What is in a Paradigm?

The 1960s were a decade of political and intellectual revolution. In 1962 Thomas Kuhn published a book that was not only a revolution in itself but also seemed to describe the very process of intellectual revolution that it exemplified. *The Structure of Scientific Revolutions* was hugely influential in the years that followed, well beyond the field of history and philosophy of science to which it was addressed; it became one of the most widely read academic books of all time. In this article I will explain some of the key ideas of that book: normal science and revolutionary science, paradigms, and incommensurability, before concluding with some remarks about how this all links to some ideas in cognitive neuropsychology.

History and philosophy of science

It would be unusual to find an eminent literary critic, or even poet or dramatist, who is largely ignorant of the works of Shakespeare or Milton. Perhaps it is not even *possible* to be a good literary critic if you are ignorant of the great poets of the past. Contrast this with the historical knowledge of an eminent scientist. Most leading physicists know very little of the detail of the work of Galileo Galilei (Shakespeare's exact contemporary). And it would not matter if they knew none at all. Furthermore, what little they do know comprises a very limited, cleaned up knowledge of Galileo's achievements, presented in modern terminology and formalism. They won't have gained it by reading Galileo's own writing.

Kuhn regarded this relationship between scientists and the history of their fields as significant in two respects. First, the partial and distorted picture of the history of science possessed by scientists has had a negative effect on the historical study of science and even more so on the philosophical study of science. Secondly, this relationship, via its limited role in scientific education, provides an insight into the way scientists learn and think.

Kuhn himself started out as a physicist, gaining his PhD from Harvard in 1949. But he then turned to the history of science and later to the philosophy of science. He experienced first hand the nature of scientific education and the place of history within it. Most scientific textbooks make little mention of history, except perhaps in passing to explain why some law or theorem bears the name that it does. If there is more discussion of history it is likely to be in an introductory chapter in which the various milestones are listed along the road which led to the field's current state. The upshot of this is a certain perspective that scientists are likely to have on the history of their subject. That history will look as if it would inevitably lead to its current state. Science progresses primarily by adding to the truths previously uncovered and also sometimes by correcting earlier errors. Newton's famous remark that he had seen further than others only by standing on the shoulders of giants is quoted by scientists with approval and seems to provide evidence of the cumulative nature of scientific development. Since the purpose of a scientific textbook is to make the logic of scientific reasoning as clear as possible, it is a natural corollary of this limited history that even if it took genius to make some of the greatest past discoveries, the truth of those discoveries, once made known, would have been obvious to those contemporary observers who possessed open minds.

Correspondingly those who held false views in the past either lacked the relevant evidence or must have been stupid, blind, or prejudiced in some way.

As we shall see, Kuhn thought this view of the history of science a mistake in several respects. The mistake matters if it affects the writing of the history of science outside science textbooks, as it might well do in the case of the sort of history of science written by retired scientists (as it once typically was). More importantly, the mistake matters if it affects the philosophy of science. Since many philosophers of science (like Kuhn himself) had themselves been practicing scientists, they would have gained in the course of their studies just the picture of the history of science painted above. And this in turn would affect their conception of the philosophy of science. If the history of science is linear and cumulative (follows a direct path from past to present, adding at each point to the achievements of earlier generations), then it is natural to think of science as aiming at the truth and as succeeding in this aim. In which case, one must ask why is it that science succeeds in this aim. And in answer to this question it is natural to think that there is some logic of scientific discovery or scientific method. In which case one readily thinks that it is the task of the philosophy of science to uncover and articulate this logic or method. Although Kuhn thought that this misleading picture of the development of science is bad for history and philosophy of science, he did not think it bad for science itself. Indeed, he seemed to regard it as entirely appropriate for science students as scientists.

Kuhn's cyclical view of scientific change

In what ways did Kuhn think that the traditional conception of scientific development is mistaken? In most ways. Science does not posses a logic or fixed method. It does not have a grand overall aim such as truth or knowledge. It therefore does not progress by getting closer to the truth adding to the stock of known truths. The contrast between heroes and villains is too stark, with often each being just as reasonable (or unreasonable) in their context as the other.

