends.⁷ The emphasis may shift with the advance of science and the immediate purposes of enquiry. The ‘standard observables’ of one period will differ from those of another. What one treats as basic and uninterpreted will also vary according to the theory one is testing. Those descriptions which one considers more stable and more directly accessible will be taken as data, but that judgment will itself reflect theoretical assumptions. Hopefully this kind of account can represent both the more observational and the more theoretical poles of science and the interaction between them. It accepts the idea that there is no pure observation language, but it does not accept the claim that theories are incommensurable.

2. On the Falsifiability of Theories

Let us look next at the debate as to whether or not theories can be verified or falsified. To the positivists, *verification* had seemed a clear-cut and straightforward process. It was assumed that theories are verified by their agreement with experimental data. Knowledge, it was said, consists of proven propositions established by the hard facts. The famous ‘Verification Principle’ went on to assert that, apart from formal definitions, the only meaningful statements are empirical propositions verifiable by sense-experience. To rehearse the inadequacies of positivism now would be whipping a dead horse, but some of the reasons for the rejection of the idea of verification in science should be mentioned.

No scientific theory can be *verified*. One cannot prove that a theory is true by showing that conclusions deduced from it agree with experiment, since (i) future experiments may conflict with the theory, and (2) another theory may be equally compatible with present evidence. From a finite set of particular observations one cannot derive a universal generalization with certainty (the much debated logic of induction can provide no inferential grounds for making assertions about *all* cases when only a particular group of cases has been examined). In science, all theoretical formulations are tentative and subject to revision. Newtonian physics, one of the most extensively developed and experimentally supported theoretical systems in the history of science, was overthrown by relativity; we have seen that Einstein challenged almost all of Newton’s basic concepts. No theory today is immune to modification or replacement.

Cannot theories at least be *falsified*, then? Even if many instances of agreement with experiment do not prove that a theory is true, it would seem that even a single counterinstance of data which disagrees with theory should conclusively prove it false. Karl Popper, acknowledging that scientific theories are never verifiable, contended that they must be in principle falsifiable. Science advances by bold conjectures and stern attempts to refute them. Popper dwelt on the importance of ‘crucial experiments’ through which an hypothesis is definitively eliminated. Intellectual honesty, he said, requires the scientist to specify in advance experiments whose results could disprove his theory. Statements which are in principle unfalsifiable have no place in science.⁸

But Popper’s view has in turn received considerable criticism. Discordant data *do not always falsify* a theory. One can never test an individual hypothesis conclusively in a ‘crucial experiment’; for if a deduction is not confirmed experimentally, one cannot be sure which one, from among the many assumptions on which the deduction was based, was in error. A network of theories and observations is always tested together. Any particular hypothesis can be maintained by rejecting or adjusting other auxiliary hypotheses.⁹ As Quine puts it, theories form a field which is only loosely tied to the data at its boundaries:

The total field is so under-determined by its boundary condition, experience, that there is much latitude of choice as to what statements to reevaluate in the light of a single contrary experience. No particular experiences are lined up with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole... Any statement can be held true, come what may, if we make drastic enough adjustments elsewhere in the system.¹⁰

In practice the scientist works within the framework of accepted assumptions, and throws all the doubt on one new hypothesis at a time; but it might be just the accepted assumptions which should be questioned.

Many authors have criticized the idea of ‘crucial experiments’, but I want to comment particularly on the recent writings of Imre Lakatos because they show how far he has moved from Popper’s position, even though he presents his own view as a modification of Popper’s. In some cases of discrepancy between theory and data, he points out, it is the *implicit theoretical*

assumptions in the data which have been challenged. The use of data presupposes theories about the operation of instruments and the interpretation of experimental procedures, any of which may be questioned. Lakatos writes that ‘stubborn theoreticians frequently challenge experimental verdicts and have them reversed.’ Newton, for example, told the Astronomer Royal, Flamsteed, to correct some astronomical data because it disagreed with theoretical predictions; several factors, including refraction of light by the atmosphere, were later proposed to justify the corrections.¹¹

Auxiliary hypotheses may be introduced to remove a disagreement. A classic instance was the beta-decay of the nucleus, in which experimental data seemed clearly to violate the law of conservation of energy. Rather than abandon this law, physicists postulated an unobservable particle, the neutrino, to account for the discrepancy. Only at a considerably later point was there any independent evidence for the existence of the neutrino. Another case was Prout’s theory that the atoms of all elements are composed of hydrogen atoms, which implied that the atomic weights of all elements should be whole numbers (integers). Experiments giving 35.5 as the atomic weight of chlorine seemed to refute his theory, but he insisted that the assumptions implicit in the techniques for purifying the gas must be erroneous. He was unable to support this auxiliary hypothesis. Yet one can see a partial vindication of his theory in the later discovery that samples separated by physical rather than chemical means into pure isotopes do indeed have atomic weights which are almost exactly integral multiples of the atomic weight of hydrogen.¹²

Whether or not a given procedure is considered a ‘*crucial experiment*’ will vary with the changing theoretical context. At one point Fizeau’s measurement of the velocity of light in water seemed a conclusive refutation of the corpuscular theory of light; but the latter returned, in a new form, in Einstein’s theory of photons. Again, Michelson designed in 1881 a ‘*crucial experiment*’ whose results, he claimed, were a proof that the ether is dragged along with the earth, disproving the stationary ether theory. But Lorentz showed that the latter is not refuted if bodies change dimensions when moving (the Fitzgerald -- Lorentz contraction). Later Einstein on other grounds developed the theory of relativity; only then did the Michelson-Morley experiment, performed twenty-five years earlier, appear as important evidence against all ether theories.

