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President—Sir WALTER LANGDON-BROWN, M.D.

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Lavoisier and the History of Respiration

By E. ASHWORTH UNDERWOOD, M.A., B.Sc., M.D., D.P.H.

FEW great scientists have suffered more than Lavoisier from a lack of critical writings on their works. The biography by Grimaux [1] was not published until nearly a hundred years after his death. The complete edition of his *Œuvres* appeared in the years 1864-93 [2]. Berthelot's valuable extracts from Lavoisier's note-books were published in 1890 [3]. Important papers on his work in scientific journals are relatively few. Recently a number of biographies and other studies have appeared—each indebted to Grimaux, and each coloured by the writer's own interests so far as the scientific aspect is concerned; the most valuable are the works of Meldrum, Cochrane, Aykroyd, McKie, French and Hartog [4 to 9]. Nevertheless few of these works deal even briefly with Lavoisier's physiological studies. The celebration of the second centenary of his birth suggests that this aspect should receive more attention, and the following study is preliminary to a more extensive work which the writer has in preparation.

The Development of a Brilliant Amateur

Antoine-Laurent Lavoisier was born of good stock in Paris on August 26, 1743. He profited by an excellent education at the Collège Mazarin—one of the few schools which had a good science course. At the University he studied law; but in addition he took voluntary classes in many scientific subjects. Guettard the geologist and Rouelle the chemist especially influenced his further work. To Guettard we owe his final desertion of the law for science; he accompanied Guettard as assistant on a protracted geological tour of France. At the early age of 25 years Lavoisier was elected to the French Academy. Just when he seemed set for a scientific career he took the surprising step of accepting a responsible administrative post in the *Ferme générale*, the wealthy company to which was entrusted the task of farming the nation's revenue. Lavoisier was a capable administrator and business man, and he remained a member of the *Ferme* for twenty-three years, rising steadily to the post of Farmer General. Throughout his life he put in each day sufficient time to get through his administrative work, which often involved much travelling; in addition he devoted six hours daily to scientific pursuits.

In 1771 Lavoisier married Marie Anne Pierrette Paulze, a girl of 14 who developed into a brilliant woman. She devoted herself to her husband and his work. She studied Latin and English. She took painting lessons from David, and the figures in the *Traité* were drawn and signed by her. David's famous picture (fig. 1) was painted when Lavoisier was 45 and she was 30. As McKie says, in every way she carried enough guns for her husband. The marriage was very happy. In March 1775 Lavoisier was appointed an inspector of gunpowder for the government, and till 1792 he and his wife resided at the house in the Arsenal which they soon made very famous. It was in his own laboratory here that nearly all his scientific work was done and his guidance enabled France to maintain the large stocks of powder which were necessary for the revolutionary wars. He had other interests, including politics. He did much for the Academy, reporting on a considerable proportion of the diverse problems which were submitted to it for investigation. The Lavoisiers were originally of peasant stock, and Antoine always had a deep respect for the habits of the country people. He had a successful experimental farm on his estate

FIG. 1.—LAVOISIER and Mme. LAVOISIER by DAVID. This portrait was painted in 1788, and is now in the Rockefeller Institute, New York. According to Grimaux, it is the only authentic portrait of Lavoisier. [Reproduced by kind permission of the Rockefeller Institute for Medical Research, New York.]

at Fréchines, and in 1785 he became a member of the Committee of Agriculture in Paris. Two years later he was a member of the Assembly of Orleans. Prison and education reform also claimed his attention. Such was the busy life which Lavoisier further enriched by his scientific work, some of which will now be discussed. Throughout his life he remained an amateur in science. At the University he officially studied law, and his scientific studies were voluntary. He never held a paid post in chemistry—except for that at the *Régie des Poudres,* which was largely administrative.

Lavoisier's Chemical Work—The RÉVOLUTION CHIMIQUE

Lavoisier is known to the most junior student as the great founder of the modern science of chemistry. The purpose of this tribute to his memory is mainly to discuss some of his other discoveries which are less widely known, and to which therefore most space must be given. His chemical researches must, however, first be dealt with in some detail, since they determined the lines of his physiological work, which would have been impossible had he not paved the way by his brilliant chemical synthesis. It would be easy to discuss in detail the *Mémoires* and the contents of his laboratory journals, but this would require almost a book. The writer has therefore attempted to summarize the main lines of Lavoisier's ideas on chemistry, and it will be seen that in this chemical section his debt to the careful work of Meldrum, of McKie and of Hartog is manifest.

Chemistry was essentially mediæval when Lavoisier was a student. Boyle's definition of an element had remained an abstract conception, and there was no list of elementary substances. Practically the four "elements" of the Greeks and the three "principles" of Paracelsus held the field. It is important to realize that both in chemistry and in physiology the phlogiston theory was a bar to further advances. The theory was first advanced by Johann Joachim Becher in *Physicæ subterraneæ* (1669), and was greatly developed by Georg Ernst Stahl in his *Fundamenta Chymiæ* (1723). On this theory combustion implies the escape of phlogiston from the burning body. Sometimes the process can be reversed. For example, when a metallic calx is heated with charcoal, phlogiston passes from the charcoal to the calx and the metal is reproduced. A metal should therefore be heavier than its calx. Since the time of Boyle it had been known that the reverse was the case, and two alternative explanations of the conflicting facts were in common use. Despite what Lavoisier says [10], there was not really much support for the view that phlogiston had negative weight. Most accepted Boyle's view that increase of weight on calcination was due to the passage of particles of fire through the glass of the retort [11]. The phlogiston theory certainly "explained" many known facts and was an attempt at systematization. As Patterson [12] says, if placed in the position of the philosophers of the time—most of whom except Boerhaave [13] adopted it—we could hardly do otherwise. Another belief of the time was that matter was transmutable. In Van Helmont's willow tree experiment it was shown that vegetable substance was produced from water alone. Boyle's "elastic fluid" was mainly common air; but Van Helmont and Hales had done more than merely suspect that there were different kinds of common air. Yet there was a gleam of true understanding in this darkness. In 1756 Joseph Black published the extended English version of his epoch-making dissertation on *magnesia alba* in which the existence and properties of "fixed air" as a separate entity were clearly demonstrated [14]. His experiments added to the quantitative foundation of the science which had been laid down by Van Helmont and Boyle. The science of chemistry was still saddled with the outmoded names and symbols of the Middle Ages. As Lavoisier later pointed out in the *Méthode de Nomenclature* (1787) it needed much practice and a great memory to remember the substances expressed by some of the current names. Not only were the names *oil of vitriol, butters of arsenic and antimony,* and *flowers of zinc* ridiculous—since there were neither butter, oil, nor flowers in the mineral kingdom; what was worse was the fact that all of these substances are violent poisons [15].

