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LAVOISIER THE EXPERIMENTALIST

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Historians have paid more attention to Lavoisier the theorist than to Lavoisier the experimentalist. His conceptions of heat, the gaseous state and the composition of the atmosphere, his theories of combustion and of oxygen as the acidifying principle, his definition of an element and the reordering of chemical composition, his attacks on the phlogiston theory and his reform of the nomenclature of chemistry have all been thoroughly analyzed. Much scholarship has been devoted to the origins of his interest in these subjects, to the genesis of his ideas concerning them, and to the influences of other thinkers on his views. In part because the Chemical Revolution is treated as the construction of a new conceptual foundation for that science, Lavoisier has been viewed predominantly as a great theorist. It is frequently pointed out that at critical points he borrowed the experimental findings of others - especially those of the experimentally brilliant Joseph Priestley - and reinterpreted their results to fit his emerging theoretical framework. Some have even maintained that Lavoisier himself did not make major experimental discoveries.

Lavoisier is also known as the author of the fundamental principle of the conservation of mass. In the *Traité Élémentaire*, whose bicentennial we are celebrating this year, he wrote (1):

> ... nothing is created, either in the operations of art, or in those of nature, and one can state as a principle that in every operation there is an equal quantity of material before and after the operation.

It is recognized that this statement was the operating principle on which Lavoisier based his "balance sheet" method of experimentation; but the priority given to Lavoisier as a theoretician has prompted historians to wonder why he located the statement of so general a principle in a detailed discussion of fermentation rather than in a broader context. If one follows closely Lavoisier's prolonged investigation of fermentation, however, a very reasonable explanation for this connection becomes apparent. The fermentation reaction he viewed as a difficult, almost climactic test of his experimental method. As he put it in an earlier paper on fermentation that he did not publish, for a simple case there is no difficulty following a chain of reasoning in which the equation between the materials and the products of a chemical change is implicit. It is in handling a complicated case like fermentation that it is most important to keep this principle firmly in mind (2). That example alone should suggest that it would be fruitful to place more emphasis than is commonly done on the details of Lavoisier's experimental practice.

Historians here frequently noted that Lavoisier practiced quantitative "balance sheet" methods long before he stated the general principle on which they are based. His first notable experiments on the transmutation of water in 1768-70 relied on that method, and it pervaded all of his experimental investigations through the next two decades. There has been, however, an implicit assumption that Lavoisier's most significant experimental achievement was simply to adopt this criterion and the quantitative methods necessary to implement it. Making them actually work has not been viewed as a major problem once the "airs" in which Lavoisier was interested had been incorporated into the balances. When we follow Lavoisier's investigative pathway, however - in particular when we reconstruct his experimental ventures at the intimate level recoverable from his laboratory notebooks - we find that he did not have a global method for ensuring that his balance sheets would balance out; that they frequently did not; that he encountered myriad errors, the sources of which he could not always identify with certainty; that he often had to calculate indirectly what he could not measure directly; that he exerted great ingenuity in the management of his data so as to make flawed experiments support his interpretations; and that he devoted much care and effort to the design of experiments so as to obviate such difficulties, but that he often settled for results he knew to be inaccurate, using his faith in the conservation principle to complete or correct the measured quantities. Much of his scientific success, I would claim, is rooted in the resourcefulness with which Lavoisier confronted the many pitfalls that lay along the quantitative investigative pathway he had chosen. He was, in fact, the most innovative experimental chemist of his age. He invented a whole new way to perform chemical experiments, and it required all of his considerable technical skill and critical judgment to make it succeed.

Recently there has been some discussion over the question of whether the innovations that Lavoisier introduced into experimental chemistry were modeled on experimental physics. It is argued that, in breaking with the qualitative methods of earlier chemistry, he emulated a quantitative approach already manifested in the physics of his time (3). This point of view has some interest; but unless we can identify specific aspects of Lavoisier's experimental apparatus and methods that he derived from experiments carried out in the domains that were regarded as parts of physics, it will not carry us far toward an understanding of Lavoisier's investigative approach. It is, moreover, a question which I do not believe would have concerned Lavoisier as much as it does current historians. Lavoisier's scientific interests and background were multifaceted. He had studied physics, mathematics, chemistry, mineralogy, geology, and other subjects, and he exploited ideas or methods drawn from any of them that he found pertinent to the problems in which he engaged himself. The most realistic portrait of Lavoisier's scientific orientation, I think, is that given by the late Carl Perrin in a beautiful article entitled "Research Traditions, Lavoisier, and the Chemical Revolution." From the mid-1760's, when he committed himself to a career in science, Perrin points out, Lavoisier was "continually on the lookout for what he called 'une belle carrière d'expériences a faire', a fine course of experiments to run" (4). He sought, in other words, problems that would open up lines of investigation that promised to lead him to novel insights. He was ready to pursue such opportunities into whatever disciplinary areas they might lead. That pragmatic quality remained characteristic throughout his career.

