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Teaching Chemistry for All Its Worth: The Interaction Between Facts, Ideas, and Language in Lavoisier's and Priestley's Chemistry Practice: The Case of the Study of the Composition of Air

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Teaching chemistry for all its worth-the interaction between facts, ideas, and language in Lavoisier's and Priestley's chemistry practice: the case of the study of the composition of air

Kevin de Berg

Abstract Both Lavoisier and Priestley were committed to the role of experiment and observation in their chemistry practice. According to Lavoisier the physical sciences embody three important ingredients; facts, ideas, and language, and Priestley would not have disagreed with this. Ideas had to be consistent with the facts generated from experiment and observation and language needed to be precise and reflect the known chemistry of substances. While Priestley was comfortable with a moderate amount of hypothesis making, Lavoisier had no time for what he termed theoretical speculation about the fundamental nature of matter and avoided the use of the atomic hypothesis and Aristotle's elements in his *Elements of Chemistry*. In the preface to this famous work he claims he has good educational reasons for this position. While Priestley and Lavoisier used similar kinds of apparatus in their chemistry practice, they came to their task with completely different worldviews as regards the nature of chemical reactivity. This paper examines these worldviews as practiced in the famous experiment on the composition of air and the implications of this for chemistry education are considered.

1. Introduction

How should one go about the teaching and learning of a subject like chemistry? This is an ever present question resident in the mind of a chemistry teacher who knows that students will find chemistry an inherently difficult subject to understand. Henry Bent (1986) once inferred that a falling match, a reference to physics, was easier to describe and explain than a burning match, a reference to chemistry. Should one give preference to the discoveries, ideas, concepts, and language of *modern chemistry* realizing that to dwell on the ideas and experiences of the past would make chemistry an even more difficult subject to learn and understand? Or is it the case that chemistry is inherently historical and that in order to understand current ideas one must understand their epistemology? Should chemistry be taught predominantly as a laboratory-based subject, as a theoretically-based subject, or as a blend of the experimental and theoretical? Is there a 'scientific method' for doing chemistry and is there a 'teaching-learning method' for understanding chemistry? Are there well defined terms and procedures which represent the 'nature of chemistry' and if so, should students be introduced to them at the commencement of their chemistry education or at a later stage? And what might Lavoisier's and Priestley's chemistry practice, now over two hundred years old, have to do with these questions?

During the 1960's the *School Science Review* published a series of articles on chemistry teaching by J. Bradley from the University of Hull. These articles make for challenging reading even fifty years after they were written. Bradley (1965, p. 65) views chemistry teaching as "heuristic, historical, and formal". By heuristic, he (Bradley 1964a, p. 364) means the "kind of teaching and learning in which the pupil creates for himself the necessary concepts to interpret his own experience"; by historical, he (Bradley 1966, p. 707) means those ideas and experiments of the past which are "still a part of the living body of the science"; and by formal, he (Bradley 1964a, p. 364) means "that kind of teaching in which each essential type of experiment is carried out by the student, or shown to him, before the concept, or the item in a conceptual scheme or theory, is employed". Formal teaching also included the presentation of information that could not be demonstrated by experiment which

Bradley (1964a, p. 365) called "the dishonest appropriation of goods which happen to be lying around (or)… intellectual theft". So one can see how central experiment was to Bradley's teaching.

When students are in the early phase of their chemistry education, Bradley (1964a, pp. 364-368) introduces them to what he calls *The Copper Problem*. Students get to heat a piece of copper in a Bunsen flame and notice that the copper puts on two coats, a scarlet inner one and a black outer one. In answer to the question as to why the copper has gone black, one student suggests: "Oh sir, it has lost some vapours out of its inside" (Bradley 1964a, p. 366). Now this response would have resonated with Joseph Priestley's understanding of combustion-that the principle of change resided within the metal. Another possibility is that the copper could have been attacked from the outside, from the air, and this possibility would have resonated with Antoine Lavoisier's view of combustion.

Bradley recognizes how foolish it would be to expect students, by way of a heuristic or inquiry model, to stumble across the crucial experiment of the heating of mercury and its oxide in air[1]. Students in the class have tried to regain the air that has coated many metals by heating but to no avail. For student Robert, "He cannot turn his back, because the key of the gate to the kingdom of chemistry is the experiment of 1777. The circumstantial history of 1774 and 1777, the exciting tale of the good Priestley and the great Lavoisier, must be told and exemplified experimentally, for neither Robert nor teacher is clever enough to discover it. Such history lifts Robert up the sheer face of the precipice on to the next slope, where he may begin again to walk for himself" (Bradley 1964b, p. 127).

Other descriptions of Bradley's teaching, particularly in a student's early chemistry experience, could be summarised as follows: one must teach for insight, usually achievable by the heuristic method, as well as for information (1964b, p. 130); too much time has been devoted to atomic theory, chemical formulae and equations instead of determining the intrinsic and comparative properties of materials in the laboratory (1964b, p. 130); some measure of authoritarian teaching is permissible and even necessary (1964b, p. 127); at times the teacher must yield up their own personal authority before that of historical greats like Lavoisier (1964b, p. 127); and one should not focus on formal definitions or formal language too early in a student's chemistry experience (1964b, p. 131). So, in response to the questions raised in the opening paragraph of this introduction for students in the early phase of their chemistry education, Bradley favoured an approach which was laboratory focussed in the context of an inquiry or heuristic model for some of the time; an approach which did not rely heavily on theory; an approach which did not formalise chemistry to any great extent; an approach which drew upon key historical episodes; and an approach which recognised that there was a place for information giving in chemistry. Others, of course, may respond differently to the questions in the first paragraph but what is attractive about the Bradley approach is the way the historical Priestley/Lavoisier experiment was used in a heuristic setting. But is there something more in the Priestley/Lavoisier composition of air episode, including their understanding of the role of facts, ideas, and language in chemistry, that might prove of value to chemistry teaching at higher levels both for teacher and student? This paper is dedicated towards answering that question as well as considering any further issues in relation to the early years of a chemistry education.

2. Lavoisier and Priestley Background

Antoine Lavoisier (1743-1794) and Joseph Priestley (1733-1804) were French and English contemporaries respectively of the late 18th century with an interest in the composition of air. Their personal and scientific background feature in the biographies written by Bensaude-Vincent (1993); Donovan (1993); McKie (1935; 1952); Poirier (1996) [Lavoisier] and

Schofield (1997; 2004) [Priestley]. Biographies that compare Priestley and Lavoisier include Aykroyd (1935); Davis (1966); and Jackson (2005). In this paper we focus on that part of their chemistry practice related to the determination of the constituents of air. Chemists of the 18th century had been able to show that air could no longer be considered as one of the principle elements of nature, an idea that had been popular for centuries. Air was transformable and both Priestley and Lavoisier recognized this. The big question was related to how one could determine its constituents. History appears to be somewhat kinder to Lavoisier owing to his positive role in the chemical revolution, and rather dismissive of Priestley due to his reluctance to remove the idea of 'phlogiston', that invisible principle of combustion, from his chemical arsenal of concepts used to understand chemical change. This is in spite of the fact that there were many similarities between the two men both in practice and ideas.

Both Priestley and Lavoisier used equipment in their laboratories which was typical for 18th century chemistry: pneumatic troughs or basins, furnaces, large heating lenses, retorts, earthenware vessels, gas jars, bladders, syringes, delivery tubing, balances, glassware and so on. The only major difference here was that Lavoisier's equipment was more elaborate in its use of brass and finery whereas Priestley's laboratory resembled more of an elaborate kitchen. Basu indicates that both men "employed reactivity as a key chemical property to establish chemical distinctness", and that property-bearing principles like "the acidity or alkalinity of a substance would be explained by appealing to the presence of the appropriate principle in that substance" (Basu 1992, p. 447; p. 454). In fact, both chemists agreed that oxygen gas (Lavoisier's term) or dephlogisticated air (Priestley's term for the same gas) contained the acidifying principle. This was because when nitrous air (NO) was exposed to it in the presence of water, an acid was produced. In addition, the accompanying decrease in the volume of air exposed to nitrous air, due to the enhanced solubility of the air, was used by Priestley as a test for the presence of dephlogisticated air or oxygen gas (Priestley 1790b, pp. 115-116). In the 18th century chemists typically used the term *principle* to refer to that component of matter exhibiting certain chemical properties. The term *atom* would not be consistently used until the 19th century and beyond.

