16 Kuhn and his View of Scientific Revolutions

This chapter will discuss the second part of Kuhn's theory--the final and more controversial part--the question of how we deal with this phenomenon that Kuhn calls scientific revolutions. It is on this issue of scientific revolutions that Kuhn has caused most waves, and unpopularity for himself amongst philosophers, epistemologists, methodologists and other believers of rationality and progress, because as we will see, Kuhn's theory undermines simple ideas about progress and straightforward rational decision-making in science. Before, we continue I just want to refresh your memory about the last chapter (fig. 1). This is Kuhnian life-cycle of a typical science: it is not Science (capital S) it is a science. Each individual science starts somewhere by emerging from pre-science, whether that is the 18th century AD or 4th century BC depending upon the science (chemistry and astronomy respectively). You have normal work under the first paradigm that has emerged. You have the revolutionary overthrow and installation of a second paradigm, and later perhaps a second revolution to a third paradigm dominated normal period.

In what follows I shall be giving Kuhn's views and when I'm stating my own opinions you will notice the difference. The aim of Kuhn's theory is to show that scientific revolutions grow out of normal science. Normal science must give birth to revolutions. You cannot predict them, but you know that the very nature of normal science is to create revolutions that undermine the previous normal dispensation. There is a pattern according to Kuhn; a set of stages or if you like, moments in the emergence and ripening of a revolution. There are four or five steps in this. Kuhn would see them, rightly or wrongly, as always present in every event that he calls a scientific revolution. So we are looking microscopically at the onset and the end or resolution of revolutions. I will briefly go through these moments or stages and then talk more extensively about them.

According to Kuhn there are always problems within the paradigm that have not yet been solved. This is always the case and it is necessary, because the paradigm would not be in business if there were not problems to be solved. Remember that the problems are typically of the form of 'fit'--closing the gap between the paradigm and the relevant data from either end--or problems of 'extension'--extending the paradigm to cover new realms of data with a degree of 'fit' which it is a further problem to improve. This is how a scientist gains credibility, by working on and solving problems.

Now, occasionally, Kuhn claims, a problem or a set of problems surprisingly resists solution by the normal scientists who work within their currently prevailing paradigm. They cannot close the gap or extend the paradigm over a new area of data the way they would like to and expect to be able to. Such a problem, that resists solution and that annoys the practitioners because it cannot be solved, is what Kuhn calls an '*anomaly'* that is, a recalcitrant unsolved problem in the paradigm.

According to Kuhn, the existence of anomalies will trouble some members of the scientific community (it may be only one at first). They will be bothered that an anomaly that has not yet yielded to solution is around. In this situation these people, and I reiterate it may only be one person, will experience a sense of a *lack of confidence in their paradigm*. Kuhn calls this a crisis and he says that it is typical in a crisis situation for those that are bothered to make a bold stake of this form: the anomaly is so bothersome that they are willing to change the paradigm in order to resolve it. The cost

of solving the anomaly is changing the paradigm. This constitutes a bid to launch an embryonic new paradigm.

There follows, according to Kuhn, a debate which has very interesting properties which cannot be resolved straight-forwardly by logic, method or facts. The debate is over which paradigm to choose, the embryonic one or the new one. In the end however, Kuhn claims that these debates are closed (closure is achieved but *not* by predominances of facts, assessment of method or application of a common rational standard). If that closure involves the acceptance of the new embryonic paradigm by a preponderant part of the community then a **revolution** has occurred. If the new paradigm is not accepted then maybe you could say that it was a revolution that was aborted and does not show up in the historical records as a revolution at all. 'Predominant' does not mean 'take a vote' it means something akin to 'how do caucus room decisions about parliamentary leaders get resolved?' Ultimately by a vote, but all the action begins beforehand. 'Predominant' means that a lot of the people with the most influence have finally moved in a certain direction. If a new paradigm has emerged there has been a revolution, and work then proceeds within the new paradigm. The few stragglers refusing to accept the new set-up are judged not to be practitioners of that science any more, because they have not moved with that science and then everyone goes about their business until the next time some anomalies emerge, and the cycle is replayed.