If the traditional view is wrong, what then replaces it? In summary Kuhn's answer is this. Scientific development is not smooth and linear; instead it is episodic—that is, different kinds of science occur at different times. The most significant episodes in the development of a science are *normal* science and *revolutionary* science. It is also cyclical with these episodes repeating themselves. Nor is it cumulative, since revolutionary science typically discards some of the achievements of earlier scientists. Science does not itself aim at some grand goal such as the Truth; rather individual scientists seek to solve the puzzles they happen to be faced with. There is no logic of science or fixed scientific method. Instead scientists make discoveries thanks to their training with exemplary solutions to past puzzles, which Kuhn calls *paradigms*. Furthermore, whatever the motive and motor of science, we are not in a position to say that science has advanced towards the truth and that recent scientists got things right where earlier thinkers got them wrong. This is because certain kinds of comparison between later and earlier theories are problematic due to what Kuhn calls *incommensurability*, which is the lack of a common measure of theories.

This may sound all very sceptical, and indeed there is a strong sceptical strain in Kuhn's philosophy. However, in his earlier writing, including *The Structure of Scientific Revolutions*, Kuhn took more of a neutral line on questions of knowledge and truth. His view was that a satisfactory explanation of scientific theory change need not consider whether the theories in question are true or false. Let us start with those episodes Kuhn calls normal science. A period of normal science is dominated by an exemplary piece of science, one held up as a model for practicing scientists. An exemplary scientific achievement, such as Newton's laws of motion and gravitation and their application to the problems of the orbits of the Moon and planets sets the agenda and standards for subsequent science. Kuhn sees normal science as a period of *puzzle solving*. The paradigm—the exemplary scientific achievement—provides examples of worthwhile puzzles, provides a guide for solving those puzzles, and sets the standards for assessing proposed solutions. We shall see later exactly how paradigms-as-exemplars work. How they do work is independent of whether the theory at the core of paradigm is true or not; that theory and the exemplary puzzle solution incorporating it can be a template for future science even if false.

That an exemplary scientific achievement provides a fruitful example upon which subsequent scientific successes are modelled does not guarantee that all subsequent puzzles can be solved by reference to the original paradigm. Some puzzles may resist solution; there may be observations that scientists cannot successfully reconcile with the preferred theory. Such puzzles are known as anomalies. Kuhn emphasizes that this is a natural state for normal science. We do not expect to be able to solve all puzzles immediately; perhaps more data needs to be collected or old data needs to be checked; perhaps new techniques need to be developed first; perhaps the scientist who has chosen to work on this puzzle just isn't clever enough to solve it. However, if enough anomalies pile up that resist continued attempts at solving them, then scientists begin to question the paradigm itself. Perhaps the source of the difficulty lies not with the data or the techniques available nor with the competence of the researchers; perhaps instead the paradigm itself is at fault.

If enough scientists think like this then science enters a phase of crisis. Bolder, younger scientists in particular will no longer seek solutions to the anomalies within the model set by the old paradigm. Rather the hunt is on for a new paradigm to solve those outstanding puzzles. Success in this search will come in the form of a new piece of science that differs in important respects from the previous paradigm piece of science. It will solve anomalies because of innovative changes to scientific theory; it may 'dissolve' the anomalies by showing them to be products of parts of the previous paradigm that are now rejected. If that new piece of science is accepted by scientists in place of its predecessor then a scientific revolution has taken place. An example of this is the supplanting of Newtonian mechanics by Einstein's special theory of relativity. The Newtonian paradigm was supplemented by Maxwell's account of electromagnetism which seemed to imply that light must travel though a medium, called the aether, just as sound travels through air. Albert Michelson and Edward Morley set out to detect the aether and to measure the Earth's speed through it. Yet they detected no aether. Hendrik Lorentz and George Fitzgerald sought to explain this anomaly within the existing paradigm, by hypothesizing that objects contract while in motion through the aether. Einstein, however, was able more effectively to remove the puzzle in a revolutionary fashion by postulating that the speed of light is constant for all observers, whatever their motion relative to one another. This requires rejecting the geometry and the conception of space and time upon which Newtonian science was based. Although Einstein's ability to resolve this anomaly was not one of his reasons for believing in his theory, it did play a part in persuading others to accept the revolution he instituted. Once the revolution is accepted the new great scientific achievement (such as Einstein's two theories of relativity) provides a paradigm, an example of how science should be done, upon which other scientists model their own work. A new period of normal science ensues.