I would now like to illustrate Lavoisier's experimental approach by discussing in some detail several concrete examples. From the time he engaged himself in 1773 in a broad investigative program to study the processes that fix or release "elastic fluids" from other bodies, until the end of his career 20 years later, there was a remarkable continuity in the experimental problems he pursued, the methods he applied, and the apparatus he used. During those two decades, the problems, methods, and apparatus evolved together from simple beginnings toward ever greater complexity and refinement. In his early experimental set-ups we can readily discern improvised adaptations of equipment that had long been in use. Later he increasingly resorted to equipment designed and constructed especially for each type of investigation; but the descent of his elegantly crafted later apparatus from the crude early ones is self-evident.

My first example is from a series of experiments that Lavoisier published early in 1774, that were performed during the course of the preceding year, on the reduction of lead ore. His purpose was to measure the quantity of elastic fluid given off in the reduction by a given quantity of lead calx. Prior experiments showing that lead calx did release an air had constituted one of the prime discoveries that prompted him to initiate his long research program on the fixation and release of airs. The apparatus is shown in figure 1. On the left is a furnace containing a retort fabricated from four pieces of iron soldered together. In it Lavoisier placed the lead ore mixed with charcoal. The bell jar in the middle was inverted in a basin of water, and the water level raised inside the bell jar by partially evacuating the space above it. The surface of the water was covered with a layer of oil to prevent the air released into the air space from dissolving in the water. As the retort was heated, the elastic fluid released passed from the retort into the recipient at the base of the inverted bell and up the vertical tube, causing the water level to descend. After the apparatus had cooled, so that the expansion effect due to heating had been

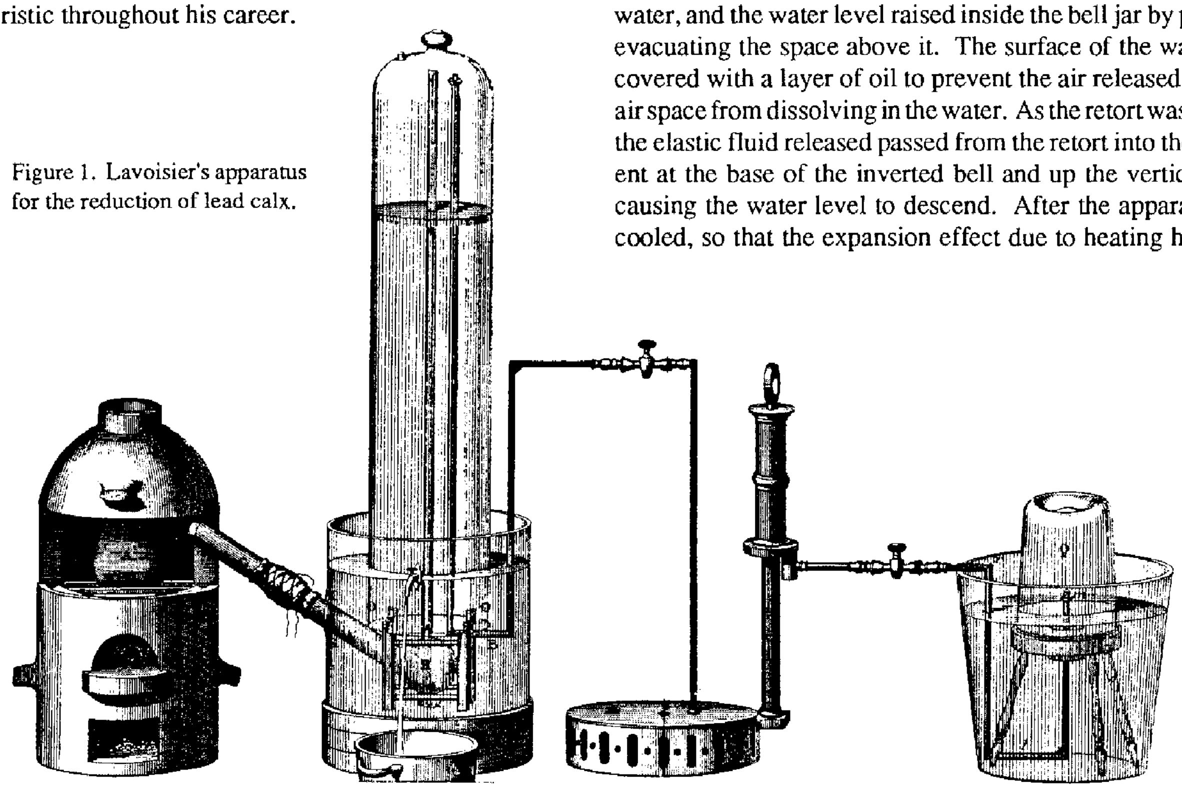


Figure 1. Lavoisier's apparatus for the reduction of lead calx.

eliminated, Lavoisier marked the change in the level of the water and from that he calculated the volume of the elastic fluid released from the lead calx (5).