Let us go through those steps in more detail, for they are interesting and very rich in ideas and also very problematical in a way. The essence of normal science is problem solving, and, as I stated before, there are always going to be problems to solve. When a problem is not solved by a first assault upon it, that does not mean that it is an anomaly. Scientists in the community have plenty of work to do and if a problem has proved a little difficult they can always choose to drop it, let it ride for a while and do something else. Sometimes failure to solve a problem will lead to a concerted effort to solve it--the stakes have been raised and the person who solves the problem will receive a lot of credit -- symbolic, professional, material--so therefore it is worth the time and effort in trying to be the first one to solve it. So, some problems that are a little more difficult attract attention and get solved. Note that these tough problems have not occasioned the jettisoning of the toolkit, and are not taken to have 'falsified' the reigning paradigm. It is only in those cases where the problem is seen as important and resists solution (even though a lot of attention is paid to it) that you get an anomaly. I would like to add here that Kuhn does not stop to ask why the different members of the community judge different problems to be of different levels of importance. There I think context comes into play; institutional, social and even larger political ideological contexts come in as to why somebody thinks the problem is really crucial and someone else thinks it is something you can leave aside--recall Copenicus and his initally private view that the equant was unacceptable. In other words, how do they pick or give weight to different problems?

Kuhn gives an example of a problem that should have been an anomaly but wasn't, it is the problem of explaining the motion of the Moon in Newton's theory of gravity. In 1687 Newton published his *Principia*, his system of physics and gravity which solved many problems and established a paradigm. There was lots of work to do and it was very successful, but there were some difficult problems. One problem was a complicated aspect of the motion of the Moon, on which Newton's theory could only achieve 50% accuracy, compared to the existing data (which was not good in celestial mechanics). Newton and his readers knew this and it presented a nice problem for his followers. Unfortunately, it took over 50 years for anyone to dramatically improve that

matter (solve it) for it was only in the 1740s that a man named Clairault, a French mathematician, showed how by a little manipulation of Newton's new mathematics you can bring the predictions and the data into better agreement. Clairault received a lot of credit for solving the problem, but at no stage was the problem ever an 'anomaly' for there was no crisis of the Newtonian paradigm centering on this problem.

Anomalies, I think, are really in the eye of the beholder. Kuhn does not really explain why anomalies induce crisis. Let's take a closer look at an anomaly according to Kuhn. When a few people get worried about an anomaly or a set of anomalies there is going to be a person or a few people, who are willing to place this wild 'bet' that the anomaly/s will only get solved if the paradigm is changed. Usually the people working within the particular science do not want to change the paradigm in order to solve problems. Kuhn would say that the great individuals of the history of science whom we look back upon from a Whig historic view as heroes, eg. Copernicus, Newton, Einstein, Darwin, were people who did not discover new things by induction; and they were not people who falsified old theories by Popperian testing. These people were gamblers, people who were 'poker' players--people who, for whatever reason, perceived an anomaly or set of anomalies and put their stakes on an alternative paradigm to solve those anomalies, and they won. If they had lost they would have been remembered as cranks in the history of science. The heroes of Whig history were not great rational men who have seen the truth better, or used the scientific method better. They are people who played a certain situation in a fairly reckless way and won. This is not a way of looking at great scientists that will ingratiate you to your average philosopher or scholar of scientific method (or old-fashioned historians of science) because it says that the scientists' game is different than usually considered and their behaviour is actually consists of quite different actions than is usually thought.

So, say that we have entered a crisis stage (or a few individuals have) and somebody places a huge bet on an alternative paradigm, claiming the only way to deal with the 'anomaly' is to use and work out a new paradigm. What happens then? This is where the community debate comes into play. The community negotiation, or struggle, is going to determine the outcome of this revolution. This debate, according to Kuhn, may as well be placed on a tape loop for it keeps going around and around in an identifiable pattern.