Sometimes Kuhn's cyclical account of science is misleadingly presented as if a scientific revolution is a root and branch rejection of the previous paradigm, as if the slate is wiped clean and science starts again from scratch. That is clearly not the case. A revolution will revise *some* of the previous paradigm but not necessarily all of it. To be accepted a proposed new paradigm must retain at least the bulk of the puzzle-solving power of its predecessor. And the scientists trained in the old paradigm, including young and radical scientists, must be able to recognise the new scientific achievement *as* a new scientific achievement. So it must share some similarity to its predecessor. There is progress then in science, not only in normal science but also through revolutions. A scientific field progresses by increasing in overall puzzle-solving power. Note the contrast between this view and the more traditional one that science progresses by getting ever closer to the truth. Kuhn's view is that scientific progress is not 'teleological'; it is not progress *towards* anything (such as truth). Rather it is progress in a more Darwinian way—just as species evolve by acquiring an improved ability to cope with localised conditions (and not towards some perfect form), scientific theories evolve in response to localised puzzles.

Kuhn's characterisation of normal science as 'puzzle-solving' may seem to devalue it, but that was not his intention. Young scientists may aspire to being the next Einstein; but almost all will spend their entire careers engaging in normal science. Even most Nobel prizes are for normal science (and revolutionary science may find it difficult to attract Nobel prizes—the two theories of relativity did not). The point of the term 'puzzle' is to bring out certain aspects of an analogy with puzzles such as chess or crossword puzzles. First, one gets to understand and to be good at crossword puzzles by practising them. The more one does crossword puzzles the easier one finds it to see the solutions. We'll return to this aspect later. Secondly, solving puzzles takes place within a framework. Certain ways of doing things are taken for granted. In solving a chess puzzle one does not propose a solution that involves an adjustment to the rules of chess. Furthermore, the context makes one confident that there is a solution to the puzzle. One should not *need* to adjust the rules of chess to solve it. Similarly, in a period of normal science the paradigm that shapes ones research is not questioned; it is taken for granted and provides the resources for solving the puzzle. I once saw a bumper sticker bearing the words 'Subvert the Dominant Paradigm!'. While Kuhn popularised the use of the now-clichéd word paradigm, his view was that subverting the dominant paradigm is something that scientists should almost *never* do. Science could never make progress if scientists typically sought to solve puzzles by changing the rules. Despite being the philosopher of scientific revolutions, Kuhn's thinking is importantly conservative. In a conference dedicated to broadening the minds of scientists, Kuhn pointed out that a certain kind of narrow-mindedness is important for science to progress. For normal science to solve puzzles in the quantity that it does, most scientists must accept the status quo most of the time. It is therefore appropriate that the little history of science that scientists read in their textbooks should show scientific progress as the accumulation of knowledge or the increasing nearness to the truth, since that is a picture of science as *always* in a state of normal science. A history of science that emphasized revolutions and incommensurability would not encourage the willingness to take paradigms for granted that permits the normal-scientific progress that is characteristic of science to be made. It would be in tension with the very method of scientific education which is the employment of past scientific achievements as paradigm-exemplars that young scientists should seek to emulate.

Paradigms and exemplars

Let us look more closely at Kuhn's notion of a paradigm. Kuhn popularised the use of this term, which has come to mean something like a framework, a dominant way of thinking and doing things, shared expectations and rules. These somewhat vague phrases do reflect something in Kuhn's use of the term; that 'something' he also called the 'disciplinary matrix'. But the central notion of a paradigm for Kuhn, as I have tried to emphasize above, takes us back to the original meaning of 'paradigm' according to which a paradigm is a excellent example, a model to which others aspire. This, Kuhn said, was the most novel and least understood aspect of *The Structure of Scientific Revolutions*.