Did Lavoisier model the design of this experiment on experimental physics or did he derive it from existing chemical practice? If we examine the apparatus closely, we see that it includes components from three distinct sources. On the right is a piston-operated vacuum pump. Such vacuum pumps descended from the pump invented by Robert Boyle in the 17th century. Boyle was both a chemist and a natural philosopher, but we associate his experiments using the pump mainly with physics, and it was part of the repertoire of 18th century physics as well. The vacuum pump served here, however, only the subsidiary purpose of raising the water level in the bell jar.

The furnace and retort on the left derive from the most traditional equipment of the chemical laboratory. Lavoisier had, in fact, begun with an ordinary glass retort, but had found it unusable because the lead ore attacked the glass. He then tried ordinary clay retorts, but they were porous enough to leak a little air. Requiring an absolutely air-tight system, he was, after a number of failed attempts, driven to have a special retort fabricated in iron. This modification is, I believe, typical of the pragmatic moves through which Lavoisier began early in his career to adapt standard chemical apparatus to the new demands of his methods.

The inverted bell jar central to the experiment is, of course, a modification of the pneumatic flask invented nearly a half century earlier by Stephen Hales. Lavoisier himself wrote that "the idea" for the apparatus "came originally from Hales" (6). Figures 2 and 3, showing two of Hales' experimental arrangements, confirm visually that they were the source for Lavoisier's apparatus (7). Can we say that Hales' experiments constituted a part of physics or of chemistry? Readers of Hales' *Vegetable Staticks* will know that the inspiration for his measurements, whether of the blood pressure in a horse, the height to which sap can rise in plants, or the quantities of air that can be "fixed" in solid bodies, was Isaac Newton; and therefore that it is traceable to one of the greatest achievements in physics (8). Hales' interest in the fixed airs derived more immediately, however, from his study of plants and was incorporated into a book about his experiments on plants. We might, therefore, just as well derive the pneumatic apparatus from botany as from physics or chemistry. Subsequently, Guillaume-François Rouelle, the popular teacher of chemistry in France, incorporated Hales' pneumatic experiments into his chemical lectures, where Lavoisier undoubtedly first encountered them (9). Moreover, a plate in the well-known *Encyclopedia*, published in 1777, depicting a typical chemical laboratory, shows a pneumatic apparatus among the more traditional equipment of the chemistry laboratory (10). On balance, therefore, the methods that Lavoisier adapted to this crucial experimental problem seem to be associated more directly with experimental chemistry than with experimental physics. Nevertheless, the composite sources for his overall design, as well as the experimental problem itself, reinforce my earlier suggestion that Lavoisier's approach is not easy to classify primarily as physical or chemical. He was practical enough to find his models wherever they could help him.

