5 Was Oxygen Discovered or Constructed by Lavoisier?

"*Social Constructs are the 'realest' things there are.. .*"

The last chapter ended with an outline of the world of phlogiston theory--a world of concepts, a connected world of chemical facts and manipulations. We have already seen, in addition, that Lavoisier did not step into a void empty of chemical theory, and suddenly conjure modern chemistry out of thin air. Neither did he invent chemistry by applying 'scientific method' to chemical phenomena. He actually stepped into a situation already dominated by a quite mature, effective theory--the phlogiston theory, even if to our eyes (conditioned by Lavoiser) it seems a little bizarre.

We have seen that despite what the chemistry textbooks say, Lavoisier's chemistry was quite different from our own. It is an ancestor of our chemistry, but then so is phlogiston based chemistry! In this chapter you will see that Lavoisier's chemistry also bears a relation of descent to phlogiston chemistry. For example, both chemistries were concerned with 'weightless' matters and with problems of explaining combustion and the nature of acids.

In this chapter we are concerned with three key issues.:

> (1) What does it mean to 'discover' something in science? What does it mean to 'discover' oxygen? Is it just like finding or seeing a thing for the first time, something that was there all the time but previously hidden in some way, but now uncovered and revealed? Is it like walking into a strange room and 'discovering' a chair you have never seen before. Is it like jumping into a ship and sailing until you bump into a previously unknown continent. Is discovery in science like that, or is it more complicated? Are discoveries in some important sense social or cultural events in which the so-called 'discoverer' is in some way contributing to the invention or construction of what is taken to have been discovered?
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> (2) How do we compare theories when they are in conflict? When there is a scientific controversy at the research frontier between proponents of two fairly good theories, how do the contending parties compare and assess these theories? How do rational human

beings ever decide to favour one theory over the other? Can we use 'scientific method' or some single, unequivocal, objective standard of measurement to decide between competing theories. Can we use some simple test to decide the issue, or, is the situation more complicated. Is theory choice a social and political process in the scientific community, a matter also of struggle, negotiation, persuasion and recruitment?

(3) Finally, if one theory wins in such a conflict what does winning actually mean? What is scientific progress when such 'progress' occurs through the conflict of theories and the ultimate defeat of one contender?
Let's consider the discovery of oxygen. Lavoisier was not the first person to manipulate in a lab the substance he came to call oxygen. That was handled, manipulated, if you like, 'discovered' by others before Lavoiser, but was not called 'oxygen' by them. One of the first 'discoverers' of oxygen, although he would reject that title was Joseph Priestley (1733-1804). Priestley was a leading British phlogiston chemist. In the 1780s and 90s he was a determined opponent of Lavoisier's attempted 'take-over' of chemistry.

In the early 1770s Priestley took the calx of mercury (recall a calx is something like our oxides/ores of the corresponding metal) and produced some mercury from it. In post-Lavoisier terms we would explain this reaction as follows

$$\mathbf{2HgO \text{ -------->} 2Hg + O_2}$$

This, after all, is what one does with a calx--you try to produce the corresponding metal from it. The interesting thing was that Priestley did this in an odd way. He placed some calx of mercury in a glass bell jar and heated up the calx by using a magnifying glass to focus the sun's rays on it. **He did not place some burning charcoal inside the jar--the usual idea, for the charcoal is the source of the phlogiston that turns the calx into the metal.**

(Remember calx + Phlog. ---------> metal)

In this odd case, the calx heated and bubbled away under the sun's rays and turned into nice mercury anyway. Inside the jar Priestley trapped a gas or 'an air' as he would have called it. Like other late 18th century chemists Priestley was very interested in airs and gases for they had just developed techniques for trapping the gases that go into or come out of chemical reactions. Priestley was a great expert at this technique, and so were other leading chemists, including Lavoisier. This approach focussing on gases was called 'pneumatic chemistry', a term you will find in the other readings for this topic.

Having found this 'air' arising in the odd reaction producing mercury, he naturally tried a few standard 18th century pneumatic chemistry tests to determine some of its properties, and perhaps what 'air' it was.