Kuhn was working against a philosophical tradition that held that the process of scientific discovery, or at least the task of assessing a theory on the basis of the evidence, is a matter of following rules of method or of inductive logic. Although scientists' use of such rules might be to large extent unreflective or unconscious, it was thought to be the task of the philosophy of science to uncover those rules. The rules would lead towards the truth and would thus promote scientific progress (conceived as getting more of or closer to the truth). Not only did Kuhn reject the picture of science as aiming towards the truth, he also rejected the picture of science as operating according to rules of logic or method. The paradigm notion is intended to explain how science does function without such rules. Instead of following rules, scientists seek to match their work to the paradigm in a way the depends on their seeing similarities between their work and the paradigm. Seeing similarities is an ability that cannot be reduced to rules, just as recognising a face or seeing a family resemblance is not reducible to rules. Kuhn felt that the operation of paradigms could explain the whole process of scientific development, without recourse to truth and rules. Paradigms would set the puzzles for scientists to solve, give them the tools to solve those puzzles, and provide the standards by which those puzzles could be assessed.

Let us see how this idea works as applied to the Newtonian paradigm that was at the core of mechanics and mathematical astronomy and much of the rest of physics until the late nineteenth century. Here the paradigm can be identified with Newton's *Principia Mathematica* and the puzzle-solutions it contained. In that book Newton provided explanations of the observed motions of the planets as well as solutions to several problems of motion on the Earth. But his work was not the last word on all such questions. In some cases the results were only approximate because of simplifying assumptions used by Newton. In other cases (such as the motion of the Moon) his conclusions seemed to be inaccurate. When new planets were discovered, their orbits needed to be explained also. There remained a host of more sophisticated motions on the Earth that might also be investigated. So Newton's work created a considerable opportunity for his successors—there were a myriad of questions and puzzles raised by *Principia* or by subsequent discoveries that other scientists could work on. Furthermore, *Principia* provided the tools whereby later scientists could solve those puzzles. The basis of any puzzle solution would be Newton's laws of gravitation and motion. Not only did Newton supply the laws to be used in puzzle solutions, he also invented the calculus (differentiation and integration) that was necessary analyse continuously changing or accumulating quantities (velocity, momentum, energy and so forth). Newton's *Principia* also provided the tools for subsequent puzzle-solving in a more subtle way. Knowing the laws and mathematics will not allow most people to solve the puzzles in question. What most of us need is training. From A level through to early graduate studies a young scientist learns to solve an ever wider and more sophisticated range of puzzles. He or she acquires this capacity by practice with existing puzzle-solutions. By following worked examples in a textbook or classroom the student gets a feel for how such puzzles are to be tackled, which versions of the equations are appropriate, which transformations of those equations might help get an answer, and so on. This sense of how to 'see' a problem is honed by tackling puzzles to which the answers are already known (but not by the student!).

In these way the paradigm provides the puzzles and the tools for solving them. It also provides the standards by which attempted puzzle-solutions are judged. By working with Newton's own puzzle-solutions a follower of Newton acquires not only a sense of what a worthy puzzle should look like and a sense of how to solve such puzzles but also a sense of what a satisfactory solution to such puzzles should be like. A puzzle-solution had better look like one of Newton's own puzzle-solutions to be acceptable. Of course this is not strictly correct. By the nineteenth century few scientists were learning from Newton's own puzzle-solutions. Thanks to changes in symbolism and advances in mathematics, Newton's own puzzle-solutions were presented on a modern form, and it is the modern forms of the solutions that would be the model for young scientists. And thanks to advances in physics they would also be learning by reference to new puzzle-solutions, for example in electrodynamics, which was developed well after Newton. Nonetheless, the modern puzzle-solutions could trace their ancestry back to Newton—Coulomb's law of electrostatic attraction was readily accepted precisely because of its similarity to Newton's law of gravitation. More importantly the key idea is that whatever the current exemplars are, puzzle-solutions are judged not by the application of rules of method or a logic of induction, but are instead assessed on the basis of a perceived similarity to those exemplars.

