

# 8 Kuhn and the Nature of Science and Scientific Revolutions

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## 1. Introduction: Kuhn and the History & Philosophy of Science

During the course so far we have looked at two accounts of scientific method, Popper's account and the traditional account based on the generalisation (induction) of facts. We have seen that they do not throw much light on what we know about the history of science and the conduct of scientific research. Scientific change, debate and work is more complicated, for social and political reasons, than those stories tell us. The conclusion we have reached is an artificially constructed version of the situation that pertained in the field of History & Philosophy of Science, 25 to 30 years ago. At that point work in our own field of history and philosophy of science was very much affected by the appearance of Thomas Kuhn's book *The Structure of Scientific Revolutions* (1st ed.1962; revised ed.1970).

Kuhn's work on the nature of scientific change has affected thinking in many areas--not only the history & philosophy of science but also general history, sociology, political science, anthropology and even art history. Although an educated person today can afford to be ignorant of Popper, one cannot afford to be ignorant of Kuhn. We may say that Popper was moving in the wrong direction while Kuhn was moving in the right direction, although very little of what he actually said is agreed to today even by his followers.

Kuhn, who was born in 1922, was a physicist turned historian of science and then philosopher and sociologist of science. He was aware of the difficulties of theory-loading and the problems that it posed for the story of traditional method. He was aware that Popper's method story does not really grasp the dynamics of scientific change. Kuhn's project was to develop a general theory of how natural science works and develops. But, unlike most general theories of how science works, Kuhn's theory does not depend upon believing that there is some sort of scientific method that gives the answers. Kuhn did not construct a philosophy of science or method, for he was trying to examine the *dynamics* of scientific change: how any given science *changes* over time.

One of the main things to grasp about what he was trying to do (whether you accept it or not) is that Kuhn believed that he had discerned or outlined a common pattern (dynamics or life story) that each individual science

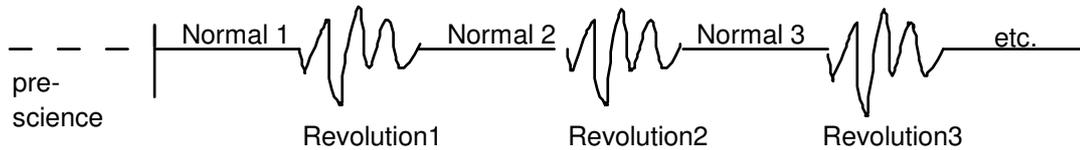
undergoes. In other words, the reason we can have a general theory of science is that each field of science has a life history that bears analogies and resemblances to the development of the other fields of science. It was this common pattern that Kuhn was trying to elucidate: Not as a common pattern of method, but as a common pattern of social and political behaviour among scientists which produces the similar life histories or patterns of development and change within those different sciences.

There are a number of premises which make up Kuhn's viewpoint and it is important to remember them. The first premise is that there is no such thing as Science (capital S). We cannot say "Science began with the Greeks" or "Modern Science started during the 17th century". If we talk in this way we do not achieve a decent understanding of the sciences or their histories. Kuhn was interested in the **histories of the sciences**. He was not interested in Science (capital S) which he viewed as an invention of public relations and rhetoric. If you like, it is also an invention of the method story. There is no 'Science' there are only 'sciences'. The next point is that Kuhn did not believe that there is a scientific method. His theory partially explains how scientists work and yet not share any *common* method. The third point he made concerned the common pattern of development and change which we see unfolding in the history of each science.

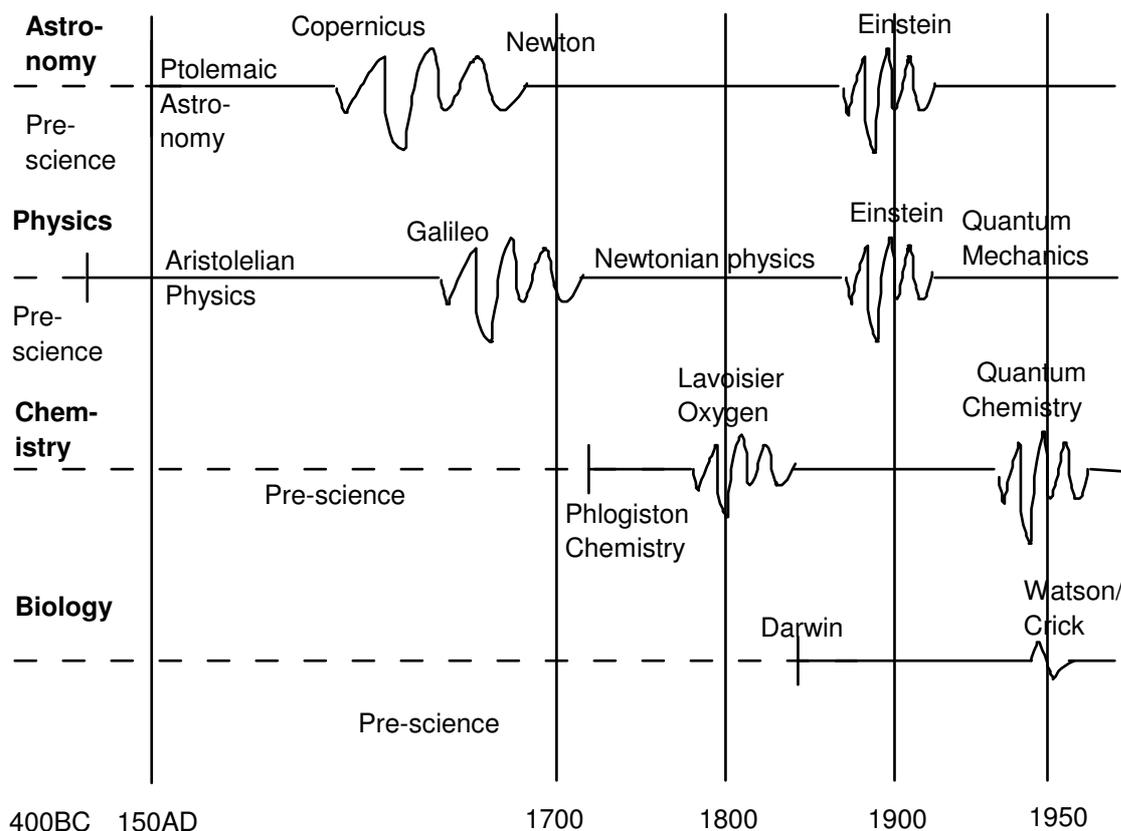
Kuhn started by being impressed by what he considered to be an important historical fact: In examining any science we find that its history alternates or appears to alternate between two qualitatively different phases or periods. (fig. 1) The first period which Kuhn called a 'normal' period is characterised by great stability and agreement about the basic theory. During a 'normal period' the basic theory is used and applied but not questioned or undermined. We have periods like that according to Kuhn in the history of astronomy and the history of the other sciences. However, there are also other kinds of periods which he calls 'revolutionary' periods or 'scientific revolutions'. In scientific revolutions there is no consensus or agreement about basic theory, for there is always debate, conflict and disagreement, because the fundamentals are under question. According to Kuhn, such periods of scientific revolution end when one theory (a new and very different one from what was accepted previously) emerges out of the revolutionary 'ruck' and is accepted on an agreed basis forming a new period of normal consensual scientific work. Until, of course, another revolution occurs. There is one other thing to keep in mind: when you look at an area of science, such as astronomy, physics or chemistry, according to Kuhn there is always a period prior to the emergence of a first normal