Lavoisier's first chemical paper, published at the age of 21, was an attempt to explain the binding of plaster by analysis of gypsum [16]. It is notable for the fact that he declined to make conjectures which he could not prove. Four years later he began an experiment which was to shake to its foundations the current theory of the transmutability of matter. It had been noted by many chemists that when distilled water was heated in a glass vessel, there was always a slight residue of earthy matter: clearly a case of transmutation of water into earth. By a painstaking experiment Lavoisier showed that the earth came from the glass, and he also obtained some valuable standards of purity for water used in chemical operations [17]. This paper was not actually published until 1773, although that volume of the *Mémoires* was really for the year 1770. A great difficulty in assessing Lavoisier's debt to others, and theirs to him, is this late publication of the *Mémoires.* Hence between the date of the reading of a paper and its actual date of publication it was possible for a member of the Academy to incorporate the results of many later experiments. Lavoisier always took full advantage of this facility—as is seen in his next important Memoir on the combustion of the diamond. He showed that a diamond can be burned, and that the product of combustion is fixed air. This work, though read in 1772, did not appear till 1776 [18].

During this memorable year 1772 other things were happening. In his note-book Lavoisier noted on September 10, 1772, that he had carried out an experiment on the combustion of phosphorus. This was his first attempt at the problem of combustion. On October 20 he presented a note to the Academy informing them of what he was doing, and on November 1, 1772, he deposited a sealed note with the Secretary [19]. Until 1932, when the first two of these notes were published by Meldrum [20], only the last was available. They show that Lavoisier had conceived the theory that when phosphorus and sulphur burned, the gain in weight was due to their combination with atmospheric air; he further suggested that the increase in weight in the calcination of metals was due to the same process. On February 20, 1773, he drew up a memorandum in his note-book. This is virtually a private programme of the experiments which he intended to carry out, his reasons for doing so, and a statement of what he hoped to discover. He says definitely that he intends to carry out a long series of experiments on the elastic fluids derived from any chemical changes. He feels that experiments must be performed to determine whether these elastic fluids are common air unaltered or modified in some way, or on the other hand whether they are emanations "of the minute and ultimate parts of substances". He recognizes that a vast number of experiments will have to be performed, but "the importance of the end in view prompted me to undertake all this work, *which seemed to me destined to bring about a revolution in physics and in chemistry*" (italics mine). This must be one of the most confident and prophetic statements in the history of science. He finished his memorandum with these words: "The processes by which one can succeed in fixing air are: vegetation, *the respiration of animals,* combustion, in some conditions calcination, also some chemical changes. It is by these experiments that I feel bound to begin" [21] (italics mine). This very important memorandum shows that Lavoisier had already, at the age of 29 years, visualized the future progress of the *Révolution chimique.* He seems to have had his attention directed towards the study of gases mainly as a result of his immature experiments on sulphur and phosphorus noted above. There had been in France no interest in the study of elastic fluids; as he says in the Introduction to the *Opuscules,* this was essentially a subject which had received the attention of English, German and Dutch chemists [22]. It is fairly definite that Lavoisier must have read widely on this subject about the end of 1772 and the beginning of 1773. Indeed, he devotes nearly a half of his *Opuscules Physiques et Chimiques,* which was written in 1773, to a history of experiments on air.

A point of vital importance to the present investigation which is brought out by this memorandum is the fact that by February 1773, and probably by the end of 1772, Lavoisier had sensed that a study of the elastic fluids involved a study not only of combustion but of respiration, and that he had already decided to undertake experiments on respiration.

The experimental work for the *Opuscules* was carried out at high pressure in a space of about six months, and the work was submitted for the approval of the Academy on December 7, 1773. In the early experiments recorded (Chaps. I to IV) it is clear that he thought that the gas liberated by the action of acids on metals was the same as the gas used up in calcination, and that this was therefore fixed air. In the next chapter he describes his famous experiment on the heating of minium and powdered charcoal in a bell-jar over water [23]. He concluded that the gas formed arose neither from the minium nor the carbon, but from a union of the two. There followed the five deductions on which much of his later work is based. Although, as Meldrum [24] has pointed out, these are not unexceptionable, they indicate that Lavoisier saw that calcination does not continue indefinitely in a closed vessel, and that the process consists essentially of a union between the metal and the air. The next series of experiments deals with the combustion of phosphorus in a bell-jar; experiments 6, 7 and 8 in this series [25] are especially important. From them Lavoisier showed that whatever it was in the atmospheric air which combined with the phosphorus, it was not water vapour. His final words are that the substance which combines with the phosphorus is "either air itself, or another elastic fluid contained, in a certain proportion, in the air which we breathe". From his note-books we see the doubts which he had in his mind [26]. In February 1773 he suspected that the air in minium was not "fixed air". In July, however, he had reversed his opinion and had tried to re-create common air by adding fixed air to the residual air left after the burning of phosphorus.

The missing link in the chain was provided by Priestley's discovery of "dephlogisticated air". Priestley, with the discovery of a number of new "airs" already to his credit, had obtained this gas in November 1771, but he had not then recognized it as another new "air". On August 1, 1774, he obtained an "air" by heating *mercurius calcinatus per se* (now known as mercuric oxide) and recognized from his tests that it was extraordinarily vigorous as a supporter of combustion. Towards the end of the same month Priestley met Lavoisier and his circle at the latter's house at the Arsenal, and it is known [27] that he talked of the properties of his new gas. Priestley thought it might be the gas we now call nitrous oxide, but Lavoisier immediately perceived that this was the clue which he was seeking. It was not until March 1, 1775, that Priestley realized that his new "air" was respirable, and soon thereafter that it could be used medically and to raise artificially the temperature of combustion. The actual date of the discovery of "dephlogisticated air" (oxygen) was therefore March 1, 1775 (*see* Hartog [9, 46, 46*a*]). It is important to note that Priestley's first public announcement of his discovery was not made until March 23, 1775, when a letter of his was read to the Royal Society. Fortified by the possession of the key which Priestley had given him the previous August, Lavoisier had meanwhile been repeating Priestley's experiments from the aspect of the hypothesis which he had set out to investigate. On April 26, 1775, he read to the Academy his "Memoir on the nature of the principle which combines with metals during calcination, and which increases their weight" [28]. McKie points out—a statement with which I agree—that Lavoisier here referred to "the purest part of the air", meaning purified air, and not a particular constituent [29]. This paper was revised and read again to the Academy on August 8, 1778, and in the revised form it then appeared in the *Mémoires de l'Académie* for 1775, which was not published until 1778 [30]. In the new version the "purest part of the air" has become "eminently respirable air" and "most salubrious air". He now realized that common air consisted of "eminently respirable air" and an inert *mofette.* In other words, in 1775 Lavoisier had distinguished between oxygen and fixed air, but not definitely between oxygen and common air. Reference will be made later to the experiments which he carried out on the respirability of the new gas. His theory of combustion and calcination dates from 1777.