Incommensurability

Despite the importance he attached to the paradigm/exemplar idea in his early work, Kuhn scarcely mentions it in his later writing. Instead he concentrated on different approaches to explicating his notion of incommensurability. Kuhn noted that the scientific work of a past scientist, Aristotle for example, might seem entirely irrational at first. But on further investigation and with familiarity the reasonableness of his thinking becomes apparent. Some of this might be attributed to language problems, especially where apparently familiar words have changed their meaning. For example, King Charles II exclaimed on first seeing the new St Paul's Cathedral that it was 'awful and artificial'. Far from being a derogatory remark, the King was giving the edifice his highest praise. By 'awful' he meant much what we do by 'awesome'. That is just a case of a simple shift in meaning. The case of 'artificial', however, is more complex. The core of the meaning of that term was not so different for Charles from what it is for us—meaning constructed, designed, not natural. In the seventeenth century the term carried positive connotations: designed with careful intelligence, created with skill, artistic, sensitive, rational. Both for Charles and for us there is an intended contrast with 'natural', but in his day the contrast was a positive one. The aspects of nature with which the artificial is contrasted are the irrational, brutish, unrefined sides of nature. Since the late eighteenth century the dominant conception of nature has changed: nature is supposed to be pure, wholesome, free, intuitive, original. And so for us 'artificial' carries with it the connotations of the impure, unhealthy, imitative. And so even if the core of the meaning of 'artificial' has remained the same (though this could be argued over), the connotations and so the uses of the word have changed. And they have changed because of the changing perception of man's relation to nature: from a view of nature as something dangerous and to be escaped from or improved to something pure but harmed by humans, to which we should try to return. (Contrast Hobbes' and Rousseau's deeply opposed conceptions of human life in the state of nature: nasty, brutish, and short versus free, healthy, and invigorated.)

The case of 'artificial' may help illustrate the sort of problem that Kuhn was dealing with under the heading 'incommensurability'. Changes of that kind make it difficult to compare theories from different epochs. The dominant philosophy of science before Kuhn denied that there should be such difficulties: as regards evaluating the theories for their truth-content, we need only look at their observational consequences; and the meanings of the theories also depend on the relation of the theory to observation. So as long as what is observable remains the same and the meanings of observation words are unchanged, we can use observation as a basis for comparing theories for truth-content and for establishing a shared basis for explaining meaning. One of Kuhn's most important legacies was to persuade philosophers of science that this assumed invariability of observation is untenable. Kuhn pointed to gestalt images (such as the Necker Cube or duck-rabbit) which present one appearance at one moment and a different appearance at another; he also cited research that shows that how we perceive objects depends on what we expect to see: our perceptual experience does not match the way things actually are, if they are unusual. Kuhn believed that ones' perceptual experience and so also one's observations could be influenced by the theory one holds. Hence observation cannot be a shared basis for all theory-comparison.

Although Kuhn's earlier conception of incommensurability is primarily psychological, relating to perception in particular, his later work focussed on linguistic aspects of incommensurability. There has been considerable debate on whether Kuhn succeeded in explaining what incommensurability is and whether it has serious consequences for the philosophy of science (e.g. by showing that theories cannot be compared at all for truth-content). It is probably fair to say that most philosophers of science hold that while there may well be a phenomenon of incommensurability, conceived of as a certain kind of untranslatability, incommensurability nonetheless does not have significant consequences for theory comparison.

Paradigms and incommensurability revisited

Although Kuhn regarded the paradigm idea as the most innovative and least understood aspect of *The Structure of Scientific Revolutions* it played little part in his later thinking, which was dominated by only partially successful elaborations of the concept of incommensurability. Why did he make this perhaps largely fruitless switch? There are at least two reasons. One is that the move from the psychological idea of a paradigm to the more philosophical notion of incommensurability mirrors his professional switch from being an historian of science to a philosopher of science. Another reason is that the psychological message that Kuhn was propounding fell (as he noted) on deaf or uncomprehending (philosophical) ears. Part of this is just another illustration of a psychological incommensurability. The audience, the philosophical audience in particular, was unable to see that gestalt switches could be a useful pointer to the sorts of psychological shift involved in switching paradigms. To them this seemed a weak metaphor that introduced a mystery that explains nothing about theory change. As a way of introducing a new and controversial idea, this was not helpful. Does being told that changing paradigms is like seeing a picture one moment as a rabbit, another moment as a duck, really explain much on its own? Part of Kuhn's problem was that he did not know of the mechanism underlying gestalt switches, pattern recognition, and so on. This meant that he was unable to show how that mechanism might extend to the apparently quite different sphere of theoretical belief.

