10 Understanding the History and Social Aspects of Science : Rethinking Discovery and 'Progress'

This lecture concludes and summarises the first section of the subject. I shall begin with three key points, A, B and C that we have been attempting to develop during this section of the course.

One of the things that should now be clear to us about the social, political and historical aspects of science is that we do not get anywhere if we maintain the old naive view that nuggety given facts exist in nature just waiting for us to discover them. So the first key point is that:

> **(A) • There are no purely given facts - none given to be discovered, or given to end a test.**

This is entailed by all of our material about the perception of facts depending upon the humans involved and their cultural/theoretical and language grids. We learned that human facts are not in nature but result dynamically from human interaction with nature--facts arise when humans encounter nature, bringing their cultural maps and grids with them.

You may still think that that is all very nice, but that having a cultural or theoretical grid is a kind of 'bias', and surely somewhere there are 'good guys' without any such biases. The argument here has been that there is no such thing (although the method story, and common myths about science, technology and society, tend to assume such is the case.) So there is no privileged position of 'gridlessness'.

We have also learned that in a given culture or given sub-culture of a particular culture, just about everybody is gridded up in the same fashion. The differences only really exist at the margins; nobody ever totally throws away their grid. The jockeying for position, the difficulties, the negotiations of facts tend to take place at the margin.

This is what we have seen in the scientific cases we have studied. Galileo, for example, argues with his best friends, not with his worst enemies--they are going to judge differently the meaning of a test, and the meaning of a 'gap', making different investments in ever so slightly differing accounts of the gap and the relevant 'facts' arising from the test.

For, Galileo, the test 'proved' his law of fall allowing him to proceed further in the main arena of physics. His colleagues had doubts about the meaning of the gap, and saw a chance to make a claim (stake a possible fact) that would place them in the game, inside the negotiational scrum--perhaps the 'gap' was too big and they could 'discover' something in trying to close it themselves.

Or, take Priestley and Lavoisier working with similar if not identical lab procedures and arguing about what conceptual map or grid is to be applied to them. We saw that Lavoisier posed a very interesting case of an individual scientist differing **with himself** over the shape of the grid, at different stages in his career. He started as a phlogiston chemist, and later redrew the grid to rule out phlogiston, then he created a new grid space for 'things' called oxygen and caloric. He did not **discover** oxygen, let alone caloric, nor did he **prove** phlogiston did not exist--he redrew the grid of theory, and remapped its links to material practices--experiments and tests.

We saw John Bahcall do similar things in the solar neutrino test case. At one stage in his career he was cautious and defensive about the size and meaning of the gap. Later, when he was more established in the field--he embraced the gap as a 'problem' imposing a challenge upon himself to provide an answer.

So it is at the margins that differences and changes of grid are primarily raised and negotiated, leading to what we call the 'discovery' of new facts (their construction and negotiated acceptance) and the 'disproof' of 'fallacious' old facts (their deconstruction, dismantling and marginalisation).

We can say that science is a human institution devoted to the continual transformation of its own conceptual grids--but mainly at the margins, rather than the cores of the then ruling theory/grid. (This is related to Kuhn's idea of normal science, puzzle solving within a ruling 'paradigm'. It is also related to my refinement of Kuhn to include the idea that every novelty in normal science demands slight renegotiation of one or another aspect of the ruling paradigm [cf chapter 8]).

Science is in business to produce cultural change in its own substance and practices, but mainly and usually at the margins--although such changes can unintentionally force major remappings from time to time (That is a more advanced way of talking about Kuhn's idea of 'revolutions' in science as wholesale shifts of 'paradigms.)

> **(B)• All that I have just said seems to focus on individual scientists' grids: Galileo's grid, Lavoisier's grid, Bahcall's grid. The next important point therefore is that yes, important moves and bids are made by individuals in science, but nothing relevant and definitive ever happens in science unless a significant part of the scientific community accepts the claim, the bid, the suggested shift in grid--whether marginal or large.**

Decisions in science are ultimately not based on brilliant individuals--who of course often may initiate change. An individual may frame a new claim, a bid to shift the grid and so alter in some way the given pattern of fact and theory. But ultimately the decisions are **social**, they depend on what we have called reaching a consensus about the status of the new claim on the basis of expert negotiation.

An **individual** does not really make a discovery in science. Like Lavoisier, an individual may bid to make a discovery, but he has to carry the community, or a significant portion of the community with him. Bahcall and Davis each bid for a solar neutrino telescope: they did not quite agree with each other about how to make that object exist in the context of slightly modified grids of background fact and theory, and in the end, no relevant community of specialists has agreed consensually on either bid, and so the drycleaning fluid telescope has not come into existence as an accepted object or instrument of science. [cf chapter 9]

Consensus formation is the key to understanding how scientific knowledge (facts and discoveries and altered theories) are made and unmade in the history of science. Now, clearly in the human process of debate, claim, counter-claim, negotiation, interpretation and judgement there is scope for historians and sociologists to study the humans who are engaged in this peculiar science-making and science-breaking behaviour.

All this further suggests that the grids, theories and facts that scientists hold at any one moment are ultimately the product of the latest rounds of debate, negotiation, and struggle in that particular field, or specialty. The currently accepted facts, theories and experimental/instrumental practices are the sedemented residue of the last few rounds of negotiation in the field. They result from the complex human history of the field, how its has developed internally and been influenced, if at all, externally. The current grids etc are not a sanctified pile of nuggests of truth--bricks of fact in the wall--won by applying a method: No, they are a revisable, re-negotiable bank of cultural resources--intellectual and material, which will be used, recycled and certainly changed, at least at points on the margin, during the next rounds of 'research'.

That is indeed what it means to have achieved something in science--that your claims have been (for the time being) accepted and form part of the base line with or against which further rounds of claims are made and debated. Your own claims may later disappear (or be 'proven wrong' just as Stahl's phlogiston was), but that does not make you a fool or even mean you made a mistake--that is how Whig history of science would look at such an episode. It just means that once your claims were part of the grid and later they were revised or dropped or reinterpreted. Just to have been a part of the game in this way makes one's work significant. If your claims never play a role in the grid, you are nobody; if your claims once formed a significant part of a grid, but are now gone, you are a great scientist. Great scientists do not discover eternal nuggety truths--they construct robust claims that are big parts of grids for significant passages of research--even if later they must drop out.

A major discovery does not have to be a permanent nuggety fact--how could it be? A major discovery, like oxygen, is a major reconstruction of a grid and a set of practices, shaping perceptions and reports (facts) that are accepted by the relevant group, until they are later changed or dropped. Lavoisier was not wrong about (modern) oxygen or right about (modern) oxygen--both are whiggish views. Rather he made a major and successful bid in his time--and he discovered (better constructed) an object called oxygen that has an historical relation to later constructed objects of the same name. Similarly Stahl was not some fool--as Whig history would have it--who was not smart enough or objective enough to 'discover' oxygen. No, he 'discovered' (that is constructed) phlogiston, and oxygen, when it was 'discovered' (constructed) owed part of its being, its geneology in concept and in material practices, to Stahl's temporarily triumphant grid or paradigm.