‘Crucial experiments are only recognizable by hindsight, relative to the historical development of a theoretical system. The term is honorific, bestowed long after the event by the victorious party. For, in Lakatos’ words, a theory can lose several battles and yet come back to win the war, if its supporters do not give up too easily.’¹³

Finally, a recurrent discrepancy may simply be set aside as an *unexplained anomaly*. Newton’s theory of gravitation predicted that the apogee (most distant point) of the moon’s elliptical orbit around the earth should move forward $1^{1/2}$ ° each revolution. Newton admitted in his *Principia* that the observed motion was twice that predicted. For sixty years this disagreement, which was far beyond the limits of experimental error, could not be accounted for, yet it was never taken to ‘falsify’ the theory. More recently, the advance of the perihelion of Mercury was treated as an anomaly for eighty-five years, and only after the advent of relativity theory was it taken as evidence against Newtonian mechanics. The history of science is replete with such anomalies which for varying, periods have been left unexplained.’¹⁴

It is worth noting that a theory of great generality is usually abandoned only in favour of an *alternative theory*, not just because of conflicting data. A theory which seems defective at a few points is better than none at all. In the absence of an alternative, one can usually doctor up the old theory with suitable amendments, though there may eventually be so many patches and *ad hoc* adjustments that in the interests of simplicity one starts looking for alternatives. In practice, then, discordant observations are important, but they do not have any absolute power to falsify a theory, especially a comprehensive one.

One of the points at which Lakatos differs most markedly from Popper, and most resembles Kuhn, is his defense of *commitment* to a ‘research programme’. He urges that our attention be directed, not to individual hypotheses, nor even to theoretical networks at any one point in time, but to developing research programmes over a span of time -- such as the Newtonian programme to treat the universe as a mechanical system, or Bohr’s programme for the quantization of atomic systems. Lakatos’ programmes, like Kuhn’s paradigms, are not falsifiable in any direct way. For he holds that the ‘hard core’ of a programme (Newton’s Laws, for instance) is by deliberate decision made *exempt* from *falsification* so that its positive possibilities can be explored; any adjustments to accommodate

counter-instances are confined to non-essential secondary assumptions. 'This core is "irrefutable" by methodological decision of its protagonists; anomalies must lead to change only in the "protective" belt of auxiliary "observational" hypotheses and initial conditions.'¹⁵ This decision is not a declaration that the programme is true; it is a methodological device, a useful strategy for systematically developing the 'positive heuristic' without too many distractions. It is a policy for determining which hypotheses are to be considered essential to the programme, to be retained as long as possible, and which hypotheses are non-essential, to be sacrificed when difficulties occur.

Lakatos defines a *research programme* as 'progressive' if in the long run it leads to the discovery of novel phenomena and previously unexpected facts as well as accounting for facts already known. A programme is 'degenerative' and should be abandoned when (1) it has stalled for long enough and (2) there are promising alternatives. In such a degenerative stage, there will usually be an accumulation of *ad hoc* modifications for which there is no independent evidence. There will be no growth, over a protracted period of time, in the corroborated empirical content of the hard-core theories. But Lakatos maintains that there are no clear-cut rules for judging when a period is protracted enough, or the novelty slight enough, or the alternatives promising enough, to warrant relinquishing a programme. Here Lakatos, like Kuhn, holds that only scientists themselves can decide, in particular historical contexts, whether to stick with a research programme or not. In the next chapter, such commitment to a programme will be compared to commitment in religion.

3. Commitment to Paradigms

Of the exponents of new views of the relation of theories and observations, Thomas Kuhn has been the most influential. One discussion of his ideas lists thirty-six reviews of *The Structure of Scientific Revolutions* in journals whose fields range from philosophy and science to psychology and sociology.¹⁶ Many scientists feel at home in the volume because it gives frequent concrete examples from the history of science and seems to describe science as they know it. But others hold that he gives far too much prominence to subjective aspects of science. Workers in new research fields in the natural sciences, and in areas of the behavioural sciences where basic concepts and fundamental assumptions are in dispute, often find Kuhn's writing illuminating. I will summarize four themes of his book as it

originally appeared, and then indicate some of the criticisms it has evoked and his subsequent reply to his critics. The debate reveals a new understanding of the nature of science which has far-reaching implications.