First, he burned a candle in a sample of this air and found that the candle burned brightly and intensely for quite a long time: In all respects better and longer than a candle would burn in a similar sample of ordinary atmospheric air. He also put a standard 18th century lab mouse into a closed sample of this air and was pleased to see the animal run around and live a lot longer before suffocating than it would have in a similar sample of ordinary air.

Well, reasoning very logically within his expert phlogiston chemistry grid, Priestley concluded this was very good air indeed--supporting combustion and respiration (life) much better than atmospheric air. He had certainly discovered something interesting. What was it? To decide what it was; name it; place it in a conceptual grid, he had to manipulate and stretch his own seemingly true and reliable grid or theory of phlogiston. According to Priestley, what he had discovered was a new gas, which he called, 'de-pholgisticated air'!

Why did he call it this, why did he name it this, why did he say this is what I now have in my laboratory. It's not because he was ignorant, it's because he was smart and he lived and worked within the phlogiston theory. He knew exactly what this gas was, because his theory helped him to know.

Remember, we learned that according to phlogiston theory, when you burn a candle in a closed space, after a while the candle goes out. According to Stahlian phlogiston theory this is because the air in the container becomes saturated with phlogiston emitted from the candle. When the air has absorbed all the phlogiston it can take, no more can spew out of the wick of the burning candle and its emission of phlogiston is choked off! Now what do we make of an air in which candles burn brightly and for a long time? The 'scientific' deduction would be that we have an air that has much less than the normal, natural 'background' level of phlogiston. It can absorb much more phlogiston from the burning candle before being saturated than normal atmospheric air can. This is scientific thinking of the highest order!

So you have a sample of an air in which candles burn longer and brighter than in an identical sample of atmospheric air with the same volume and pressure. Animals live longer in it than in an identical sample of atmospheric air, before suffocating, because they can no longer give off the phlogiston that results from their vital processes.

Since atmospheric air has a saturation point for phlogiston, it must be that this air starts with less background phlogiston, or it has a higher saturation point. Thus a good name for it is de-phlogisticated air. This is a good scientific way of proceeding. Priestley lived and worked within his trusted theory, he found something interesting, and he probed it, tested it, described it and named it. Within the framework of his conceptual grid, he had found something new and exciting within his theory and within the world.

Don't be an old fashioned historian of science about this and say, "Look how unsophisticated Priestley is, he has oxygen but doesn't want to realise it." Now that itself is an unsophisticated way to look at the history of science, because it totally distorts the historical reality of Priestley's situation. We must try to place ourselves in his shoes and see how the chemical world looked through his set of theoretical spectacles--we want to know what he thought and why. If we wish to understand Priestley, his friends and their opponents, Lavoisier and his friends, we have to be good historians (like good anthropologists) and sympathetically put ourselves into the thought-worlds, the cultures of these 'strange' people.

What then, according to phlogiston theory, has happened to the calx of mercury which produced this de-phlogisticated air. Two possibilities come to mind within the 'grid' of basic phlogiston theory. Either the phlogiston to join the calx and make mercury came out of the bell jar air leaving it de-phlogisticated, or, and this is Priestley's preferred hypothesis, mercury calx contains fixed in its substance 'air' (They knew that solid and liquid substances have 'airs'--gases--fixed in solid or liquid form in them--they did a lot of work on this). So Priestley decided that mercury calx contains some air in its solid substance, when heated the phlogiston content is rearranged joining the calx more intimately to form mercury, and an air deprived of phlogiston is given off into the bell jar. Either way we produce a new kind of air with less phlogiston than natural air, a new discovery. Admittedly it is easy to name and test this air. The tricky part for further research concerns what is happening to the calx, in this case where no charcoal has been used. Lavoisier will hone in on issues and problems in this area, but Priestly has still very rationally, within his own grid and theory, discovered not oxygen, but de-phlogisticated air.

Now, de-phlogisticated air is not a thing we believe in. It was accepted as a fact by phlogiston chemists over the next 15 or 20 years, until the final abandonment of phlogiston theory by the last few phlogiston chemists in the early 19th century. So for a while this was considered a fact. We don't now think of it is a fact, but it was for many in the chemical community a fact in the past.