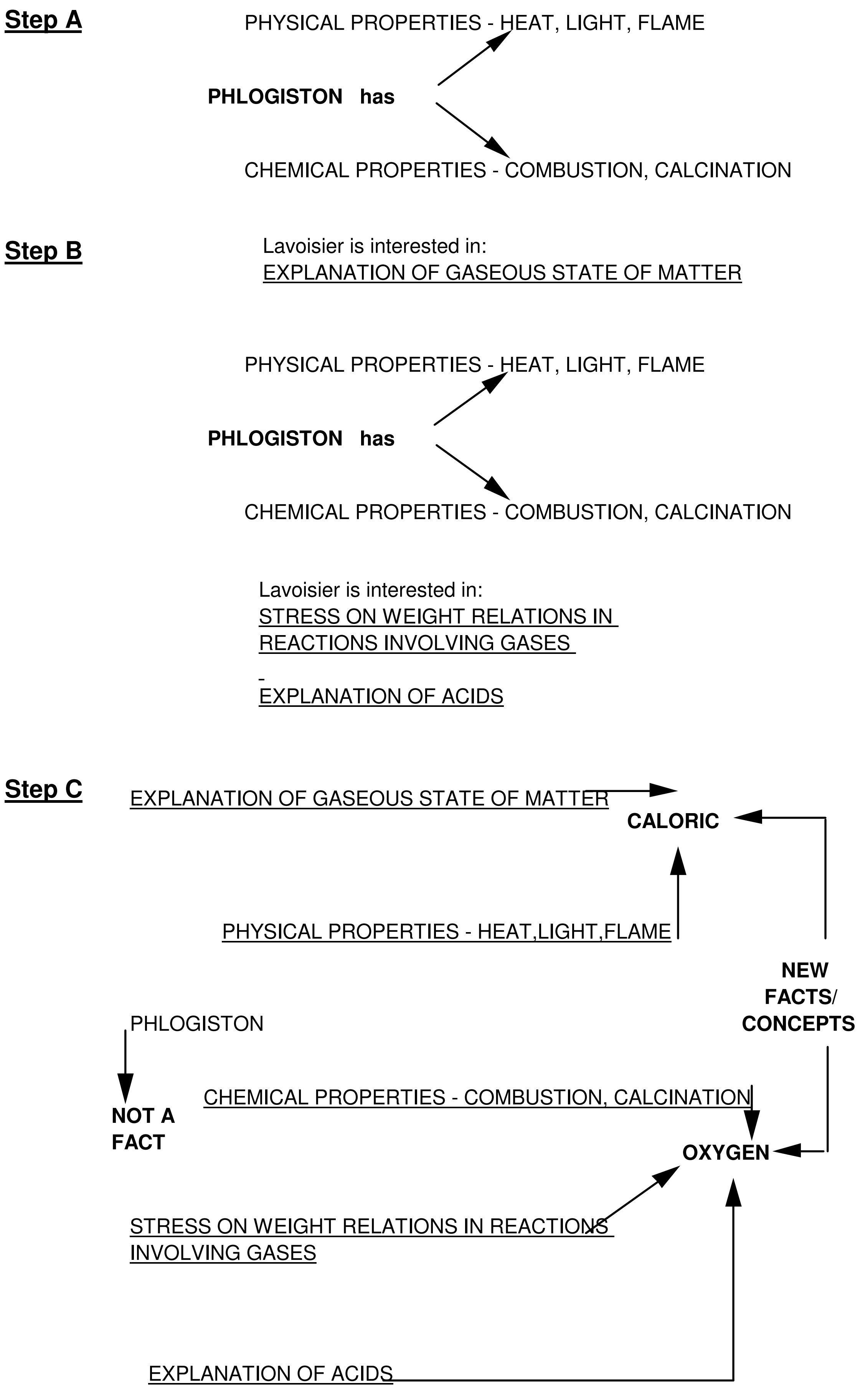
So we have gone from a psychological point about perception and grids, to a sociological point about consensus being a social product, and consensus/closure being what makes a discovery or a fact.

(C) • Now we come to a crucial point--whatever has been agreed in the last round of work--for example, this 'thing' was 'discovered'; this test had the following result; this fact was established--whatever is agreed implies some shifts somewhere in the pre-existing network of accepted scientific knowledge--facts, theories, ways of using and understanding hardwares.

Every decision about a discovery, or a test, or a hardware, implies some shifts have to be negotiated in the pre-existing framework of ideas, and web of practices. We reached this conclusion in improving upon Kuhn's ideas--and we did so by reflecting on the cases we had already studied. [Chapter 8]

As I have stressed all along, this is what a discovery really is. It is not like bumping into the American continent and finding pre-existent nuggety facts. It involves rejigging the previously accepted grid. You will recall figure 1, used in Chapter 5, showing how Lavoiser constructed (discovered) not only oxygen, but caloric as well. He started from phlogiston, and we then saw him rejigging that concept in the light of his interests. Oxygen and caloric were created at once as ideas and as related practices, that borrowed from and displaced the old ideas (and practices) concerning phlogiston. You can't 'discover' oxygen and caloric without rejigging the old map, the old culture, in which phlogiston was a 'fact' an 'object' indeed itself a 'discovery' that had been made by Stahl years before.

FIGURE 1


Step A
PHYSICAL PROPERTIES - HEAT, LIGHT, FLAME
PHLOGISTON has
CHEMICAL PROPERTIES - COMBUSTION, CALCINATION
Step B
Lavoisier is interested in:
EXPLANATION OF GASEOUS STATE OF MATTER
PHYSICAL PROPERTIES - HEAT, LIGHT, FLAME
PHLOGISTON has
CHEMICAL PROPERTIES - COMBUSTION, CALCINATION
Lavoisier is interested in:
STRESS ON WEIGHT RELATIONS IN REACTIONS INVOLVING GASES
EXPLANATION OF ACIDS
Step C
EXPLANATION OF GASEOUS STATE OF MATTER
CALORIC
PHYSICAL PROPERTIES - HEAT,LIGHT,FLAME
NEW FACTS/ CONCEPTS
PHLOGISTON
NOT A FACT
CHEMICAL PROPERTIES - COMBUSTION, CALCINATION
OXYGEN
STRESS ON WEIGHT RELATIONS IN REACTIONS INVOLVING GASES
EXPLANATION OF ACIDS

And in figs 2,3,4 and 5 which we have seen before in Chapter 9, we saw how the solar neutrino telescope **could** have been slotted into some possibly changed re-negotiated background grids in chemistry physics and astro-physics, if it were to become a solid scientific instrument.