period--a time before there is any technical, theoretical or goal consensus about how to pursue issues in that general area of natural inquiry. Kuhn calls these periods before a first normal period, the 'pre-science' period of the science in question.

**FIGURE 1**



In fig. 2, we have pictured the alternating patterns of normal and revolutionary stages in the sciences of astronomy, physics, biology and chemistry and their respective 'pre-science' periods of varying length. Take chemistry; for example: according to Kuhn, the pre-scientific stage in chemistry lasted throughout the period of the Greeks, all through the Middle Ages and up to the 17th century. The first really serious technical and theoretically sound chemistry (according to Kuhn) only occurred around 1720, and in fact it was our old friend, the so called phlogiston chemistry of the 18th century!

**FIGURE 2**

As we know, this chemistry was built around the idea that when something burns there is a substance, phlogiston, given off by the burning or flammable substance. Kuhn would say that this was the first normal period of chemistry, but rather soon, the phlogiston chemistry was challenged then overthrown. A revolution occurred and the new theory of Lavoisier based on the concept of oxygen replaced the old chemistry. This led on to further revolutions in chemistry concerning firstly atomic theory, and, in this century, quantum mechanics.

To take another example: Physics is the study of motion and causation. According to Kuhn, the first serious version of a science of Physics; that is, the first normal period of research in physics occurred under the guidance of Aristotle. The pre-scientific period in physics ended during the 4th century BC with Aristotle, and it was a long time until *the* Scientific Revolution of the 16th and 17th century when Aristotle's physics were challenged and overthrown. This led to a new and completely different physics, the so-called Classical Physics of Galileo and Newton. Newton's physics reigned supreme for 200 years until it suffered a dual defeat and displacement in the early 20th century with the advent of Einsteinian physics and quantum mechanics. This is how Kuhn tends to see the map of the history of science. These are the basic facts

that Kuhn is working with and how he sees the basic history. Now come the problems of explanation and understanding.

Following in Kuhn's footsteps we have two tasks: We want to know what he means by a 'normal' period of science. For example, what goes on in the 'normal period' of physics from Newton to Einstein, or in the normal period of phlogiston chemistry from Stahl to Lavoisier? What are the social and institutional mechanisms and dynamics of science in such normal periods. Secondly, what is a 'Scientific Revolution'? Why do they happen? How do they relate to the normal science that comes before them? How do they relate to the normal period that always seems to follow them? Kuhn's answer to those questions combined with the information in figure 2 constitutes his theory. It is quite a different way of asking the questions and setting up the questions than the Whiggish or methodological way where the story of Science is the story of some one, unique method.

## 2. Kuhn on Periods of 'Normal' Science

In general what is a 'normal' period of science like? What do scientists do in their field when they are in a period of 'normal science'? This is what they do *not* do--they do not collect facts in order to generalise them into theories: they are not following the old method story. Another thing they do *not* do is desperately run around trying to falsify the theories that they hold. According to Kuhn, scientists do not behave the way Popper suggests they behave (at least in normal periods of science and in fact he does not believe that they do this in a Revolutionary period either). As scientists during normal periods of our particular science we operate within an all-embracing theoretical framework which is peculiar to our science at that point in time. If we do not believe in the prevailing theory, then we do not count as professional members of the community. This effectively means that our work will not be accredited by members of the community and will not at this stage be considered a part of 'real' science.

This all embracing theoretical framework is what makes work possible in a field at that point in time. Our theoretical framework is what loads our experiments, observations and descriptions; it controls the problems we define; it is what controls the solutions to our problems. Accordingly, scientists are very reluctant to give up their theoretical framework. Kuhn has a name for this all-embracing framework which defines work in a given field of science during a given normal period: it is called a '**paradigm**' (a term that has

now passed into literature). So there occurred in chemistry a phlogiston or Stahlian paradigm, and then an oxygen or Lavoisierian paradigm; and in physics, first an Aristotelian paradigm and then a Newtonian paradigm. **A paradigm is an all embracing theoretical framework that defines scientific work in a given moment or period within one particular field of science.**

Basically a paradigm consists of three parts:

(1) The basic laws and concepts of the science at that time. Kuhn states this itself is not enough, because philosophers have often talked about science as consisting entirely of its basic laws and concepts. They have missed the other elements that go into a paradigm and so have missed the real guts of how science works.