What is de-phlogisticated air? We don't want to say Priestly didn't discover anything at all. If we are good historians of science, we don't want to say that he discovered oxygen, because that gives a totally false picture of his pattern of reasoning, his motives, his aims and his claims to being, within his framework a competent, rational researcher. We might as well say that non-Europeans are dumb, say that, Aboriginal Australians are dumb, because they do not have the same space or time concepts that Europeans do. This is quite unacceptable, not only morally, but in terms of good social science, good history and good anthropology. We must take different belief systems at face value and try to see how and why people behave within them. What 'facts' and goals make sense within those frameworks and how those frameworks evolve and change.

I have an idea about this, which I think we should consider--it will help us understand the nature of scientific discovery, scientific change and scientific progress:

(1) De-phlogisticated air is not a naturally occurring substance that falls off trees the way apples do. It is not something existing in nature that Priestley found, because we do not think it is there anyway. But by the same token it is not just an idea, a mental figment, a concept held by Priestley--after all he made de-phlogisticated air in the lab; he handled and manipulated it--he burned candles in it and suffocated mice in it--in some sense it had a material existence, given his theoretical grid and modes of practice.

(2) I would argue as an historian of science that de-phlogisticated air as a discovery is a linkage. It exists or consists of a linkage of two things: A linkage between a slightly changed version of phlogiston theory on the one hand, and

a set of laboratory practices and procedures on the other. That is, de-phlogisticated air may be described as the following linkage of theory and practices:

changed ideas about phlogiston & 'air' <==> certain lab practices & manipulations

De-phlogisticated air is not something that we still think exists, the way we firmly believe in apples and oranges, but de-phlogisticated air was not simply an idea that Priestly had, because he could produce it, use it, hold it and test it etc. De-phlogisticated air existed as a linkage between a subtle change in existing ideas regarding phlogiston theory, and a set of very particular lab practices and procedures.

This understanding of de-phlogisticated air explains why it could be a fact for some people in history and then stop being a fact for others, it's simply that the linkage broke down or was rejected. Chemists stopped believing the relevant theory, or they stopped indulging in the relevant practices, or they linked those practices to a different theory (ie. made a new discovery-- hint: that's what Lavoisier did), and so 'the substance' no longer exists, except in historical recollection. However, de-phlogisticated air did exist and was real for as long as that linkage existed for some relevant sections of the scientific community. In sum, de-phlogisticated air equals:

"de-phlogisticated air" = [changed phlog theory <===> certain lab practices]

This analysis is very important because we might begin to see that discoveries in science--all discoveries in science--have a structure like this. They are linkages of certain changes of existing theory with certain specified material practices--they are not just new ideas, nor are they the uncovering of 'things' existing in nature waiting to be discovered by scientists. Discoveries are the association of slightly changed theories with certain material procedures. Discoveries have to do with human interaction with nature, and with the human imposition of grids on nature. A discovery occurs when a change of human ideas, a change of human cultural belief is associated with certain actions and things in the world.

Here is an analogy that will help you understand this notion of what a discovery is: Think of a car manufacturer producing a new model. First there is a set of ideas or designs; then they gradually gear up to manufacture and produce that design. Eventually the new product rolls off the assembly line. Now what is a new model car? Do they exist in nature? Do they fall off trees? No, human institutions produce them. But cars are real, and they are not just ideas, they are physical objects in and of the world. They are the products of the relations between our planning and designs and our practices of production and manufacture. I think scientific discoveries like de-phlogisticated air are like technological artefacts--they are like new product lines. For 2000 years we have thought about discoveries in science in a naive way as the uncovering of pre-existing objects. It is much more like humans making new objects, which involves human ideas, human theories, human plans, and human practical implementation of those ideas. This does a lot to bring the study of science into line with the study of technology.

Now consider that Lavoisier did not do anything very different from Priestley. It isn't the case that Priestley did the sort of thing schematised above, but more that Lavoisier did something different when he 'discovered' oxygen. He wasn't bumping into 'the truth' where Priestley was just linking ideas and practices. If Lavoisier made a scientific discovery, and he certainly did, then the structure of that discovery is just the same as this, as we shall see in what follows. My argument therefore is that Lavoisier's **oxygen is the same sort of linkage, an historically specific linkage of certain (altered) concepts with certain lab procedures.** We are now partially ready to believe this, because as I have already suggested to you Lavoisier's oxygen is not our oxygen. That is, his oxygen, just like Priestley's de-phlogisticated air, is a fact, a discovery, that once was very true for certain people, but now is not literally believed by anybody.

