From: What is this thing called science?, third edition, by A.F. Chalmers. (part of chapter 8).

his displacement current, again with the aid of mechanical analogies, it was possible to bring great coherence to the theory in the form of Maxwell's equations, which clearly specified the interrelationship between all the electromagnetic quantities. It was not long before the ether, which had been considered to be the mechanical seat of the fields, could be dispensed with, leaving the fields as clearly defined concepts in their own right.

In this section I have attempted to construct a rationale for approaching science by way of the theoretical frameworks within which scientific work and argumentation take place. In this and the following two chapters we look at the work of three important philosophers of science who have pursued this idea.

Introducing Thomas Kuhn

Inductivist and falsificationist accounts of science were challenged in a major way by Thomas Kuhn (1970a) in his book *The Structure of Scientific Revolutions*, first published in 1962, and then republished with a clarificatory PostScript eight years later. His views have reverberated in the philosophy of science ever since. Kuhn started his academic career as a physicist and then turned his attention to the history of science. On doing so, he found that his preconceptions about the nature of science were shattered. He came to believe that traditional accounts of science, whether inductivist or falsificationist, do not bear comparison with historical evidence. Kuhn's account of science was subsequently developed as an attempt to give a theory more in keeping with the historical situation as he saw it. A key feature of his theory is the emphasis placed on the revolutionary character of scientific progress, where a revolution involves the abandonment of one theoretical structure and its replacement by another, incompatible one. Another important feature is the important role played by the sociological characteristics of scientific communities.

Kuhn's picture of the way a science progresses can be summarised by the following open-ended scheme:

pre-science — normal science — crisis — revolution — new normal science — new crisis

The disorganised and diverse activity that precedes the formation of a science eventually becomes structured and directed when a single *paradigm* becomes adhered to by a scientific community. A paradigm is made up of the general theoretical assumptions and laws and the techniques for their application that the members of a particular scientific community adopt. Workers within a paradigm, whether it be Newtonian mechanics, wave optics, analytical chemistry or whatever, practise what Kuhn calls *normal science.* Normal scientists will articulate and develop the paradigm in their attempt to account for and accommodate the behaviour of some relevant aspects of the real world as revealed through the results of experimentation. In doing so, they will inevitably experience difficulties and encounter apparent falsifications. If difficulties of that kind get out of hand, a *crisis* state develops. A crisis is resolved when an entirely new paradigm emerges and attracts the allegiance of more and more scientists until eventually the original, problem-ridden paradigm is abandoned. The discontinuous change constitutes a *scientific revolution.* The new paradigm, full of promise and not beset by apparently insuperable difficulties, now guides new normal scientific activity until it too runs into serious trouble and a new crisis followed by a new revolution results.

With this resumé as a foretaste, let us look at the various components of Kuhn's scheme in more detail.

Paradigms and normal science

A mature science is governed by a single paradigm.[1] The paradigm sets the standards for legitimate work within the science it governs. It coordinates and directs the "puzzle-solving" activity of the groups of normal scientists who work within it. The existence of a paradigm capable of supporting a normal science tradition is the characteristic that distinguishes science from non-science, according to Kuhn. Newtonian mechanics, wave optics and classical electromagnetism all constituted and perhaps constitute paradigms and qualify as sciences. Much of modern sociology lacks a paradigm and consequently fails to qualify as science.

As will be explained below, it is of the nature of a paradigm to belie precise definition. Nevertheless, it is possible to describe some of the typical components that go to make up a paradigm. Among the components will be explicitly stated fundamental laws and theoretical assumptions. Thus Newton's laws of motion form part of the Newtonian paradigm and Maxwell's equations form part of the paradigm that constitutes classical electromagnetic theory. Paradigms will also include standard ways of applying the fundamental laws to a variety of types of situation. For instance, the Newtonian paradigm will include methods of applying Newton's laws to planetary motion, pendulums, billiard-ball collisions, and so on. Instrumentation and instrumental techniques necessary for bringing the laws of the paradigm to bear on the real world will also be included in the paradigm. The application of the Newtonian paradigm in astronomy involves the use of a variety of approved kinds of telescope, together with techniques for their use and a variety of techniques for the correction of the data collected with their aid. A further component of paradigms consists of some very general, metaphysical principles that guide work within a paradigm. Throughout the nineteenth century the Newtonian paradigm was governed by an assumption something like, "The whole of the physical world is to be explained as a mechanical system operating under the influence of various forces according to the dictates of Newton's laws of motion", and the Cartesian program in the seventeenth century involved the principle, "There is no void and the physical universe is a big clockwork in which all forces take the form of a push". Finally, all paradigms will contain some very general methodological prescriptions such as, "Make serious attempts to match your paradigm with nature", or "Treat failures in attempts to match a paradigm with nature as serious problems".