1. *Paradigms dominate normal science.* Kuhn maintains that every scientific community is dominated by a cluster of very broad conceptual and methodological presuppositions embodied in the 'standard examples' through which students learn the prevailing theories of the field. Because such examples also serve as norms of what constitutes good science, they transmit methodological and metaphysical assumptions along with key concepts. A paradigm, such as Newton's work in mechanics, implicitly defines for a given scientific community the types of question that may legitimately be asked, the types of explanation that are to be sought, and the types of solution that are acceptable. It moulds the scientist's assumptions as to what kinds of entity there are in the world (Newton was interested in matter in motion) and the methods of enquiry suitable for studying them. 'Some accepted examples of actual scientific practice -- examples which include law, theory, application and instrumentation together -- provide models from which spring particular coherent traditions of scientific research.'¹⁷

Normal science, says Kuhn, consists of work within the framework of a paradigm which defines a coherent research tradition. Scientific education is an induction into the habits of thought and activity presented by text books, and an initiation into the practice of established scientists. It leads to the acquisition of 'a strong network of commitments, conceptual, theoretical, instrumental, and methodological'. Paradigms illustrate ways of attacking a problem -- for instance, by analysis in terms of masses and forces. Thereby they guide the direction of normal research, which is 'an attempt to force nature into the preformed and relatively inflexible boxes that the paradigm supplies'.¹⁸ Like solving a puzzle or playing a game of chess, normal science seeks solutions within an accepted framework; the rules of the game are already established. A shared paradigm creates a scientific community -- a professional grouping with common assumptions, interests, journals and channels of communication. This stress on the importance of the community suggests parallels in the role of the religious community which will be explored later.

2. *Scientific revolutions are paradigm shifts.* Kuhn holds that in normal research fundamental assumptions are not questioned. Anomalies are set to

one side, or accommodated by *ad hoc* modifications. Ptolemaic astronomy went on adding planetary epicycles to remove discrepancies; defenders of the phlogiston theory were driven to postulate negative chemical weights in order to maintain their paradigm. But with a growing list of anomalies, a sense of crisis leads the scientific community to examine its assumptions and to search for alternatives. A new paradigm may then be proposed which challenges the dominant presuppositions.

Kuhn shows that when a major change of paradigm does occur it has such far-reaching effects that it amounts to a revolution. Paradigms are incompatible. A new paradigm replaces the old; it is not merely one more addition to a cumulative structure of ideas. A revolution from Aristotelian to Newtonian physics, for instance, or from Newtonian physics to relativity, is 'a transformation of the scientific imagination' in which old data are seen in entirely new ways. For a period, adherents of two different paradigms may be competing for the allegiance of their colleagues, and the choice is not unequivocally determined by the normal criteria of research. Kuhn writes:

Though each may hope to convert the other to his way of seeing his science and its problems, neither may hope to prove his case. The competition between paradigms is not the sort of battle that can be resolved by proofs.. Before they can hope to communicate fully, one group or the other must experience the conversion that we have been calling a paradigm shift. Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like a gestalt switch it must occur all at once or not at all.¹⁹

Scientists resist such revolutions because previous commitments have permeated all their thinking; a new paradigm prevails only when the older generation has been 'converted' to it, or has died off and been replaced by a new generation. As Kuhn portrays it, a paradigm shift is thus a highly subjective process. He claims that scientific revolutions, like political revolutions, do not employ the normal methods of change.

3. *Observations are paradigm-dependent.* Kuhn agrees with Feyerabend and Hanson that there is no neutral observation language. Paradigms determine the way a scientist sees the world. Galileo saw a swinging pendulum as an object with inertia, which almost repeats its oscillating motion; his

predecessors, inheriting the Aristotelian interest in progress towards -- final ends, had seen a pendulum as a constrained falling object, which slowly attains its final state of rest. (Note the recurrence of the expression 'seeing as', whose use by Wittgenstein and Wisdom was mentioned earlier.) As with a gestalt switch, the same situation can be seen in differing ways. Scientists with rival paradigms may gather quite dissimilar sorts of data; the very features which are important for one may be incidental to the other. Rival paradigms, says Kuhn, solve different types of problems; they are, like Feyerabend's basic theories, 'incommensurable'.²⁰

4. *Criteria are paradigm-dependent.* Competing paradigms offer differing judgments as to what sorts of solution are acceptable. There are no external standards on which to base a choice between paradigms, for standards are themselves products of paradigms. One can assess theories within the framework of a paradigm, but in a debate among paradigms there are no objective criteria. Paradigms cannot be falsified and are highly resistant to change. Adoption of a new paradigm is a 'conversion'. Each revolution, says Kuhn:

... necessitated the community's rejection of one time-honoured scientific theory in favour of another incompatible with it. Each produced a consequent shift in the problems available for scientific scrutiny and in the standards by which the profession determined what should count as an admissible problem or a legitimate problem-solution. And each transformed the scientific imagination in ways that we shall ultimately need to describe as a transformation of the world within which scientific work was done.²¹

Yet in one of his final chapters Kuhn does state that there are reasons, even 'hard-headed arguments', for the adoption of a new paradigm. Its proponents must try to show that it can solve the problems which led to the crisis of the old paradigm. They can sometimes point to quantitative precision or to the prediction of novel phenomena not previously suspected. But in the very early stages the enthusiasts for a new paradigm may have little empirical support to offer, while the traditionalists may have many solved problems to their credit, despite unresolved anomalies. And even at later stages there is seldom anything approaching a conclusive proof of the superiority of one paradigm over another.²² This question of criteria for choice of paradigms is perhaps the most important issue in the controversy over Kuhn's book.