Fundamental as are the other discoveries of Lavoisier in the history of chemistry, they are not so important with reference to the subject of this paper, and they must therefore be dealt with briefly. In a paper on the combustion of phosphorus published in 1780—but referring to experiments read in 1777—Lavoisier showed that common air could be re-created from the gaseous residue left after combustion—but by the addition of "eminently respirable air" and not "fixed air" as he had previously thought [31]. In 1777 he also began unobtrusively his attack on the phlogiston theory. A memoir which was read on November 23, 1779—though actually submitted two years earlier—contains the first appearance of the word "oxygène" [32]. The term "air vital" did not come until later. Lavoisier made his great attack on the phlogiston theory in a paper read in 1783 and published in 1786 [33]. He pointed out that the known facts could be explained as satisfactorily without the help of phlogiston as with it, and that therefore, on the principle of Occam's razor, it was probable that it did not exist. The theory was an error disastrous to the development of chemistry. The paper contains no new facts, but it is one of the most brilliant exhibitions of analytical reasoning which has ever been written. Macquer in his *Dictionnaire* had attempted to retain the facts and to fit the phlogiston theory to them, but Lavoisier showed that in all the arguments "phlogiston" was usually applied to two contradictory properties. It is notable that in one section [34] he refers to the changed conditions during the slow combustion of a metal—an example of the beginnings of his line of thought on respiration. As he says, physicists and geometers were then already abandoning the theory, but most of the chemists were still supporting it. This memoir was the beginning of the gradual disappearance of this famous theory.

The history of the discovery of the composition of water concerns us here only indirectly. McKie [35] in particular has recently disentangled the web of contradictory evidence. Lavoisier invariably gave full credit in his papers to the work of the British chemists, but for once he failed here to acknowledge his indebtedness to others. In June 1783 Lavoisier learned from Blagden what Cavendish had done. On June 24, in the presence of Blagden and others, he and Laplace made a rough experiment, and on the following day he took the precipitate step of reporting to the Academy that with Laplace he had synthesized a quantity of pure water; the report does not mention Cavendish. The careful results of Cavendish were not published until 1784. Meanwhile, on November 12, 1783, Lavoisier read a paper to the Academy which was published elsewhere in the following month [36]. The paper was then revised and was finally published in the *Mémoires de l'Académie* in 1784—*not* in the volume for 1783, but in that for 1781 [37]. Although Lavoisier cannot claim to be the discoverer of the composition of water, he was the first to explain the facts. Cavendish could not explain them correctly, since he was a confirmed phlogistonist. He thought water came from the two gases by deposition, and he did not realize that it was a compound. Indeed Lavoisier was one of the few at that time who *could* have explained the facts correctly.

The language of chemistry was still alchemical and iatro-chemical in nature, and from 1782 onwards three other chemists—de Morveau, Fourcroy and Berthollet—collaborated with Lavoisier in drawing up a nomenclature which would be in accordance with the known facts. Lavoisier was chosen to introduce the system to the world of science, and this he did at a public session of the Academy on April 18, 1787. The details were given later by de Morveau, and the whole of the contributions by the four men were published in 1787 as the *Méthode de Nomenclature chimique*. The numerous names of chemical substances, which had been so picturesque and yet so unscientific, disappeared; in their place were established the names which we use to-day.

Lavoisier had no doubt realized for some time that the new theory of chemistry which he had created had rendered out of date all existing textbooks on the subject. An authoritative statement of the new chemistry was required, and this Lavoisier himself provided in 1789 in the *Traité élémentaire de Chimie* [38]. Even a short examination of this great work is beyond the scope of this paper, but a few remarks are necessary to determine Lavoisier's attitude to subjects which will be discussed later. In the first place, although he did not give any place to phlogiston, he reasoned that the matter of heat ("caloric") is a material substance and he included it in his table of elements. Next, he stated in his preface that he had imposed upon himself, as a law, never to advance but from what is known to what is unknown, and never to form any conclusion which was not an immediate consequence necessarily flowing from observation and experiment. He thus impressed upon all who read the book—and not merely on the fairly select body who read the *Mémoires de l'Académie*—the lines upon which chemistry was to develop in the future. Further, he made in it the first clear statement of the Law of Conservation of Mass as applicable to a chemical change. Finally, he applied Boyle's definition practically, and gave a table of the chemical elements, thirty-three in number. Apart from the inclusion of light and caloric among the elements the table marks a tremendous advance on anything which had gone before. The third part of the *Traité* was devoted to a description of the methods and apparatus used by Lavoisier. The illustrations of apparatus were drawn and signed by Mme. Lavoisier.

It remains only to mention briefly the importance of Lavoisier in the sphere of organic chemistry. Scheele had discovered a number of organic acids but organic analysis was hampered by the phlogiston theory and by a lack of understanding of the *organic radicle* [39]. As early as 1784 Lavoisier had determined the proportion of carbon in spirit-of-wine, in olive oil and in wax respectively by burning them in oxygen and absorbing the carbon dioxide in potash solution. (This paper was not published until 1787 [40].) The last of his extant laboratory note-books shows that in April 1788 he was working on the analysis of sugar by combustion with red oxide of mercury [41]. He later used oxide of manganese and chlorate of potash for the reduction, and he experimented on various resins and other organic substances. Dumas published in the *Œuvres* a previously unpublished Memoir of Lavoisier—no doubt incomplete—on his spirituous fermentation in which he sets out quite clearly the algebraic quality of an equation in organic analysis; he also indicated that vegetable substances must be regarded as a loose combination of oxygen, hydrogen and carbon, and that this combination can be easily upset by a medium degree of heat [42]. According to Dumas this paper probably dates from 1788. At this time Lavoisier must have been writing the *Traité*. In this he devotes chapters to the decomposition of organic substances by the action of fire, to vinous and acetous fermentation, and to putrefaction. Here again he stresses and illustrates the Law of Conservation of Matter [43]. We know, he says, that animal substances are composed of hydrogen, carbon, azote, phosphorus and sulphur, "all of which in a state of quintuple combination, are brought to the state of oxide by a larger or a smaller quantity of oxygen. We are, however, still unacquainted with the proportions in which these substances are combined, and must leave it to time to complete this part of chemical analysis, as it has already done with several others" [44]. Lavoisier had laid well and truly the foundations of organic analysis and a most important branch of physiology.

Before passing on to Lavoisier's work on respiration it might be well to emphasize that in the past much injustice has been done to his great British contemporary, Priestley. It used often to be said that Priestley was a brilliant experimenter with no theory except that of phlogiston behind his work. Even Meldrum [45] said that Lavoisier "was in earnest about what Priestley passed over lightly and amiably". Sir Philip Hartog [46, 9] has recently exploded this myth in brilliant fashion. He shows that many of Priestley's experiments exhibit constant planning, thought, and scientific imagination. To the end of his life he preserved a light-hearted suspicion of every hypothesis that seemed to him unverifiable, and the phlogiston theory was one of these hypotheses. His apparent adherence to the theory was not due to conservatism, and he remained a "sceptical chymist" to the last.