Normal science involves detailed attempts to articulate a paradigm with the aim of improving the match between it and nature. A paradigm will always be sufficiently imprecise and open-ended to leave plenty of that kind of work to be done. Kuhn portrays normal science as a puzzle-solving activity governed by the rules of a paradigm. The puzzles will be of both a theoretical and an experimental nature. Within the Newtonian paradigm, for instance, typical theoretical puzzles involve devising mathematical techniques for dealing with the motion of a planet subject to more than one attractive force, and developing assumptions suitable for applying Newton's laws to the motion of fluids. Experimental puzzles included the improvement of the accuracy of telescopic observations and the development of experimental techniques capable of yielding reliable measurements of the gravitational constant. Normal scientists must presuppose that a paradigm provides the means for the solution of the puzzles posed within it. A failure to solve a puzzle is seen as a failure of the scientist rather than as an inadequacy of the paradigm. Puzzles that resist solution are seen as *anomalies* rather than as falsifications of a paradigm. Kuhn recognises that all paradigms will contain some anomalies (for example the Copernican theory and the apparent size of Venus or the Newtonian paradigm and the orbit of Mercury) and rejects all brands of falsificationism.

Normal scientists must be uncritical of the paradigm in which they work. It is only by being so that they are able to concentrate their efforts on the detailed articulation of the paradigm and to perform the esoteric work necessary to probe nature in depth. It is the lack of disagreement over fundamentals that distinguishes mature, normal science from the relatively disorganised activity of immature *pre-science*. According to Kuhn, the latter is characterised by total disagreement and constant debate over fundamentals, so much so that it is impossible to get down to detailed, esoteric work. There will be almost as many theories as there are workers in the field and each theoretician will be obliged to start afresh and justify his or her own particular approach. Kuhn offers optics before Newton as an example. There was a wide diversity of theories about the nature of light from the time of the ancients up to Newton. No general agreement was reached and no detailed, generally accepted theory emerged before Newton proposed and defended his particle theory. The rival theorists of the pre-science period disagreed not only over fundamental theoretical assumptions but also over the kinds of observational phenomena that were relevant to their theories. Insofar as Kuhn recognises the role played by a paradigm in guiding the search for and interpretation of observable phenomena, he accommodates the sense in which observation and experiment can be said to be theory-dependent.

Kuhn insists that there is more to a paradigm than what can be explicitly laid down in the form of explicit rules and directions. He invokes Wittgenstein's discussion of the notion of "game" to illustrate some of what he means. Wittgenstein argued that it is not possible to spell out necessary and sufficient conditions for an activity to be a game. When one tries, one invariably finds an activity that one's definition includes but that one would not want to count as a game, or an activity that the definition excludes but that one would want to count as a game. Kuhn claims that the same situation exists with respect to paradigms. If one tries to give a precise and explicit characterisation of some paradigm in the history of science or in present-day science, it always turns out that some work within the paradigm violates the characterisation. However, Kuhn insists that this state of affairs does not render the concept of paradigm untenable any more than the similar situation with respect to "game" rules out legitimate use of that concept. Even though there is no complete, explicit characterisation, individual scientists acquire knowledge of a paradigm through their scientific education. By solving standard problems, performing standard experiments and eventually by doing a piece of research under a supervisor who is already a skilled practitioner within the paradigm, an aspiring scientist becomes acquainted with the methods, the techniques and the standards of that paradigm. The aspiring scientist will be no more able to give an explicit account of the methods and skills he or she has acquired than a master-carpenter will be able to fully describe what lies behind his or her skills. Much of the normal scientist's knowledge will be *tacit*, in the sense developed by Michael Polanyi (1973).