Given that there was much in common between Lavoisier and Priestley, what was it, then, that contributed to their differences? Basu (1992) suggests that Priestley thought that chemical distinctness depended only on the presence or absence of a constituent rather than on the appropriate proportion of constituents in a substance whereas Lavoisier gave precedence to proportion. Perhaps the underlying reason for this suggestion was Lavoisier's greater dependence on gravimetric mass measurements in his chemistry. Not that Priestley didn't use gravimetric mass measurements, but it was more consistently used by Lavoisier. However, there is some evidence that Priestley did recognize the role of proportion in chemical distinctiveness. When Lavoisier suggested to Priestley that water was not a simple substance but consisted of Priestley's phlogiston combined with the principle of acidity, Priestley responded as follows: "It must be acknowledged, that substances possessed of very different properties, may, as I have said, be composed of the same elements in different proportions, and different modes of combination" (Priestley 1790c, p. 543). Priestley knew that this would have to be the case if his nitrous air (NO), with different properties to water (H_2O), also, like water, consisted of phlogiston and the acidifying principle. Phlogiston was thought to be the entity or principle responsible for a substance's combustibility in air and the acidifying principle that entity responsible for acid production when dissolved in water.

Priestley and Lavoisier were different, not so much in their practice, if by practice we mean the laboratory apparatus and manual skills brought to the task. They were different in the worldview of chemical change they brought to their practice. Kuhn (1970, p. 118), in his chapter ten on worldviews, observes that, "Lavoisier..saw oxygen where Priestley had seen dephlogisticated air and where others had seen nothing at all”. Jackson (2005, p. 38) suggests that the different worldviews of chemical change arose from different scientific traditions: “Just as Priestley and Lavoisier were born into different classes, they were heir to competing scientific traditions-quality versus quantity; a deep search for *essence* versus a faith in things that could be measured”. The best way to illustrate the different worldviews is with the example where steam is passed over heated iron. In terms reminiscent of the times, the reaction[2] can be represented as follows.

$$\text{Iron + Steam} \rightarrow \text{Iron calx} + \text{Inflammable air}$$

Priestley describes the French understanding of this reaction at the time as follows. “Water, they say, is completely decomposed when it is made to pass over red hot iron, the iron imbibing the acidifying principle (oxygen), and the remainder going off in the form of inflammable air (hydrogen)” (Priestley 1790c, p. 546). The bracketed words in the quote have been inserted to enhance the clarity. According to Priestley’s understanding however, since metals consist of the metal calx and phlogiston, the iron releases its phlogiston when heated, leaving behind the iron calx (what the French called an oxide of iron), and the phlogiston combines with water to produce inflammable air (what the French eventually called hydrogen). Again, bracketed sections have been inserted for clarity. This is a fundamentally different way of understanding the reaction. To Lavoisier, the iron metal was a simple substance or element; to Priestley, it was a substance made up of calx and phlogiston. To Lavoisier, water was made up of two simple substances; to Priestley, water was a simple substance or element. To Lavoisier, what triggered the reaction was an engagement between the heated iron and the steam; to Priestley, what triggered the reaction was the release of phlogiston from heated iron.

Scholarship in the humanities thrives on the diversity of human expression and experience brought to bear on whatever happens to be the topic in question. Human diversity is thought not to play any significant part in the scholarship of the natural sciences since scientific advance is constrained by experiment. This is often the view communicated to our students across all educational levels. A study of the scientific practice and associated ideas of Priestley and Lavoisier is designed to show how important theoretical ideas are in the business of doing science. After comparing Priestley’s and Lavoisier’s general orientation to practice the paper will focus on their air composition experiments and how they interpreted the data. Both had an interest in chemistry education and commented on the role of history in that education in relation to their work on the composition of air. The paper concludes by providing readers with an example of how the 18th century composition of air experiment can be of value in a student’s chemistry education.

3. General Orientation to Practice: Facts, Ideas, and Language : Priestley

At the beginning of a section entitled, *Of the constituent Principles of the different Kinds of Air*, Priestley outlines his general scientific objective and disposition to facts and theories: “It is always our endeavour, after making experiments, to *generalise* the conclusions we draw from them, and by this means to form a *theory*, or *system of principles*, to which all the *facts* may be reduced, and by means of which we may be able to foretell the results of future experiments” (Priestley 1790c, p. 533). Priestley maintained that he spent the daylight hours in his laboratory and the evening hours doing his writing. This illustrates his commitment to the generation of experimental facts and his commitment to making sense of the facts by seeking generalisations or theories that might explain the facts. There were occasions when he gave the impression that experimental facts were more important than ideas, hypotheses, or theories: "our business is still chiefly with facts, and the analogy of facts…." (Priestley 1767, p. 480); and occasions when he indicated how valuable even roughly formulated hypotheses might prove: "For when a sufficient number of new facts shall be discovered (towards which even imperfect hypotheses will contribute) a more general theory will soon present itself; and perhaps to the most incurious and least sagacious eye" (Priestley 1790a, p. xliii).

There seems to have been a progression in Priestley's appreciation of hypothesis and theory from his early work on electricity to his work on the constitution of airs. In reviewing his experiments on electricity he says, "It may be said that I ought, at least, to have waited till I had seen the connection of my new experiments with those that were made before, and have shown that they were agreeable to some general theory of electricity. But when the facts are before the public, others are as capable of showing that connection, and of deducing a general theory from them as myself" (Priestley 1767, p. 579). However, in attempting to summarise his findings on the principles and constitution of the airs, he didn't hesitate to launch into theoretical speculation even if he had to later retract his ideas. This is typical of Priestley to the point where Lavoisier appears conservative and cautious in comparison. Here is Priestley being brutally honest and opening up his vulnerability:

> In my former publications I have frequently promised, and sometimes attempted, to give such a general theory of the experiments in which the different kinds of airs are concerned as the present state of our knowledge of them enabled me to do, and I cannot well decline attempting something of the same kind in this new edition of all that I have published before; though I acknowledge that I am very far from being able to satisfy myself with respect to it, and therefore cannot expect to give much satisfaction to others. When I published the first of my six volumes, I was not aware of much difficulty on this subject, but new experiments soon unhinged whatever I had thought the best established; and this has so often been the case, that my diffidence increases in full proportion to the increase of our knowledge. Fluctuating, however, as the present state of this branch of knowledge is, I shall not decline to give my present views of it; nor shall I find any more difficulty in retracting any opinion I shall now advance, than I have hitherto done in retracting what I have advanced before (Priestley 1790c, p. 534).

Priestley was extremely reluctant to introduce new terms to the language of his science and did so only out of necessity. For example, he saw no need to replace the term 'air' with the term 'gas' even though 'gas' was becoming more fashionable. He also saw no need to replace the term 'phlogisticated air' with 'azote' since nothing new would be communicated in such a change, both terms referring to stagnant air. The language he chose implied "no attachment to any hypothesis whatever" (Priestley 1790a, p. 10), except, he agrees, in the case of phlogisticated and dephlogisticated air, which depend on the hypothesised existence of 'phlogiston'. More will be said about language later.

4. General Orientation to Practice: Facts, Ideas, and Language : Lavoisier

In the 1930's the then professor of chemistry at University College London, F.G. Donnan, proposed that, while the first great synthesis of *physical principles* occurred toward the end of the 17th century and culminated in Newton's famous *Principia Mathematica,* the first great synthesis of *chemical principles* occurred toward the end of the 18th century and culminated in Lavoisier's great *Traité élementaire de chimie* (McKie 1935, p. 7). In the preface to this famous work Lavoisier sets forth his commitment to facts as determined from experiment and observation as follows: "We must trust to nothing but facts: These are presented to us by Nature, and cannot deceive. We ought, in every instance, to submit our reasoning to the test of experiment, and never to search for truth but by the natural road of experiment and observation" (Lavoisier 1965, p. xviii). It is in this context that Lavoisier gives no deep consideration to the constituent and elementary parts of matter as detailed in Greek philosophy such as in Aristotle's four elements of earth, air, fire, and water or indeed the atomic hypothesis of Democritus. He classifies a discussion of such as of a metaphysical nature, grounded in a philosophical tradition, and without any experimental justification. However, he does use the particle model (*molécules* in the French original) when describing the influence of caloric on a substance: "The same thing takes place, with respect to natural bodies; the intervals left between their particles are not of equal capacity, but vary in consequence of the different figures and magnitudes of their particles, and of the distance at which these particles are maintained, according to the existing proportion between their inherent attraction, and the repulsive force exerted upon them by the caloric" (Lavoisier 1965, p. 17). In categorizing his chemistry as a branch of physical science Lavoisier understood his chemistry to involve not only the facts of experiment which he sees as the objects of science but also the ideas which represent the facts and the language by which the ideas are expressed.