(2) All the experimental and instrumental procedures for attaching the concepts and laws to concrete situations. There is a very important insight involved in this: It is that instruments are not neutral. Instruments are embodiments or materialisations of theory. Instruments are theory-laden or if you prefer -- instruments are paradigm-loaded. Previously we have seen Galileo struggling to conduct experiments on motion--his experimental apparatus were highly theory and assumption laden.

(3) Any paradigm has a set of underlying deep cultural assumptions which shape it. The set of deep cultural assumptions is called the paradigm's metaphysics.<sup>1</sup>

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<sup>1</sup>What this means is that scientific laws are stated in terms of general concepts that are available in our cultural & conceptual grid -- we cannot assert a law about a concept that is not in our conceptual grid. The concepts in our conceptual grid are already tied up and connected with each other before we can assert a new connection. Those pre-existing relations and connections can shape and colour the type and kind of newer relations that we draw as laws.

To put it another way, a theory and the concepts used in a theory always have a context, a surrounding atmosphere of other beliefs and theories that have shaped how those concepts are used in the new law or how a new theory is put together. What historians of science have found is that these background beliefs that shape a new theory or a new law, can be beliefs of any kind ie: religious, political, or philosophical; they can also be beliefs based on some other science which is already established. Any bits of prior belief can provide the presuppositions or the **metaphysical background** for a new law or a new theory. And it obviously follows that there are never new laws or theories that do not have presuppositions behind them or some metaphysical background.

This is important because we think that in the history of science and in science generally, the metaphysical background of a theory or law affects the meaning of the theory. Secondly, it affects the facts that are relevant to that theory, and thirdly, the metaphysical background of a theory affects the goals and the tactics of research within that theory.

Each paradigm is defined by the basic laws or concepts, the paradigm-loaded experimental and instrumental procedures and the metaphysical background that has shaped that paradigm. During a scientific revolution what we *change* is the paradigm. Kuhn does not talk about changing 'theories', only 'paradigms'. He means that the basic laws and concepts change, the instruments and experiments change or they are construed differently. Often, the metaphysical background to the new paradigm is different from that of the previous paradigm. The Newtonian and Aristotelian paradigms of physics did not have the same metaphysical background, and similarly, the metaphysical backgrounds of Newtonian and Einsteinian physics are different.

Now we come to the final set of points that are important for us to grasp about normal science. We come to the question of what the scientists do inside their paradigms, where they seem trapped until the next revolution. Actually Kuhn recognises that point and states that scientists are happy to be trapped because then they know what to do and what tools they have for doing it. If they were not 'trapped' they would be confused and would not know what to do. What we do inside a paradigm is pose, and solve, problems. Our paradigm is our life-blood and life-line because it: (1) helps us to define problems; (2) gives us the tools for solving the problems, and (3) gives us the standards or the criteria for judging whether we have done a good job in solving the problems. This sounds rather closed and narrow; but, according to Kuhn, there are important things to be done. There are two broad categories of problem which we call 'problems of fit' and 'problems of extension'. These are not Kuhnian terms but my own, for they interpret a lot of what Kuhn says.

The problem of 'fit': it could be problems of fit in Stahl's chemistry during one period, or problems of fit in Einstein's physics in another. We have the paradigm and we try to make predictions from it in order to explain various phenomena. What is a prediction matched to? What is an explanation an explanation of? Predictions are predictions about 'data'. Explanations are explanations about data. Kuhn used an inappropriate word here when he stated that scientists try to make predictions that 'fit the paradigm to nature'. Now no-one is fitting any paradigms to *nature* for people are fitting predictions to *data*. Data are, of course, theory-laden, selected and interpreted.

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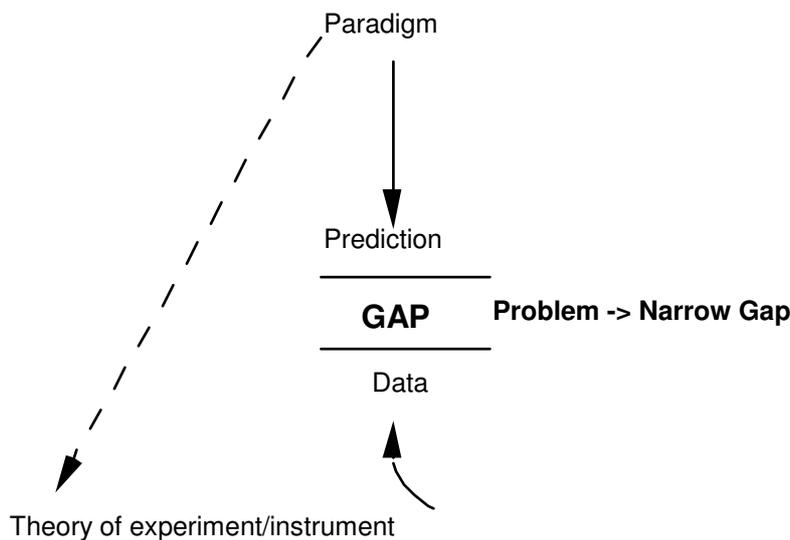
Historians of science before and after Kuhn have extensively traced the existence of these deep cultural presuppositions residing within theories--especially in the period of the Scientific Revolution of the 17th century and in the Darwinian Revolution of the 19th century. Such issues are explored in HPSC 2100 **The Scientific Revolution**.

In fact it could be that it is the paradigm that is loading the production of the data.