4. Paradigms Reconsidered

Since its first appearance, Kuhn's volume has provoked extensive discussion. He has had enthusiastic supporters and strenuous critics. Each of the four theses outlined above has been attacked:

1. *Criticisms of 'normal science'*. Kuhn's critics complain that his concept of paradigm is vague and ambiguous. Masterman lists twenty-one different senses of paradigm in the book. Kuhn's portrayal of the authoritarian character of normal science has also been challenged. Popper argues that in science there is continual criticism of fundamental assumptions; only beginning students or routine workers in applied science would uncritically accept dominant presuppositions. The scientist, he asserts, can challenge prevailing views whenever he wants to. 'If we try, we can break out of our framework at any time.' Feyerabend maintains that there is, and should be, a multiplicity of basic alternatives present at all times, rather than the exclusive monopoly by one paradigm which Kuhn describes and defends. Normal science is more diverse and more self-critical than Kuhn recognizes.²³

2. *Criticisms of 'scientific revolutions'*. Apart from the difficulty in identifying when a change is a 'revolution' and when it isn't, the sharp contrast between normal and revolutionary science has been questioned. S. E. Toulmin finds frequent small changes more typical of science -- 'micro-revolutions' which do not fit either of Kuhn's two classifications. In addition, he alleges, the struggle of alternative views occurs not simply in rare crises but more or less continuously. There are many gradations between routine and extraordinary science, differences of degree rather than of kind. There is also more continuity across a revolution than Kuhn depicts; there may be changes in assumptions, instrumentation and data, but there are no total discontinuities.²⁴

3. *Criticisms of 'the paradigm-dependence of observations'*. Even if a new paradigm directs attention to new problems and new variables, the old data need not be discarded and much of it may still be relevant. Dudley Shapere insists that under successive paradigms there are partly overlapping vocabularies; otherwise, there could be no possibility of communication or public discussion. If two paradigms really were 'incommensurable', they could not be 'incompatible'; to be considered 'rivals' they must at least apply to a jointly identifiable phenomenon, describable in predicates shared

by both protagonists. Moreover, though a paradigm determines which variables to study, it does not determine what the values of those variables will be. It may be resistant to falsification, but an accumulation of discordant data cannot be dismissed if empirical testing is to be maintained.²⁵

4. *Criticisms of 'the paradigm-dependence of criteria'*. If observations as well as criteria are paradigm-dependent, there is no rational basis for choice among competing paradigms. Each paradigm determines its own criteria, so any argument for it is circular. The choice seems arbitrary and subjective, a matter of psychology and sociology more than of logic. Lakatos writes:

For Kuhn scientific change -- from one 'paradigm' to another -- is a mystical conversion which is not and cannot be governed by rules of reason and which falls totally within the realm of the *(social) psychology of discovery*. Scientific change is a kind of religious change... There are no rational standards for their comparison. Each paradigm contains its own standards. The crisis sweeps away not only the old theories and rules but also the standards which made us respect them. The new paradigm brings a totally new rationality. There are no super-paradigmatic standards. The change is a band wagon effect. Thus *in Kuhn's view scientific revolution is irrational, a matter of mob psychology*.²⁸

It is on this point that Kuhn's critics are most vehement, accusing him of *relativism, subjectivism and irrationality*. Paradigm preference can be discussed only relative to a particular community. Watkins contrasts the dogmatism in Kuhn's 'closed societies' with the continuous criticism in Popper's 'open societies' and concludes:

'My suggestion is, then, that Kuhn sees the scientific community on the analogy of a religious community and sees science as the scientist's religion.'²⁷ Popper himself says: 'The Myth of the Framework is, in our time, the central bulwark of irrationalism.... In science, as distinct from theology, a critical comparison of the competing theories, of the competing frameworks, is always possible.'²⁸ Kuhn's portrayal of normal science as dominated by unchallenged dogmas, his failure to specify criteria for paradigm choice, and his talk of 'conversion' and 'persuasion' all seem to these critics to threaten the objectivity and rationality of the scientific enterprise.

In response to his critics, Kuhn has added a Postscript in the second edition of his book, and has written several essays, in which he clarifies his earlier views and at some points significantly modifies them. Since his final position does answer some of his critics' objections, his more recent treatment of each of the four themes presented above should be outlined:

1. *The diverse meanings of 'paradigm'*. Kuhn now tries to distinguish some of the various features of science which were formerly lumped together. Paradigms in their primary meaning are shared crucial examples, for which he suggests the term *exemplars*. One learns science by concrete examples of problem-solving, rather than by explicit rules. A formula, such as $f = ma$, is of little use until one learns how to approach a new situation so that it can be applied. One 'learns to see situations as like each other', and to recognize similarities which have not been formalized. Kuhn holds that the extension of such similarities, embodied in exemplars, is important for normal research as well as for the science student.²⁹

The more general 'constellation of group commitments' Kuhn now wants to call *the disciplinary matrix*. One component consists of widely held *values*, such as simplicity, consistency and predictive accuracy (these will be examined in connection with criteria below, since Kuhn acknowledges that they are widely shared among different scientific communities). Another component consists of metaphysical commitments transmitted by *particular models*:

Re-writing the book now I would describe such commitments as beliefs in particular models, and I would expand the category models to include also the relatively heuristic variety: the electric circuit may be regarded as a steady-state hydrodynamic system; the molecules of a gas behave like tiny elastic billiard balls in random motion. Though the strength of group commitment varies, with non-trivial consequences, along the spectrum from heuristic to ontological models, all models have similar functions. Among other things they supply the group with preferred or permissible analogies and metaphors. By doing so they help to determine what will be accepted as an explanation and as a puzzle-solution; conversely, they assist in the determination of the roster of unsolved puzzles and in the evaluation of the importance of each.³⁰

By introducing these distinctions, Kuhn has modified his earlier idea of the unity of a paradigm as a total coherent viewpoint, though it is not clear just how he thinks of the separate components of the disciplinary matrix as interacting with each other.