The debate is like this: You have had an anomaly, and you have some innovator claiming that the anomaly is so important that it requires a change of paradigm. The rebels say first of all that the new paradigm solves their crucial anomalies--that is their reason for getting people to follow the new paradigm. But, obviously if you are not a rebel but part of the old guard who may still win, you can say several different things: You can say, 'What anomaly?' (Kuhn does not actually opt for that, but I think subsequent work since Kuhn has made us realise that anomalies are in the eye of the beholder). There is another fallback position, that there might, or might not be an anomaly, but that the rebels haven't solved it yet. The third position might be that there is an anomaly, you solved it, but we still think it is better to stay with the old paradigm. This old paradigm has worked well in the best and still has a richness and life to it with the potential for further development. Another possibility is the one we saw with the initial response to Copernicus by the 16th century astronomical community--just reabsorb and reinterpret part of the novel challenger--as with their acceptance of Copernicus's banishment of the equant. Of course, the advocates of the new paradigm can come back and say that their new paradigm is a little embryonic and has not had time to develop and hence solve a lot of problems, but if we stick with this new paradigm, it will be much more fruitful that the old paradigm. To these remarks the old

guard could say: That is only a promise, a wish compared to solid past achievements of the existing paradigm. At this point the tape loop closes because the advocates of the new paradigm will probably retort: Your solid past achievements ended with this terrible anomaly that only we and the new paradigm can solve.

You find a lot of these kinds of arguments in scientific controversies and Kuhn has caught the flavour of how many of these controversies are carried out. (Of course, before 1962, people did not talk paradigm language whereas scientists of today carry out scientific debates using Kuhnian language or Popperian language but, they are still carrying out debates of this structure.)

Kuhn has stumbled upon something that is very characteristic and important and hitherto unacknowledged about scientific debates which is that the existence of what Kuhn calls an anomaly does not disprove the old paradigm. Inverting Popper's language: you cannot *disprove* a theory. One unsolved problem does *not* disprove a theory. On the other hand, the new paradigm cannot be proved during this period of negotiation; it cannot in any straightforward way be proved to be superior, so the Inductivists and the Popperians drop away, because the old paradigm cannot be falsified and the new one cannot be established as true. What is going on in a revolution?

Fundamentally why this debate is difficult and why it has no simple solution is that the two paradigms load two somewhat different sets of facts and problems. (fig. 2) According to Inductivism and Popper, when one theory replaces another there is a straight comparison between the problems that were previously solved and those that the new one solves and the new one is better. According to Kuhn there is no absolutely strict comparison because they have only certain problems or facts in common.

You must be careful here, for when Kuhn wrote, he often made a mistake or a slip of the pen (or perhaps there were deeper reasons). He said something which is absurd. Kuhn writes that sometimes the two paradigms create two entirely different worlds of facts or problems. (fig.3) This is historically implausible and probably humanly impossible, in the kind of situation that he is describing. What I think he means is the former case, that there is not a total overlap between the two competing paradigms.

The reason you cannot have two completely different sets of facts is that you have people from the same community and tradition who make that new paradigm. Copernicus is not from Mars--he is working in the tradition of Greek astronomy. His view of astronomy is going to be different from others but not totally different. There is not totally complete overlap, for if there were Popper and the Inductivists would be right. The situation needs only to be something like figure 2: Paradigm 1 and Paradigm 2 with the overlap where all the problems and facts are the same. As long as there are little sectors of fact(s) or problem(s) that hang out at the ends, the two paradigms cannot be straightforwardly judged.

Kuhn has a word for this situation which he calls the 'incommensurability' of paradigms, which means there is no single agreed measure of deciding which one is better. Commensurable and incommensurable are mathematical terms which work in this way: (fig. 4) we have two straight lines and I ask which one is longer then we have a debate/controversy. There would be no controversy if we all agreed that the one on the right is longer. This is possible because we have a common unit of standard that we can apply to both. We are agreed on the method of applying the unit measure to both lines and we are agreed on the outcome of the measurement.

What Kuhn is saying, is that paradigms don't have single agreed measures and so they are incommensurable. It does not mean that they cannot be compared at all; it does not mean that one flips a coin to see which is better; it means there is *no single agreed measure*. We saw this when Copernicus invoked the same criteria that Ptolemy and Aristotle did, but he wanted to add an extra criterion and wanted to weight it differently. Copernicus is said in effect, 'judge my paradigm a new way' and the other people, the Aristotelians and Ptolemaics said in effect, 'we do not have to judge the paradigm by your standards and we can judge our theory in our own way'.

Incommensurability is an important concept in Kuhn's writing. If only he had expressed it more clearly, for he makes it sound as though one paradigm came from Mars and the other from Venus and that they had nothing in common. This is impossible. Incommensurability is crucial because it means that no single method, no simple rule or criterion, can decide this debate and that is the richest and most important result in Kuhn. From this comes the dismissal of searching for a scientific method and our asking instead, "what do these people do when they debate and how do they ever close their debate?" The answer can only be the result of social, psychological investigation of their behaviour, rather than pretending that they used a method which gives a single, agreed measure of the two competing paradigms.