In contrast to the speculative understanding of what is meant by 'element', Lavoisier relies on a practical definition of element or principle of a body using the

> idea of the last point which analysis is capable of reaching. (Thus) we must admit, as elements, all the substances into which we are capable, by any means, to reduce bodies by decomposition. Not that we are entitled to affirm that these substances we consider as simple may not be compounded of two, or even of a greater number of principles; but since these principles cannot be separated, or rather since we have not hitherto discovered the means of separating them, they act with regard to us as simple substances, and we ought never to suppose them compounded until experiment and observation has proved them to be so (Lavoisier 1965, p. xxiv).

Lavoisier did not deny the existence of atoms as the ultimate constituents of substances but did not have at his disposal any experimental test for their existence. Bensaude-Vincent and Simon note that, "Lavoisier's famous definition (of element) cited above, while it puts into play what might be termed a positivist scepticism concerning the accessibility of the ultimate components of the material world, is not necessarily anti-realist…It is not the case that the ultimate constituents of substances are not real just because they are not yet known" (Bensaude-Vincent & Simon 2008, pp. 180-181).

Lavoisier's chemistry program involved determining the constituent principles of bodies, in terms of his practical definition of elements or principles, through the processes of analysis and synthesis. In the famous pair of air experiments Lavoisier believed that his heated mercury and mercury calx were able to divide air into two components, one that supported combustion and one that did not support combustion. How did one know that these components resided in the original air sample and were not the product of mercury or glass changing air into a different element or combination of elements? One way of checking this possibility was to combine the two separated components and check if the product of the combination had the same properties as the original air. However, it bears remembering that these procedures of analysis and synthesis did not originate with Lavoisier. Bensaude-Vincent and Simon indicate that alchemists "had long practiced the complementary processes of destruction and reconstitution as a way to counter accusations of trickery" (Bensaude-Vincent & Simon 2008, p. 86). Moran links Lavoisier's so-called modern chemistry with alchemy by suggesting that, "Separating the supposed rational purity of chemistry from the alleged logical impurities of alchemy as a way to establish the compelling features of a new chemical discipline is also misdirected because chemistry itself did not so much replace alchemy as subsume it" (Moran 2005, p. 184).

There is no doubt that when Lavoisier devised, conducted, and subsequently interpreted his composition of air experiment he depended upon ideas which were not just speculative but had some justification in experiment and observation. For example, he considered all substances to consist of a BASE component surrounded by heat matter which he called CALORIC. In the transition from solid to liquid to gas more caloric was added to the base and this idea was consistent with the experimental fact that one needed to heat a solid to convert it to a liquid and further heat was required to convert the liquid to a gas. One could say Lavoisier depended on the caloric theory in his investigations but the idea was consistent with practical circumstances. If Lavoisier was to comment on the nature and constitution of caloric he would have regarded himself as entering the field of speculation, an area he attempted to eschew at all costs. Just because an idea might be classified as theoretical doesn't mean it is necessarily speculative. Such theories can have profound practical applications. For example, in the composition of air experiment, if one regards the red solid left in the flask as resulting from the combination of mercury and a component of air, one would expect heat to be liberated in the reaction since there has been a conversion from gas to solid leading to a release of caloric.

Both Lavoisier and Priestley were anxious to use a language for substances which betrayed their chemical properties. This is why Lavoisier wanted to remove any reference to phlogiston from the names of gases since, as far as he was concerned, chemistry could be explained without reference to it and , in addition, there was no substantial evidence for its existence. Thus, 'dephlogisticated air' became 'oxygen' (meaning *acid producer*) and 'phlogisticated air' became 'azote' (meaning *depriving life*).

5. The Composition of Air Experiment: Priestley

When Priestley directed rays of sunlight onto a sample of *mercurius calcinatus* (mercuric oxide) with his large glass lens, he collected an air which had very similar properties to common air, that is, the air that we all breathe (Figure 1). The air was called *dephlogisticated air* because the *mercurius calcinatus* had to imbibe phlogiston from the surrounding air to again become metallic mercury thus suggesting that the surrounding air had become dephlogisticated in the process. After examining the air produced in some detail, Priestley says, "After this, I had no doubt but that the air from *mercurius calcinatus* was fit for respiration and that it had all the other properties of genuine common air" (Priestley 1790b, p. 113).

There were three properties Priestley relied on to reach this conclusion: a candle continued to burn brightly in the air; the volume of the air diminished when nitrous air (nitric oxide) was admitted to the dephlogisticated air over water (because nitrogen dioxide produced from nitric oxide and oxygen is very soluble in water); and the air supported the life of a mouse. Even though the air from *mercurius calcinatus*, possessing the same properties as common air, proved even better than common air in supporting respiration, Priestley concluded that, "all the constituent parts of the air were equally, and in their proper proportions, imbibed in the preparation of this substance (that is, the *mercurius calcinatus*), and also in the process of making red lead" (Priestley 1790b, p. 113).

So how did Priestley understand what happens when mercury is heated in common air to produce *mercurius calcinatus*? It appears that this presented somewhat of a problem for Priestley. Since a metal was thought of as consisting of its calx and phlogiston, heating in common air resulted in removing phlogiston from the metal, thus phlogisticating common air and leaving behind the calx (*mercurius calcinatus*). In Priestley's mind this still happened with mercury but, in addition, to explain the experiment shown in Figure 1, the constituents of common air must have been incorporated into the calx as well during the heating of mercury. The proof of this lay in the fact that when the calx was heated as shown in Figure 1, an air similar to common air was produced. Phlogisticating the air during the heating of mercury reduced its volume and contaminated the air thus reducing its capacity to support life.

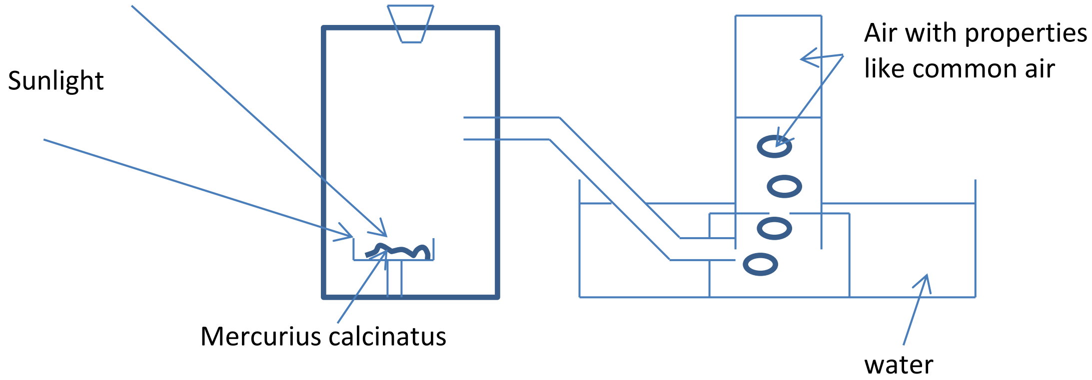

Fig.1 Heating of *mercurius calcinatus* to produce *dephlogisticated air* (oxygen) over water

But was Priestley able to identify the constituents of common air? It is at this point that the issue becomes rather confusing. At one point Priestley was sure that common air contained at least dephlogisticated air as previously described. However, in 1775 he proposed that, "there remained no doubt in my mind, but that atmospherical air, or the thing that we breathe, consists of the nitrous acid and earth, with so much phlogiston as is necessary to its elasticity….." (Priestley 1775, p. 23). And in 1790 he claimed that, "according to my latest observations, water…..is the basis of all kinds of air" (Priestley 1790c, p. 535). If one examines all the different kinds of air Priestley identified, including common air, it appears that the basic constituents were water, phlogiston, and the acidifying principle with different proportions leading to different types of air. Dephlogisticated air, for example, consisted predominantly of water and the acidifying principle. But Priestley never seemed to clarify unambiguously how many constituents were involved.

Priestley refused to call metal calxes the 'oxides of the metal', proposed by the French chemistry school. Both Priestley and Lavoisier agreed that the air obtained from *mercurius calcinatus* contained the acidifying principle, but there was a disagreement about some of the details. When red lead, called *minium*[3], is heated, yellow *massicot* [4] is formed with oxygen being liberated. Berthollet, the great French chemist, wanted to call *massicot*, an oxide of lead, since it produced water when heated in inflammable air (hydrogen). Priestley disagreed with this proposal since the acidifying principle (oxygen) had already been given off in the heating of *minium*[5]. So Priestley retained 'phlogiston' in the names of some of the airs or gases and the name 'calx' for the products of burning or combustion.