As we saw studying Galileo, and later Popper, there is always a gap between our prediction and our data. Trying to make that gap smaller is a problem of fit. (fig.3) Anything we can do **within** the paradigm to make the gap smaller on a given problem is a 'successful' piece of work within the paradigm. So, we might juggle the paradigm a little and the predictions a little, or we might alter our production of data: through different processes of selection we might interpret it differently, alter our theory a little -- anything to close the gap! Problems of fit are, as Kuhn says, problems of matching paradigms to nature; but as I said, they are more correctly problems of closing the gap between paradigm predictions and relevant, selected, interpreted, theory-laden data. The difference between these two remarks is the difference between 1962 and the 1990s. Kuhn talked about nature in the raw, as it were nature presenting itself directly to the objective, passive recording gaze of the methodical scientist. But such a naive view of facts and testing is the last thing we should be discussing in the history of science during these 'post-Kuhnian' times.

**FIGURE 3**

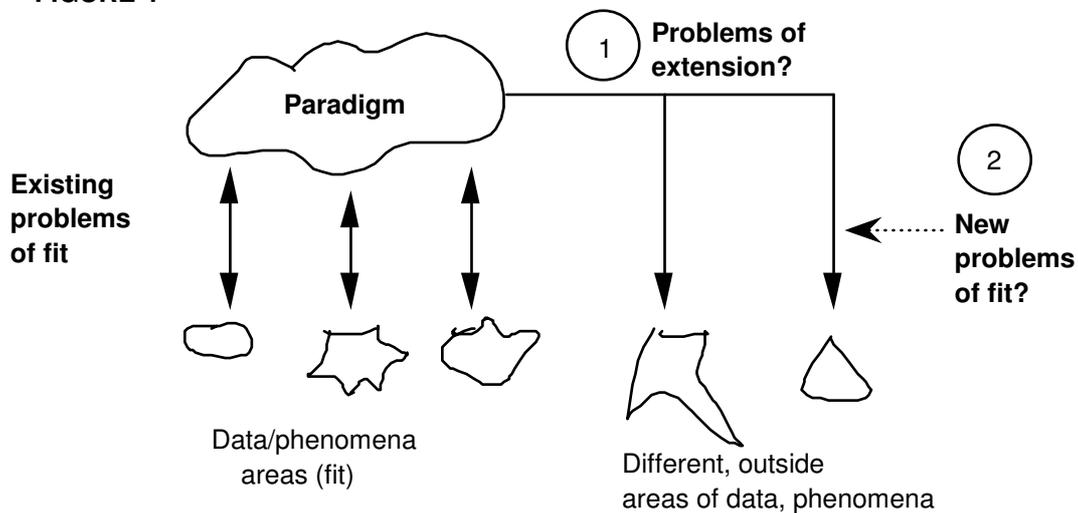
**PROBLEMS OF FIT**



Problems of extension on the other hand can be illustrated in this way: the problem of extension is a challenge to extend the paradigm so it makes explanations and predictions about new areas of phenomena. The term 'phenomena' in this context means relevant pieces of selected interpreted data. Let's say in figure 4 that we have a paradigm that already explains or predicts

some realms of data and people are struggling to get closer and closer. Someone may say that we haven't looked laterally at some other type of phenomena, or can we perhaps use our paradigm to explain them? Scientists in this way would be looking to *extend* their paradigm. Of course as soon as we extend our paradigm we then have questions of 'fit' in the newly 'conquered' areas of phenomena. So the ideal paradigm is one that creeps all over every realm of relevant data and phenomena, and covers them with smaller and smaller gaps. In that sense, the most successful paradigm ever was probably Newtonian physics which went from strength to strength in explaining more and more different types of phenomena and explaining them on an increasingly accurate basis. This in fact is Kuhn's prime example of a very successful paradigm.

FIGURE 4



We should at this point note a few things before we finish our discussion of 'normal science'. There is no scientific method and there is no general method for constructing Lavoisier's chemistry or Newton's Physics. The way to do Lavoisier's chemistry is to learn to do chemistry Lavoisier's way. The method of Newtonian Physics is entirely different because we first learn Newtonian Physics, and then do it. The method of Quantum Mechanics is again entirely different because we study Quantum Mechanics for 6 or 10 years then do Quantum Mechanics. This is Kuhn's viewpoint; there is no general method; there are only the paradigms of different sciences at different moments in their histories.

There is a useful analogy which we use to explain what normal science is actually about. The normal scientist is to his/her paradigm as trained professional tradespeople are to their tool kits and the techniques of their trade. Therefore, scientists are like trained master electricians or master carpenters in the sense that they only take on problems that their tool kits and

techniques show can be solved. They never change their tool boxes and techniques in a radical way *if they can help it*. They hold on to them at all costs, for they are the very way of defining and solving the problems that arise in their particular paradigm. If they do not succeed on occasions (if the plumbing leaks or the wiring develops a short) that does not mean that our electrician discards the box of tools, far from it, this simply goes down as a problem that was not addressed the right way on the day and that can be sorted out sooner or later using the same tools. The problem is never with the toolbox. We can draw the analogy to the paradigm in the "normal" course of events: the problem is never with the paradigm; we never falsify the paradigm for we ultimately live and die as professional scientists by the paradigm.

The question now becomes: If Kuhn's analysis of normal periods is correct, why are there ever any Revolutions. For these normal scientists sound very boring, trapped by their own paradigms and involved in nit-picking little exercises of problem-solving. Kuhn's answer is that the very process of using the given paradigm ultimately helps to undermine it!