The solar neutrino telescope exists or does not exist--takes its place in these maps or not, depending upon whether the relevant communities decide that the test is 'closed', that the gap is 'small enough', by agreeing on what changes and alterations in current grids need to be made in order to agree that the gap is 'small enough' and that the test is 'over'. Since no agreement on the size of the gap is made, and no agreement is reached on how one might alter existing disciplinary grids to make that gap small enough--no telescope is ever agreed consensually to exist. Had the changes of grid been made, and the telescope agreed to exist as a reseach artefact, then the rules and resources for the next rounds of research in the relevant fields would have been altered. Major 'discoveries' would have been made in 'getting the telescope to work'. Research grants, for example, would have been awarded for other scientists to go out to Montana and use Davis's 'telescope' in their own researches. Others would have tried to repeat and improve the performance of Davis's new telescope, and a whole new specialty in astronomy would have been opened.

FIGURE 2

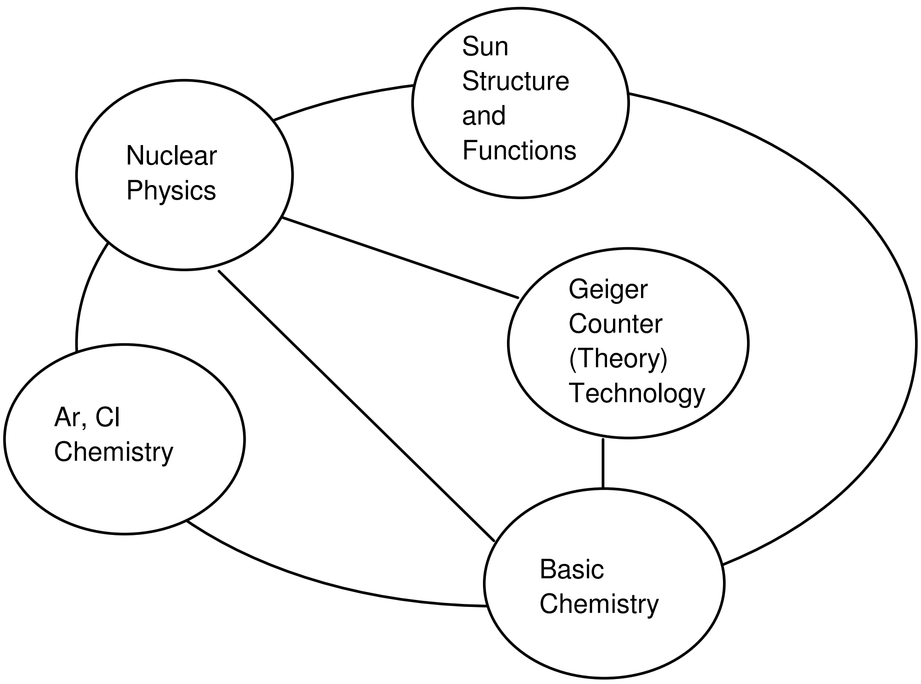


FIGURE 3

2) If 'gap' in telescope test is 'small enough'
a) telescope <u>works</u>
b) all related knowledge reinforced
c) particular solar reaction paths reinforced

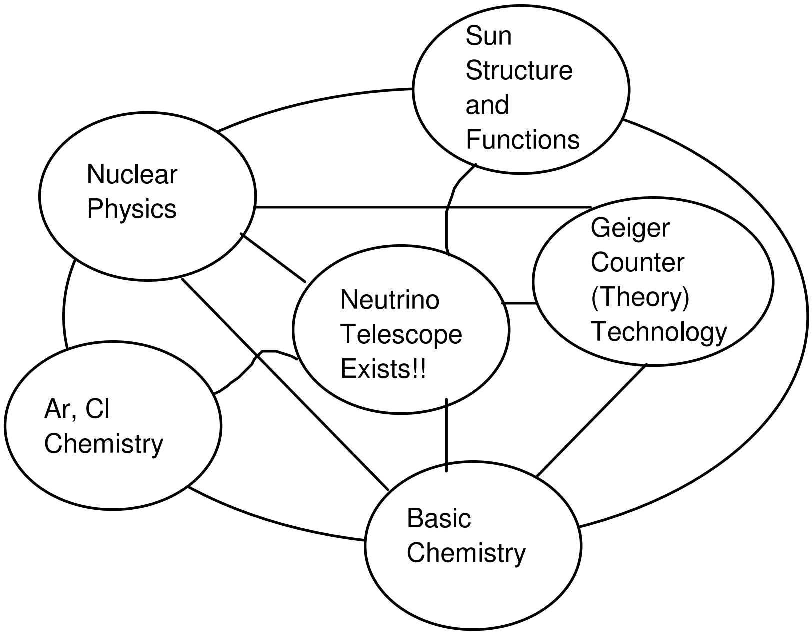


FIGURE 4

3) If 'gap' in telescope is not 'small enough'
- the telescope does not exist
- all other knowledge <u>constant</u>