You may still be troubled by the conclusion that the two competing paradigms load or shape two somewhat different sets of 'facts' and 'problems', so I will give some examples. Let's look at Ptolemy and Copernicus. Now consider the statements, 'Mars is a planet', 'Venus is a planet'. These are facts in both the Ptolemaic and Copernican views. But, in Ptolemy it is also a fact that 'the Sun rises and the Sun sets'. This is not a fact in the Copernican view, for there are other facts that has replaced this Ptolemaic view. Similarly, for 'problems'. 'Make up a model for the motion of Mars', is a problem in Ptolemy and for Copernicus. It is the same problem. They share the problem. 'Make up a model for the motion of the Sun around the Earth', is not a problem you can solve in Copernican view, for it does not happen. Another problem: 'Make up a model for the motion of the Earth'. This not a problem for Ptolemy but it is for Copernicus. Here we see what Kuhn means by incommensurability. How can they decide which paradigm is better by comparing facts and problem solving, if they do not completely agree on the facts or the problems or on the criteria of selection of facts or of solution of problems?

How, then, is a decision ever reached? Here, I suppose I have to go a little beyond Kuhn because I am not sure that he fully answers this question. Kuhn waved in the direction of answers which further problematised the issue for research by other historians of science. The first thing is, we have a debate that is not going to be resolved by method. We do not want to hear fairy stories about rational method and decision-making for there is no common measure. The other side of the story is, that it is not an invitation to irrationality or to flip a coin, which is what Kuhn has been accused of by philosophers (who say Kuhn is stating that science does not follow any particular method).

There are plenty of places in human life where decisions are made and no scientific method is used and neither do we roll a dice or flip a coin. For example, in courts of law; in industrial relations arbitration; in party caucus rooms; in policy-making bodies. In politics in general. In human institutions in general.

If a group of people are operating in an institution or a court of law etc., they may negotiate, but in the long run they know they have to decide because they would not be

in business if they do not decide. I think what Kuhn means, is that the scientists have to make a decision otherwise the business of science stops in this particular speciality. Scientists negotiate, make arguments; people have different positions in the argument. If I am a young rebel who has just invented a new paradigm, my neck is on the line therefore I will fight very hard for the acceptance of the new paradigm or my professional standing will be non-existent. What if I am an established professional: I have long standing investments in equipment, post-graduate students; I have a reputation based on my skills within the old paradigm. I am not going to move my position unless something really good can be shown to exist in the new paradigm so that I will move on the issue. People have investments and interests, and positions within the debate. Everyone tries to persuade and exert pressure, possibly make concessions. It is politics (the kind of politics you find in a human decision-making situation). Some people have more power than others; some have more followers than others; some people are more persuasive than others. What Kuhn is saying, I think, is scientific decision-making is nothing special. If you are going to investigate scientific decision-making it is just the same as walking into the Arbitration Court and observing the behaviour of the parties in the game. There is no magical recipe by which scientific decision making is accomplished.

So, how did the Copernican Revolution unfold--at least as far as we have thus far studied it? No-one we have studied so far--Copernicus, Tycho, Kepler, Galileo--has produced a 'method' which will resolve the issue. They are still battling and will continue to battle. We continue to need sociological and historical explanations as to how it went. Kuhn's view is not what philosophers have been telling us since Aristotle. Kuhn may not have given a very good account of paradigm, scientific debate and incommensurability, but at least he set us on that kind of empirical path to investigate those kind of activities.

Finally let's look at a related problem that Kuhn raises that has really upset philosophers: the question of progress which is closely related to the question of method. If there is a method, clearly you can make progress. According to Kuhn there is progress during normal phases, in the sense that if scientists improve the fit and extend the scope, then the paradigm is making progress; but to what end? Not towards mirroring nature, because all you are doing is closing the gap with data or covering new data. You should not state that normal science is making progress towards reality, but it does make progress in the problems to which it poses to itself. Those solutions may be quite useful and effective in the real world.