I want to show you with a series of diagrams what we mean by Lavoisier's discovery of oxygen; what I mean by Lavoisier making a change of theory and linking it to a specific set of material practices and procedures. (And by the way, the procedures are just about the same ones that Priestley used, at least in the beginning of Lavoisier's program of research.) Since the procedures are similar, we can attend mainly to how Lavoisier in effect re-negotiated a set of concepts to produce that particular constellation of theoretical ideas which linked to the practices constituted (or was) the discovery of 'oxygen'.

Figure 1. is a picture of the standard concept of phlogiston. Phlogiston explains heat, light, flame--**physical** manifestations or phenomena--and it explains the processes of combustion and calcination--**chemical** phenomena, processes of

material transformation. So the phlogiston concept does work to produce both physical and chemical explanations.

FIGURE 1

PHYSICAL PROPERTIES - HEAT, LIGHT, FLAME

PHLOGISTON has

CHEMICAL PROPERTIES - COMBUSTION, CALCINATION

Figure 2: has that same picture of phlogiston from fig 1; but around that I have placed some of the ideas and some of the aims or concerns of Lavoisier as a research chemist. One of his concerns was to explain why there are gases, and to explain what a gas is. For Lavoisier, ultimately a gas is material particles surrounded by "caloric"--that is his explanation of the gaseous state of matter--an issue that was very hotly contested by 18th century chemists, who had recently gained some control over gases in chemical reactions.

FIGURE 2

Lavoisier is interested in:
<u>EXPLANATION OF GASEOUS STATE OF MATTER</u>

PHYSICAL PROPERTIES - HEAT, LIGHT, FLAME

PHLOGISTON has

CHEMICAL PROPERTIES - COMBUSTION, CALCINATION

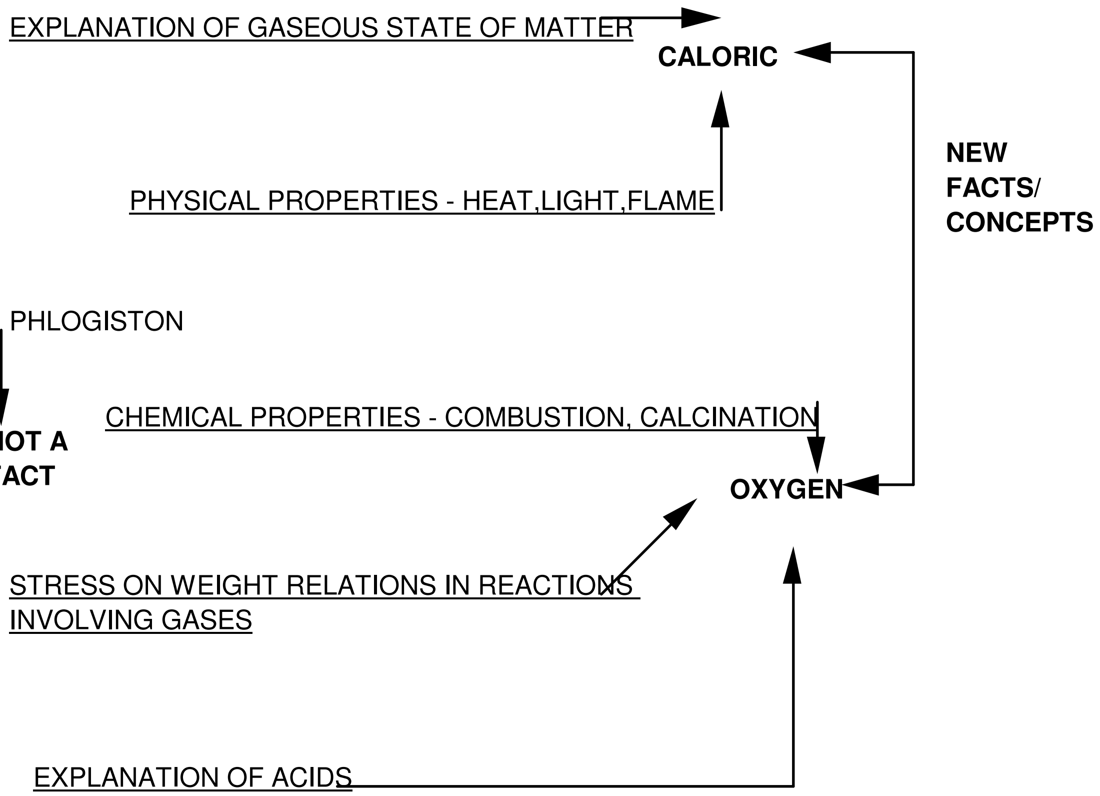
Lavoisier is interested in:
<u>STRESS ON WEIGHT RELATIONS IN REACTIONS INVOLVING GASES</u>