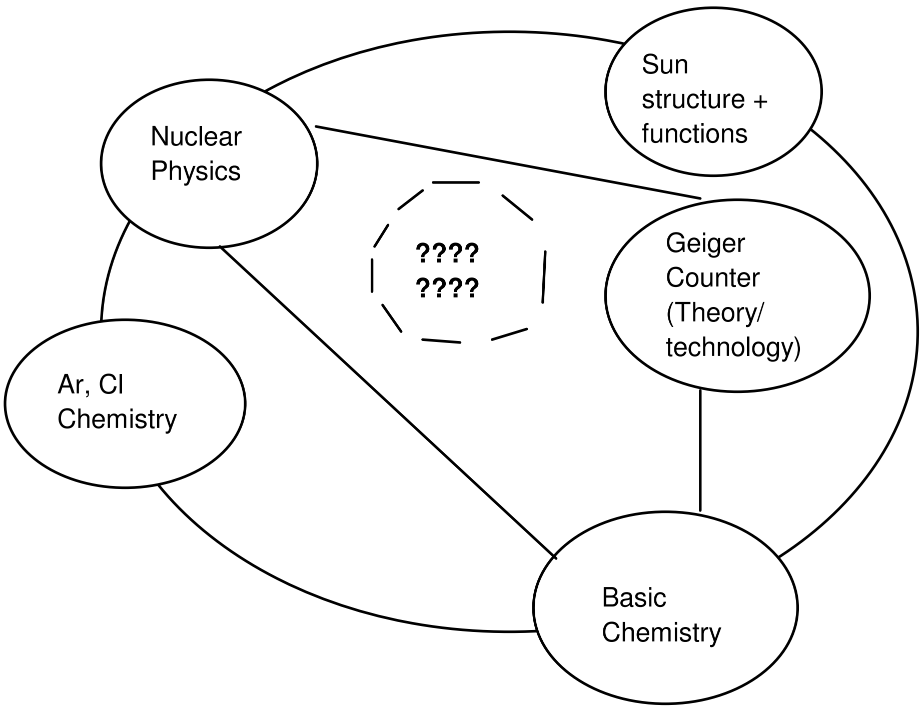
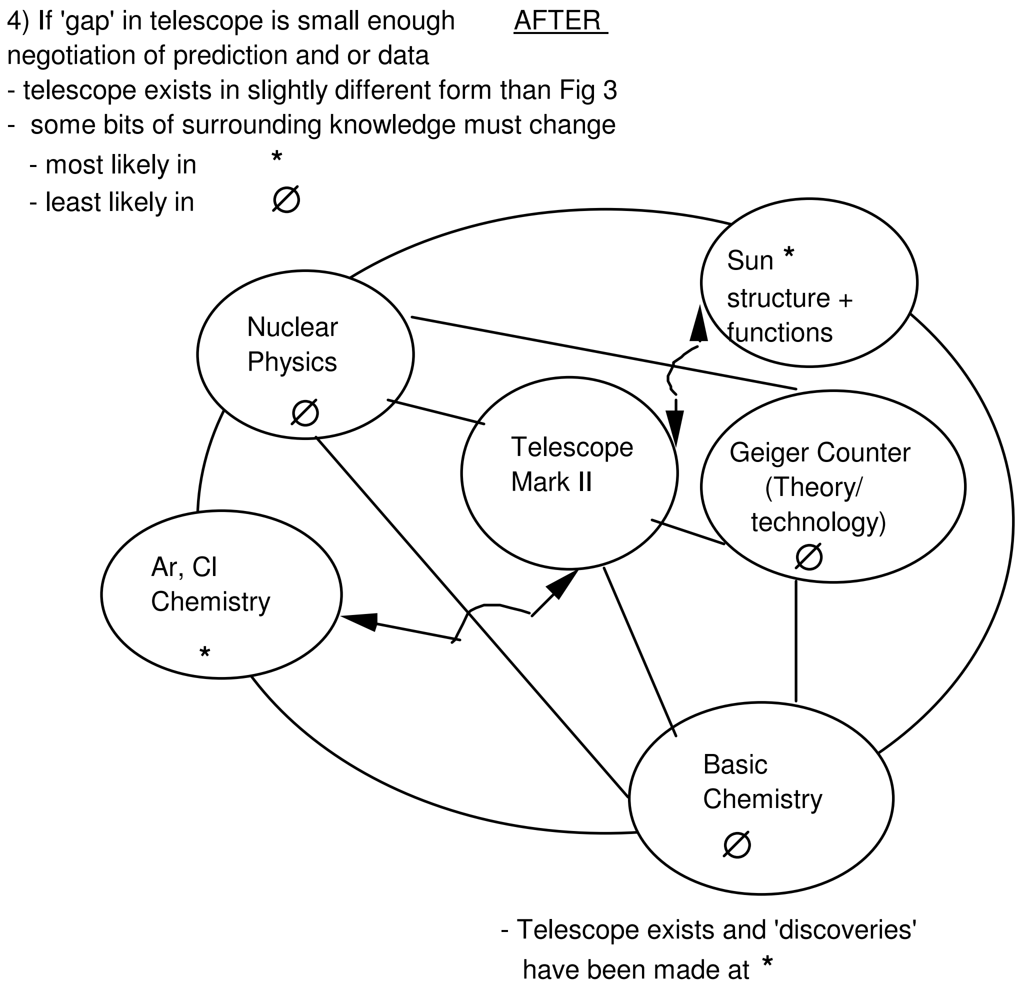


FIGURE 5



Having surveyed points A, B and C, we are, I think, led back to an interesting and crucial issue which we have addressed from time to time in these chapters and which we can now formulate in more mature terms. This is the problem of the nature of progress. But before attacking this issue directly, we need to return briefly to our opening chapters and to our overview of method. This will help our understanding of what 'progress' should be taken to be.

We began with the first of the four interlinked common stories about science, technology and society. That stated that “Science Discovers the Truth about Nature". And we saw that according to the story, this happy outcome is produced by utilising the Scientific Method. According to the story, moreover, scientific method rests on the following assumptions:

1 - Nature is an objective system of facts.
2 - Humans can objectively observe and report facts.
3 - Scientific knowledge is based on facts alone.
4 - Theories are generalisations of facts and are proven true or 'confirmed' by tests.
5 - Science makes progress: Collecting more facts and successfully testing truer and more powerful theories.
6 - Scientific knowledge is objective and proven, and therefore has no social, personal or political bias.

Now, whilst assumption 5 defines the meaning of purely scientific progress within the method story, we also saw that from assumption 6 it follows that "What Science Proves True about Nature by Means of Method is the Sole Objective Basis for Technology", hence whatever technological progress might occur, depends in part on the method story. Finally, we also saw that (until recently at least) most people further believe that "Society must adapt to technological change, and that humankind automatically benefits from this". Hence we arrived at an initial idea of social or economic progress, linked back through technological progress to the fundamental idea of science progressing by use of the method.

Clearly, all three senses of progress--scientific, technological and socio-economic--point back to the story of method. We, however, have become highly sceptical of the relevance and plausibility of the method story as a way of understanding science and its history. We have more or less deconstructed most of these key assumptions about method. Let's review what has happened to the assumptions, paying special attention to the implications for ideas of progress.

First, as to assumption 1, let's leave nature aside--we can still assume it exists out there as a one off, objective, system of facts.

Assumption 2: Humans can objectively observe and report facts: well within specific cultures you can "objectively" observe and report facts, given always that those facts are constrained, indeed, made within the cultural grids and resources of that culture. We therefore changed that old sentence a bit and now we understand it differently.

Assumption 3: Scientific knowledge is based on facts alone. Well, fair enough, as long as you recall that facts only exist in the light of prior belief and prior theory.

Assumption 4: Theories are generalisations that have been proven true or confirmed by test. Here is the traditional language of 'true' and 'false', 'confirmed' and 'falsified', whereas now we know there are always gaps to be negotiated in tests. Make a decision that the gap is 'small enough' and we call the prediction 'confirmed'; we make a decision that the gap is 'too big' and we call that the 'falsification' of the prediction. As we have seen, Popper, and traditional method theorists as well, all missed the politics of testing, the negotiating and judging a consensual view of the the gap.

We have thus deconstructed the old assumptions 2, 3 and 4 and we've learned to look at science as a much more social and political undertaking than the old method story would or could allow. We come, therefore, to the issue of Progress. Assumption 5: Science makes progress: collecting more facts and successfully testing truer and more powerful theories.

The idea of progress is clearly very important within the idea of method and very important in understanding everyday ideas about science. And as we shall see in section 2 of this subject, it is also very important in understanding how people think about technology and technological change and its social consequences. We can begin to see that by reflecting on the ideas of technological and socio-economic progress introduced above.

> **What can we now say about progress as a result of our deconstruction of the method story and our post-Kuhnian study of the nature of science and scientific change?**

Returning to the old method story, we had there an image of scientific progress piling up bricks in the wall of objective, proven knowledge of nature; a brick wall that gets bigger, longer, wider, and higher over time. The bricks are congealed facts known as laws, or congealed laws, known as theories. This is an image that represents the idea of scientific progress within the old story of given facts and scientific method. What we have seen in these chapters makes this image a little difficult to accept: that progress should be so cumulative, inevitable, unproblematic and obvious.