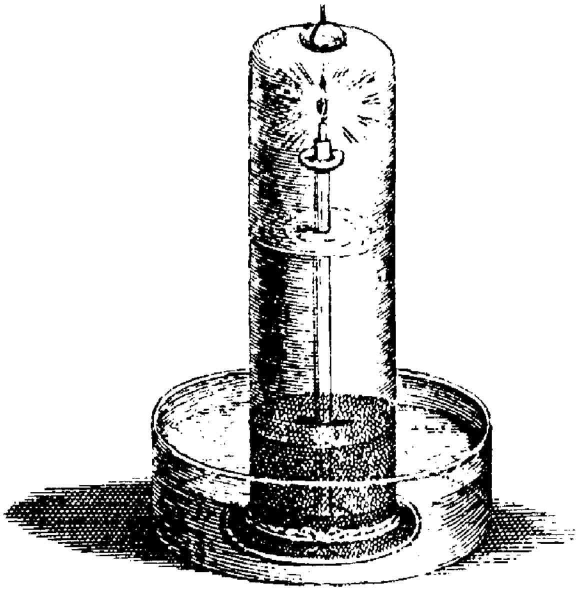


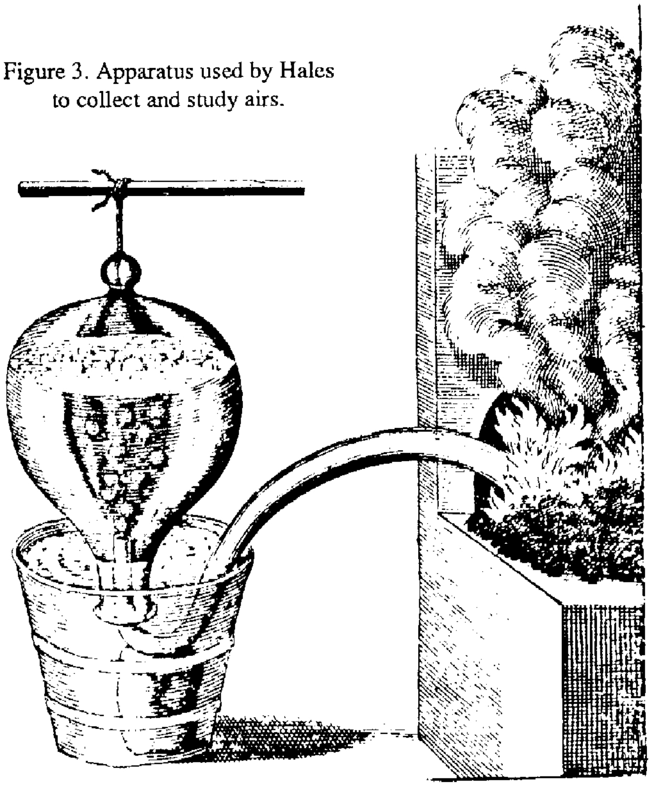
Figure 2. Apparatus used by Hales to collect and study airs.

In the execution of this experiment Lavoisier encountered obstacles typical of many of his efforts to balance the matter existing before and after an operation. Ideally he would have shown that the difference between the lead ore he had placed in the retort and the lead he collected from it afterward equaled the weight of the elastic fluid disengaged; for his aim was to confirm that the lead ore was composed of the lead and the elastic fluid. There were, however, complications. The difference between the ore and the lead was 6 gros 6 grains. He had measured, however, the volume rather than the weight of the elastic fluid. Since he was not certain of the nature of that fluid, he could not be certain of its density either. If it were ordinary air, the volume of 560 cubic inches would have weighed only 3 gros 41 grains. If it were the same elastic fluid (fixed air) released in the reaction of an acid with lime, the density would be somewhat greater - from the results of such an experiment he had estimated a density of 575/1000 grain per cubic inch. Even then the weight would be only 4 gros 34 grains, "and there still remains a *deficit* by weight of 1 gros 44 grains" - that is, of about one-fourth (11).

Seeking to account for this discrepancy, Lavoisier suspected, since a few drops of water had accumulated in the small receiver, that perhaps the lead ore had contained "a portion of water." To check that possibility he reduced the same quantity of lead in an ordinary retort with a large recipient in which he hoped more of the water might collect. He obtained, however, only 24 grains of water, far too little to cover his deficit. His only remaining recourse was to assume that additional water vapor had been carried away by the current of the elastic fluid produced in the reduction (12). Thus in this, as in so many other cases, Lavoisier's balance sheet did not balance. There were factors he could not control. His faith in the principle was not an outcome of his experimental experience, but an axiom without which he could not conduct his experiments at all. Lavoisier did not need proof of its validity - he simply could not imagine any rational alternative to the view that the weight of the matter present before an operation is equal to that resulting from it.

In this experiment Lavoisier had to contend also with the possibility that the elastic fluid had been disengaged not from the lead but from the charcoal essential to its reduction. Through additional experiments he was able to show that the charcoal consumed was not sufficient to supply all of the elastic fluid, so that some of it, at least, must have come from the lead ore. At the time he published these experiments, however, he still did not know how much that was (13). That he was willing to make public an investigation leaving so much to be desired from a quantitative standpoint may be viewed in part as a mark of his youthful ambition, in part because he regarded his results as a progress report on ongoing investigation; but a more basic reason is that the result sufficed for his present purpose, which was only to confirm that the calx of a metal was a combination of the metal with an elastic fluid. A more complete balance sheet would have been nice, but was not necessary for the pragmatic argument he wished to make.

Figure 3. Apparatus used by Hales to collect and study airs.