While Priestley was not averse to measurement it does not feature prominently in his analysis of air. His is certainly more of a qualitative approach than that adopted by Lavoisier.

6. The Composition of Air Experiment: Lavoisier

The apparatus used by Lavoisier is shown in Figure 2. Four ounces of pure mercury were heated over the furnace for a period of 12 days. The volume of air in contact with the mercury in the retort and bell jar decreased from 50 cubic inches to about 42 cubic inches over this time and 45 grains of red solid had formed on top of the mercury. The 42 cubic inches of air remaining in the system extinguished a glowing taper. When the 45 grains of red solid were heated in a retort over a furnace 41.5 grains of running mercury were collected and 8 cubic inches of elastic fluid with properties similar to that reported by Joseph Priestley were formed. That is, this elastic fluid supported respiration and combustion better than common air. All volumes were recorded at the same temperature and pressure (10^0 and 28 inches of mercury), all units referring to 18^{th} century French units.

It is surprising that Lavoisier didn't rely on experimental technique to ensure constancy of temperature and pressure given his emphasis on experiment and observation in chemical techniques. Instead he relied on a mathematical calculation based on Mariotte's

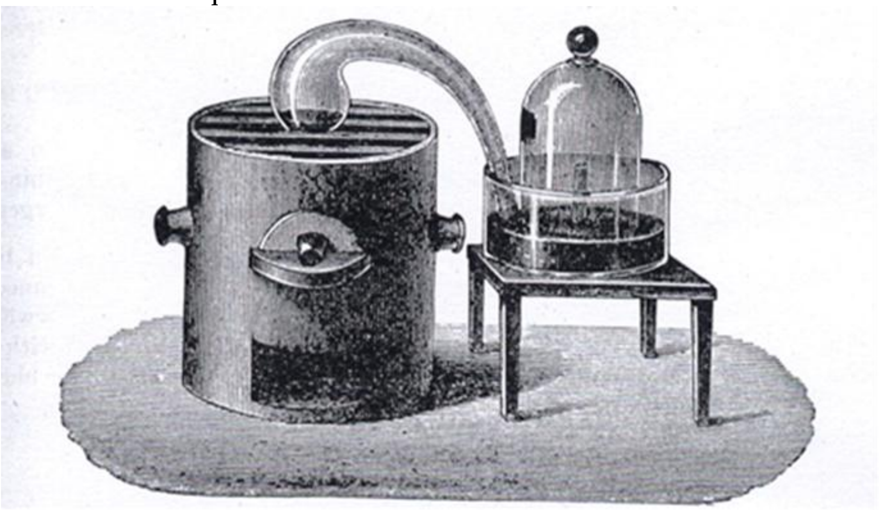

Fig. 2 Lavoisier's apparatus for studying the composition of air

(French) or Boyle's (English) law for pressure constancy and the then known expansion characteristics of a gas with temperature for temperature constancy. On the other hand, this may not be so surprising given Lavoisier's respect for the Abbé de Condillac, who promoted the use of algebra as an exact language, and the experimental difficulties associated with such a requirement of constant temperature and pressure. In the preface to his great work Lavoisier (1965, p. xiii) pays tribute to the Abbé de Condillac in these words: "While engaged in this employment (that is, the completion of the Nomenclature of Chemistry), I perceived, better than I had ever done before, the justice of the following maxims of the Abbé de Condillac, in his System of Logic, and some other of his works. 'We think only through the medium of words…Languages are true analytical methods… Algebra, which is adapted to its purpose in every species of expression, in the most simple, most exact, and best manner possible, is at the same time a language and an analytical method…The art of reasoning is nothing more than a language well arranged' ". Lavoisier (1965, p. xiv) then applies this logic to the relationship between facts, ideas, and language as follows: "Like three impressions of the same seal, the word ought to produce the idea, and the idea to be a picture of the fact. And, as ideas are preserved and communicated by means of words, it necessarily follows that we cannot improve the language of any science without at the same time improving the science itself; neither can we, on the other hand, improve a science, without improving the language or nomenclature which belongs to it". This, in a sense, represents the passion that motivated Lavoisier to write his *Elements of Chemistry*.

Albury (1986, pp. 207-208) claims that it was the:

> Linguistic, algebraic treatment of the classic methodology of resolution and composition that gave Condillac's *Logic* much of its initial appeal, because it seemed to hold out the promise that reasoning in the non-mathematical sphere could approach the analytic power and clarity which algebra achieves in mathematics, if only the languages of the various non-mathematical fields of study could be systematically reformed……What he (Lavoisier) found, then, in Condillac's methodological combination of constituent analysis and linguistic algebra was a means of expressing his theory of composition in a quasi-algebraic fashion through the language of chemistry. If the science of chemistry was not yet ready to become wholly mathematical, it could at least lay claim to an approximation of the theoretical rigour and precision of mathematics in its language.

Thus, in the case of chemical nomenclature, the name, sulphuric acid, indicated that the substance belonged to the 'genera' of *acid*, that is, a substance containing the acidifying principle, oxygen, and contained sulphur as its *base* which identified the 'species' of acid. Sulphurous acid was "formed by the union of oxygen with sulphur by a lesser degree of oxygenation than the sulphuric acid" (Lavoisier 1965, p. 223). Thus, quantity of oxygen was linked to the suffix of the name of the acid; '*ic'* indicating more oxygen than 'ous'. Also, a name like 'sulphuric acid' communicated more information than the old 'vitriolic acid'.

What conclusions did Lavoisier draw from his composition of air experiment? It is suggested that the conclusions may be categorized in four ways as follows.

1. Air can be divided, or decomposed (a term sometimes used by Lavoisier) into two elastic fluids, one that supports combustion and one that doesn't support combustion.

This conclusion appears to be consistent with Lavoisier's practical definition of 'element' and his endeavour to appeal to his experimental observations. One could argue, of course, that there could be more than one elastic fluid present that supports combustion and more than one that doesn't support combustion. This argument could be true, of course, but it is speculative, going beyond what Lavoisier's experiment revealed. Lavoisier gives no indication of this possibility in his text. Priestley doesn't appear to be as bold as Lavoisier here. Given that air consists predominantly of nitrogen and oxygen Lavoisier's conclusion was fairly close to what the composition of air turned out to be but of course things would have been different if argon had occupied say 30% of the air.

2. In Lavoisier's own words: "Although this experiment furnishes us with a very simple means of obtaining the two principal elastic fluids which compose our atmosphere, separate from each other, yet it does not give us an exact idea of the proportion in which these two enter into its composition" (Lavoisier 1965, p. 37).

From the quoted experimental values one would expect oxygen and nitrogen to constitute 16% and 84% of the atmosphere by volume respectively. Lavoisier does suggest later that the percentages are more like 27% and 73% respectively. While he does refer to some of the experimental difficulties associated with this experiment and the need to repeat the procedure a number of times his main reason for inaccuracy appears to reside in a speculation about the relative attraction of the respirable part of air (oxygen) to mercury, to caloric, and to the non-respirable part of air (nitrogen). In other words, according to Lavoisier, not all the respirable or vital air was attracted to the mercury. Some remained attracted to its caloric and some remained attracted to the mephitic or non-respirable part of the air. This approach appears to be out of character with Lavoisier's insistence on referencing his ideas to his experimental results. Perhaps Joseph Priestley was partially justified in accusing Lavoisier of relying on metaphysical speculation (Brooke 1995, essay 1, p. 18).

3. If the base of respirable air is combined with mercury it follows that caloric must be disengaged during the process.

Because of the slowness of the mercury reaction and the close proximity of the furnace to the reaction chamber it was difficult to demonstrate that the reaction was exothermic. So Lavoisier chose a faster reaction, one with iron, first noted by Ingenhouz (Lavoisier 1965, p.39), to demonstrate this principle. A large amount of heat and light were detected from the iron reaction which confirmed Lavoisier's understanding of the role of caloric in the states of matter. It is interesting to note that $\Delta_r H^o$ for the mercury reaction is -181.7 kJ/mol while for the iron reaction it is -1648.4 kJ/mol, so the difference would have been quite noticeable.