For one thing, you may recall that Karl Popper and many other thinkers in this century have been worried by what seemed to be the reality of large scale revolutions in science. (Kuhn too was trying to make sense of this purported phenomenon.) You will also recall that the idea of occasional big revolutions in science posed problems for the traditional, smooth, continuous and evolutionary image of the growing brick wall--based on the traditional method story--because revolution would seem to involve wholesale destruction and reconstruction of sections of the wall which previously had seemed firm, proven and solid.

Popper tried to show that revolutions could still give you smooth progress, but there were fatal problems with his account. Kuhn, on the other hand, admitted incomensurability between successive theories or paradigms, and so he gave up on the idea of smooth, continuous easily judged progress. [cf Chapters 7 and 8]

Therefore, I think that we can no longer believe in the brick wall image of progress and therefore we can ask from our new perspective "what does it mean that someone has achieved something in science?" What does it mean to be a "winner" in science? In the method story a winner is somebody who finds a brick and puts it in the wall. But on what we have seen a winner is somebody like Lavoiser, somebody who says let us rejig, at least a bit, the existing grid or paradigm; let us link these rejigged concepts to certain material practices; and let us base the next rounds of research on that revised grid and set of linkages. He really wins by convincing his colleagues to buy this revision of things, a revision vulgarly called "discovering" something, in this case oxygen, but which we have seen is more a constructing or manufacturing of a revised grid-practice linkage.

However, notice that Lavoiser's winning position includes a substance, caloric, that later gets totally thrown out: later 19th century researchers did to his caloric what he did to phlogiston, they argued for a shift of grid and grid-practice linkages and said that caloric did not exist. They said instead that energy (and entropy) exist, except they said it as "we have proven that energy exists and that caloric does not exist". **But as we have seen caloric certainly did exist as a grid category and set of practices in the chemistry and physics of the early 19th century.**

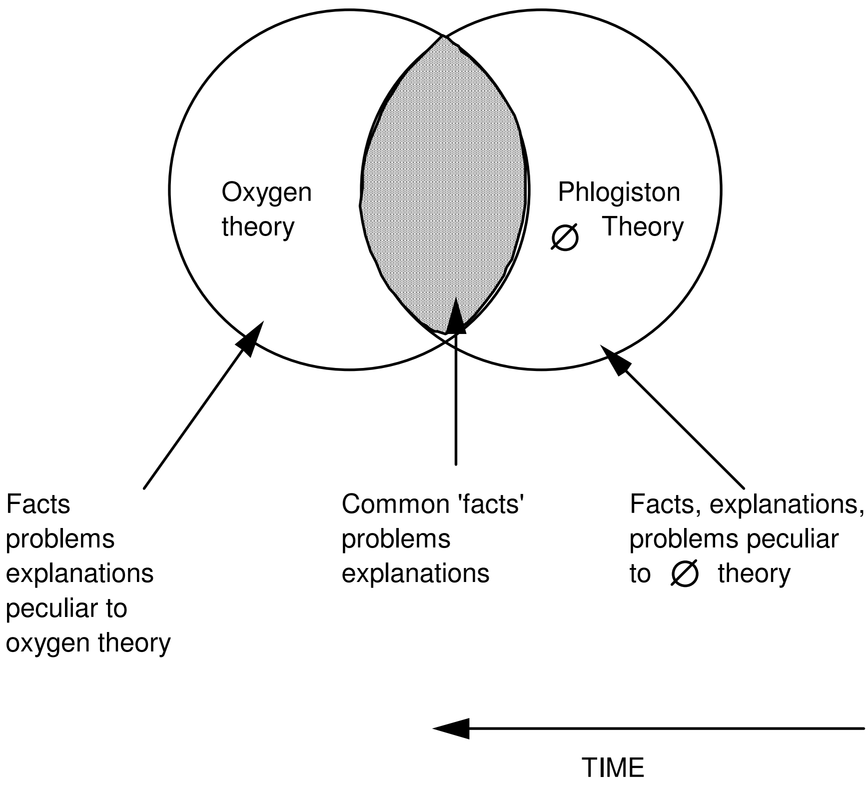
Moreover, even Lavoisier's oxygen is not our oxygen. After 1800 it underwent negotiated changes in grid and links to material practices, whilst still bearing the earlier name, oxygen. Lavoiser had not found a permanent brick for the wall--oxygen, nor had he made a mistake--about caloric. Rather, for a number of years, and a number of rounds of subequent research, his new objects (i.e. grid-practice linkages) existed and were resources for and objects of research. So he was a great winner, a famous winner, but he had not discovered a pure nuggety fact, nor were his 'discoveries' permanent or stable over time through rounds of subsequent research.

The winner's winning position is not permanent, not some revelation of the nuggety facts of nature. The winner's position is a winning position for a while, for a few rounds of subsequent research--a couple of days, weeks, years, decades, or even, in Newton's case a couple of centuries.

In the end it is likely that a winning conception will be changed, grids altered, and new linkages agreed to. Hence new facts and discoveries will emerge, and old facts and discoveries will no longer be facts or discoveries, they will not be permanent bricks in a wall.

How then may we describe progress? Well to answer that we had better first ask what is change in science? You will recall figure 6, which showed the relation between oxygen theory and phlogiston theory illustrating the relationship between two successive paradigms in a field of science, according to Kuhn. The overlap is only partial, that is the key, that is what Kuhn called incomensurability, that means that not all the facts and problems in the old theory are parts of the new theory, and that the new theory envisions some facts and problems that have no counterparts in the old theory. There is no totally conserved brick wall standing during the transition; only parts of it may survive the change.