Because of the way they are trained, and need to be trained if they are to work efficiently, typical normal scientists will be unaware of and unable to articulate the precise nature of the paradigm in which they work. However, it does not follow from this that a scientist will not be able to articulate the presuppositions involved in the paradigm should the need arise. Such a need will arise when a paradigm is threatened by a rival. In those circumstances, it will be necessary to attempt to spell out the general laws and metaphysical and methodological principles involved in a paradigm in order to defend them against the alternatives involved in the threatening new paradigm. The next section summarises Kuhn's account of how a paradigm can run into trouble and be replaced by a rival.

Crisis and revolution

Normal scientists work confidently within a well-defined area dictated by a paradigm. The paradigm presents them with a set of definite problems together with methods that they are confident will be adequate for the solution of the problems. If they blame the paradigm for any failure to solve a problem, they will be open to the same charges as the carpenter who blames his tools. Nevertheless, failures will be encountered and such failures can eventually attain a degree of seriousness that constitutes a serious crisis for the paradigm and may lead to the rejection of a paradigm and its replacement by an incompatible alternative.

The mere existence of unsolved puzzles within a paradigm does not constitute a crisis. Kuhn recognises that paradigms will always encounter difficulties. There will always be anomalies. It is only under special sets of conditions that the anomalies can develop in such a way as to undermine confidence in the paradigm. An anomaly will be regarded as particularly serious if it is seen as striking at the very fundamentals of a paradigm and yet persistently resists attempts by the members of the normal scientific community to remove it. Kuhn cites as an example problems associated with the ether and the earth's motion relative to it in Maxwell's electromagnetic theory, towards the end of the nineteenth century. A less-technical example would be the problems that comets posed for the ordered and full Aristotelian cosmos of interconnected crystalline spheres. Anomalies are also regarded as serious if they are important with respect to some pressing social need. The problems that beset Ptolemaic astronomy were pressing ones in the light of the need for calendar reform at the time of Copernicus. Also bearing on the seriousness of an anomaly will be the length of time that it resists attempts to remove it. The number of serious anomalies is a further factor influencing the onset of a crisis.

According to Kuhn, an analysis of the characteristics of a crisis period in science demands the competence of the psychologist as much as that of the historian. When anomalies come to be seen as posing serious problems for a paradigm, a period of "pronounced professional insecurity" sets in. Attempts to solve the problem become more and more radical and the rules set by the paradigm for the solution of problems become progressively more loosened. Normal scientists begin to engage in philosophical and metaphysical disputes and try to defend their innovations, of dubious status from the point of view of the paradigm, by philosophical arguments. Scientists even begin to express openly their discontent with and unease over the reigning paradigm. Kuhn (1970a, p. 84) quotes Wolfgang Pauli's response to what he saw as the growing crisis in physics around 1924. An exasperated Pauli confessed to a friend, "At the moment, physics is again terribly confused. In any case, it is too difficult for me, and I wish I had been a movie comedian or something of the sort and had never heard of physics". Once a paradigm has been weakened and undermined to such an extent that its proponents lose their confidence in it, the time is ripe for revolution.

The seriousness of a crisis deepens when a rival paradigm makes its appearance. According to Kuhn (1970a, p. 91), "the new paradigm, or a sufficient hint to permit later articulation, emerges all at once, sometimes in the middle of the night, in the mind of a man deeply immersed in crisis". The new paradigm will be very different from and incompatible with the old one. The radical differences will be of a variety of kinds.

Each paradigm will regard the world as being made up of different kinds of things. The Aristotelian paradigm saw the universe as divided into two distinct realms, the incorruptible and unchanging super-lunar region and the corruptible and changing earthly region. Later paradigms saw the entire universe as being made up of the same kinds of material substances. Pre-Lavoisier chemistry involved the claim that the world contained a substance called phlogiston, which is driven from materials when they are burnt. Lavoisier's new paradigm implied that there is no such thing as phlogiston, whereas the gas, oxygen, does exist and plays a quite different role in combustion. Maxwell's electromagnetic theory involved an ether occupying all space, whereas Einstein's radical recasting of it eliminated the ether.