-
<u>EXPLANATION OF ACIDS</u>

Lavoisier was also interested in explaining acids--what is an acid, what makes a substance be an acid? He was in fact mainly interested in that question, and we know that his mature answer to that question is that all acids contain oxygen, that oxygen (the 'acid maker') is what makes substances into acids. Finally he is also interested in tracing the weight balances between what goes into a reaction and what comes out. This was a new, trendy, exciting area of chemistry, dependent again, upon the recent invention and improvement of ways of trapping the gases involved in chemical reactions. Lavoisier likes to pursue the problem of balancing the weights between what goes into a reaction and what goes out.

The next diagram (Fig 3) actually summarises about 10 years of work, thinking, struggle and negotiation on the part of Lavoisier. It is essentially the same as the previous diagram, but with a few structural modifications.

FIGURE 3



This diagram suggests that Lavoisier constructed the concepts of oxygen and caloric by in effect repositioning and juxtaposing aspects of what phlogiston was with what his own interests where. That is, oxygen and caloric were basically modifications, revised nodes as it were of the existing phlogiston conceptual grid. Except that in Lavoisier's system phlogiston itself becomes redundant and is ruled "not to exist'.

If you were to ask what caloric is, it is the explanation of the gaseous state of matter and it also explains the physical properties of heat, light and flame. A new conceptual map is being drawn by Lavoisier and oxygen in this new conceptual map explains the chemical properties of combustion and calcination, the nature of acids and it is crucial in controlling the weight relations, the weight balances between what goes into many reactions and what comes out.

By the time you have removed the physical aspect of phlogiston and put it into caloric (along with the problem of the gaseous state) and have ripped off the chemical aspect of phlogiston (and put it with the problem of acids and weight relations), there is virtually no phlogiston left as a concept. **Its explanatory work has been hived off to two new, interrelated concepts--oxygen and caloric. Phlogiston as a concept has become redundant and bits of it have been repositioned on the conceptual map.** Oxygen and caloric for their parts go together like hand and glove for Lavoisier and his followers. If there is no oxygen, there is no caloric, and if there is no caloric, there is no oxygen. He did not 'prove' that phlogiston does not exist, although of course that is the way his rhetoric proceeded. He rather looked at critically, and rearranged the conceptual landscape so that phlogiston became literally' redundant.

So here we have conceptual or theoretical work redrawing or revising the existing conceptual map or grid of chemistry. Lavoisier did that; it was not that easy or that apparent, but this captures the sense of what he did over a period of time.
Now this is what I have called above, the modification or alteration of existing ideas or concepts. He attaches all of this to some of the same set of procedures that people like Priestley had already accepted; and that linkage, of these rearranged concepts to certain specific lab practices and procedures is the discovery not only of oxygen, but also of caloric!

Lavoisier's concepts came from the juxtaposed and modified versions of available ideas, refracted through his own nexus of particular research concerns and interests. Then these concepts are connected to a set of practices and procedures, and he can say, “I have a thing called oxygen in my hand--I can touch and feel and control oxygen (and caloric)”; just as Priestley could say, “I have de-phlogisticated air in my hand, I can touch feel and control de-phlogisticated air”. Yet all of this occurred despite the fact the we today do not believe in the existence of either of those substances as they conceived of them. **In other words Lavoisier can say he ‘discovered’ oxygen and caloric, and Priestley can say he ‘discovered’ de-phlogisticated air. We do not agree either discovered a thing that now exists, but both were engaged in the same**