Early Work on Respiration

As an example of the theories on respiration which were held up to the time of Priestley and Lavoisier, we may summarize the views of von Haller, the great physiologist who died in 1777, the year in which the new theory of combustion was formulated, and in which Lavoisier read his first paper on respiration proper. Haller admitted that the reasons why animals cannot live in vitiated air were not clearly recognized. He discussed the view that the air itself, or something vital in it—the subtle aerial particles of Mayow—reaches the blood and acts chemically, but was inclined to reject it. However, he believed that air exists in all the humours of the body, and he thought that possibly something was derived from the respired air. He was unwilling to commit himself regarding the nature of this substance, and on the whole he believed that air plays the part of a cement holding together the earthy elements of the body [47]. So far as animal heat was concerned he was driven back to the theory of Stahl that it was due to the friction of the blood as it passed through the blood-vessels. He could not quite understand how this theory explained the constant temperature of the body.

In leading up to Lavoisier's experiments on respiration it should be recalled that in 1757 Black had recognized "fixed air" as a distinct gas which arose from the burning of charcoal. At first he thought that it constituted the irrespirable part of the atmosphere, but later, on the discovery of nitrogen by Rutherford in 1772, he changed his views. Meanwhile Priestley was intrigued by the possibility of re-creating common air from vitiated air. In 1771—the year in which he discovered oxygen without recognizing it —he thought that, since common air is necessary for the continuance of both plant and animal life, the effect on each would be the same. He wrote later of his experiment: "I own I had that expectation, when I first put a sprig of mint into a glass jar, standing inverted in a vessel of water: but when it had continued growing there for some months, I found that the air would neither extinguish a candle, nor was it at all inconvenient to a mouse, which I put into it. The plant was not affected any otherwise than was the necessary consequence of its confined situation" [48]. Later, in August 1771, he showed that the same applied to air in which a candle had burnt itself out, and that the effect did not take place with leaves of mint, but that a growing plant was necessary. These experiments were not published until 1775 [49]. In the meantime Priestley had been carrying out numerous observations on the properties of dephlogisticated air, as was mentioned previously.

At this period Lavoisier was drawn almost unconsciously into his earliest observations on the respirability of various gases. At that time the British chemists at least, if not the French chemists in general, had two tests which they used by rote on any unknown air. One was the use of a candle or taper; the other was a test with a small animal. In order to put his investigations in correct perspective I have in writing this section compared the notes which Lavoisier made in his journals with the text of the original memoirs. It has already been noted that in the brilliant memorandum which he wrote in his laboratory note-book on February 20, 1773, he showed his belief that air was fixed by respiration, and that the process deserved further investigation [20]. In March 1773 he wrote in his note-book: "The apparatus for testing the effect of air by means of animals has been ordered and is almost ready. . . . In experiments on animals do not forget the frog" [50]. It is clear therefore that at this early period he had in mind a broader investigation than the mere placing of an animal in a gas to see how it reacted. On July 3 of the same year he was investigating the reduction of minium by carbon and a rat was used to test the resulting gas [51]. This is probably one of the experiments described in the *Opuscules* in which he gave the first proof that the gas produced by reduction of calces with carbon was identical with fixed air; he says there that in his examination of the gas, sparrows, mice and rats which were placed in it perished immediately [52]. It will be remembered that the *Opuscules* were published in January 1774. In the same work he described how a lighted candle was instantly extinguished when plunged into air in which phosphorus had been burned; a bird placed in the same air could breathe easily for half a minute [53]. Boyle had already made the same observation a century earlier. Despite this Berthelot, in commenting on the original note of this experiment in Lavoisier's note-book, remarks that the result is evidently erroneous and due to some accidental admixture of air [54]. Had he been able to recognize the fact at that period, Lavoisier would have obtained from this experiment a clue to the role of increased carbon dioxide tension in producing a stimulatory effect on the respiratory process. At this time he attributed the death of animals in the gases given off by effervescing fluids to the fact that these gases were easily soluble in water, and therefore could not retain sufficient tension to expand the lungs [55]. Thus up to this point Lavoisier's knowledge was limited to the lethal effects of certain concentrations of "fixed air". He had no idea of the function of the air which was inspired.

The years 1775 and 1776 were very important in the development of Lavoisier's interest in the respiratory process. It has already been mentioned that, although Priestley had obtained a strange air in August 1774 it was not until March 1 of the following year that he had any idea of its respirability [56]. There followed a series of brilliant and exciting experiments in which Priestley not only shook his assumption that there could be no air better than common air, but that the new gas was actually four or five times better; later he obtained air which was five or six times better. By the time he published his second volume of *Experiments and Observations* in 1775 he had, by breathing the new air through a glass siphon, "reduced a large jar full of it to the standard of common air. The feeling of it to my lungs was not sensibly different from that of common air; but I fancied that my breast felt peculiarly light and easy for some time afterwards. Who can tell but that, in time, this pure air may become a fashionable article in luxury. Hitherto only two mice and myself have had the privilege of breathing it" [57]. Lavoisier's great debt to Priestley in putting him on the right track has already been noted. In the final version of his famous paper on the principle which combines with metals in calcination, Lavoisier says (1778) that this principle "is none other than the purest part of the air itself which surrounds us, which we breathe, and which passes, in this process, from a state of expansibility to one of solidity" [58]. He emphasized the fact that the whole of the air was not respirable, and to the purest part he gave the name "eminently respirable air". This final form of the memoir had evidently been modified by experiments which he had carried out in the meantime. His note-books show that from October 13 to 15, 1776, he carried out tests on the respiration of birds in dephlogisticated air. A red-breast lived for one hour in common air; another lived for three hours in dephlogisticated air, and a third, introduced into the air in which the second had died, lived for an hour. It is very interesting to see from this note that he thought that the birds were suffocated in a little atmosphere of fixed air, over which there was an atmosphere more pure than common air [59]. In reading Lavoisier's papers on respiration, therefore, it should be remembered that the laws of diffusion of gases were not at all understood at this period. Towards the end of this year (1776) he wrote in his note-book: "It seems that respiration in absorbing air vitiates a part of it", and we may make a shrewd guess that for Lavoisier the emphasis in this sentence was on the suggestion that a particular constituent part of the air suffered the alteration [60].