2. *The distinction between 'normal' and 'revolutionary' science.* Kuhn qualifies this distinction but still defends it. He now wants a 'scientific community' to be identified sociologically (e.g. by its patterns of inter-communication) before its shared paradigms are studied. Some 'communities' turn out to be quite small -- as few as a hundred scientists. There can be considerable variation among competing 'schools of thought'. We are told that members of a community can disagree about some rather fundamental issues; nineteenth-century chemists did not all have to accept atomism as long as they all accepted the laws of combining proportions. Further, there can be 'small-scale revolutions' and 'micro-revolutions' (without a preceding crisis) affecting specialized subgroups within a larger community. Nevertheless Kuhn still maintains that the most fruitful strategy of normal science is to develop and exploit the prevailing tradition, extending its scope and accuracy; the examination of assumptions and the search for alternatives, he holds, seldom occurs except during major crises.³¹

3. *The 'translation' of observations.* Kuhn has also qualified his 'incommensurability' thesis, though he continues to maintain that there is no neutral observation language. Communication is by no means impossible between men with rival paradigms. 'Both their everyday and most of their scientific world and language are shared. Given that much in common, they should be able to find out a great deal about how they differ.'³² Each can try to see a phenomenon from the other's viewpoint, and eventually even anticipate how he would interpret it. The problem, says Kuhn, is like that of translation between two language communities, which is difficult but not impossible. This analogy allows Kuhn to retain some vestiges of his idea of 'conversion' -- for a person can go beyond translation to the actual adoption of a new language in which he thinks and speaks.

4. *The 'rationality' of paradigm-choice.* Kuhn objects strongly to the charge of irrationality. If science is not rational, he asks, what is? But to understand what scientific rationality really requires, we have to look at science with care. Kuhn reminds his critics that he always has maintained that there are 'good reasons' and 'hardheaded arguments' for choosing paradigms. In his

Postscript he spells out more fully the values which are shared by all scientists:

Probably the most deeply held values concern predictions: they should be accurate; quantitative predictions are preferable to qualitative ones; whatever the margin of permissible error, it should be consistently satisfied in a given field; and so on. There are also, however, values to be used in judging whole theories: they must, first and foremost, permit puzzle-formulation and solution; where possible they should be simple, self-consistent, and plausible, compatible, that is, with other theories currently deployed. (I now think it a weakness of my original text that so little attention is given to such values as internal and external consistency in considering sources of crisis and factors in theory choice)³³

Kuhn insists, however, that these shared values provide no automatic rules for paradigm choice, since there is inevitable *variation in individual judgment* in applying them. Moreover, not all persons will assign the same relative weights among these values. After stating that debates over fundamental theories do not resemble logical or mathematical proofs, Kuhn concludes:

Nothing about that relatively familiar thesis implies either that there are no good reasons for being persuaded or that those reasons are not ultimately decisive for the group. Nor does it even imply that the reasons for choice are different from those usually listed by philosophers of science: accuracy, simplicity, fruitfulness, and the like. What it should suggest, however, is that such reasons function as values and that they can thus be differently applied, individually and collectively, by men who concur in honouring them. If two men disagree, for example, about the relative fruitfulness of their theories, or if they agree about that but disagree about the relative importance of fruitfulness and, say, scope in reaching a choice, neither can be convicted of a mistake. Nor is either being unscientific. There is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision.³⁴

Kuhn offers a pragmatic justification for this variability of individual judgment. For if everyone abandoned an old paradigm when it first ran into

difficulties, all effort would be diverted from systematic development to the pursuit of anomalies and the search for alternatives -- almost all of which would be fruitless. On the other hand, if no one took alternative paradigms seriously, radically new viewpoints would never be developed far enough to gain acceptance. Variations in judgment allow a distribution of risks, which no uniform rules could achieve. Yet the fact that there are agreed values encourages communication and the eventual emergence of a scientific consensus. Finally, these values provide standards in terms of which one can see genuine progress as one looks at a succession of theories in history. 'That is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress.'³⁵ Kuhn thus denies the allegations of irrationality and subjectivism.

Some of Kuhn's critics are still far from satisfied in this regard. Thus Shapere, in a review of Kuhn's recent writings, repeats his earlier epithets:

It is a viewpoint as relativistic, as antirationalistic, as ever... He seems to want to say that there are paradigm-independent considerations which constitute rational bases for introducing and accepting new paradigms; but his use of the term 'reasons' is vitiated by his considering them to be 'values', so that he seems not to have gotten beyond his former view after all. He seems to want to say that there is progress in science; but all grounds of assessment again apparently turn out to be 'values', and we are left with the same old relativism... The point I have tried to make is not merely that Kuhn's is a view which denies the objectivity and rationality of the scientific enterprise; I have tried to show that the arguments by which Kuhn arrives at this conclusion are unclear and unsatisfactory.³⁶

Shapere does not define 'rationality', but he evidently identifies it with rule-governed choice. Kuhn is called 'anti-rationalistic', it seems, because he still holds that the choice of paradigms is not unequivocally specified by the values accepted throughout the scientific community. Such name-calling, however, sheds little light on the question of how choices in science are or should be made.