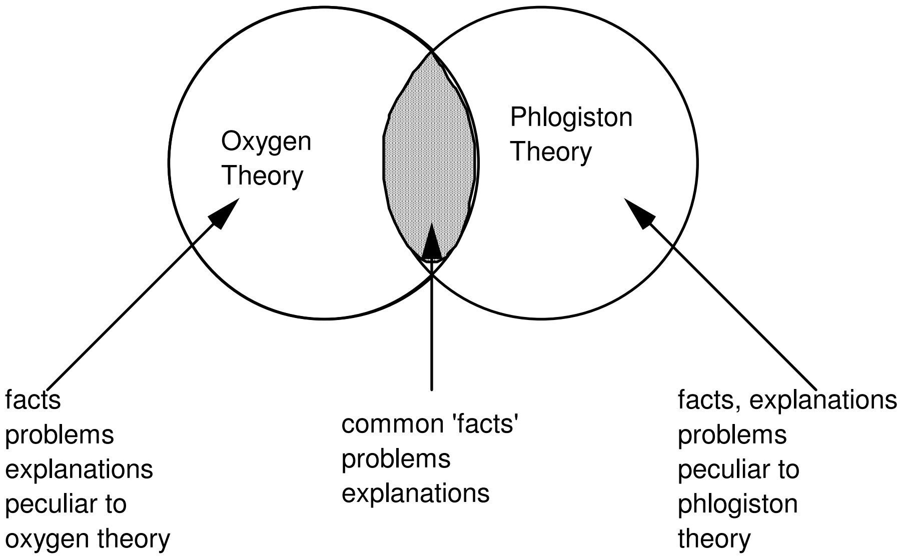
type of activity. It is not the case that you can avoid this pattern and simply go out and find the simple truth of nature--a discovery is always of this type--a reconnecting of revised theory with selected human practices. There is no special revelation of reality in Lavoisier's work as though he grasped reality and Priestley missed it. They were both in the 'linkage' game, struggling to convince themselves, each other and the other chemists of the time that this or that linkage was the way forward for chemistry. Of course they described this situation in mythical language as a contest over real versus erroneous 'discoveries of the facts of nature'.

Let's now compare the two theories. And again in doing this we are going to be learning some quite general things about the history of science. Very often in the history of science two quite reasonable theories have competed, and it is not obvious that one wins out and the other loses simply on the rather mythical grounds that good guys who grasp reality made up one theory, and bad guys who make mistakes about reality made up the other: Or that the winning theory was based on scientific method while the loser was not.

These two competing theories of chemistry oxygen vs phlogiston could not be separated by some decisive test or method. **There was no simple, single agreed test that could prove one of these theories was better than the other.** There was no simple method for making a hard decision as to which one was false and which one was true. **The reason for this is that the two theories are not strictly and totally comparable--they are only partially comparable. And this situation of only partial comparability, rather than total and complete comparability, is typical of virtually all major scientific controversies eg. Copernicus vs Aristotle, Darwin vs his enemies, Einstein vs the classical physicists of the late 19th century.**

In (Fig 4) we have two partially intersecting ovals, one oval is phlogiston theory with all the 'facts' it accepts (loads) and explanations it can make; the other is oxygen theory. Note there is an area of overlap, where the two theories effectively describe the same range of facts and offer comparable explanations of them (they describe these facts in somewhat different languages, but we would be pressing too far to say that the players did not recognise that here effectively the same facts and problems were at stake.) So in the common area each one can explain combustion, calcination and the balance of nature, etc.

FIGURE 4



The interesting thing is that there are areas of non-overlap sticking out at either end of the diagram. Here each theory could do things the other theory could not do. Oxygen could do some things that phlogiston theory could not, and vice versa. Some examples: oxygen theory could in its own terms explain why all acids are acids (all acids have oxygen); it was also very good on weight balances, tracing and checking weight balances before and after reactions. (Although if you wanted to do so, you could pursue that sort of thing in phlogiston theory as well. It is true that phlogiston theorists always seemed to be following rather than leading in this area--but too much has been made of this factor in the histories--giving an unfair view of the actual state of competition between the theories.)