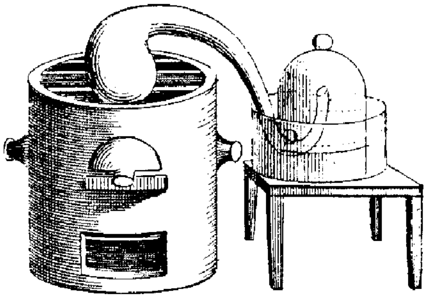


Figure 4. Lavoisier's apparatus for the oxidation of mercury and the reduction of mercury calx.

During those formative years that led Lavoisier by the Fall of 1777 to a general theory of combustion and his first full break with the phlogiston theory, he relied upon experimentation and reasoning that was essentially quantitative, but that did not require precise quantitative results. Experiments combining conventional chemical processes and apparatus with the pneumatic bell jar and the identification of elastic fluids continued to play the central role. The most decisive and brilliant experiment during these years, Lavoisier's famous analysis and synthesis of the air by calcining mercury and reducing the resultant calx without charcoal, typifies his approach. Figure 4, taken from the *Traité*, depicts the apparatus first used in April 1776 for that experiment. The physical resemblance to Stephen Hales' original experimental arrangements is obvious. An ordinary chemical retort has its neck curved so that it will connect with the interior of a pneumatic flask rather than with a conventional receiver. In the experiment Lavoisier showed that "about 1/6" of the air in the flask is removed during the calcination. Replacing that portion with about the same amount of "dephlogisticated air" - as he still called the air obtained by reducing mercury calx without charcoal - he restored the original atmospheric air. The tests by which he confirmed that he had reconstituted ordinary air were partly qualitative, but included also Priestley's semi-quantitative nitrous air test. Quantitative measurements were thus basic to the conclusions Lavoisier reached. In the first step the mercury gained weight while the air lost volume; in the second step the same volume of an air produced in an operation in which mercury calx loses weight was added to the air remaining after the first step. For Lavoisier's purpose, however, quantitative accuracy was not essential (14).

It was during the years 1781-1785, when Lavoisier extended the conceptual structure and methods that he had established during the previous decade to more complex situations and during which he encountered technically more difficult problems than those in the early years, that he was pressed to strive not merely for quantitative results, but results reliable enough and precise enough to use as foundations for further calculations. He was probably also influenced during this period, through his collaboration with two able mathematicians, Pierre-Simon Laplace and Jean-Baptiste Meusnier, to seek greater vigor both in his experimental measurements and the calculations he made with his data.

A key transition point in Lavoisier's movement from rough quantification toward a drive for accuracy was the calorimetric experiments that he performed with Laplace during the Winter and Spring of 1783 to measure specific heats and the heat released in combustions and respiration. Their aim in the design of the ice calorimeter, they wrote afterward, was to find "a method appropriate to determine those quantities with precision." Under certain restricted conditions, most notably that they were able to operate only on those rare days when the ambient temperature was within two degrees of the freezing point of water, they were able to achieve remarkably good results, given that these were the very first measurements of their kind. Their investigative goals propelled them, however, to also seek more accurate results in other types of experiments (15-16).

A prime objective of the calorimeter experiments was to confirm Lavoisier's theory that respiration consisted of the combustion of carbon. They wished to show that respiration released the same quantity of heat in producing a given quantity of fixed air that the combustion of charcoal yielded. To do so they required four separate measured quantities: calorimetric measurements of the heat released by a guinea pig over a given time period; of the heat released in burning a given quantity of charcoal in oxygen; the amount of fixed air produced by the animal in the given time; and the fixed air released by burning a given quantity of charcoal. Attaining accurate measurements of the latter two quantities proved to be more difficult than the calorimetric measurements (17).

I wish to concentrate for now on the measurement involving charcoal. Although Lavoisier had concluded in 1777 that fixed air is composed of carbon and oxygen, this was primarily a deduction from the overall theoretical framework he had by then constructed. Having earlier shown that phosphorus and sulfur absorb oxygen to form acids, he reasoned analogously that charcoal absorbs oxygen to form the fixed air which Torbern Bergman had shown also to have acidic properties. To prove this relationship experimentally was, however, more difficult than for the other two cases, because the product was also an "air;" and there is no evidence that Lavoisier had done so at that time. Now, in 1783, he had to tackle the problem not only of establishing that relationship empirically, but of determining quantitatively the proportions between the charcoal consumed and the fixed air formed (18).

Lavoisier carried out the operations for the combustion of charcoal in a pneumatic apparatus similar to the one shown in figure 5. Although it appears simple in comparison to some of the apparatus that he had by this time employed, much like the pneumatic troughs that Priestley and other predecessors had used, this apparatus too bears refinements reflecting Lavoisier's imaginative experimental craftsmanship.