FIGURE 6

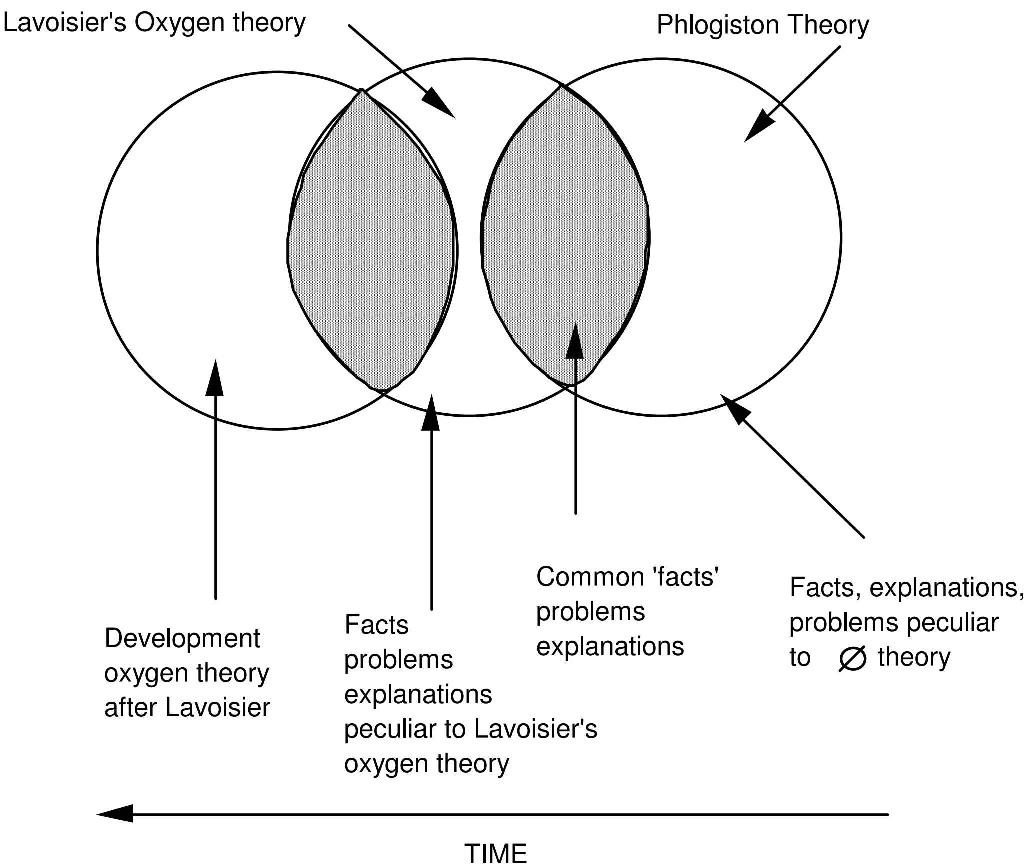


But, the new theory does not totally replace the old theory; the new theory partially does the same things as the old theory; the new theory can do new things, but these have no counter parts in the old theory. And things crucial to the old theory have simply been lost, sacrificed in the change, things the new theory cannot do.

From Lavoiser's standpoint there is 'improvement'--e.g. he now knows what he thinks an acid is--something with oxygen in it. But certain facts, problems, opportunities linked to the old theory have been lost. Lavoiser could not explain the family resemblance of metals, but Stahl could, using his phlogiston theory. Lavoiser could say, "Well maybe I don't know that, but you guys are wrong anyway, and I'll get that answer some day". But at the moment of decision, there is this kind of Kuhnian incomensurability.

Recalling our points about Lavoisier's oxygen chemistry not being the final word about oxygen, we could modify figure 6, producing figure 6A. Here we add to another circle for later post-Lavoisier, oxygen chemistry--where oxygen is redefined as not the acid-maker, and where energy replaces caloric. Here again there would be loss of some things in 'old oxygen theory' that are not made up, at least at first, in the later oxygen theory--the same kind of non brick wall incommensurability.

FIGURE 6a



Now we can ask what is 'progress'--because these pictures of change like figure 6 or 6A are not pictures of piling bricks on bricks. What we seem to arrive at is a notion that progress is not a thing or an event in history; rather, it is a word that the winners use to describe the improvements they claim to have made. Hence, 'progress' is a social label or a rhetorical label used by the players, in particular the winners, to label their new circle(s) in the above diagrams. It does not fully take account of what has been lost in the shift to the new theory, the costs involved in the shift, the new difficulties openned up by rejecting what had been facts and established procedures in the old theory. '**Progress' is a loaded term for one-sidedly describing the process of scientific change: an historical and cultural process of change.**

Progress means an "improvement" (in some sense) in the judgement of those enforcing the new set up, the new dispensation. Lavoisier and his friends endorsed the virtues and advantages of their new theory and the improvements it made over phlogiston theory. The losers in this process do not get to define progress, or to point out the down side, the costs, the failures of the new theory or what they could do that the new theory could not do. I am not denying therefore that scientific theories improve, I simply deny that there is a God's eye view of improvement, of progress. The 'virtues' and 'improvements' are always selectively pointed out by the winners.

Progress is not some God given phenomenon, some set of concrete developments in the world, open and agreed to by all observers through the progressive, cumulative revelation of natural facts. Rather, it is a self-interested player's word, used inside the process of change to describe and legitimate this sort of incommensurable shift. It is a label winners apply to the bits of these diagrams that they like. They get away with it, if they have won, and **that** is why progress is 'inevitable', to hear the winners tell it, because it is the winners' version of the historical process of change.

The same idea applies in politics: those in charge do something, with some noticeable effect and they claim progress: all the costs, and losses, become forgotten issues and are simply ignored. More importantly, as you will see later in this subject, the same idea applies to technological change. Whatever innovation is implemented and successfully marketed constitutes 'progress'. That is why a healthy society demands educated people who can step outside such processes of change and ask embarassing questions about what has been lost, what real costs are involved, what worthwhile things have been lost in this "change" called "progress". The core of this western myth starts with science and the idea of scientific progress; the ideas of technological progress and social progress rest on that basis and gain legitimacy by pointing back to the supposed rock solid and objective "progress" of science. That is why it is so important to be aware of all this, starting with a close analysis of the human nature of science and scientific change.

This is a very important insight for us. For 300 years in the West people have believed that progress is something built into history, or at least western history as a series of events and concrete, unassailable achievements. They have thought that western science and technology make western society different and better, a difference and a betterness grounded in our undergoing Progress. I am saying yes, science changes, the business of science is to change, but that change is not based on the old idea of method and the accompanying image of the brick wall. Our analysis here is influenced by Kuhn, it sees scientific change as a social, negotiational process in which any 'discovery' involves the modification, change and partial loss of what was before called knowledge. It is a process of historical and cultural change.

Change is not progress, progress is a label that winners in change apply to the aspects of the change they wish to stress and highlight. **So our challenge is not to view progress, to document or idolise progress, but rather to study scientific change, to study its social and human character and to watch the players struggle to label changes as progress.** Similarly with technology, we will see that our job is not to celebrate technological change, or to idolise changes, but to anatomise the complex factors that shape technological changes, and to study the players as they contend to label favoured changes as 'progress'.

We need a history and politics of "progress". Today this is not such an odd idea as it would have been over the last couple of centuries when the myth of progress reigned supreme. Today even average people in the street are just a little sceptical about science, medicine and technology. Sceptical of the constant litany of claims that what is new or changed equals progress. What we are really after are the tools for studying change in science and technology. Tools from history and philosophy, from politics and econonomics; to see science and technology as human, social institutions that are shaped by social factors and causes that ultimately have complex social consequences--not as just simple unarguable "progress".

Having said this about progress, that it is a system of rhetoric for the winners to label the aspects of change they wish to push to the audience while avoiding discussion of any costs or down sides or knowledge losses, we can see easily what the story of method must be, and how it has functioned for so long in our society in relation to these phenomena.