4. New language for the two elastic fluids can be based on their chemistry.

We know that Lavoisier was assisted by de Morveau, Berthollet, and de Fourcroy in the development of a new nomenclature for chemistry (Lavoisier 1965, p. 5). As far as the elastic fluids associated with the composition of air experiment are concerned, Lavoisier retains "the word *air* to express that collection of elastic fluids which compose our atmosphere" and the term *gas* as "a generic term expressing the fullest degree of saturation in any body with caloric; being, in fact, a term expressive of a mode of existence" (Lavoisier 1965, pp. 50-51). He then draws attention to the two gases composing atmospheric air and defines the base of each gas according to its chemical properties. "We have given to the base of….the respirable portion of the air, the name *oxygen*, from, *όξύς*, *acidum*, *γείνομαι*, *gignor*; because, in reality, one of the most general properties of this base is to form acids by combining with many different substances. The union of this base with caloric we term *oxygen gas*, which is the same with what was formerly called *pure* or *vital* air" (Lavoisier 1965, pp. 51-52). This gas was also called *dephlogisticated air* by Priestley based on his use of the phlogiston theory which Lavoisier set out to replace with his oxygen theory of combustion. The term *oxide* was to be used to name substances formed by the binary combination of oxygen with simple substances or elements. So, oxygen combined with lead was to be called 'the grey oxide of lead'. Such a term reflected the fact that the name which was to be chosen for the base of the gas "had to be changeable into adjectives and verbs" (Lavoisier 1965, p. 50).

As far as the non-respirable part of atmospheric air was concerned, Lavoisier says, "we have been satisfied to derive the name of its base from its known quality of killing such animals as are forced to breath it, giving it the name of *azote*, from the Greek privitive particle, ά- and, ζωή *vita*; hence the name of the noxious part of atmospheric air is *azotic gas*" (Lavoisier 1965, p. 52). Lavoisier indicates that there was some thought about calling this gas *nitrigen gas* (Lavoisier's spelling) given the fact that this element was known to be a part of *nitric acid*. However, the decision was finally made in favour of *azotic gas*. Binary substances formed from the combination of this element with simple substances were to be called *azides*. The common English[6] term used these days is, of course, nitrogen gas and its nitrides, although sodium nitride is also known as sodium azide, the substance used for filling the car air bag.

7. Significance of the composition of air experiments for studies in the nature of science

Schofield reminds us that Priestley was, "not a philosopher of science, in the sense of his having constructed a logical and coherent view of nature and of scientific activity.....He did, however, set forth a few general principles on the nature of science..." (Schofield 1997, p. 153), which we have already noted in relation to fact, theory, hypothesis, experiment and observation. Neither could one call Lavoisier a philosopher of science in the modern sense. In fact there is much with which both Priestley and Lavoisier could agree when it comes to the nature of scientific practice.

Scholars have found it interesting to compare the scientific approaches of Priestley and Lavoisier. Lavoisier's approach to writing accounts of his experiments is suggestive of carefully pre-planned objectives and a rigorous approach to measurement and the recording of results. Priestley's approach was to record a story of what failed, what worked, how many times he tried an experiment, and a record of serendipitous events along the way. Brock notes that, "Priestley's practice was to write literary 'cookery books' that encouraged everyone to participate, urging that by repeating or conducting their own experiments, men and women could draw their own conclusions rather than having conclusions handed down to them by specialists and experts" (Brock 2008, p. 66). While one must admire Lavoisier's insights and rigour, Gillispie has noted that, "Perhaps there is always a danger that it will impoverish inquiry to elevate the logic of existing science into precepts of method" (Gillispie 1960, p. 218). While Priestley's account of his air experiments appears messy and convoluted while Lavoisier's account is logical and well-prescribed, both accounts have proved pertinent to chemistry. According to Gillispie, "Chemistry profited, therefore, from the curious, the almost symbiotic relationship between Priestley and Lavoisier, however unwelcome to both" (Gillispie 1960, p. 218). After all, it was Priestley who directed Lavoisier's attention to the *mercurius calcinatus* experiment.

Experimental anomalies were critical facts for Priestley but consequently, as Brock points out, "he was unable to 'idealize' chemical reactions and see them in a simple form....When science idealizes, it leaves anomalies for later followers to add explanations such as 'side reactions', the presence of impurities, altered physical conditions etc. But, as examples from the past repeatedly show...., simplification is a necessary feature of scientific progress and the first step towards advancing knowledge" (Brock 2008, p. 78).

Lavoisier's self-confessed emphasis on facts derived from observation and experiment and his self-confessed avoidance of speculation sounds very much like Chalmers' definition of a widely held common-sense view of science.

> Scientific knowledge is proven knowledge. Scientific theories are derived in some rigorous way from the facts of experience acquired by observation and experiment. Science is based on what we can see and hear and touch, etc. Personal opinion or preferences and speculative imaginings have no place in science. Science is objective. Scientific knowledge is reliable knowledge because it is objectively proven knowledge (Chalmers 1982, p. 1).

This is an example of scientific induction which implies that there is such a thing as a rigorous scientific method. In contrast, Priestley communicates the results of an experiment not designed to test any theory necessarily but simply to find out what happens. There is a haphazardness and chance-like nature associated with his experiments. When he commences his account of the preparation and properties of the air obtained from heating *mercurius calcinatus* (mercuric oxide), he reflects that, "more is owing to what we call chance, that is, philosophically speaking, to the observation of events arising from unknown causes, than to any proper design, or preconceived theory in this business" (Priestley 1790b, pp. 102-103). In relation to testing the goodness of the air he had prepared, he observed, "If, however, I had not happened, for some other purpose, to have had a lighted candle before me, I should probably never had made the trial; and the whole train of my future experiments relating to this kind of air might have been prevented" (Priestley 1790b, p. 114).

Feyerabend viewed science in a less prescriptive way than many. He (Feyerabend 1993, p. 18) was adamant that "the idea of a fixed method, or a fixed theory of rationality, rests on too naïve a view of man and his social surroundings". The boundary between facts and theories was fuzzy or blurred according to Feyerabend's understanding of science. For example: facts and theories are much more intimately connected than is admitted by the autonomy principle (1993, p. 27); facts and theories are never as neatly separated as everyone makes them out to be (1993, p. 51); theories, observations, and experimental results are not as well defined as we think (1993, p. 51); and facts that enter our knowledge are already viewed in a certain way and are, therefore, essentially ideational (1993, p. 11). It would seem that Feyerabend's opposition to a fixed method would have resonated more with Priestley than Lavoisier, but in both Priestley's and Lavoisier's mind the boundary between facts and theories would have been more clearly defined than that described by Feyerabend. But were Priestley and Lavoisier simply unaware of their commitment to some overarching model or theory in their observation statements? The question of the extent to which Priestley's and Lavoisier's observations were guided by a commitment to some idea or theory will be taken up in the next section.

One should note that Feyerabend's view of facts and theories was obtained from the Galileo episode of falling bodies where the observation language used depended very much on one's belief in either a moving earth or a motionless earth. The question is: Does this view of facts and theories translate into other sciences as well? In response to a criticism from Ian Hacking, Feyerabend (1987, p. 293) admits that "the sciences are more complex and many-sided than I assumed". In other words, there may be some circumstances in other sciences where the observation language is not as starkly dependent on a theory as might have been the case in the Galileo episode.

Sir Peter Medawar expresses his opposition to the rigid view of scientific method in this way: "The essential point is that there is no logically rigorous procedure by which an inductive "truth" can be proved to be so", and, "there is no such thing as a calculus of discovery or a schedule of rules by following which we are conducted to a truth" (Medawar 1984, pp. 14,16). While these statements refer to the impossibility of proving a universal law statement from some specific cases for which the law seems to apply, they also imply what philosophers refer to as the *theory-dependence of observation* and that *scientific ideas or theories are underdetermined by experiment.* Are these ideas relevant to the composition of air experiments and to the teaching of chemistry?

7.1 Theory-dependence of Observation

During the course of his twelve-day experiment Lavoisier observed that the 'bulk of air' in contact with the mercury had decreased from 50 cubic inches to a value between 42 and 43 cubic inches and that the red particles formed on the surface of the mercury being heated by the furnace had amounted to 45 grains. Philosophers like Chalmers (1982) and Feyerabend (1993) would argue that even seemingly precise observation statements like those made by Lavoisier pre-suppose theories about bulk and mass even though these are well-established theories. So, according to Chalmers, "Precise, clearly formulated theories are a pre-requisite for precise observation statements. In this sense theories precede observation" (Chalmers 1982, p. 29). Such well-formulated theories are often classified as low-level theories probably because their formulation is such that they border on being classified as a fact. Godfrey-Smith addresses this point as follows: "For example, maybe observational reports assume 'theories' that are so low-level that the testing of real scientific theories will never be affected. We can think of the assumption that objects generally retain their shape when we are not looking at them as 'theoretical' in a sense, but the effect of this assumption on observation reports does not usually matter to testing in science" (Godfrey-Smith 2003, p. 157). In this sense we would agree that Lavoisier's statements about the bulk of air and the mass of red matter are factual enough for our purposes here. The question is, is there any evidence that Lavoisier showed a commitment to theories or ideas which were not as well-established at the end of the 18th century to be classified as facts at the time he did his experiments on the composition of air?