5. Criteria of Assessment in Science

In this section, a view of criteria for scientific choice is proposed which incorporates what I take to be the most significant insights of Kuhn's

reformulated position and the most important contributions of his critics. Such a position may be less exciting than either the early empiricists' 'objectivism' or the 'subjectivism' which many readers found in Kuhn's first edition. But hopefully it can better represent an accurate description of what scientists actually do and a fruitful prescription for the continuation of the distinctive achievements of science. Its implications for the critique of religion are analysed in the next chapter.

I will distinguish the following aspects of science:

- (1) observations,
- (2) theories and theoretical models,
- (3) 'research traditions' (Kuhn) or 'research programmes' (Lakatos), over a span of time, embodied in key examples ('exemplars'), and
- (4) metaphysical assumptions about the nature of entities in the world

In subsequent chapters I will use the term 'paradigm' to refer to the third component above, namely, a tradition transmitted through historical exemplars, but in this section I will avoid using the term because Kuhn used it in a variety of senses in his earlier writing. Exemplars have an important practical function in this scheme; as key examples, rather than explicit rules, they serve to initiate the student into the methods of attacking a problem which are accepted within a research tradition, and they guide the projected research programme of a particular scientific community. But exemplars do not determine the criteria for theory choice, and they can be considered separately from metaphysical assumptions. Traditions influence the type of model which is proposed in a new situation. Particular theoretical models (such as the billiard ball model of a gas) are treated here along with the theories which they generate and by which they are tested. A number of my conclusions in the first part of this chapter can now be applied within this scheme.

First, *all data are theory-laden, but rival theories are not incommensurable*. There is no pure observation language; the distinction between theory and observation is relative, pragmatic and context-dependent. But protagonists of rival theories can seek a common core of overlap in observation languages, on a level closer to agreed observations to which both can retreat. This seems a more accurate way of describing communication concerning

observations during basic controversies (such as those over relativity and quantum theory) than Kuhn's recent analogy of 'translation', which assumes no common terms. It also allows more continuity and carry-over at the level of observations and laws before and after a revolution, and hence a more cumulative history, than Kuhn and Feyerabend recognize.

Something rather like a '*gestalt* switch' does occur in moving from one comprehensive theory to another. Different features of the phenomenon are selected for attention; new problems, new variables, new relationships are of interest. A familiar situation is seen in a new way. Further, it may be necessary to challenge and reinterpret the interpretive component of observations; to that extent, the data can be said to change. But this usually involves a retreat to observations whose interpretive component is not in doubt. Even in a *gestalt* switch, after all, there are lines in the picture which remain unchanged. Unlike a *gestalt* switch, however, there are in science criteria for favouring one interpretation over another -- though I will suggest that in the very early stages, when a comprehensive theory of wide scope is first proposed, these criteria seldom yield definitive conclusions.

Second, *comprehensive theories are highly resistant to falsification, but observation does exert some control over them.* There are no 'crucial experiments' which can be specified in advance. But the degree of vulnerability to counter-instances varies considerably among the various components of science. If unsupported by a theory, a law stating relationships between variables which are relatively 'observable' will be thrown into question by a few persistent discrepancies. Theories, especially comprehensive ones, are more resistant to falsification, but an accumulation of anomalies, or *of ad hoc* modifications having no independent experimental or theoretical basis, cannot be tolerated indefinitely. An accepted comprehensive theory is overthrown not primarily by discordant data but by an alternative theory; we should visualize not a two-way confrontation of theory and experiment, but a complex confrontation of rival theories and a body of data of varying degrees of susceptibility to reinterpretation. *A research programme is even more resistant to change than a theory, but may eventually be abandoned in favour of a new programme which has greater promise of explaining known data, resolving anomalies, and predicting novel phenomena.*

Commitment to a research tradition and *tenacity* in a research programme are scientifically fruitful (on this Kuhn and Lakatos agree). Only if scientists

stick with a programme and do not abandon it too readily will its potentialities be systematically explored and exploited. What balance between criticism and commitment is possible and desirable? Here Kuhn's revised picture of normal science allows for considerable diversity within a scientific community -- including the presence of rival small groups and competing 'schools of thought'. Popper's advocacy of 'continual criticism' ('we can break out of our frameworks at any time') and Feyerabend's plea for a plurality of basic alternatives in every field at all times ('proliferation of theories', 'perpetual revolution') seem unrealistic and, even if they could be achieved, wasteful of scarce scientific manpower. There is both historical and strategic justification for Kuhn's view that, for most scientists, fruitful work is achieved within a framework of accepted assumptions, except when major difficulties in dominant theories are evident.

Third, *there are no rules for choice between research programmes, but there are independent criteria of assessment*. Criteria are indeed acquired more from studying past exemplars than from learning explicit principles; but they are common to many exemplars and can be stated apart from any of them. A scientist usually has some training in several related fields and some familiarity with their exemplars; his criteria are not dependent on one tradition alone.³⁰ As outlined earlier, the most important criteria are simplicity, coherence, and the extent and variety of supporting experimental evidence (including precise predictions and the anticipation of the discovery of novel types of phenomena). But there are no rules, no specific instructions, that is, for the unambiguous application of the criteria; there is, in Kuhn's words, 'no systematic decision procedure which must lead each individual in the group to the same decision'. Yet the criteria provide what Kuhn calls 'shared values' and 'good reasons' for choice; they are 'important determinants of group behaviour, even though the members of the group do not apply them in the same way'.