### **3. Kuhn and his view of Scientific Revolutions**

It is on this issue of scientific revolutions that Kuhn has caused the most controversy, and if you like, the most unpopularity for himself amongst philosophers, epistemologists, methodologists and other believers of rationality and progress. This is because, as we shall see, Kuhn's theory undermines simple ideas about progress and straightforward rational decision-making in science. In what follows I shall be giving Kuhn's views and when I'm stating my own opinions you should hopefully notice the difference.

The aim of Kuhn's theory is to demonstrate that scientific revolutions evolve out of normal science. Normal science must give birth to revolutions. We cannot predict them, but we 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 moments in the emergence and ripening of a revolution. There are four or five steps to this process and Kuhn would see them, rightly or wrongly, as always present in every event that he calls a scientific revolution. Let us briefly go through these stages and then discuss them in more detail.

According to Kuhn there are always problems within the paradigm that need to be solved. This is always the case and it is necessary, because the paradigm

would not be tenable if there were no 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 within a particular paradigm.

Kuhn claims that occasionally a problem or a set of problems surprisingly resists solution by the scientists working 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.

Kuhn stated that the existence of anomalies would necessarily trouble some members of the scientific community (it may be only one member at first). They will be bothered by the existence of an anomaly that has not yet yielded a solution. In this situation these people, and I reiterate it may only be one person, will experience a *lack of confidence in their paradigm*. Kuhn called this a 'crisis' and he said that it is typical in a crisis situation for those that are bothered to make a bold stand of this form: **to them 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 old one. In the end, however, Kuhn claims that these debates are closed (closure is achieved but *not* by factual 'proofs' [whatever they may be]; assessments 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 perhaps we could say that it was a revolution that was aborted and so did not show up in the historical records as a revolution at all.

'Predominant' does not mean 'take a vote'; it means something akin to the way in which caucus room decisions about parliamentary leaders get resolved:

ultimately by a vote, but all the action begins beforehand. 'Predominant' means that 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. Then everyone goes about their business until the next time anomalies emerge, and the cycle is repeated.

Let us go through these steps in more detail, for they are interesting and very rich in ideas, but also very problematical in a way. The essence of normal science is problem solving, and, as 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 concentrate on 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, and material. Therefore it is worth the time and effort in trying to be the first one to solve it!

Some problems that are a little more difficult attract attention and get solved. 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 we 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 degrees of importance. In other words, how do they pick or give weight to different problems? It is here, I think, that context comes into play; institutional, social and even larger political ideological contexts apply as to why somebody thinks the problem is crucial, while someone else thinks it is something that can be safely left aside.

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 with 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. 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. 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 the 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 could bring the predictions and the data into closer 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 centring 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 will be a person or a few people, who are willing to place a wild 'bet' that the anomaly/s will only be solved if the paradigm is changed. Usually, however, the people working within the particular science do not want to change the paradigm in order to solve the problem. Kuhn would say that the great figures of the history of science whom we look back upon from a Whig historic view as heroes, eg. Copernicus, Newton, Lavoisier Einstein, Darwin, were not people who 'discovered' new things by use of method; they were not people who falsified old theories by Popperian testing. These people were in fact 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 won! If they had lost they would have been remembered as cranks in the history of science.

For Kuhn, the heroes of Whig history were not great rational men who have seen the truth more clearly, or used the scientific method better. They were people who played a certain situation in a fairly reckless way and yet won. This is not a way of looking at great scientists that will ingratiate you to your average philosopher of scientific method (or old-fashioned historians of science) because it says that the scientists' game is different than usually considered and their behaviour actually consists of quite different actions than is usually thought.

Let us assume 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 attempted 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 unfolds like this: We have an 'anomaly', or at least some members of the community claim to perceive one, and then we 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, 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 this position, but I think subsequent work since Kuhn has made us realise that anomalies exist in the eye of the beholder).

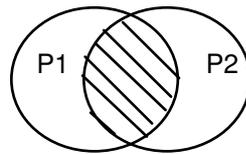
There is another fallback position, that there may, or may not, be an anomaly but the rebels haven't solved it yet. The third position might be that there is an anomaly, you rebels solved it, but we still think it is better to stay with the old paradigm. This old paradigm has worked well in the past and still has a richness of life to it and the potential for further development. Of course, the advocates of the new paradigm may return and say that their new paradigm is a little bit embryonic. It 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 than 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: our solid past achievements ended with this terrible anomaly that only we and the new paradigm can solve.

One finds a lot of these 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 may carry out scientific debates using Kuhnian language or Popperian language but, they are still carrying out debates of this structure.)

Kuhn, stumbled upon something that is very characteristic, 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: we cannot *disprove* a theory. One unsolved problem does *not* disprove a theory. On the other hand, the new paradigm cannot be proven during this period of negotiation. It cannot in any straightforward way be proven to be superior so the old method story and the Popperian method story both drop away. This is because the old paradigm cannot be falsified and the new one cannot be established as being true simply by appealing to "the facts".

Fundamentally why this debate is difficult and why it has no simple solution is because the two paradigms load two somewhat different sets of facts and problems. (fig.5) According to the old method story and Popper's method story, when one theory replaces another there is a straight comparison between the problems that were previously solved and those that the new one solves. According to Kuhn there can be no absolutely strict comparison because they have only certain problems or facts in common.

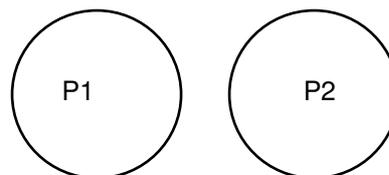
**FIGURE 5**



We must be careful here, for when Kuhn wrote, he often made mistakes or slips of the pen (or perhaps there were deeper reasons). He said something which to us today seems quite absurd. Kuhn wrote that sometimes the two paradigms create two entirely different worlds of facts or problems. (fig.6) 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, in terms of the facts and problems 'loaded'.