Why do so many people still believe in scientific method--the old version or the Popper version--why have so many people always believed in scientific method if it is now in the late 20th century so easy to knock the stories down? Well, we have to see that the method story is seductive, it is easy to believe, it trades on the simple beliefs that we have about facts and about objective human perception; it is linked directly to attractive and simple ideas of progress, and of whig history. The story is believable, very believable if you do not dissect it, and moreover it plays a crucial social role in science, and in society. It gives a simple account of what it is that scientists do and how they achieve their results.

Scientists and others find this packaging rhetoric useful and pursuasive, so it continues to be used. For example in science teaching the method story is used to motivate students and explain in brief and at first what science is: what is it?...It is this great method. The details come later, and then one tends to neglect or forget the opening oratory about method--but it is there in the overall packaging of science pedagogy. Another example of where it is used is at the actual research front, the research coal-face of science. Method stories provide the means for scientists to package, set out and sell their claims. Historians and sociologists of science have found both past and present scientists using the rhetoric of method to package their own claims and to try to knock down the claims of their competitors.

The final area where the story of method is used is in what we can call public images and legitimations of science--public representations and descriptions of what it is that Science "capital S" is supposed to be, why it is good and why it should be funded etc. etc. Since the 17th century the method story has been used as a good way to explain to the general public that there is a crucial institution called Science, that it deserves support and that it must not be interfered with. Remember, method depends upon objective observation of nuggety facts! Scientists are special people, because they have a method, and they are to be left alone, because that same method tells us that if we interfere we taint the results.

The study of public representations of science is increasingly important, because modern science is not only characterised by the existence of lots of specialised fields and subfields each with their little collection of esoteric experts spread across the globe, but also because modern science (and medicine) are characterised by massive public controversies that involve the media, governments, activist groups, and political parties, etc. Arguments about science and medicine now take place in larger environments, in courts, parliaments, the media, and so it makes a lot of difference how people decide to talk about and represent science. Often, the experts will rely on method stories to package their side of a controversy, but just as often, critics and reformers or aggrieved parties will also use method rhetoric to package and present their side of a controversy. Since controversies about science, medicine, technology and the environment are so prevalent, and characteristic of modern society, we had better be able to avoid being seduced and confused by the superficial rhetoric of method and learn how science really works so that we can see what is at stake in such debates. We can avoid the pull, the 'rip' of such rhetoric, if we know that it is a rhetoric that covers up the social, human and political reality of scientific work and argument!

In the third section of this subject, when we look at a case study of scientific/technological controversy--an environemntal case. You will see how this area opens right up, if you have digested what we have said here about method stories and the actual social negotiation and social construction of scientific knowledge.

Finally: *Do not misquote me*

I love to be quoted, but I hate to be *mis*quoted so please try to observe the following niceties!

I have not said that science is "nonsense"- I have said the method story is nonsense, in that it is a terrible description of what it pretends to describe: the nature and dynamics of science. But the method story is indeed useful, functional nonsense for certain interest groups who need to cover or represent things about science for relevant audiences. Science is a huge, significant and to many people authoritative social institution. I would be a fool to say it does not exist, but I would also be a fool not to ask in hard historical and social terms how it really works not how the method myth claims it works. Do I aim to knock science off some pedestal by examining it like this? I'm not sure.... Some people in HPS, and elsewhere, have an interest in moving from an analysis of the history and sociology of science to a denial and destruction of science. **But I do not.**

For studying nature, I do not know a better alternative at least in the context of how our culture and society go about their business. So I want to know how science works--fairy tales aside--I do not want to find out how it works in order to put it out of business. I don't think Divine revelation or mystical seances or astrology provide good knowledge of nature, and just because I know that scientific knowledge is a human contruct doesn't mean I deny it is better than other forms of human knowledge. I personally am sufficiently a child of our scientific culture to think it is just about the best tool we have, but that does not preclude asking questions about directions of research, consequences and costs of research, the uses of research etc. and those questions cannot be intelligently posed if we stay mired in the old method story and the ideas of progress and whig history of science.

During the 17th century this was still an issue. I told you about Bacon and Descartes around 1620 advancing ideas about the scientific method. One of the things they were trying to do was to say 'only science should provide the basis of knowledge'. Their view was that religious enthusiasm should not provide the basis of knowledge; Papal authority should not provide it; the whims and views of 'ignorant' peasants should not provide it; the views of peculiar other cultures that they were trying to take over or subdue should not provide it--only what we elite Europeans call science should determine what we believe about nature and humankind's relation to nature. So there was a political point to their method talk, it was to clear the stage for science. I don't believe science is nonsense. If I thought that I would not waste my time studying how it works and how and why it has changed throughout its history.

I have not said discoveries and facts do "not exist" just that they don't exist as the method story says they do. I told you what I thought a discovery is, what a fact is, in ways that I would claim take account of what we know about human perception and culture, and about the history and social character of scientific knowledge. Facts exist, societies are based on the existence of facts. The issue is what is a fact in a human context. We have said that it is not a nugget waiting out there for the objective, method-bearing 'good guys' to discover; that a fact for humans cannot exist outside of theory-loading and the social negotiation and communications of reports. Similar grids and social practices tend to produce similar facts for the humans involved, so facts exist. Facts exist, but they are 'cultural' not natural--they arise from different groups of humans interacting with nature and with each other.

Furthermore, I am not saying that any individual can make up or insist upon any facts he or she pleases willy nilly. I am not saying that 'everything is totally subjective'. Far from it: We can make up facts if we wish, but we would be considered odd, if not mad. Facts are cultural products, the products and possessions of groups whether whole societies, or specialist groups in a society, with specialist facts as in the sciences. Others must be brought into line, come into the consensus, join our version of the negotiation for our claimed fact to become, for the time being, and for the group involved, a fact.

We cannot hop out of bed in the morning and declare a new fact. That is precisely what we cannot do. It is the job of the social sciences: history, sociology, anthropology etc to study the constraints on fact construction and the modes and manners of fact construction in a society or sub-group.

I have not said tests do not "have meaning"; rather I am saying that they only have meanings that scientists' consensus formation may or may not give to them. We cannot walk in and say to Raymond Davis, "I don't think this means anything, Davis", or "I do think it means x, y or z, Davis". Davis would not care, because we are not proper members of the negotiating set. But that is the point--no one person can give a test a meaning or outcome, and the test has no meaning or outcome until the relevant humans (whoever they are) have been able to close the debate and negotiations about the meaning (however they come to do that). It's not that tests have no meanings; it's that the meanings come from social interactions amongst the relevant players. Mere subjective views are not important unless they can be communicated and sold to the relevant negotiators.

I have not said Nature does "not exist"; rather I have said Nature is behind all perceptions and tests, but always in view via our filters, both internal and externalised as hardwares. Nature is there, but the humans only approach Nature through their cultural grids, and the bits of grid externalised as artefacts and instruments. So what any group of humans take nature to be depends upon nature, but also, inescapably upon that group's grids, material practices, and modes of communication and negotiation.