In *the very early stages*, when a comprehensive theory and its development into a research programme are first proposed, empirical criteria seldom have a predominant role. To return once more to our historical example: in the history of the theory of relativity, the Michelson-Morley experiment did not play the determinative part most textbooks assign to it. In point of fact, all of the experimental evidence on which Einstein drew had been available for fifty years; he was unaware of the Michelson-Morley results until considerably later. He was interested primarily in simplicity and coherence - in particular, the symmetry of the forms of the equations for electrical and

magnetic fields in motion.³⁸ The variability of individual weighting among various criteria, which Kuhn describes, is also most noticeable in the early stages of a new theory. Thus the Inconsistency between Bohr's quantum theory and the assumptions of classical physics worried some physicists very much when it was first proposed, whereas others thought this inconsistency of little importance compared to the accuracy of the predictions which it yielded.

The criteria for assessing theories are relevant to the *evaluation of research programmes*, but they cannot be applied in any rigorous way. The decision to abandon an accepted programme will depend on judgments of the seriousness of the anomalies, inconsistencies, and unsolved puzzles in the old programme (these are sometimes more important than Lakatos admits), and the promise of a proposed new programme. As Lakatos maintains, there are no clear-cut rules for such decisions, and there are risks in either changing programmes too precipitously or too reluctantly. The decision may be vindicated only decades later -- which does not help much during the scientific controversy itself. Yet because there are accepted criteria common to all scientists the decision can be discussed and reasons set forth, and an eventual consensus can be expected.

Theories and programmes, then, are not verified or falsified, but *assessed by a variety of criteria*. Especially in the early stages of controversial theories of great generality, and in the decision to abandon a well-developed research programme in favour of a promising but undeveloped new one, the assessment is an act of personal judgment. In such circumstances the scientist is more like a judge weighing the evidence in a difficult case than like a computer performing a calculation. The judgment cannot be reduced to formal rules, yet it is subject to rational argument and evaluation by commonly agreed criteria. The impossibility of specifying explicit rules is one of the reasons why editors of scientific journals and panels awarding research grants must have considerable discretionary power in evaluating new ideas.

Finally, *metaphysical assumptions* are one stage further from direct empirical verification or falsification, yet even these are not totally immune to change. I agree with Kuhn that the scientist does have beliefs about the kinds of entity there are in the world, and does have ontological commitments (and not merely methodological commitments for the sake of a fruitful research strategy, as Lakatos would have it). Because Newtonian

mechanics was spectacularly successful, physicists not only used it as an exemplar of what a theory should be like, but also took its categories as indicative of the constituents of the universe. Additional assumptions were made concerning regularity, causality, action-at-a-distance and other basic features of the world. The same conceptual categories and presuppositions proved to be powerful tools in many fields, from astronomy to chemistry and biology. Less legitimately, perhaps, these metaphysical commitments were extended to a total world-view of reality as matter in motion.

But several things can happen to change the dominance of a set of metaphysical assumptions. The selection of the particular features of the research programme which had been assumed to be responsible for its success may be reconsidered; the emphasis may be placed instead on other features of the programme. Again, research programmes in one field -- or in several fields -- may be replaced by new programmes using very different basic concepts. Interest may also shift to new scientific fields, or to new areas of human experience; the earlier extension of metaphysical assumptions from one field, as wider interpretive categories for a total world-view, may then be questioned. In the course of history such assumptions have changed -- at least partially in response to changes in science, though also in response to changing views of other area of human experience.

The position I have presented is consistent with the *critical realism* defended in Chapter 3 above. Naive realism is not plausible if the history of science provides evidence of major paradigm shifts rather than simple cumulation and convergence. Thus Mary Hesse writes:

The history of science has already sufficiently demonstrated that successive acceptable theories are often in radical conceptual contradiction with each other. The succession of theories of the atom, for example, exhibits no 'convergence' in descriptions of the nature of fundamental particles, but oscillates between continuity and discontinuity, field conceptions and particle conceptions, and even speculatively among different topologies of space.³⁹

On the other hand, there is in the history of science more continuity than one would expect from Feyerabend or from Kuhn's earlier work, in which truth is entirely relative to a succession of self-contained language systems dominated by diverse paradigms. I have argued that observations and basic

laws are retained through paradigm-shifts, at least as limiting cases under specifiable circumstances; a new theory usually explains why the older theory was as good as it was and why its limitations became evident.

To summarize: the scheme I have outlined accepts the three 'subjective' theses that (1) all data are theory-laden, (2) comprehensive theories are highly resistant to falsification, and (3) there are no rules for choice between research programmes. It also preserves Kuhn's most distinctive contributions concerning paradigms: the importance of exemplars in the transmission of a scientific tradition, and the strategic value of commitment to a research programme. At the same time I have made three assertions which seem to me essential for the objectivity of science: (1) rival theories are not incommensurable, (2) observation exerts some control over theories, and (3) there are criteria of assessment independent of particular research programmes.