**FIGURE 6**

Kuhn is  
not saying



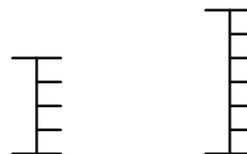
The reason we cannot have two completely different sets of facts is that people from within the same community and tradition make the new paradigm. Lavoisier was not from Mars--at first he worked in the tradition of phlogiston chemistry. His views of chemistry were thus different from others, but not totally different. Yet, there is never a complete overlap, for if there were, then Popper and the old method story would be right. The situation needs only to be something like fig. 5: Paradigm 1 and Paradigm 2 with the overlap occurring where all the problems and facts are the same. As long as there is a small set of facts or problems that hang out at the ends, the two paradigms cannot be straight-forwardly judged.

Indeed we have seen a version of figure 5 before, in Chapter 5 on the construction of Oxygen during the 18th century. There we argued precisely this point, that the two theories loaded somewhat different sets of facts, NOT the exact same set of facts and NOT two totally different sets of facts. And because there was an overlap in the facts but only a partial overlap, there could be no simple unequivocal criterion for deciding which theory to prefer. Phlogiston theory could do a few things that oxygen theory at first could not do; but oxygen theory also did that, whilst ruling non-existent large slabs of phlogiston theory. **As we shall now see, this is precisely the situation that Kuhn was identifying as the relation of old to new paradigm in any scientific revolution!**

Kuhn has a word for this situation which he calls the 'incommensurability' of paradigms, meaning that there is no single agreed measure for deciding which one is better. Commensurable and incommensurable are mathematical terms which work in this way: (fig. 7) we have two straight lines and if I ask which one is longer, then we have a potential debate or controversy--but not for long, because in such a case we would all very quickly agree 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. We reach a firm consensus and we reach it quickly, because we all accept the unequivocal standard of measure and its practical application to this case.**

**FIGURE 7**

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

**Now, what Kuhn is saying, is that paradigms do not have single agreed measures and so they are incommensurable. It does not mean that they cannot be compared at all; nor does it mean that one flips a coin to see which is better; it means that there is *no single agreed measure*.. As mentioned above, we saw this in the attempt during Chapter 5 to compare the virtues and weaknesses of phlogiston and oxygen theory of the 18th century as each side**

saw them. There was no single agreed measure or criterion for assessing the competing theories (paradigms): the criteria you did apply depended upon what problems and avenues you valued; what your professional investments in skills and reputation were; and where you stood in the field of chemistry. People did make decisions, but there simply was no one, unique, overriding "rational" criterion to work on which every relevant player accepted and used.

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 that Kuhn provides. 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 and 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'. Actually, all we need to do is recall Chapter 5 where we compared oxygen and phlogiston theory. There we saw, for example, that oxygen theory declared phlogiston non-existent (but insisted on the new substance, 'caloric'); that oxygen theory claimed to solve the problem of acids (they all contain oxygen); but that it could not solve a key problem long resolved in the view of phlogistonists--why is there a family of metallic substances. 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? This is what Kuhn really means by incommensurability.

How, then, is a decision ever reached in a scientific community undergoing a 'revolutionary debate? Here, I suppose I have to go a little beyond Kuhn because I am not sure that he fully answers this question. Kuhn wavered in the direction of answers which have been developed by later historians and sociologists 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 essentially what Kuhn has been accused of suggesting by philosophers.

There are a number of areas in human life where decisions are made and no scientific method is used, nor do we roll the dice or flip a coin. 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 must reach a decision. I think what Kuhn means, is that 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. If I am an established professional: I have long standing investments in equipment, post-graduate students, etc; I have a reputation based on my skills within the old paradigm. I am not going to move my position unless something really spectacular can be shown to exist in the new paradigm which will force me to move on the issue. People have investments and interests, and positions within the debate. Everyone tries to persuade and exert pressure and possibly make concessions.

The questions thus take on a political aspect (the kind of politics we find in a human decision-making situations). Some people have more power than others; some have more followers than others; some people are more persuasive than others. What Kuhn is saying, is that scientific decision-making is nothing special. If we 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 case. There is no magical recipe called 'method' by which scientific decision making is accomplished.

Finally let's look at a related problem that Kuhn raised that has really upset philosophers: the question of **progress** which is closely related to the question of method. As we already know, if there is a method, clearly you can make progress--both the old method story (bricks in the growing wall) and Popper's method story (lengthening lists of successful predictions) claim that progress is real, simple, measurable, obvious and explainable. According to Kuhn, progress occurs 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 we are doing is closing the gaps between paradigm predictions and relevant domains of data, or covering new domains of data with 'satisfactory' paradigm predictions. Even

Kuhn states that we should not think that a normal science is making progress towards reality. He believes it does make progress in terms of the problems which it poses to itself and that the solutions may be quite useful and effective in the real world.

What about revolutions? Is there some clear sense of progress picking up in the new paradigm where progress left off with the death of the old paradigm? We saw that Popper tried to develop a theory of scientific revolutions which gave us progress through them: **the new, better theory, picks up the trail of progress precisely where the old, falsified theory left off, with nothing lost or changed in terms of extent of predictive success!** In Popper's view the second theory, victorious in a revolution, is definitely better than the first, and the third, after the second revolution, is better than both. Kuhn, on the other hand, recognises that once we have said there is incommensurability between competing paradigms, then it is hard to speak of simple continuous progress across the revolutionary paradigm shift. For, when the new paradigm is accepted and starts making its own "normal" progress, how can we 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 the two successive paradigms where we cannot hook them up completely to see a clear linear sense of progress across the period of revolution.**

As a former student of Kuhn, and as a 'post-Kuhnian' historian of science, I do not feel uncomfortable with this position because I believe that 'progress' is a retrospective construct or social label. People look back and define 'progress' in social and cultural retrospectivity. The 'winners' in any scientific controversy look back and claim progress. After all, this is how history presents itself to us in the standard Whig version. 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') rather than, it is called 'better' because it beat the first.