Lavoisier was committed to a binary view of chemical substances in that they were classified as consisting of a *base* combined with the matter of heat known as *caloric.* While Lavoisier refused to speculate as to the constitution of a *base* and its *caloric* a commitment to the binary nature of substances suggested that when mercury combined with a proportion of atmospheric air to produce the red solid product, heat should be released since less caloric was associated with the solid state compared to the gas state. Here is Lavoisier's reasoning: "Since, during the calcination of mercury, air is decomposed, and the base of its respirable part is fixed and combined with the mercury, it follows, from the principles already established, that caloric and light must be disengaged during the process" (Lavoisier 1965, p. 38). However, as already stated elsewhere in this paper, it was difficult to observe such a phenomenon in the case of the mercury reaction which turns out to be slow with a relatively small negative enthalpy compared with other metals. The close proximity of the furnace also compounded the problem. Lavoisier then describes a reaction with iron which illustrates a large release of heat and light to confirm his prediction. The desire to illustrate the importance of caloric in the combustion reaction seems to have been Lavoisier's motivation for the iron experiment. "It is, however, easy to render this disengagement of caloric and light evident to the senses, by causing the decomposition of air to take place in a more rapid manner. And for this purpose, iron is excellently adapted, as it possesses a much stronger affinity for the base of respirable air than mercury" (Lavoisier 1965, p. 39).

During a description of the observations associated with the iron experiment, Lavoisier demonstrates a commitment to the law of conservation of mass without acknowledging such. This is how he describes his observations at one point:

> If all the attention has been paid to this experiment which it deserves, the air will be found diminished in weight exactly equal to what the iron has gained. Having therefore burnt 100 grains of iron, which has acquired an additional weight of 35 grains, the diminution of air will be found exactly 70 cubical inches; and it will be found, in the sequel, that the weight of vital air is pretty nearly half a grain for each cubical inch; so that, in effect, the augmentation of weight in the one exactly coincides with the loss of it in the other" (Lavoisier 1965, p. 44).

However, it would appear that Lavoisier's use of the word 'exactly' is somewhat unwarranted given the results he himself reports. He reports an increase in the weight of iron as 35 or 36 grains and speaks of vital air as being 'pretty nearly' half a grain per cubic inch. Priestley tried to duplicate Lavoisier's work but without success and accused Lavoisier of over-estimating the accuracy of his measurements. In the case of the mercuric oxide experiment, Brock claims that,

> Priestley's objections to Lavoisier's chemistry were often, indeed, usually, perfectly valid....For example, in the decomposition of mercuric oxide Priestley consistently got less mercury back than he started with. In any case, he observed, Lavoisier's pretence of measuring to four or five places of decimal was pure window dressing. To this Lavoisier replied that expensive and superior apparatus was needed to achieve precision which, of course, was anathema to Priestley's democratic approach to chemical experimentation (Brock 2008, p. 75).

It would appear that Lavoisier's commitment to the law of conservation of mass guided his reporting of results. This seems to be particularly the case given the difficulty he experienced in obtaining consistent results for the percentage composition of respirable and non-respirable air in atmospheric air. Gillispie (1960, p. 231) makes a pertinent comment in this respect: "Scientists have sometimes written that Lavoisier formulated the law of conservation of matter. The reality was simpler. He assumed it".

In order to add experimental justification to his proposition that atmospheric air consists of two elastic fluids, Lavoisier uses the principle of analysis and synthesis, the belief that adding together the products of analysis would lead to the synthesis of the original material. Referring to his mercury experiment he offers a proof of the 'two elastic fluids' proposition as follows:

> As a proof of this important truth, if we recombine these two elastic fluids, which we have separately obtained in the above experiment, viz., the 42 cubical inches of mephitis, with the 8 cubical inches of respirable air, we reproduce an air precisely similar to that of the atmosphere and possessing nearly the same power of supporting combustion and respiration, and of contributing to the calcination of metals (Lavoisier 1965, p. 37).

This kind of analysis looks appropriate but didn't convince chemists such as Priestley and Cavendish (Gillispie 1960, p. 217) probably because of the opportunistic way Lavoisier seemed to combine his results, particularly given the fact that he earlier reports his volume of mephitis as between 42 and 43 cubic inches. Even some of Lavoisier's own countrymen were not impressed by his analysis. Jean-Claude De La Métherie, according to Lissa Roberts (1992, p. 264), "opposed the entire web of Lavoisier's claims", and continued his advocacy of phlogiston.

It is clear that Priestley was well aware of the biases and prejudices associated with experimentation. In Section 1 of his account of dephlogisticated air (oxygen) he comments that, "when the decisive facts did at length protrude themselves upon my notice, it was very slowly, and with great hesitation, that I yielded to the evidence of my senses... (This hesitation and slowness) I attribute to the forces of prejudice (the belief that atmospheric air was a simple elementary substance), which, unknown to ourselves, biases not only our judgments, properly so called, but even the perceptions of our senses" (Priestley 1790b, p. 103). So Priestley was not as naive about observation and experiment as one might be led to believe in some of his statements.

It is true that some of Priestley's statements suggest he had a *low view* of theory, speculation, and generalization in chemistry. For example, in the preface to his studies on air he says, "We are, at all ages, but too much in haste to *understand*, ... the appearances that present themselves to us. If we could content ourselves with the bare knowledge of new facts, and suspend our judgment with respect to their causes, till, by their analogy, we are led to the discovery of more facts, of a similar nature, we should be in a much surer way to the attainment of real knowledge" (Priestley 1790a, p. xxix). What Priestley seems to be objecting to here is not the formation of hypotheses or theories to aid our understanding but the speed with which people are prone to speculate without sufficient facts at their disposal.

7.2 Scientific ideas or theories underdetermined by experiment

Using modern terminology the main finding of Lavoisier's composition of air experiment was that atmospheric air consists of the two gases oxygen and nitrogen. However, Lavoisier's conclusion was based on the application of just one experimental test, that of support or non-support of combustion. Nowhere in his *Elements of Chemistry* does Lavoisier suggest the possibility that two or more gases could be responsible for supporting combustion or conversely, the possibility of two or more gases being responsible for not supporting combustion. Of course, we must remember that Lavoisier did not have at his disposal the atomic-molecular view of matter which was to come with Dalton in the early 19th century. Lavoisier distinguished matter on the basis of its 'principles' rather than on the weight of its atoms. Since two different 'principles', that of supporting or not supporting combustion, were evident from his experiment this signified two different gases or elastic fluids. However, from a modern perspective, given the more complicated composition of the atmosphere (see Table 1), we could say that Lavoisier's experiment did not grant him a complete picture of the atmosphere's composition. That is, the composition of the atmosphere was underdetermined by his experiment. While "no single theory ever agrees with all the known facts in its domain"(Feyerabend 1993, p. 39), it is also true that no single experiment reveals all the facts of the domain being investigated. In a section dealing with underdetermination Godfrey-Smith notes that, "Over time, structures and objects in the world can move from being so inaccessible that only speculative model-building can be applied to them, to being so accessible that their study is routine" (Godfrey-Smith 2003, p. 223). While a study of the noble gases in the atmosphere could be considered routine today, it certainly wasn't the case in the 18th century. As noted, Lavoisier refused to speculate on the possible existence of more than two gases in the atmosphere, probably because this went beyond his immediate experimental results, yet he did not hesitate to speculate on mercury's incapacity to attract all the base of vital air in the atmosphere as a possible reason for inconsistent results in the air experiment.

What is rather fortuitous is that Lavoisier was amazingly close in his estimate of two elastic fluids composing the atmosphere as can be seen from Table 1. According to Table 1 nitrogen and oxygen combined constitute 99.03% of the atmosphere. It took nearly 100 years for chemists to determine that other gases were present. In 1894 Rayleigh observed that the density of nitrogen obtained from iron reduction of nitrogen oxides was 1.2505 g/cm^3 whereas the nitrogen density in the atmosphere appeared to be 1.2572 g/cm^3 (Rayleigh 1894). While one might conclude that experimental error was responsible for this small difference in density value, careful experimentation by Rayleigh and Ramsay led to the discovery of argon which is nearly 1% by volume (Rayleigh & Ramsay 1895) of the atmosphere. Imagine what the implications would have been for Lavoisier's experiment if argon had been 10% by volume. The composition of air would have been even more underdetermined.