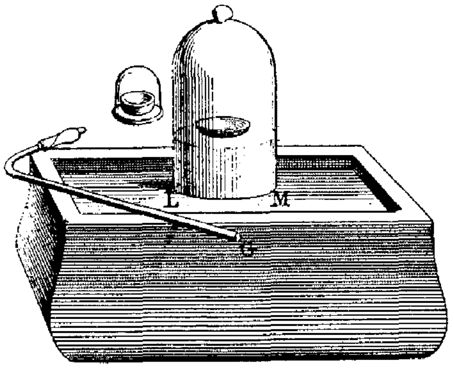


Figure 5. Simple pneumatic trough used by Lavoisier.

In the experiment, Lavoisier filled the bell jar with oxygen, placed a weighed quantity of calcined charcoal in the dish, marked the level of the mercury on the side of the bell jar, ignited the charcoal, and after the apparatus had cooled, marked the level to which the mercury had risen. He then inserted caustic alkali in another dish to absorb the fixed air formed, marking the further rise of the mercury. Disassembling the apparatus, he afterward weighed the charcoal again to estimate the quantity consumed. In principle his method allowed him to determine the quantities of oxygen and carbon used and of the product, fixed air, thus permitting a total measured balance sheet of the chemical operation. In practice he encountered serious anomalies. The total decrease in volume would be expected to represent the oxygen consumed, whereas the decrease due to the caustic alkali would represent the fixed air evolved. By now he had better figures for the densities of both airs and, after correcting for the barometric pressure and temperature, he ought to be able to calculate reliable values for the weights of the two airs. There was, however, an unresolved ambiguity, because in respiration experiments there appeared to be no decrease in volume during the conversion of oxygen to fixed air, whereas in this experiment, supposed to represent the same process, there was a substantial diminution. Still more awkward for Lavoisier was that the total weight of the oxygen and charcoal consumed was greater than that of the fixed air produced by an amount equal to nearly one-third of the charcoal consumed. Lavoisier reported in his laboratory notebook rather dryly that "there seems to be a portion of the weight lost." When he wrote up the experiment for the memoir on heat that he and Laplace presented shortly afterward to the Academy of Sciences, Lavoisier glossed over these discrepancies. For his immediate purpose he did not require the complete balance sheet, and he did not include one in the paper. He merely calculated the quantity of fixed air formed in the combustion of one ounce of charcoal, as a basis for the further calculation of the equivalence of combustion and respiration. He was circumspect enough to state that "we can ... not be entirely sure of its precision until we have repeated" the experiment several times. Here again the practical Lavoisier was willing to present publicly quantitative results he knew to be in some respects unsatisfactory, so long as they were adequate to his short-term objectives (19).

A year later such a result was no longer adequate to Lavoisier's needs, and he had moreover a new conceptual basis available to interpret in a more complex manner what was involved in the combustion of charcoal. Even as he and Laplace were making public their studies on heat in June 1783, they were participating in the momentous discovery that water can be synthesized by burning the very light air known then as "inflammable air" in oxygen in a closed container. The discovery that water is decomposable not only claimed much of Lavoisier's investigative attention during the following year, but cast new light on many of the experiments that he had previously carried out on the age-old assumption that water is one of the elements. One of the early reappraisals that Lavoisier made was of the experiment he had carried out ten years before on the reduction of lead that I described earlier. The small amount of water that had appeared in the receiver then, he could now explain in a very different way than he had done; the charcoal must have contained, in addition to carbon, a portion of inflammable air that combined with some of the oxygen in the lead ore to form the water (20).

During the spring of 1784 it became an urgent matter to determine accurately the proportions of carbon and oxygen in fixed air, because Lavoisier and his collaborator Meusnier needed that data in order to interpret an elaborate but flawed experiment they had conducted on the decomposition of water. They had passed water through an inclined gun barrel containing powdered charcoal and heated to incandescence. The carbon in the charcoal combined with the oxygen of the water to form fixed air, releasing inflammable air. The inflammable air and the fixed air collected in a pneumatic flask, but the experimental arrangement did not permit them to separate these airs, and they had moreover collected them over water, resulting in an unmeasurable loss of some of the fixed air into the water. In an effort to calculate indirectly the quantities they could not measure directly, Meusnier submitted the results to an enormously complicated analysis that required him to know the exact proportions of the carbon and oxygen in fixed air. Returning to the record of Lavoisier's single experiment on the combustion of charcoal of the previous spring, Meusnier treated the deficit that Lavoisier had then left unexplained as due to the combination of a small quantity of inflammable air contained in the charcoal with oxygen to form water. He was then able to calculate both the ratio of carbon to oxygen in fixed air and the composition of the charcoal as a combination of carbon and inflammable air (21).