As we have learned, Whig history, written by the victors, will look back and lose sight of things such as the old paradigm having a lot of life left 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. We do not hear about this in Whig history. These are the lessons of Kuhnian history and they unfortunately reflect poorly on the reality and simplicity of progress

across scientific revolutions. It is very hard to decipher what 'real' progress is in a revolution, except by bowing to the winners' post-facto whig history of their glorious revolution!

In the end, how do we classify a science according to Kuhn? A science, for example, chemistry as you have studied it, is a succession of socially constructed frameworks, or paradigms, within which for a time a group of specialists works to solve problems, one paradigm at a time. Periodically, a crisis occurs in the current 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; nor is the history of each science a story of dramatic falsifying theories and their replacement by theories that are obviously better. It is something much more messy, historical, political, and social than that.

#### **4. Kuhn vs Popper - Not Who is Right, but Who Better Promotes Improvement in our Understanding of the History & Sociology of Science?**

1. Popper attempted to save a crisp, clear criterion of progress across the divide of revolutionary changes of theory. As mentioned above, Kuhn's account of a paradigm change questions the existence of a simple, unequivocal criterion for evaluating 'progress'. The entire notion of incommensurable (only partially comparable) paradigms, and the necessity for judgment, negotiation and choice mean that differing criteria are applied by different players, and that even if a new paradigm, like Lavoisier's, triumphs, there is no trans-paradigmatic criterion of 'goodness' that unequivocally endorses the new paradigm. The winners, however, have the privilege of writing the history of science in terms of 'progress' and they then inherit the field of research, devoting effort to filling in the weak points and gaps in their newly crowned paradigm. On the whole Kuhn's account seems closer to what the study of historical cases actually reveals.

2. According to Kuhn scientists, at least during normal periods, work within their tool-kit or paradigm; it provides the tools, the problems and the criteria of solution; it therefore is not subject to attack and overthrow in routine practice. Popper's picture of 'good' and 'proper' scientific behaviour involves constant and determined attempts to 'disprove' the current theory, and ready acceptance of evidence that counts against it. While this may account for some behaviour during revolutionary crises--when advocates of one paradigm seek

to attack the basis of the competing paradigm--it does not seem to accord with Kuhn's more realistic picture of scientists as highly skilled, and somewhat conservative tool-users. Kuhn, however, may have made normal science too rigid, and normal scientists too conservative.

3. Popper seems to think that scientific decision making is not very contentious and falls into crisp patterns of easy consensus-formation. For example, for honest scientists, a test falsifying a theory's prediction has a clear agreed outcome, leading to quick rejection of the 'falsified' theory. Kuhn is closer to seeing that scientific decision making is a complex and fluid process of consensus formation, involving people with genuinely held differing views grounded in varying assessments of the state of play, the best way forward, and their own best investment of professional reputation and skills. This is particularly clear in Kuhn's account of decision making in revolutionary science. His view of normal science may, like Popper's, be a bit simplistic in sociological terms, puzzle solutions are quickly and relatively easily accepted. However, Kuhn may be wrong about this, and as we shall see below in point 1 of the next section, there may be significant negotiation and fluidity of judgement in decision making during relatively routine research as well.

4. Popper, as we stressed, stands firmly in a grand tradition of thinking about science as based on some unique, transferable method. To understand scientific method is to understand the nature of science, the pattern of its history, and the social and ethical demands of its practice. Kuhn stands radically apart from this. He rejects the existence of a single, efficacious, transferable method that is the essence of how science is done. Indeed he rejects the existence of Science per se, preferring to work in terms of actual research fields and sub-fields apparent in the historical record. In this he reflected trends that had already developed amongst historians of science prior to 1962 and which have grown greatly since his time with the assistance of Kuhn's own work. Modern work in the history and sociology of science, and in general sides clearly with Kuhn, method is not the key to science, but it must be studied as a major 'cover story' and legitimation for science in the history of the West.

## **5. Modifications to Kuhn's View in Recent Work in History & Sociology of Science**

Kuhn may have had the better of Popper as far as realistic interpretation of the history of science goes, but that does not mean that Kuhn's views are definitive and have proven impervious to alteration. The fact of the matter is that very few would now endorse Kuhn's views in a literal fashion, although a great deal of the thinking in the history and sociology of science that has modified Kuhn was stimulated by his work and was aimed at improving it. A fair bit of the material in this book actually reflects this sort of "post-Kuhnian" work in the history and sociology of science, and now that we have been exposed to Kuhn (and Popper) we can bring out some of the post-Kuhnian improvements that articulate Kuhn's original impulses, and further render method-centric accounts, such as Popper's, increasingly irrelevant.

One key difference between post-Kuhnian historians and sociologists of science and Kuhn himself is in questioning the basis of Kuhn's stark differentiation between normal and revolutionary phases in the history of a science. Kuhn's normal scientists are very conservative, their paradigm never changes and is never questioned, as it serves as a basis for problem solving. Yet, in a revolution, massive changes of paradigm are envisioned, and Kuhn, in his wilder moments, speaks of the two contending groups as living in entirely different scientific worlds, the contending paradigms remember have nothing in common. Modern and in particular workers in the history and sociology of science, tend to question this picture, making it less black and white. What emerges is a picture in which there is always negotiation and social construction of results, even in what Kuhn would call normal research, whilst in revolutions, if they occur at all, there is a shift **within** a culture or tradition, not a battle between armies from different intellectual planets. We can explicate this revision of Kuhn along several related dimensions of scientific activity that we have studied so far.