The first observation of 1777 which is of interest to us is contained in a memoir on the combustion of phosphorus. Lavoisier showed that when the combustion went on to extinction in the confined space of a bell-jar over water, the air which was left differed from common air; it was inert and he designated it *mofette atmosphérique*. If to this *mofette* he added the correct amount of dephlogisticated air which had been derived from a calx of lead or mercury, the residual air again became respirable, and the process of combustion could be repeated in it. Although in practice he had to add a little more dephlogisticated air—owing to the fact that it was seldom obtained quite pure—than the quantity of the air absorbed, he nevertheless found that the reaction was so definite that the amount of dephlogisticated air to be added could be calculated in advance [61].

Lavoisier had now reached the stage of realizing that the air of the atmosphere consisted of two parts, mixed in a definite proportion. One part was inert. The smaller and more active part was necessary for respiration and for animal life. This part was also necessary for the calcination—or combustion—of metals. No matter from what source the more active part was derived, it could be mixed in the proper proportion with the inert part to form common air. The relation may seem obvious to us, but it was a very different matter for Lavoisier. The assumption which we would make *now* involved *then* the rejection of the phlogiston theory. It was at this stage that he read a short memoir—*Experiments on the respiration of animals* [62]—which is of great importance in showing the manner in which his ideas developed. After a preliminary reference to Hales and Cigna, Lavoisier referred in generous terms to the ingenious, delicate and novel experiments of Priestley. He disagreed with Priestley's interpretation of the facts, and he proceeded to describe the experiments which had helped him in the development of his own theory. Lavoisier noted that there was a profound difference between the air in which a metal had been calcined and one which had served for respiration. Not only was the diminution of volume less in the latter; the respired air precipitated lime water, but the air after calcination did not. From this he deduced that there were two processes involved in respiration (*qu'il se compliquait dans la respiration deux causes*), and that of these he probably knew only one. He therefore carried out another experiment by which he showed that about one-sixth of the volume of vitiated air consists of chalky-acid gas (*acide crayeux aériforme*). He had now thrown overboard the indefinite term "fixed air". Therefore, to re-create common air from vitiated air, it was not enough merely to add the appropriate amount of eminently respirable air (as in his experiment on the burning of phosphorus); the existing chalky-acid must also be removed. He drew immediately the logical conclusion regarding the process of respiration. Either eminently respirable air is changed in the lungs to chalky-acid air; or an exchange takes place, the eminently respirable air being absorbed, and an almost equal volume of chalky-acid air being given up to the residual air from the lungs. To me the interesting point about this memoir is the fact that, while Lavoisier obviously favoured the change of eminently respirable air into chalky-acid air, he had to admit that there were strong grounds for believing that eminently respirable air did combine with the blood to produce the red colour. He gives full credit to Priestley for his observations on this point. Lavoisier does not seem to have realized, however, that it was not necessary that the chalky-acid which left the lung during one expiration should have been derived from the eminently respirable air which entered the lung *during the inspiration immediately preceding.* This is a small point, but it obviously proved a stumbling-block.

The Rubicon was apparently crossed the same year. At least, in the *Mémoires de l'Académie* for 1777 there appears an important memoir on combustion in general [63]. In this he propounds the false theory that all gases are compounds of solids or fluids with the matter of fire or of light. In combustion the "base of the air" combines with the burning substance, thus liberating its "solvent" as light and flame. In the combustion of sulphur and phosphorus this recombination takes place violently; in the calcination of metals, slowly. Then he puts forward the tentative hypothesis, which, however marred by errors it may appear to-day, is nevertheless a landmark in the history of physiology and in the development of his theory:

"Pure air"—that is, eminently respirable air, or oxygen—"in passing through the lung, suffers a decomposition analogous to that which takes place in the combustion of charcoal. Now, in the combustion of the latter, matter of fire is set free. Therefore, in the lung there must be in the interval between inspiration and expiration a similar liberation of matter of fire, and it is doubtless this which, carried by the blood throughout the animal economy, maintains there a constant heat of about 32½° Réaumur. This idea . . . rests on two constant and incontrovertible facts, namely: on the decomposition of the air in the lung and on the liberation of matter of heat which always accompanies the decomposition of pure air, that is, every change of pure air to fixed air." He points out that the only warm-blooded animals in Nature are those which breathe regularly, and he suggests that there is a constant relation between the heat of the animal and the quantity of air which is converted to fixed air in its lungs. This must have been speculative, and the influence of the phlogiston theory is still obvious in the passage which I have translated above.

It is of great interest to note that, although Lavoisier had now deduced by inference that the eminently respirable air which entered the lungs was involved in a chemical change by which heat was liberated, he was anchored to the idea that the change took place in the lungs in the interval between inspiration and expiration. In this change one factor was eminently respirable air; he did not know the nature of the other substance. Since in his view matter of heat was material—and was, as we have seen, one of his elements—it was perhaps not unnatural that he should conceive of it as being absorbed into the blood to raise the temperature of all parts of the body.

It is the duty of the historian to try to transport himself in spirit to the age which he is studying, and if we succeed in doing so now, Lavoisier's difficulties become less obscure. He had a fair idea that in respiration there was taking place in the body—or, as he thought, in the lung itself—a process analogous to slow combustion. The question which troubled Lavoisier was: Combustion of what? Combustion or even slow calcination involves a change which is not obviously for the better. A heap of red oxide of mercury would not be of much use to a maker of mirrors. The lung was evidently not consumed or altered by a process akin to calcination. Heat could not be liberated by eminently respirable air unless the latter acted with some other substance. The question before him now, therefore, was the nature of this other substance.

In 1778 Lavoisier was evidently pondering deeply on these subjects. At the beginning of the year he noted in his journal that Bucquet was carrying out experiments on asphyxia, and Lavoisier suggested that he should try marsh gas. A guinea-pig died in oxygen, and Bucquet did an autopsy [64].

The *Mémoires* for 1781 contain the important paper on the formation of the acid called fixed air [65], but this is not of great moment to the physiology of respiration.

In it Lavoisier defined the *principe oxygine,* and fixed air or chalky-acid air he proposed henceforth to designate under the name of "carbonic acid". The memoir also contains the term *vital air.* He gives quantitative results of his experiments on the reduction of minium by charcoal, and he allows for the formation of water by the combustion of "inflammable air" in the charcoal. This observation is accounted for by the fact that the paper must have been revised in the light of Cavendish's water experiments, since it was not actually published until 1784.

. . . . .