At times Priestley was beset by contaminated samples of solids or the presence of extraneous airs in his experiments. He sometimes observed fixed air (CO_2) being produced from reacting red lead (Pb_3O_4) with inflammable air (H_2) and he took this to signify that the acidifying principle was present in red lead but not massicot (PbO) in which case no fixed air was observed. How one could get CO_2 from a reaction between Pb_3O_4 and H_2 seems perplexing but it shows how theory can not only be underdetermined by experiment but also misled by experiment.

8. Relevance of the study of air composition for current chemistry education

Today we have much more sophisticated techniques at our disposal for analysing air, such as mass spectrometry, which was not available to Priestley or Lavoisier. Air composition has become a central component of environmental chemistry so there is no question about its usefulness in current chemistry education. However, does a historical approach to air composition have any relevance to a chemistry education which currently has a multitude of techniques and concepts already at its disposal for a student to learn? What is interesting is that Lavoisier has rendered us his opinion on this subject in the preface to his *Elements of Chemistry*:

> …if I had allowed myself to enter into long dissertations on the history of the science and the works of those who have studied it, I must have lost sight of the true object I had in view and produced a work the reading of which must have been extremely tiresome to beginners. It is not to the history of the science, or of the human mind, that we are to attend in an elementary treatise: our only aim ought to be ease and perspicuity and with the utmost care to keep everything out of view which might draw aside the attention of the student.. (Lavoisier 1965, pp. xxxii - xxxiii).

Table 1 The current values for the chemical composition of air

Name	**Symbol**	**% by volume**
Nitrogen	N_2	78.084 %
Oxygen	O_2	20.9476 %
Argon	Ar	0.934 %
Carbon Dioxide	CO_2	0.0314 %
Neon	Ne	0.001818 %
Methane	CH_4	0.0002 %
Helium	He	0.000524 %
Krypton	Kr	0.000114 %
Hydrogen	H_2	0.00005 %
Xenon	Xe	0.0000087 %

Lavoisier, then, implies that there are a "sufficient number of difficulties" already in the science without burdening students with the history of the subject. The concern expressed here appears to be in the use of extended historical accounts for beginning students of chemistry. However, Lavoisier admits that the criticism of his lack of a historical treatment in the *Elements of Chemistry* is perhaps better founded than the criticism of his new nomenclature. So it would appear that his criticism does not relate to history of science as such but to its extended use in introductory chemistry. Bradley was also cautious in the use of history unless its content was "still a part of the living body of science. It is …chemistry we are to teach-not the history of chemistry" (Bradley 1966, p. 707). The use of the Priestley/Lavoisier composition of air experiment, obviously regarded as part of the living body of chemistry, proved central, however, to the copper problem posed by Bradley. The use of history remains a challenge for chemistry educators.

Priestley took a somewhat different view of history and education. Here is what he says in the preface to his first volume of *Experiments and Observations on different kinds of air*.

> I am sorry to have occasion to observe, that natural science is very little, if at all, the object of *education* in this country, in which many individuals have distinguished themselves so much by their application to it. And I would observe that, if we wish to lay a good foundation for a philosophical taste, and philosophical pursuits, persons should be accustomed to the sight of

> experiments, and processes, in *early life*. They should, more especially, be early initiated in the theory and practice of *investigation*, by which many of the old discoveries may be made to be really *their own*; on which account they will be much more valued by them (Priestley 1790a, p. xxix).

Bradley would have been passionate about Priestley's 'sight of experiments, processes and investigations in *early life'.* In his mind these activities were to take precedence over formal theories at this stage. On one occasion he (Bradley 1964a, p. 366) lamented that, "The young people of this country come hopefully to school asking for the bread of experience; we give them the stones of atomic models". Of course, Priestley was not as constrained as Lavoisier in that he had basically not changed his system of chemistry whereas Lavoisier was proposing a complete overhaul of chemical understanding and terminology. This may be why they had a different orientation to history. While Lavoisier was proposing a new nomenclature for chemistry even for 'beginners', Bradley and Priestley, while not denying the importance of nomenclature, gave priority to investigative work in the laboratory, initially using words of their own choosing and encouraging students to do the same. Priestley wanted chemistry to remain an activity of the general public, not an activity of an elite which was his perception of Lavoisier and the European school.

Answers to the questions posed in the introduction to this paper ultimately depend on the prior chemistry experience of the students. For students with little or no chemistry experience Bradley has demonstrated how important it is to limit exposure to the models, theories, and formalisms of chemistry initially and allow students to experience as much laboratory chemistry as feasible. Students are more likely to be creative in their responses to experiences in the laboratory under these circumstances. They learn that negative results are as fundamental to chemistry as positive results. The need to arrive at an answer to a problem as quickly as possible has proved detrimental to our education. Feyerabend (1993, p. 194) speaks to this issue when he says, "how disastrous an effect the drive for instant clarity must have on our understanding". It is interesting to note that it was in a creative laboratory setting that a student of Bradley's suggested that the black copper coating arose because something escaped from the copper when heated. This something was called phlogiston by Priestley. The students recapitulated the fundamental question in 18th century combustion: was the trigger for a change to the metal, when heated, something that resided within the metal or something that resided outside the metal? It would seem that this kind of experimental background provides an excellent foundation for when the formalisms of chemistry such as the nomenclature for chemical formulae, equations, and theoretical models, are introduced later in a student's chemistry education. In particular, for students entering tertiary education with a significant background in chemistry, can the Lavoisier/Priestley episode assist students in appreciating the formalisms of chemistry?

The Lavoisier and Priestley air composition study is used with the author's tertiary students in first-year BSc study. These students have already studied chemistry to the end of year 12. The historical component is administered in the form of an assignment and is embedded within the chemistry topic on the gas laws. The Lavoisier-Priestley study introduces students to two different ways of understanding a chemical reaction. After having discussed with students the phlogiston and oxygen models of combustion, they are asked to respond in writing to questions like that shown below.

1. From the experiments already described (Lavoisier and Priestley experiments) can you think of one major objection to the phlogiston model? Outline your objection below.

2. **Some phlogistonists suggested that phlogiston might actually carry negative weight and increase the buoyancy of a metal in air. Suggest why they may have come to this conclusion. Do you think their suggestion was feasible? Explain. [Hint: What would you expect if phlogiston increased the buoyancy of a metal by acting like a parachute?]**

3. **In view of what we have said about the phlogiston model, what would a phlogistonist have understood the role of carbon monoxide to be in the reaction: $Fe_2O_3 + 3CO \rightarrow 2Fe + 3CO_2$**

4. **From the viewpoint of modern chemistry, what would you understand the role of carbon monoxide to be in the reaction: $Fe_2O_3 + 3CO \rightarrow 2Fe + 3CO_2$**

5. **From your knowledge of modern chemistry, how is the metal in the metal oxide different to the elemental metal that had reacted and how is the oxygen in the oxide different to the elemental oxygen that had reacted?**

6. **From Priestley's point of view, how does a metal calx (oxide) differ from the metal?**

The assignment does not draw upon Lavoisier's and Priestley's air composition study exclusively but includes a treatment of the principles of the mass spectrometer and concludes with an analysis of the operation of the car air bag: the emphasis being on how history informs the way we go about doing and understanding chemistry today. Students typically struggle with units of measurement, for example, and it has been found that an historical treatment is well-placed to communicate the *arbitrary* nature of our units of measurement. Of course, not only students struggle with units of measurement. Kerr (1965, p. vi), in his English translation of *Elements of Chemistry*, comments on the difficulties of unit conversion as follows: "He at first intended to have changed all the weights and measures used by Mr Lavoisier into their correspondent English denominations, but, upon trial, the task was found infinitely too great for the time allowed".