What about revolutions? Does progress in the new paradigm pick up where progress left off with the old paradigm so that the progress is continuous? Popper tried to develop a theory of scientific revolutions which gave us progress through them. We recall that in Popper's view the second theory, victorious in a revolution, is definitely better than the first, and the third, after the second revolution, better than both. Kuhn, on the other hand, recognises once you have said that there is incommensurability between competing paradigms, it is hard to speak of simple continuous progress across the revolutionary shift of paradigm. For, when the new paradigm is accepted and starts making progress how can you actually say that it makes progress from exactly the platform which was left by the previous paradigm. Remember, some of the facts change, problems change and standards change. There seems to be a chasm in between them where you cannot hook them up completely so that you could say there is a clear linear sense of progress across the period of revolution.

This is what Kuhn is stuck with and it makes him uncomfortable. I do not feel uncomfortable with this because I believe that progress is a retrospective construct. People look back and define 'progress' in social and cultural retrospectivity. The winners look back and claim progress. After all, this as how history presents itself to us in the standard Whig history. Of course, the winners write history and they are anxious to show that the second paradigm inevitably beat the first paradigm, because it was better, (for that was the reason it 'won' they say) rather than, it is called 'better' because it beat the first.

Whig history, written by the victors, will look back and lose sight of lots of things such as the old paradigm having a lot of life in it. The old paradigm could have continued solving its own problems. Whig history ignores the fact that in a revolution the orthodox view has a lot of support and that good arguments are given by people defending the old view. You do not hear about this in Whig history. Those are the lessons of Kuhnian history and they do unfortunately reflect on the problem of progress. It is very hard to decipher what 'real' progress is in a revolution.

But, here is one fly in the ointment--are there really any revolutions? Do scientific revolutions of this type actually take place? Has Kuhn got it right about this phenomena? We shall reflect further on this issue later in Chapter 25. Perhaps Kuhn has overstated the existence of the phenomenon of revolution in science--but perhaps, equally, his points about incommensurability and problems of simple progress hold in more 'garden variety' science as well--we shall see!

Finally, what is a science according to Kuhn? A science, for example, astronomy as you have studied it, is a succession of socially constructed frameworks, or paradigms, within which for a time a group of specialists worked to solve problems, one paradigm at a time. Periodically, a crisis occurs in a paradigm, leading to a debate on two competing but incommensurable paradigms, and eventually leading to some resolution that might mean some radical change of paradigm--a revolution. The history of each of the sciences is not one smooth trajectory of collection of true facts; the history of each science is not a story of dramatic falsifying theories and their replacement by theories that are obviously better. It is something very messy, historical, political, social. The message you will get in this text is that from the standpoint of current history and sociology of science, we work in a post-Kuhnian 'paradigm', a perspective not identical with the writings of Kuhn, but much influenced by him and by subsequent work responding to his theories.

FIGURE 1

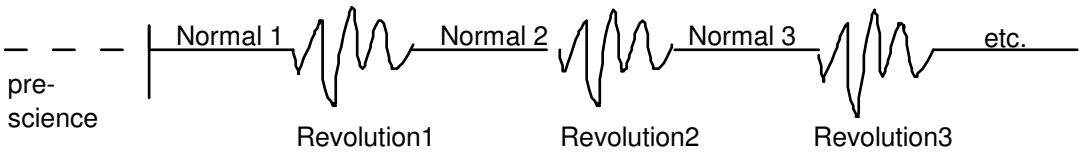


FIGURE 2

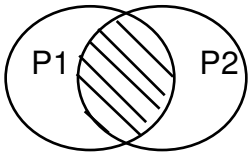


FIGURE 3

Kuhn is
not saying

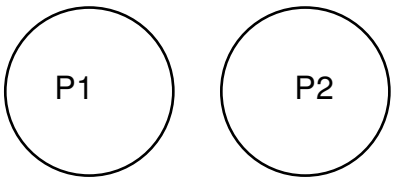
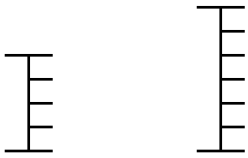


FIGURE 4

Commensurable: a common measure exists



In-commensurable: only some overlap of facts, problems, solutions
And,
No Single agreed outside yardstick for evaluating the two paradigms

There are various non-agreed yardsticks INTERNAL to one or the other paradigm for "evaluating" the two paradigms