While all these experiments were going on Lavoisier had been pursuing work on an important parallel line. In November 1777 he had begun to measure specific heats of various substances. These were interrupted, and taken up again briefly in 1781. In July 1782 the experiments began again [66]. He had, however, now obtained the collaboration of a scientist whose reputation stands as high as his own—Laplace, the mathematician. In the winter of 1782-83 very important work was carried out, and the *Memoir on heat* [67] was published in 1784, but in the volume of the *Mémoires* for 1780. Black was reputed to have used a calorimeter—but *not* an ice-calorimeter—a number of years previously, and Crawford was working on the subject of animal heat about the same time as Lavoisier; but these results do not affect the investigation which will now be described. Of the two men it was probably Lavoisier himself who invented a new type of calorimeter, of which examples were to be seen before the war in the *Conservatoire des Arts et Métiers* at Paris. The apparatus was based on the theory that if a warm body is applied to a block of ice, the heat first transforms a slice of ice into water, the remainder of the block remaining at the same temperature. If it were possible to obtain a hollow sphere of ice, the warmer atmosphere would act only on the outside layer, and the temperature of the remainder would be constant. If a warm body was then placed inside the hollow of the sphere, the inside layer of ice would be melted, but the temperature of the inner portion of the wall would be unaffected. Hence the water which collected inside the hollow would be due only to the cooling of the body inside it. The construction of the calorimeter will be understood from Mme. Lavoisier's illustration, which appeared in the original memoir and also in the *Traité.* The body to be investigated is placed in the wire basket. The wall of the sphere is represented by the middle chamber, filled with ice; the water produced is run off through the lower outlet. The middle chamber is itself insulated by the ice in the outer chamber. This long memoir contains many important observations, theoretical and experimental, but it is only the fourth, concluding, section which concerns us. The authors pointed out that recent experiments—those of Lavoisier of course—had shown that in respiration the vital part of the air is "either absorbed, or altered, or converted into fixed air by the addition of a principle which we shall call the *base of fixed air,* to avoid all discussion of its nature" (italics in the original) [68]. They then carried out experiments on the respirations of guinea-pigs in a confined atmosphere. From the average of their results they concluded that a guinea-pig produces 224 gr. of fixed air in ten hours, and by a variation of the method they showed that the changing of vital air into fixed air is the *only* effect of respiration on air. In experiments described earlier in the memoir they had shown that in the combustion of charcoal the production of one ounce of fixed air involves the melting of 26·692 oz. of ice. Hence the formation of 224 gr. of fixed air involves the production of heat sufficient to melt 10·38 oz. of ice. In an actual experiment with a guinea-pig in the calorimeter they found that in ten hours 13 oz. of ice were melted, and they thought that this represented approximately the heat which had to be renewed by the vital functions of the animal. For reasons which to us are not entirely satisfactory they also thought that this amount would have to be reduced by 2½ oz. to give an approximately true result. Hence, from the correspondence between the amount of ice melted by a guinea-pig and by the combustion of charcoal in the production of a given amount of fixed air in each case, they concluded that the principal cause of animal heat is that heat which is liberated in the transformation of vital into fixed air. They continue:

"Respiration is then a combustion, admittedly very slow, but nevertheless completely analogous to that of charcoal; it takes place in the interior of the lungs, without liberation of perceptible light, because the matter of fire is no sooner liberated than it is absorbed by the humidity (*humidité*; ? humours) of these organs. The heat which is produced in this combustion is transmitted to the blood which passes through the lungs and from there it courses through the whole animal system" [69].

Lavoisier and Laplace almost gave the true explanation; but just as the phlogiston theory had misled so many brilliant chemists, here was the theory of the material nature of heat and light standing in the way, and having to be disposed of in the convenient humours of the body. Later in the paper they give the first clear definition of the basal metabolic state. It is also noteworthy that they nowhere commit themselves any further regarding the substance with which vital air combines than that it is the *base* of fixed air.

These experiments seem to have been continued at intervals throughout the same year (1783). On May 5 Lavoisier tested the effect of dephlogisticated air, absorbed by nitrous acid, on sparrows. A week later he was observing the respiration of a guinea-pig in oxygen. On August 25 he was working on the specific weights of vital air, fixed air, and nitrous air. By December 22 he was investigating the heat produced by a guinea-pig [70]. In January 1784 he was using "apparatus A" to determine the caloric liberated by the respiration of birds [71]. About February 3 he was again working on the amount of carbonic acid liberated in the respiration of animals, and before March 16 he had weighed it [72]. Also in the winter of 1783-84 Lavoisier and Laplace made further extensive experiments on specific heats. These were not written up until 1793, and they were published posthumously many years later [73]. The paper contains nothing relevant to respiration.

In 1785 Lavoisier read to the *Société royale de médecine* a paper which carried his theory a stage further [74]. It contains what is, I think, the first clear statement in his memoirs that air is a mixture—although the evidence on which it is based rings strangely in our ears to-day. He then describes an experiment in which a guinea-pig was confined in a measured quantity of vital air for an hour and a quarter. At the end of this time there was a decrease in the volume of the vital air to the extent of about one thirty-secondth of its original volume, but the absolute weight of this air had increased. Hence, argued Lavoisier, the air breathed extracts something from the lung during respiration, and this substance when combined with vital air, forms carbonic acid. Only carbonaceous matter fulfils these conditions. By a calculation from the data Lavoisier then showed that in the experiment 52½ cubic *pouces* of vital air was used above the amount necessary to form the carbonic acid which was actually found. This additional quantity entered the lungs but did not come out in the same state. Either the oxygen combined with the blood, or with hydrogen to form water. He preferred the latter theory, and he calculated the amount of hydrogen which must have been used and the quantity of water formed. The same experiment was repeated in common air, though perhaps less satisfactorily. In this memoir Lavoisier gives the results of autopsies on asphyxiated guinea-pigs which were made by Bucquet. Now follows the significant observation that air which can be breathed easily must consist of about 25 parts of vital air to 75 of azote —variations either way are of importance; "but the ill-effects which animals experience, long before they have used up all the vital air in the air which they breathe, are indications of the irritating nature of the carbonic acid which is formed". He probably did not conceive of the irritation in the sense in which we understand it, but the observation is nevertheless important. Lavoisier finally gives the results of experiments which he carried out in public halls and in the theatre at the Tuileries. His figures for the analyses show a definite increase of carbonic acid in the vitiated air; and it is therefore not unexpected that he attributed the effects of vitiated air to this gas. No one seems to have observed previously that in this concluding section he made the observation that in a large audience each individual must breathe air which has passed a number of times through the lungs of others. This air must be charged with putrid exhalations. Is it possible, he says, that these may differ in youth and in age, in sickness or in health?

The Last Memoirs on Respiration and Transpiration

It was not until four more years had passed that Lavoisier read another paper dealing with respiration, and meanwhile he had taken as an assistant and collaborator a young man named Armand Seguin. Seguin is usually described as a physiologist in books dealing with Lavoisier. This *First memoir on the respiration of animals* was read on November 13 (or 17), 1789 [75], but it was not published until 1793. This is undoubtedly his greatest work on respiration.