Rival paradigms will regard different kinds of questions as legitimate or meaningful. Questions about the weight of phlogiston were important for phlogiston theorists and vacuous for Lavoisier. Questions about the mass of planets were fundamental for Newtonians and heretical for Aristotelians. The problem of the velocity of the earth relative to the ether, which was deeply significant for pre-Einsteinian physicists, was dissolved by Einstein. As well as posing different kinds of questions, paradigms will involve different and incompatible standards. Unexplained action at a distance was permitted by Newtonians but dismissed by Cartesians as metaphysical and even occult. Uncaused motion was nonsense for Aristotle and axiomatic for Newton. The transmutation of elements has an important place in modern nuclear physics (as it did in mediaeval alchemy and in seventeenth-century mechanical philosophy) but ran completely counter to the aims of Dalton's atomistic program. A number of kinds of events describable within modern microphysics involve an indeterminancy that had no place in the Newtonian program.

The way scientists view a particular aspect of the world will be guided by a paradigm in which they are working. Kuhn argues that there is a sense in which proponents of rival paradigms are "living in different worlds". He cites as evidence the fact that changes in the heavens were first noted, recorded and discussed by Western astronomers after the proposal of the Copernican theory. Before that, the Aristotelian paradigm had dictated that there could be no change in the super-lunar region and, accordingly, no change was observed. Those changes that were noticed were explained away as disturbances in the upper atmosphere.

The change of allegiance on the part of individual scientists from one paradigm to an incompatible alternative is likened by Kuhn to a "gestalt switch" or a "religious conversion". There will be no purely logical argument that demonstrates the superiority of one paradigm over another and that thereby compels a rational scientist to make the change. One reason why no such demonstration is possible is the fact that a variety of factors are involved in a scientist's judgment of the merits of a scientific theory. An individual scientist's decision will depend on the priority he or she gives to the various factors. The factors will include such things as simplicity, the connection with some pressing social need, the ability to solve some specified kind of problem, and so on. Thus one scientist might be attracted to the Copernican theory because of the simplicity of certain mathematical features of it. Another might be attracted to it because in it there is the possibility of calendar reform. A third might have been deterred from adopting the Copernican theory because of an involvement with terrestrial mechanics and an awareness of the problems that the Copernican theory posed for it. A fourth might reject Copernicanism for religious reasons.

A second reason why no logically compelling demonstration of the superiority of one paradigm over another exists stems from the fact that proponents of rival paradigms will subscribe to different sets of standards and metaphysical principles. Judged by its own standards, paradigm *A* may be judged superior to paradigm *B*, whereas if the standards of paradigm *B* are used as premises, the judgment may be reversed. The conclusion of an argument is compelling only if its premises are accepted. Supporters of rival paradigms will not accept each others' premises and so will not necessarily be convinced by each others' arguments. It is for this kind of reason that Kuhn (1970a, pp. 93–4) compares scientific revolutions with political revolutions. Just as "political revolutions aim to change politicial institutions in ways that those institutions themselves prohibit" and consequently "political recourse fails", so the choice "between competing paradigms proves to be a choice between incompatible modes of community life", and no argument can be "logically or even probabilistically compelling". This is not to say, however, that various arguments will not be among the important factors that influence the decisions of scientists. On Kuhn's view, the kinds of factors that do prove effective in causing scientists to change paradigms is a matter to be discovered by psychological and sociological investigation.

There are a number of interrelated reasons, then, why, when one paradigm competes with another, there is no logically compelling argument that dictates that a rational scientist should abandon one for the other. There is no single criterion by which a scientist must judge the merit or promise of a paradigm, and, further, proponents of competing programs will subscribe to different sets of standards and will even view the world in different ways and describe it in different languages. The aim of arguments and discussions between supporters of rival paradigms should be persuasion rather than compulsion. I suggest that what I have summarised in this paragraph is what lies behind Kuhn's claim that rival paradigms are "incommensurable".