Convinced by now that it was vital to determine the proportions both of inflammable air and oxygen in water and of the carbon and oxygen in fixed air "with rigor", by "comparing together at one time the results of numerous experiments," Lavoisier set out in May, 1784, to multiply his experiments on the combustion of carbon. He tried several approaches. One was to burn charcoal that was so highly purified that he could regard it as containing no inflammable air. Another was to burn wax and to calculate all of the quantities, as Meusnier had done for his earlier experiments with charcoal. None of the several experiments that Lavoisier performed during the next month, or those that he retrieved from his earlier work, was unproblematic. Some of them yielded proportions of carbon to oxygen that diverged too far from his expectations for him to accept. In others there remained substantial deficits. The best result, he thought, was that obtained by burning wax; but since the calculation depended upon the theory of the composition of water that he thought some chemists were not ready to accept, he wished to base his result also on experiments that did not rely on that theory. Calculating and recalculating the proportions, he added "corrections" for such factors as suspected losses of the air, incomplete coolings and possible changes in the weight of the residual charcoal due to the absorption of moisture. By so astutely managing his data, he was able to make the results of each of his individually flawed experiments converge upon the ratio of 72 parts carbon to 28 parts oxygen, an outcome that happens to be remarkably close to the accepted modern value (22).

We could follow Lavoisier similarly through the even more challenging experimental problems that he encountered during the next four years when he took up the analysis of plant substances, such as wax, oils, alcohol and sugar, and as he then marshalled all of his accumulated experience to bring fermentation within the compass of his balance sheets and arrived in the process at the crucial concept of the chemical equation. To do so, however, would be to crowd too much into a short presentation, and I would like to pause instead for a few general reflections.

In his published papers Lavoisier habitually claimed to have carried out numerous experiments of whatever type he was describing, giving the impression that the few he reported in detail were selected from a much larger number. My experience comparing his publications with his laboratory notebooks has, however, persuaded me that this was rhetorical exaggeration, that he actually performed relatively few experiments that he did not in one way or another incorporate into his publications. Why then, if he were the consummate experimentalist that I believe he was, did he so regularly settle for one or several imperfect experiments on a given problem? Why did he go to such great lengths to salvage the data from the few he had performed rather than to repeat them until he had reduced or removed the sources of error? There is no definitive answer to these questions, but I am persuaded that the most likely reason is that these experiments were far more difficult to prepare and to perform, more time-consuming, and more expensive than they appear to us as we look back on them from the distance of two centuries. It is easy to overlook the effort that it took to assemble apparatus that had to be luted together, the frequency with which pieces cracked, or leaks ruined the results, the difficulty of maintaining a steady temperature for several days or more by means of a charcoal fire, and numerous other obstacles.

Lavoisier was wealthy enough to spend a great deal on the apparatus he had constructed for his experiments and on the supplies required to sustain them, but his resources were not unlimited and the time he had available for laboratory work was even more restricted. Under the circumstances, the way in which Lavoisier proceeded was probably the most effective allocation he could make of his time and money. Had he persevered with each of the many experimental problems he took up until he had reached the best results he could hope to attain, he would never have been able to explore the broad scope of the investigative enterprise he had outlined for himself in 1773.

The second general point I wish to make is to reemphasize the pragmatic character of Lavoisier's investigative pathway. Although he glimpsed very early the potentially revolutionary nature of his initial discoveries in 1772 concerning the fixation and release of airs, and wrote out for himself the elements of a research program based on them which he pursued faithfully for 20 years, he could not foresee in detail where that program would lead him, nor define the general principles that he would eventually extract from the work he had done. His quantitative experimental methods, like his concepts, evolved as he went, became more tightly structured, more effective, and broader in their reach. He did not set for himself ideal goals of quantitative precision, but achieved at each stage sufficient accuracy to support the current state of his conceptual structure. When the problems he took up began to exceed the standards of his prior experimental practices, he did his best to improve his methods as far as he needed to in order to meet his more stringent requirements. Eventually he met problems so complicated that he was unable completely to resolve his experimental difficuties, but even then his efforts yielded insights of lasting value.

In the early stages of his prolonged investigative odyssey it was sufficient for Lavoisier to show that metals or combustible bodies combined with or released an air by demonstrating approximate correspondences between gains and losses of weights and increases or decreases in the volume of air in a pneumatic flask. By 1783, as the examples I have described indicate, he had reached the point at which it became important to determine accurately the combining proportions of the components of substances such as fixed air and water, and in the next years he extended this concern to plant materials. He did so not because he had derived a law of definite proportions from fundamental considerations, but because his immediate experimental problems required him to know these proportions. He made no effort to justify his implicit assumption that substances actually combine in definite proportions; he simply set out to determine them.