The opening of the memoir emphasizes the known facts regarding the composition of air, and that the results depended upon exact analyses of gases carried out by Seguin's eudiometer method. From the emphasis which is placed on "liberation of caloric" it seems to me that his whole approach to the problem of respiration may have depended upon the fundamental observation that the heat of the body is maintained under very varied conditions. The first experiments were made on guinea-pigs. Having shown in his previous memoir that carbonic acid gas is noxious, it had to be removed from the gas or air breathed. The observations were made on a guinea-pig in a bell-jar over water. The animal rested on network covering an elevated wooden dish (*sébile*) which was filled with caustic alkali by siphonage, and a similar dish containing alkali floated on the surface of the water. Individual experiments sometimes lasted several days, and Lavoisier showed that whether the atmosphere is vital air or common air, the amount of vital air consumed is always the same. Nitrogen and hydrogen were unaffected by respiration, and no more carbon or hydrogen were consumed in vital air than in common air. The very significant observation was now made that guinea-pigs consumed more vital air during digestion, and likewise during movement.

They then extended their observations to man, and Seguin *insisted* that he should be the subject. The apparatus used was shown at the meeting of the Academy, but the promise given by Lavoisier that it would be described in a later memoir was never completely fulfilled. From the brief particulars actually given in a later paper and from Mme. Lavoisier's drawing (fig. 2) it would appear that Seguin breathed oxygen through

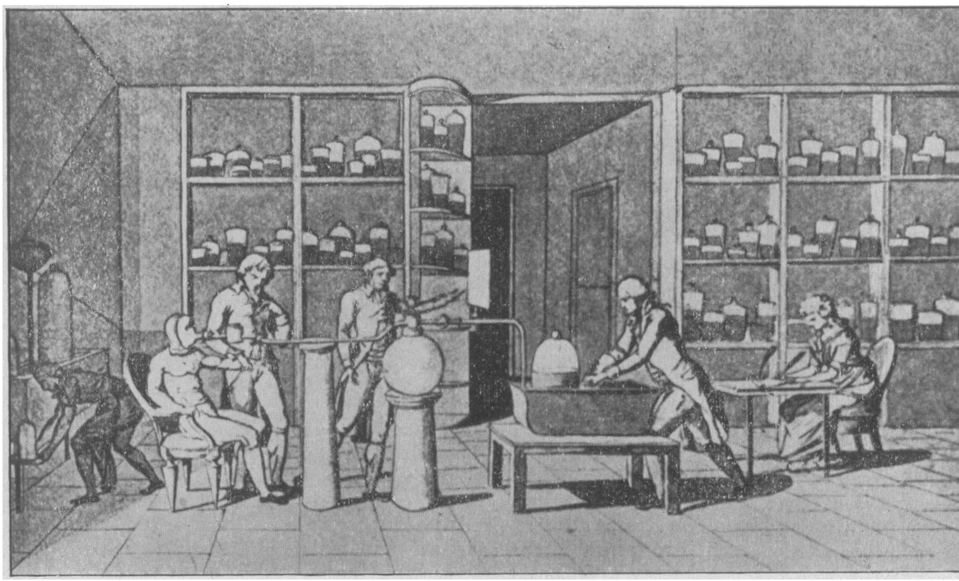

FIG. 2.—LAVOISIER IN HIS LABORATORY AT THE ARSENAL. AFTER A DRAWING by Mme. LAVOISIER. Lavoisier stands at the pneumatic trough. Seguin is the subject of an experiment on the respiration of a man at rest. Mme. Lavoisier is taking notes.

a mask which was in some manner stuck to the face (*se mastiquait sur la peau*). They emphasize that the experiments were carried out many times. The thermometer scale used was that of Réaumur. (It should be noted *passim* that very few references have ever done justice to these observations. Of the two longest known to me, Lusk [76] tabulates the results but does not name the scale; Aykroyd [77], in reproducing Lusk's table, assumes that the Centigrade scale was used.)

Lavoisier showed that a fasting man at rest at a temperature of 26° R. (32·5° C.) used up 1,210 cubic *pouces* (23·8 litres) of oxygen per hour. When the temperature was reduced to 12° R. (15° C.) the consumption rose to 1,344 cubic *pouces* (26·5 litres) per hour. During digestion at an unstated temperature, presumably 15° C., he used 1,800 to 1,900 cubic *pouces* (35 to 37 litres) per hour. After exercise during which he did the equivalent of raising a weight of 15 *livres* to a height of 613 *pieds* in fifteen minutes (10,531 foot pounds)—temperature unstated—the oxygen consumption rose to 3,200 cubic *pouces* (65 litres) per hour, and when approximately the same amount of work was carried out during digestion the intake again rose to 4,600 cubic *pouces* (90·5 litres). Though these results are remarkably accurate, it is of more importance that Lavoisier established—and recognized that he had established—the fact that the amount of oxygen absorbed depends upon the three factors, temperature, food and work. He noted that in all the experiments the temperature of the blood remains approximately constant. He also enunciated two laws which have apparently not been much followed up since then: that the increase in the pulse-rate is directly proportional to the work done; and that the oxygen consumed is proportional to the product of the respiration- and pulse-rates. In the memoir Lavoisier goes too far in saying that the absolute result must vary in individuals with such conditions as their age and state of vigour—since there is no evidence that Seguin was not the only subject. He gave 1,728 cubic *pouces* (34·0 litres) as an "average" figure of oxygen consumption for a man per hour, and he also gave provisional figures for the carbonic acid liberated, and the amount of carbon and hydrogen taken from the blood. Finally, he discussed what he considered to be the medical bearings of his discoveries. The statement is often made that Lavoisier held that the place where the slow combustion takes place is the lungs. A passage in this memoir makes one wonder whether he had not by this time a fairly open mind on the subject. He says that there is no decisive proof that the carbonic acid exhaled is produced in the lung or in the course of the circulation; it is even possible that this gas may be the product of digestion, may be carried to the lung, and liberated from the blood in proportion as the oxygen combines with it *by a superior affinity*.

The *First Memoir on Transpiration* [78] was read on April 14, 1790, but was not actually published until four years after his death. Transpiration is an aqueous emanation which exudes from the skin and from the lungs, and which is sensible only when it ceases to be in solution. Lavoisier approached the problem from the aspect of loss of weight in the individual. He conceived that the lung secreted "carbonized hydrogen"—from the *Traité* it would seem that this was his idea of the hydrocarbon radicle—and that the water formed from this and from the union of hydrogen and oxygen had to be got rid of. Seguin was equipped with a suit fitting close to the skin and made of a material similar to oiled-silk. In this way they were able to ascertain the loss of weight in the test individual due to respiration together with transpiration, and separately the loss due to respiration alone. Lavoisier estimated a man's loss of weight in twenty-four hours as 1 *livre* 13 *onces*.