The question we must now ask ourselves is what did phlogiston theory have that oxygen theory did not? For example, it has the established fact that sulphur can be synthesised. Now, you may say, hold on that is not true, that is an illusion, but what about all acids being based on oxygen, that's not 'true' either. We are looking for achievements that each theory could reasonably claim for itself, within its own terms of reference. Another factor in its favour is that phlogiston can explain why metals are metals, why there is a family of metals, and that is because metals are formed by the addition of phlogiston to calxes. The chemistry derived from Lavoisier could not explain this phenomenon for another 100 years, when it began to be explained on the basis of molecular structure and the behaviour of a newly discovered particle, the electron. Thus something routinely explained by phlogiston chemistry was missing in Lavoisier's chemistry and even for a long time in the chemistries claiming direct descendence from it.

The point here is that when big theories are in conflict, it is not the good guys, using scientific method who are obviously right and the bad guys not using scientific method who are obviously wrong. It's two sets of scientists who are working and discovering facts within their theoretical frameworks. Some of this work is more or less translatable from one theory to the other and some of it is not.

Which theory do you choose to follow? Well, there is no simple one-off test to prove which is better. The idea of method seems to suggest that things are that simple, but that is just a cover story not a guide to the nitty gritty of such controversy. We have to weigh and judge; we have to decide which issues are crucial for us, weight relations or the family of metals; and how much should we invest in the different competing, non-comparable strongpoints. **We are being asked to judge apples and oranges without a single, rigorous standard we can apply to every aspect of both competing theories. That is why the idea of a simple method is so misleading, and that is also why the competition between theories, and the eventual choice of one over the other, is a social process, a human process of struggle, negotiation, interpretation, judgment and interaction amongst the relevant expert members of the group concerned with the debate.**

This is a key idea in understanding the practice of science and technology. Conflicts between competing theories are not simply resolvable by appealing to nature or method because the theories are always to some degree not strictly comparable. There is no Divine revelation, or revelation of method that can tell us which competing facts and opportunities to support and which to reject. Deciding between such theories is like deciding which car to buy, or which legal suit to believe. It is a matter of human judgment, interpretation and negotiation. For there is no hot line to reality via method that will simply rule in favour of one theory over the other. Each theory makes its own special pleadings in favour of its facts, its discoveries and while reasonable scientists do reach decisions and consensus, they can't do this by applying a simple method or test. They must argue, judge and negotiate a consensus in their professional actions much like industrial relations advocates, juries or political caucuses do.

The decision is made through what can best be termed a process of community consensus formation, the community being the relevant groupings of experts who can take part in the debate. This is fundamentally what we have in mind by saying that method doesn't control science, and that scientific discovery, and scientific knowledge is a product of human or social actions--a 'social construct' as we say.

The scientists who make such decisions do so by in effect placing their bets. They make informed judgements as to where the field is going to evolve in the next few rounds of research and negotiation. They take an informed judgement which depends very much upon where they are situated in the pecking order and institutional network of the research community.

In this case, if you're really interested in acids, and you have staked your career on being right about the oxygen theory of acids, you are going to be Lavoisier or one of his followers. If you studied with Lavoisier or expect his patronage for your next career step you may be looking at things this way, for his theory seems to be going somewhere with acids. If you are a metallurgical chemist and are interested in the family of metals, you may think that you should hold onto your investment in phlogiston chemistry. After all oxygen chemistry seems to offers little and will strip you of your claims to expertise if it wins, since it does not readily have any answer to what you already claim to know about metals.

There could be any number of biographical, institutional, professional, economic, and cultural reasons for you to weigh up and assess the theories, there is no one single standard method or test that will get you over the need to make informed judgements. Judgements informed in part by personal, professional, institutional and perhaps other interests or goals. **At the moment of decision there is no other way to decide, no method, test, or algorithm. But notice, we say at the moment of decision, not 200 years later. Today no one would go back to a phlogiston based chemistry, nor would they return to Lavoisier's oxygen theory. Our work, competences, investments in subsequent theories and practices are too huge to make such old ideas of any value. But that is not the issue. In the 18th century, they did not have the luxury of 20th century knowledge, and in the 20th century we do not have the luxury of 22nd century knowledge. Scientists must place their bets in the contexts of their own lives, times, careers, beliefs and goals. No magic method or time travel will retrospectively get them out of this necessity to socially negotiate the next agreed consensus, the next 'real discovery'.**

However, one side won in the end, and it was not because Lavoisier and his friends were more virtuous or diligent at using scientific method. No, I believe that Lavoisier and his friends had decisive advantages in the context of persuasion and negotiation. Lavoisier for example was a very important figure in the Parisian Academy of Sciences, the most important scientific institution of the day. He was not some provincial crank with a new idea. He was already a key figure who was using the leverage he possessed to sell his theory. French science was much more centralised than was say British or German science at that time. He could therefore mobilise the resources of publication, patronage, persuasion, and for chemists in his sphere of influence, he could also exercise sanctions.