1. Normal science within a paradigm can be loosened up a bit compared to Kuhn's vision in which the paradigm tool-kit is virtually a straight jacket which is never affected by ongoing normal work until and unless there is a crisis and revolution.

A more realistic view, supported by much case study research, is that paradigms are constantly subject to partial re-negotiation and modification within and as an essential part of 'normal research'. If a problem can only be solved by advocating some shift, however so slight, in the previous conception of some aspect of the paradigm, then we can say that problem solutions may involve feed-back alterations in the paradigm being used. Alterations carried

over into the next round of research where further alterations may be suggested as part of the problem-solving process.

In this way the paradigm is not just a tool-kit, but a tool-kit that can be altered by the artisans as they go about solving problems! Such bids to alter the paradigm slightly must be accepted by the relevant community. Such small scale rounds of negotiation and jockeying for position will surround even small bids for alteration. **We may term a noticeable alteration which has been negotiated into place as a 'discovery'.**

For example, when Priestley 'discovered' dephlogisticated air, he was not just solving a problem using an unchanged phlogiston paradigm. He was also asking his fellow phlogiston chemists to accept a new phlogiston-related 'object' that could be used in further research as both a tool and target of research. When they accepted his 'discovery' they slightly altered the sum total of the phlogiston paradigm for the next rounds of play. Contra Kuhn, significant 'discoveries' can be made that modify a paradigm without overthrowing it. Those 'discoveries' are constructed claims, consensually accepted as problem solutions after disciplinary negotiation.

Similarly, when Humphrey Davy, in about 1810 showed that Lavoisier had been wrong about oxygen being the acid-maker, by showing that hydrochloric acid contains no oxygen, he was making a pretty large bid to modify the new oxygen paradigm. His claim was accepted, and oxygen theory was not 'falsified' or rejected in a revolution, but simply modified by a 'discovery' from within its own field of research.

**2. If normal science involves discoveries [constructed claims to modify the paradigm in some way that are consensually accepted] and is less boring and rigid than Kuhn thought, then correspondingly perhaps revolutionary science is not so dramatic and wild as he thought it to be. Perhaps a revolution is just a relatively large modification of a paradigm, with the key innovating players legitimating their action with a 'rhetoric' of revolutionary overthrow of the bad old theory.**

Our study of Lavoisier points toward exactly this possibility. Recall that we already know that Lavoisier was a phlogiston chemist, and that his new theory overlapped in part with the old phlogiston theory. Kuhn was wrong to speak of two totally different non-communicating theoretical worlds. Moreover, when we studied the construction ['discovery'] of oxygen [and of caloric] we

saw that Lavoisier did not prove phlogiston does not exist, rather he readjusted conceptual categories (and their related practices) to marginalise phlogiston and to create or construct 'oxygen' and 'caloric'. His discovery, his construction, was based on and related to phlogiston theory and its practices, and indeed his caloric looks a lot like a form of latter day phlogiston.

We can now see that this relates directly to the issue of a post-Kuhn view of revolutionary science. The key point is that Lavoisier was claiming a discovery: that is, he had constructed a new linkage of material practices and modified concepts that he wanted the community to accept. He could have played for a significant discovery within phlogiston chemistry by suitable description of his new linkage claim, or he could have gone for a major displacement of the existing concept, as he in fact did. In the former case he would be seen as a major, normal phlogiston chemist, in the latter case he has been seen as a successful revolutionary.

**The issue of constructing claims and fighting for their negotiated acceptance is the key, whether we are dealing with normal or revolutionary science. Kuhn's own normal science is too rigid, too lacking in serious, socially negotiated modifications (discoveries) and his revolutionary science is too wild, too fast and furious, not allowing for the fact that the same sort of debate is occurring, within one, not two communities, and that much of the difference may be the presence of some kind of revolutionary rhetoric and program for seizing the centre of the field from its former inhabitants on the part of younger practitioners on the make and playing for high stakes.**

## **6. Finale--Toward a Politics of Testing and Experiment**

It should already be clear to you that Popper really did not seriously attend to the issue of the gap. He seemed to think that the outcome of a test is clear and easily agreed to by the participating parties. Either the theory in question passed the test, or the test falsified the theory. But as we saw through using the material on Galileo's experimental tests, Popper missed the unavoidably human necessity of negotiating both the size and meaning of the gap in any given case. This denial of judgment, negotiation and micro-politics in tests vitiates Popper's account of a method of experimental falsification.

Kuhn, to his credit sees, at least in his account of normal science, that gaps are always present, for he defines problems of fit in terms of scientists trying to see if they can improve upon the size of a gap left by the last round of research on

a given problem. (He did not see, as noted above, that attempts to close a gap may involve claims to modify the existing paradigm.) But, like Popper, Kuhn seemed to think that the players would easily arrive at an agreement that a previous size of gap has now been narrowed a bit, and hence a problem 'solved'.

Our study of the Galileo case showed players--on the same side--who differed about the **size and meaning** of the gap, and hence about the necessity to work to close it further, and who no doubt would have argued about the best alterations to adapt to close the gap, had they agreed on that strategy.

In Chapter 9 we shall see just how complex judgments and negotiations can become when they concern the size and meaning of a gap, and the possible theory-alterations necessary to help define and close them. We shall see that the whole tortured and unfinished recent history of solar neutrino research revolves around these 'un-closed' negotiations, and hence that Kuhn, like Popper, was much too naive about the social and political complexity of 'finishing' a test to the players' consensual satisfaction.

Moreover, as we shall see in the case study that constitutes Section 3 of this subject, the politics of testing and of experiment resides at the basis of the huge scientific, medical, technological and environmental controversies that characterise recent times. So neither the Popperian nor the Kuhnian picture of science are adequate in this respect, although, again, it is Kuhn more than Popper who has inspired the very improvements we have just been discussing.