It is unfortunate that the physiological basis of the relevant psychological processes began to be widely discussed only after Kuhn had made his own change from a psychological to a more philosophical approach and had abandoned interest in the exemplar idea. *Connectionist* or *neural-net* models of brain function suggest that certain kinds of learning involve a Darwinian-like reinforcement of various connections between neurons. The reinforced arrangements of neural connections are those that output the relevant positive results (such as correct recognition of a face or pattern). In the gestalt cases we may suppose that the same pattern is able to output two different recognitional thoughts: 'duck' and 'rabbit'. Which of these it outputs may be sensitive to small changes (such as orientation or even the observer's direction of attention). This mechanism may be applied to non-visual learning. For example we may learn our times-tables by rote. Rote learning is a matter of reinforcing a neural pattern that outputs '42' to the input '7 times 6'. This model applies not only to rote learning. One might start by consciously calculating the answer to 7x6. But in due course the answer comes automatically. This is what is meant by 'second nature'. A certain pattern of conscious activity becomes 'natural' with repetition—'natural' in the sense of unprompted, without thinking or reasoning, or following rules, it is akin to a reflex action. Many patterns of thought may become internalised in this way. Scientific learning and inculcation with exemplars is ripe for this sort of treatment. It is the repetition of laboriously worked through examples in the textbook that allows a science student in due course simply to 'see' without further thought how a certain kind of problem should be tackled. Kuhn was right in thinking, thought he could not know how, that the neural-net/connectionist mechanism underlying the learning of sophisticated patterns in science and the learning of simple patterns in recognizing a gestalt diagram as showing a duck, are the same.

A difference between the gestalt and the scientific cases is that in the former two patterns are available to be recognised, one immediately after the other whereas sciences students are taught to react in just one way to a given problem. However, it might be that it is perfectly possible to get people to respond differently to the same problem. So it might have been natural once to respond to a problem in mechanics using purely Newtonian tools whereas today's students will automatically see the same problem as requiring the application of relativistic mechanics. What can be learned in this way can be unlearned and so scientists who accept a scientific revolution will unlearn one way of seeing things and learn a new way. It is perhaps not impossible for them to be in a gestalt-like situation, so that they can see both the Newtonian and the relativistic approaches as natural (remember that all students learn the Newtonian way first before learning the relativistic way—and they need not completely unlearn the Newtonian way). But now consider someone who comes across the writings of a scientist of long ago. If a scientific inference is second-nature to that ancient scientist and his contemporaries, then they will take that form of inference for granted. They won't spell out the background assumptions or intermediate steps. So that scientist may make a leap of reasoning that will be for him completely natural that for the modern reader, not trained in that tradition, will seem an utter non-sequitur. The problem for the modern reader will not be so much that the ancient author seems to have said something false; rather the modern reader will find it difficult even to see *why* the ancient writer says what he says; the ancient text will not make sense.

If this is right, connectionism not only explains how paradigms-as-exemplars work but also explains what incommensurability is and how it comes about. Incommensurability comes about when different ways of thinking become second nature due to training in different paradigms. Because the ways of thinking are second nature, and so are fairly deeply embedded in the scientists' psychologies, it is difficult or impossible for them to reconstruct an argument or proof in place of the inferential leaps it is natural for them to make. While the case is clearly worse in the interpretation of ancient texts, this kind of incommensurability can arise for thinkers who are contemporaries but who have acquired different patterns of thought.

Conclusion

Kuhn's *The Structure of Scientific Revolutions* was highly controversial among philosophers in the 1960s. I have suggested that this is in part due to the psychological nature of some of the key ideas as well as the lack of a theoretical underpinning to those ideas. Kuhn abandoned this work for a more philosophical line of enquiry that I believe was less fruitful, even if more familiar to philosophers. The time is ripe for a reassessment of Kuhn's earlier work in the light of connectionist and neural-net research.