In contrast Priestley also held some status and resources in the field of chemistry, but of a more provincial and less powerful nature. Furthermore, during the 1790s he got into political trouble for being too sympathetic toward the French revolution, and he was forced to leave England for America.

Tactics also come into this process of struggle, negotiation and placing of bets. A lot depends on how you decide to play or present your position. Lavoisier played for high stakes, he played 'hard ball'. He put his claims forward in the form "I have disproved the existence of phlogiston and discovered the existence of oxygen, and you must henceforth follow only me". But there were other ways to have played it, and had he had less resources he might have played it in a more temperate way, saying merely that caloric was mainly phlogiston, or phlogiston better understood, and that maybe oxygen should be considered as well. Had he been less radical in his claims, the path of negotiation may have been different and we might view him as a great phlogiston reformer, linking him more to his sources in earlier chemistry. In addition the pattern of support and rejection might have been different, leading to a different history in detail.

Here we can see that discoveries are not constructs just because they are linkages of concepts with human practices, but also because as with legal cases, styles of cars, political packages, their precise contours depend upon the political/institutional judgment of their inventors. Different political bets would produce different claims and discoveries.

Finally, new theories and the new facts they entrain are a package like new product lines or a new commodities. They are manufactured, they do not grow on trees. Oxygen theory, its facts and discoveries, are the joint social product of Lavoisier and his allies; phlogiston theory and its facts and discoveries are the joint social product of the phlogiston chemists. These products, theories and their facts are not sold accepted or rejected because they are true or false or even because they could be shown to be so. What actually happens is that scientists place their bets and wagers; they assess the uses of the theory and its pitfalls; the strengths of the theory and its weaknesses; the costs in career, resources, prestige etc and the benefits of accepting or rejecting the theory. The community struggles and negotiates and when enough scientists with sufficient power and influence have moved in one direction over the other then that theory has triumphed and will guide the next rounds of research.

This allows us, in concluding, to make some perhaps initially odd remarks about 'what it means to have won such a conflict of theories'. Does it mean that Lavoisier was absolutely correct and that Priestley was wrong. No it does not. We can see that this was a social process of consensus formation; there were good arguments on both sides, and complex calculations of personal, professional interest and investment prompted different chemists to place different bets. The fact that Lavoisier won does not show (except in mythic stories about science) that he had some special merit or insight that poor Priestley did not have--at the time the issue was a close one. Moreover, Lavoisier's own concepts were later modified and deformed, even by self-proclaimed followers of his, as they made new 'discoveries' and improvements to Lavoisierian chemistry. Today nobody believes the theory that Lavoisier claimed to have established by 1790. So again, we must ask ourselves what does it mean to have won?

We usually think of winning in science as 'finally finding the truth'--a brick in the wall has been found and mortared in. Now we can begin to see that winning in science means that your winning fact or discovery becomes the baseline for the next few rounds of debates and negotiations about further facts and discoveries. New or modified facts and discoveries that may well displace or radically change the very facts or discoveries in which your 'victory' earlier consisted, and this is precisely what happened to Lavoisier's chemistry. Priestley momentarily was a winner, then Lavoisier, whose concepts, suitably reshaped and renegotiated, seem to have had better staying power, or more clearly marked descendants, but for a moment Priestley had been a winner, a discoverer, just as Lavoisier was to be, and yet neither said what we now exactly believe to be the facts.

A winning achievement in science is not a lasting achievement, it is a license to be in on the ground floor of the next few rounds of significant negotiation. In the end all great discoveries become fossils. When you fully appreciate this, you will see that science must have a history that is not the simple one of good guys vs bad guys, but rather a history that is a complicated process of social and institutional interaction amongst scientists within a larger social context.