A scientific revolution corresponds to the abandonment of one paradigm and the adoption of a new one, not by an individual scientist only but by the relevant scientific community as a whole. As more and more individual scientists, for a variety of reasons, are converted to the new paradigm, there is an "increasing shift in the distribution of professional allegiances" (Kuhn, 1970a, p. 158). If the revolution is to be successful, this shift will spread so as to include the majority of the relevant scientific community, leaving only a few dissenters. These will be excluded from the new scientific community and will perhaps takes refuge in a philosophy department. In any case, they will eventually die.

The function of normal science and revolutions

Some aspects of Kuhn's writings might give the impression that his account of the nature of science is a purely *descriptive* one, that is, that he aims to do nothing more than to describe scientific theories or paradigms and the activity of scientists. Were this the case, then Kuhn's account of science would be of little value as a *theory* of science. Unless the descriptive account of science is shaped by some theory, no guidance is offered as to what kinds of activities and products of activities are to be described. In particular, the activities and productions of hack scientists would need to be documented in as much detail as the achievements of an Einstein or a Galileo.

However, it is a mistake to regard Kuhn's characterisation of science as arising solely from a description of the work of scientists. Kuhn insists that his account constitutes a theory of science because it includes an explanation of the *function* of its various components. According to Kuhn, normal science and revolutions serve necessary functions, so that science must either involve those characteristics or some others that would serve to perform the same functions. Let us see what those functions are, according to Kuhn.

Periods of normal science provide the opportunity for scientists to develop the esoteric details of a theory. Working within a paradigm, the fundamentals of which they take for granted, they are able to perform the exacting experimental and theoretical work necessary to improve the match between the paradigm and nature to an ever-greater degree. It is through their confidence in the adequacy of a paradigm that scientists are able to devote their energies to attempts to solve the detailed puzzles presented to them within the paradigm, rather than engage in disputes about the legitimacy of their fundamental assumptions and methods. It is necessary for normal science to be to a large extent uncritical. If all scientists were critical of all parts of the framework in which they worked all of the time then no detailed work would ever get done.

If all scientists were and remained normal scientists, a particular science would become trapped in a single paradigm and would never progress beyond it. This would be a serious fault, from the Kuhnian point of view. A paradigm embodies a particular conceptual framework through which the world is viewed and in which it is described, and a particular set of experimental and theoretical techniques for matching the paradigm with nature. But there is no *a priori* reason to expect that any one paradigm is perfect or even the best available. There are no inductive procedures for arriving at perfectly adequate paradigms. Consequently, science should contain within it a means of breaking out of one paradigm into a better one. This is the function of revolutions. All paradigms will be inadequate to some extent as far as their match with nature is concerned. When the mismatch becomes serious, that is, when a crisis develops, the revolutionary step of replacing the entire paradigm with another becomes essential for the effective progress of science.

Progress through revolutions is Kuhn's alternative to the cumulative progress characteristic of inductivist accounts of science. According to the latter view, scientific knowledge grows continuously as more numerous and more various observations are made, enabling new concepts to be formed, old ones to be refined, and new lawful relationships between them to be discovered. From Kuhn's particular point of view, this is mistaken, because it ignores the role played by paradigms in guiding observation and experiment. It is just because paradigms have such a pervasive influence on the science practised within them that the replacement of one by another must be a revolutionary one.

One other function catered for in Kuhn's account is worth mentioning. Kuhn's paradigms are not so precise that they can be replaced by an explicit set of rules, as was mentioned above. Different scientists or groups of scientists may well interpret and apply the paradigm in a somewhat different way. Faced with the same situation, not all scientists will reach the same decision or adopt the same strategy. This has the advantage that the number of strategies attempted will be multiplied. Risks are thus distributed through the scientific community, and the chances of some long-term success are increased. "How else", asks Kuhn (1970c, p. 241), "could the group as a whole hedge its bets?"

The merits of Kuhn's account of science

There is surely something descriptively correct about Kuhn's idea that scientific work involves solving problems within a framework that is, in the main, unquestioned. A discipline in which fundamentals are constantly brought into question, as characterised in Popper's method of "conjectures and refutations", is unlikely to make significant progress simply because principles do not remain unchallenged long enough for esoteric work to be done. It is all very well painting a heroic picture of Einstein as making a major advance by having the originality and courage to challenge some of the fundamental principles of physics, but we should not lose sight of the fact