Footnotes:

1. Richard Braithwaite, *Scientific Explanation*; Carl G. Hempel, *Aspects of Scientific Explanation*, The Free Press 1965; Karl R. Popper, *The Logic of Scientific Discovery*, Hutchinson's University Library 1956. Popper calls his view critical rationalism rather than empiricism because observation-sentences are used to falsify rather than to verify theories. In Ernest Nagel, *The Structure of Science*, the distinction between theory and observation is less absolute than for these other authors.
2. Toulmin, *Foresight and Understanding*; N. R. Hanson, *Patterns of Discovery*, Cambridge University Press 1958; Michael Polanyi, *Personal Knowledge*, University of Chicago Press 1958. References on Feyerabend and Kuhn are given below.
3. P. K. Feyerabend, 'Explanation, Reduction and Empiricism', in H. Feigl and G. Maxwell (eds.), *Minnesota Studies in the Philosophy of Science*, vol. 3, University of Minnesota Press 1962; also 'Problems of Empiricism', in R. Colodny (ed.), *Beyond the Edge of Certainty*, Prentice-Hall 1965.
4. P. K. Feyerabend, 'Problems of Empiricism, Part 2', in R. Colodny (ed.), *The Nature and Function of Scientific Theory*, University of Pittsburgh Press 1971.
5. Israel Scheffler, *Science and Subjectivity*, The Bobbs-Merrill Co. 1967, p.44.
6. *Ibid.*, p.119.
7. Mary Hesse, 'Theory and Observation: Is There an Independent Observation Language?', in Colodny (ed.), *The Nature and Function of Scientific Theory*.
8. Popper, *The Logic of Scientific Discovery*. Also *Conjectures and Refutations*, Routledge & Kegan Paul 1963.
9. See Irving M. Copi, 'Crucial Experiments', in E. H. Madden (ed.), *The Structure of Science*, Houghton Muffin Co. 1960.
10. W. V. Quine, *From a Logical Point of View*, Harvard University Press 1953, p.43.
11. Imre Lakatos, 'Falsification and the Methodology of Scientific Research Programmes', in I. Lakatos and A. Musgrave, *Criticism and the Growth of Knowledge*, Cambridge University Press 1970, p.130. This volume is cited as CGK in subsequent footnotes.
12. CGK, p.128.

13. *CGK*, pp. 159ff.; also Lakatos, 'History of Science and Its Rational Reconstructions', in R. Buck and R. Cohen (eds.), *Boston Studies in the Philosophy of Science*, vol. 8, D. Reidel Publishing Co. 1971, p.100.
14. See R. G. Swinburne, 'The Falsifiability of Scientific Theories', *Mind*, July 1964, p.434; W. Whewell, *History of the Inductive Sciences*, rev. ed. 1847, vol.11, p.220. Other examples are given in Polanyi, *op. cit.*, pp.148-158.
15. *CGK*, p.133.
16. Eugene Lashchuk, *Scientific Revolutions*, Ph.D. dissertation, University of Pennsylvania 1969.
17. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press 1962, p.10.
18. *Ibid.*, p.24.
19. *Ibid.*, pp.147, 149.
20. *Ibid.*, chap. 10.
21. *Ibid.*, p.6.
22. *Ibid.*, chap. 12.
23. Margaret Masterman, 'The Nature of a Paradigm'; K. R. Popper, 'Normal Science and its Dangers'; P. K. Feyerabend, 'Consolations for the Specialist'; all in *CGK*.
24. S. E. Toulmin, 'Does the Distinction between Normal and Revolutionary Science Hold Water?', in *CGK*.
25. Dudley Shapere, 'Meaning and Scientific Change', in R. Colodny (ed.), *Mind and Cosmos*, University of Pittsburgh Press 1966. See also Scheffler, *op. cit.*, chap.4.
26. *CGK*, pp.93, 178.
27. J. W. N. Watkins, 'Against "Normal Science"', in *CGK*, p.33.
28. *CGK*, pp. 56, 57.
29. Thomas Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. University of Chicago Press 1970, pp.187-191. See also his 'Second Thoughts on Paradigms', in Frederick Suppe (ed.), *The Structure of Scientific Theories*, University of Illinois Press 1973.
30. *Structure of Scientific Revolutions*, 2nd ed., p.184.
31. *Ibid.*, p. 181. Also Kuhn, 'Reflections on my Critics', in *CGK*, p.249.
32. *Structure of Scientific Revolutions*, 2nd ed., p.201.
33. *Ibid.*, p.185
34. *Ibid.*, pp.199-200
35. *Ibid.*, pp.205-206. See also Kuhn, 'Notes on Lakatos', in *Boston Studies in Philosophy of Science*, vol. 8, pp. 144ff.
36. Dudley Shapere, 'The Paradigm Concept', *Science*, vol. 172, 1971, pp. 708-709.
37. See William Austin, 'Paradigms, Rationality and Partial Communication', *Journal of General Philosophy of Science* (to appear in 1973).
38. F. Schillp (ed.), *Albert Einstein: Philosopher-Scientist*, Library of Living Philosophers 1949, p. 53.
39. Mary Hesse, 'Models of Theory Change', in Proceedings of the IVth International Congress of Logic, Methodology and Philosophy of Science, Bucharest, 1971 (to be published).