I have not said there is "no improvement" in science, rather I have said that changes are often not totally cumulative, that they involve conceptual and other tradeoffs and that any improvement is in the eye of a (winning) beholder. There are always costs and benefits involved in any discovery or shift of theory. Those costs and benefits can be in terms of knowledge, practices, values, aims etc. We, as humans, operating within grids of knowledge and value can and always do evaluate the costs and benefits, and if the balance, in our view, is with the latter we call it improvement or progress. It is just that we do not have a God's eye view of progress or improvement--such terms are judgements made by interested humans, and of course we constantly make them. For example, nobody today is going to return to phlogiston theory, it has been left behind, and we have only the descendants of Lavoisier's theories to work with. With such hindsight we can say that (present) oxygen theory is probably much better than (18th century) phlogiston theory. But that is not the issue. We need to understand why Lavoisier's embryonic oxygen theory won in the 18th century so that it could benefit from 200 years of additional work. And the answer to **that** question cannot be Lavoisier's progress rhetoric that his theory in 1789 was just clearly better, because in 1789 it was **not clearly and unequivocally better**, he was offering a winner's rhetoric. Moreover, even the improvements in oxygen theory of the last 200 years do not constitute a brick wall of progress, because even within the theory, every major adjustment or discovery has involved a negotiated loss of some benefits or resource previously present in the theory-grid, but now adjusted out of existence.

I have not said hardwares "do not work", but rather that they are the embodiments of theories, concepts and goals, and that their "outputs" are further conditioned by the aims and theories of the observers. The experimental apparatuses and instruments are human artefacts, and on this view they start to show their family likeness to technological artefacts, manufactured commodities or the means of making the same. In the oxygen chapter I said the construction of oxygen is like the design and implementation of a new car model. It involves decisions, it implements a number of theories and goals, it involves tradeoffs of costs and benefits of various sorts. Just as a newly introduced model of car surely 'exists' so surely did Lavoiser's non-modern 'oxygen' exist, for a while at least, until modified out of existence, in favour of a new model. Scientific hardwares are even more obviously like that because they embody theories and aims; and what they are and what they do, are the products of social negotiation by the experts, and they exist as long as there is agreement about their nature and their output, and relevance of their performance.

What we have not done
- in this Section of the Subject

We have looked at what post-Kuhnian historians and sociologists of science call the micro-politics and sociology of fact constructions, discovery and test negotiation.

We've looked at small expert groups--Galileo and his friends, Lavoiser and his friends, Raymond Davis and his friends, to see how such specialised experts actually go about making and breaking knowledge claims in science given that they do not proceed according to the myth by using a simple, single method.

Now, such specialist groups always exist in a multi-layered network of larger social institutions, forces, belief systems etc. They always exist, as we say in HPS, in larger **social contexts** that include the State, the economy, the system of classes, belief systems, media and communications systems, churches, political parties etc etc. The method myth says that for science to prosper, scientists using the method must be insolated from such bias or interest-setting forces. But one of the most basic findings of HPS and of its sub fields such as history and sociology of science in recent years, is the realisation that larger social contexts can always variously influence and shape the making and breaking of scientific knowledge. The whole question is what forces are in play in a given case, and how they work in detail. Our problem is that so far in this section we have not had the time to widen our scope from the micro-politics and sociology of expert groups out to wider realms of possibly shaping social contexts.

We've not looked at the role of larger society, economy or culture in shaping scientific knowledge claims. We've looked at bits and pieces of cases: Galileo, Lavoisier, and solar neutrinos. We have looked closely enough to challenge and remould our ideas about method, facts, discovery and progress in science. We have seen the social political, institutional shaping of scientific knoweldge at the level of professional communities, jockeying for position, persuading, negotiation, selling and receiving new arrangements of conception and practice.

But scientific communities exist amid larger social relations: the maneuvering, the changing of grids and of facts can be influenced by wider social factors, wider contextual factors. This needs closer study. Full-on study of the history, sociology and politics of science, or medicine requires attention to the wider contexts and their roles in the shaping of knowedge. And we shall see particularly good cases of the embedding of scientific claim-making and claim-breaking within larger social contexts in the case study on offer in Section 3 of this subject, where we shall see that for over a century now, what has counted as scientific and medical knowledge in the field of sewage and waste engineering and management has been shaped in part by a play of larger social interests and forces.

Now, in this case we shall also see a kind of 'flip side' of the issue of larger social shaping of scientific knowledge, and that is the political and ideological use of scientific knowledge claims in the push and shove of larger social debates and struggles. Science is not neutral in either its creation in specialist sub-communities nor in its deployments in wider social spheres. We have studied the detailed reasons why science is a social construct of its expert communties, but these wider contexts are ever present in reality and need to be added to the equation of HPS analysis of science.

So, in short we have not looked at the ways larger social, political, religious, economic contexts can affect the ways micro-negotiations take place. And, as I say, full understanding of science requires this, and invites us to full scale historical and sociological analysis. We do this, for example in HPSC 2100 on the `rise of modern science from 1500-1700' where we do not just look at the experts negotating what we came to call the birth of modern science, but we place them in their larger contexts. In this case contexts of religious belief and conflict, and of social and economic dynamism of 17th century Europe.

What then of technology, our topic in Section 2 of the Subject? This signals our return now to the main flow of this subject:"Science discovers the truths about nature by use of scientific method" has been criticised and reconstructed. We are now about to move to technology better able to relate a new view of science to what will be a new view of technology. Our everyday view of science, technology and society, introduced in the opening chapter, holds that after science discovers, "technology applies what science proves to be true", and that "society in adapting to such inevitable and objective technological change undergoes a progressive, benign development".

We have seen enough about science to disspell the simplistic idea that science uncovers nuggety truth by use of an objective, value-neutral method. Hopefully I have sufficiently revised your view of science so that we are now ready to competently tackle the issues of technology. **But remember that the 'science' we now talk about in section 2 when we talk about technology is not the 'science' we started with in this subject: a science produced by method which is then applied to produce technology, because we now have a demystified view of how science is produced and what scientific change or history is really about.** If science is not what we took it to be at the start, then the relations of science to technology may be different than the simple one of "technology applies method-gained science", and indeed technology may turn out to be more complex and socially and historically shaped than the simple story implied.

We shall see that technology is difficult to define and certainly cannot be taken to be simply the application of what has been found in science. We shall see that technological innovations, like scientific discoveries, are socially shaped and do not fall from the sky in some neutral, inevitable and simply beneficent manner. We shall also see that social change is not simply the result of technological change in some one-to-one relation; and moreover we shall find that as in science, so in technology, "progress" is in the eye of the winners and that a simple linear, heroic history of technological progress makes even less sense than a simple, linear, heroic, whiggish history of science. By the time you finish section 2, technology will never appear the same to you, and having critical understandings of both science and technology under your belts, you will be able to proceed to the the case study in Section 3 which concludes this subject.