To those who wish mainly to know whether Lavoisier was a reformer or a revolutionary; whether the essence of his revolution was the overthrow of phlogiston, the oxygen theory of acids, a new conception of the gaseous state or the reversal of accepted orders of comparison; whether he supplanted an existing chemistry with a new science or created a science where none had existed; whether he perceived himself as a physicist or a chemist; and to those who view the highlight of the Chemical Revolution as the new chemical language that linked Lavoisier with the broader currents of the French Enlightenment, to such people tracing the details of his experimental procedures as I have sought to do here may seem a narrow enterprise. It is, however, in my view, the foundation on which all else we can say about Lavoisier as a scientist must rest. Without the ongoing movement of the investigative enterprise that he pursued day-by-day in his laboratory and at his writing desk as he interpreted the results of his completed experiments or planned future ones, none of the great events surrounding him that we celebrate this year could have taken place.

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16. F. L. Holmes, *Lavoisier and the Chemistry of Life*, University of Wisconsin, Madison, WI, 1985, pp. 160-167.
17. Reference 15, pp. 393-408.
18. Reference 16, pp. 116-118.
19. Ibid., pp. 180-182; and reference 15 pp. 394-398.
20. Reference 16, pp. 202-204, 208-210.
21. Ibid., pp. 211-219.
22. Ibid., pp. 224-236.

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INSTRUMENTS OF THE REVOLUTION: LAVOISIER'S APPARATUS

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The development of the new chemistry required the design and use of new apparatus. In this respect, Lavoisier's experimental *modus operandi* marks another departure from the procedures of his predecessors and contemporaries. In contrast, Joseph Priestley seems to have performed many of his experiments with conventional equipment - retorts, receivers, furnaces, and burning glasses. Indeed, the drawings of Priestley's equipment show something that looks like a common basin used as a pneumatic trough and ordinary wine glasses and jars used as his glassware. But Lavoisier's laboratory at the Arsenal of Paris was equipped with the products of some of Europe's finest instrument makers, much of it designed to the scientist's exacting specifications and constructed for a specific investigation (1).

A study of the instruments of Lavoisier's revolution is facilitated by the superb engravings that illustrate the *Traité Élémentaire de Chimie* and his other works. For the most part, they were based upon drawings made by Antoine's wife, Marie Paulze Lavoisier. This gifted woman's formal convent-based education had concluded shortly before her marriage, at the age of 13. Nevertheless, she played a major role in her husband's busy life - especially his scientific researches. She studied English and translated into French a number of important chemical works, including Kirwan's *Essay on Phlogiston* (1788). Following Antoine's death, she edited, published, and privately distributed his *Mémoires de Chimie* (1805) (2).

Mme. Lavoisier's natural talent for drawing, enhanced by her studies with David, are evident from her illustrations. Almost all the original sketches, drawings, and proofs have survived, so one can trace her method. She began with watercolor sketches and then copied these, in pencil, on squared paper corresponding in size to the desired plates. The pencil drawings were, in turn, transferred by stylus to the copper engraving plates. Like her husband, Mme. Lavoisier appears to have been a demanding perfectionist. Denis Duveen and Herbert Klickstein, in their bibliography of Lavoisier's works (3), report that a number of revisions were sometimes required before the proof warranted her stamp of approval - the word "Bonne" followed by her initials. It is also worth noting that Marie Lavoisier painted a portrait of Benjamin Franklin that greatly pleased the subject. Unfortunately, it is lost.

Madame Lavoisier (Marie Anne Pierette Paulze) as a young girl.

If nothing more than the plates to Lavoisier's works had survived, one could probably reconstruct his apparatus without much difficulty. But, somewhat surprisingly given the circumstances of his death, much of his equipment has actually been preserved. The Musée des Techniques of the Conservatoire National des Arts et Métiers has an extensive collection. Indeed, that rather dusty and sleepy institution is something of a sacred shrine for chemists. The museum, which includes a deconsecrated church, is an eclectic mixture of early airplanes and automobiles, clocks and watches, Jacquard looms, and a preliminary model of the Statue of Liberty. Its centerpiece is the Lavoisier exhibit.

In his biography of Lavoisier, Douglas McKie calls the laboratory at the Arsenal "remarkable." "Up to that time," he writes (4):

> ... there had been nothing to compare with it; and many years were to pass before such a collection of instruments, especially of precision instruments and chemical apparatus, would be put together again as the working tools of a laboratory - probably not until the rise of the