It so happens that Lavoisier used old French units of measurement in his reporting of results. These units prevailed predominantly before the 19^{th} century with the decimal system being adopted somewhat later on. In *Elements of Chemistry* mass is given in *grains* where 1 French grain is equivalent to 53.11 mg; volume is given in cubic inches where 1 French cubic inch is equivalent to 19.836 cm^3; length is given in inches where 1 French inch is equivalent to 2.707 cm; and temperature is quoted in Reaumur degrees where 1 Reaumur degree is equivalent to 4/5 of a Celsius degree. The Reaumur thermometer was based on the freezing point of water taken as 0^o and the boiling point of water taken as 80^0. To form a connection with the ideal gas law of modern chemistry, one can get students to evaluate the gas constant *R* using the data associated with Lavoisier's experiment. Lavoisier quotes his oxygen sample as weighing 0.5 grains per cubic inch and the temperature and pressure conditions as being 10^o and 28 inches on the barometer. Converting these units into the Standard International (SI) units, assuming ideal gas behaviour, and taking the molar mass of oxygen as 32 g/mol, leads to a value of *R* of 8.48 J $K^{-1}mol^{-1}$ which is within 2% of the known value (8.314 J K^{-1} mol^{-1}). One could do this kind of calculation in other ways such as using the known *R* value to calculate the molar mass of oxygen and so on.

Stoichiometric calculations can also be done using Lavoisier's data. For example, the following questions are asked in the context of the ideal gas law and units of measurement.

7. **What was the mass in grams of mercuric oxide (HgO) (red calx) produced in Lavoisier's experiment? Show calculations.**

8. **Write down a balanced chemical equation for the reaction between mercury and oxygen to produce mercuric oxide. Use subscripts (s, l, g) to indicate state.**

9. **Use the guidance above (about unit conversions, mole calculations, use of the ideal gas law) to determine how many cubic inches (French) of oxygen would theoretically have been required to produce 45 grains of red calx (mercuric oxide).**

10. **Deviations from the experimental volume (assuming this volume to be correct) could be due to our assumptions of temperature and atmospheric pressure. The deviation could also be due to an inaccurate measurement of the mass of calx (HgO) produced. Considering these three variables separately what changes to temperature, atmospheric pressure, and amount of calx would bring the theoretical volume closer to the experimental volume [8 cu.in]?**

The use of the historical assignment has also shown that students have difficulty in interpreting what an experiment has achieved and what it has not achieved. They are not familiar with the following style of question used to assess an understanding of Lavoisier's air experiment.

Assess your understanding of this topic by selecting <u>one or more</u> alternatives for the following question by circling the appropriate letter(s). In each case you are asked to justify your choice(s). Keep in mind that in the late 18th century when Lavoisier and Priestley did most of their work scientists had little idea of the composition of air and formulae and atomic weights were not known as we know them today. Think of the enormous task this presented to chemists of the 18th century.

Lavoisier's experiment on the heating of mercury in air

(a) categorically proved that air consisted of a mixture of only two gases.

(b) showed that air does participate in combustion reactions.

(c) demonstrated that air consists of a part that supports combustion and a part that doesn't.

(d) proved that mercury and oxygen react in the ratio of two parts mercury to one part oxygen.

Justify your choice(s) from the incidents in this story.

While we may be expecting too much of students to transport themselves into an 18th century context, the kind of analytical skills required to answer the question are just the kind of skills necessary to interpret their own experience in a current laboratory.

A consideration of Lavoisier's air composition experiment also gives one an opportunity to address other ways of determining the composition of air. With internet services at their disposal students are quite capable of locating other ways of determining air composition. There are a range of experiments, for example, in the *Journal of Chemical Education* for determining the oxygen content of the air ranging from the use of pyrogallol (Munro 1928), nitric oxide (Najdoski et al 2000), and steel wool (Vera et al 2011). Adaptations of some of the original historical experiments can be found in Fowles (1937). Thinking about what is common or different about these approaches to that used by Lavoisier can be quite instructive.

9. Conclusion

In relation to the questions raised in the introduction to this paper, it has been suggested that the answers depend very much on whether one is dealing with introductory chemistry or chemistry at a more advanced level. In both cases, however, historical episodes can be used creatively to enhance the learning experience of students. The composition of air experiment dating from the end of the 18^{th} century is an example of such a case. For students just beginning their study of chemistry Bradley made use of the Priestley/Lavoisier experiments to act as a 'launching pad' or 'precipice climber' that enabled the students to continue their own investigation of metals and metal oxides. Here was a metal oxide (HgO) that could yield its imbibed air at a Bunsen temperature. Bradley's admission that one could not have expected the students to stumble across the Priestley/Lavoisier experiments as a result of their own investigation or construction parallels the concern expressed by Matthews (1997, p. 12) about constructivism: "If knowledge cannot be imparted and if knowledge must be a matter of personal construction then how can children come to a knowledge of complex conceptual schemes that have taken the best minds hundreds of years to build up"? For students with significant chemistry experience the Priestley/Lavoisier experiments can be used to probe a little deeper into the significance of our chemical nomenclature and to provide some insight into the arbitrary character of units of measurement.

What can one say about Priestley's and Lavoisier's use of facts, ideas, and language in their air composition experiment? Lavoisier expected thermal energy to be released in his mercury-air reaction based on his commitment to the role of caloric in nature. While this release of thermal energy could not be observed in the case of mercury due to the slowness of reaction and close proximity of the furnace, it was confirmed in the case of the burning of iron. Also, the reporting of results seemed to mirror a commitment to some notion of chemical affinity and to the law of conservation of mass in chemical processes although the latter was not stipulated. Priestley was committed to the role of phlogiston in the composition of materials including air even though it was difficult to isolate as a material substance. Lavoisier and Priestley were committed to using a language for describing the components of air that was related to their chemical properties. Lavoisier selected *oxygen* and *oxide* (acid producer and product of combustion respectively) for the component of air that supported combustion and respiraton. This was considered superior to the former names of *dephlogisticated air* and *calx* used by Priestley. *Azote* and *azide* were chosen by Lavoisier for mephitic or noxious air and the product of burning an element in this air respectively. Thus, with some small variations, we owe much to Lavoisier for our current chemical nomenclature.

The philosophical discussion of the underdetermined character of experiment is particularly relevant to a student's laboratory experience. Students find it very difficult to decide what can and cannot be deduced from an experiment. This is really not surprising given the fact that Lavoisier also had difficulty. In the gravimetric determination of the formula of a metal oxide, for example, students will tend to decide on a formula that is expected rather than one suggested by the experimental results. In the determination of the formula of a tin oxide, the experimental results might suggest the possibility of SnO_2 or

Sn_2O_3 as the formula but if students have already studied the theory of tin oxides they will invariable select SnO_2 and ignore Sn_2O_3 even though the experimental results are not decisive one way or the other.

The language used in *Elements of Chemistry* and *Experiments and Observations on different kinds of Air* to describe a chemical process will appear very convoluted to a current student. For educational settings, therefore, there is a case for producing modern English versions of the original works suitable for student reading. This remains a major task for those convinced of the value of using history to assist in the teaching of chemistry.

Lavoisier argued that the language and concepts of chemistry were already difficult enough for a neophyte in chemistry without introducing the added complications associated with the history of the subject. Therefore, how can those of us committed to the history of chemistry justify our use of it in the teaching and learning of chemistry? Isn't the language and conceptual structure of even upper-level chemistry difficult enough without adding components of history to the mixture? However, Priestley argued that doing historical experiments gave students a sense of ownership of the practice and ideas of the science. It has been argued in this paper that there are benefits in using history if it is embedded within or amidst current concepts and informs those concepts. Of course, philosophers and historians may argue reasonably that this is too pragmatic an approach. What about the place of our chemistry in the grand scheme of intellectual and cultural life, of its place in the history of ideas? Feyerabend (1993, p. 21) argues that the "history of a science becomes an inseparable part of the science itself". In the introduction to his four-volume treatise on the history of ideas, Peter Watson (2009, p. xl) observes that many philosophers have divided human intellectual history into three stages. While there is nothing sacrosanct about a threesome, Watson chose the *Soul*, *Europe*, and the *Experiment* as his three stages. While Francis Bacon (1561-1626) was one of the first in the modern period to give a clarion call to the important role of observation and experiment in science, Lavoisier and Priestley, along with others, championed the call in the 18th century. Granting our students the opportunity to witness a big picture approach to chemistry and the diversity of its ideas, that is, one which positions chemistry in the broader context of the history of ideas, remains a challenge but one we envisage will be ultimately rewarding.

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Endnotes

1. Mercury is the only metal which imbibes air and releases air within the temperature range of the Bunsen burner.
2. In modern terms the reaction is represented as : $Fe(s) + H_2O(g) \rightarrow FeO(s) + H_2(g)$.
3. Pb_3O_4 in modern terms.
4. PbO in modern terms; massicot was well known to the French as a mineral used in painting.
5. One should note here that PbO doesn't liberate oxygen on heating whereas PbO_2 and Pb_3O_4 ($PbO_2.2PbO$) do liberate oxygen.
6. Azote in French; azoto in Italian; azot in Polish.

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