A letter which Lavoisier wrote to Black of Edinburgh on November 13, 1790 [79], is his last authentic and untouched account of his views on respiration. He repeats the conclusions expressed in his first respiration memoir, and emphasizes that animal heat is constant in all circumstances, that respiration is the same in any concentration of oxygen provided that the carbonic acid is removed, and that nitrogen plays no part in the process. The results are given, apparently with much greater detail—which on examination proves to be largely superficial—in the *Second Memoir on Respiration* [80]. This paper was one of several communications read to the Academy by Lavoisier between March 9, 1791, and February 21, 1792; all of these have been lost with the exception of this and the following memoir [81]. Neither of these saw the light until 1814, when they were published, perhaps altered, by Seguin. The work for this present memoir was carried out on guinea-pigs, and the results are stated in detail. But although many experiments were carried out, the presentation of the data leaves much to be desired. Among new points introduced are the statement of the itching effect of carbonic acid gas on the skin—throwing some light as it does on Lavoisier's views regarding the noxious effect when the gas is respired—and the observation that in any respirable air, oxygen in excess of that necessary for combustion is inert.

The *Second Memoir on Transpiration* was read to the Academy on February 21, 1792, and published by Seguin in 1814 [82]. They believe that the transpired material would block the pores of the skin were it not for a surface respiration which takes place on the skin. That being the case, there must be alteration of the air near the skin surface. This point was investigated by withdrawing specimens of air for analysis from within Seguin's elastic suit. The action of air in complicating transpiration *per se* was also excluded by weighing Seguin before and after he entered a bath, and by analysing the bath water. Loss of weight in water was in all circumstances less than that in air. This was explained by the assumption that the skin contained both absorbing (*inhalant*) and excretory (*exhalant*) vessels, and that in a bath the absorbing vessels took in a little water. Throughout this memoir too much emphasis is placed on the possibility of the individual being able to maintain his body temperature by muscular work. They finally attempted to apply their findings to everyday life, and especially to the effect of clothing on the metabolism.

A full discussion of these memoirs must be reserved for another place, but meanwhile comment should be made on certain points. Lavoisier was undoubtedly the real discoverer of the true functions of oxygen in combustion, and he extended his results in brilliant fashion to the process of respiration in animals and in man. As he says, "the torch of life is lighted at the moment when the infant draws his first breath, and is extinguished only on his death" [83]. His experiments on metabolism—if they are correctly expressed in these last memoirs—were no doubt circumscribed by the fact that he thought that combustion probably took place in the lungs; that free hydrogen, or at least the hydrogen base or radicle, was combusted to form water; and that he had no idea that food could be stored in the liver and other organs for future use. Yet there is reason to believe that he might have solved these problems had he been granted a longer life. It has already been noted that he admitted that the blood might contain carbonic acid, and he says in one memoir that they intended to carry out blood analysis and experiments on digestion [84]. It is at least conceivable that the penetrating mind of Lavoisier would have ultimately noticed that if hydrogen from the body is consumed in the lungs to form water, it is very unlikely that it can be breathed unaltered in respired air—a fact which he had already proved. Lavoisier's ideas were probably not fully worked out at the time of his death. Certainly the presentation of the results in these last memoirs is scarcely adequate, and it is particularly unfortunate that Berthelot found no laboratory note-books relating to this period [85]. So far as oxygen consumption was concerned Lavoisier was on his own ground, and a master. He chose to follow this up by experiments on the loss of weight in metabolism. Considering his earlier experiments on calorimetry, he might quite possibly have switched over to heat loss had he lived longer—and then our already great debt to him might have been even greater. The fact that he had not done so at the time of his death was probably due to the difficulty of constructing a calorimeter for human experiments.

Reference is sometimes made to the highly-coloured passages in these last memoirs in which Lavoisier visualizes metabolism experiments of the future and drags in comparisons of metabolism in the rich and in the poor. In my view these passages are of no great importance. It should be remembered that Lavoisier had always been interested in economic matters, and that the Revolution was afoot. The style is different from that of earlier memoirs, and Seguin may have been in part responsible. This supposition would not be inconsistent with his later history. Seguin was 21 years old when the *First Memoir on Respiration* was read. He consorted with Fourcroy, Berthollet and other scientists who had revolutionary sympathies. In the year of Lavoisier's death he had, at the age of 26, become one of the budding capitalists of the Revolution. He had evolved improved processes for making leather, had had tanneries built for him, and in a few years was a very rich man. In his later years he was a leading authority on banking and finance, and a well-known eccentric [86].

On internal evidence there appear to be grounds for the suggestion that Lavoisier was only in part responsible for these last memoirs as they remain in their published form. Perhaps he intended them only as interim communications to the Academy, to be added to, corrected and polished—as was his wont—at leisure. No doubt the thoughts expressed are his, and he probably directed most of the experiments; but Seguin or some other person possibly wrote down Lavoisier's observations, the results of the experiments in brief, and his deductions from them. To these apparently Seguin added the extraneous analogies which are scarcely like the products of Lavoisier's logical and practical mind. A possible exception is the *First Memoir on Respiration*, which, apart from the fact that the experimental results are not given in great detail, is clearly on a different plane. Two years after Lavoisier's death his wife quarrelled with Seguin over the publication of the *Mémoires de Chimie*. Grimaux [87] records that in his proposed preface to the work Seguin attributed to himself an equal share with Lavoisier in the conception of the project. Mme. Lavoisier was indignant at his presumption, and the project was abandoned until 1806, when she herself edited the only two volumes of the *Mémoires* which appeared. From a detailed study of the last memoirs on respiration and transpiration I have formed the tentative opinion that Seguin merely acted as an assistant, and that conception and execution were both the work of Lavoisier himself. His generous acknowledgments of Seguin's part constituted a step which Seguin himself seems to have known very well how to use in his ascent. Until I have completed my study of Seguin's own writings on other subjects, however, I must advance this view with a certain reserve.

The Last Years

The last few years of Lavoisier's life, when these memoirs on respiration and transpiration were being written, belong to the history of the French Revolution. In 1789 the hurriedly assembled States-General became the National Assembly. Lavoisier, a member of the 89 Club, was possibly not out of sympathy with some of the aims of the Revolution; but the *Ferme* was anathema to the nation in its new mood. In January 1791 he was the object of a scurrilous attack by Marat, and in March the *Ferme* was suppressed by decree of the National Assembly. From that time Lavoisier threw himself with renewed vigour into the work of the Commission of Weights and Measures, and into a defence of the Academy against its enemies within itself and in political circles outside. His work on the Commission survives, but the Academy in its old form was suppressed in August 1793. Difficulties also arose over the *Régie des Poudres,* and on August 15, 1792, Lavoisier moved from the house at the Arsenal which he had occupied so happily for seventeen years. His cup was not yet full. Although the *Ferme* no longer existed, its former activities were very suspect, and accusations were being made—quite wrongly—against the former Farmers. The inquiries were very protracted, and on November 14, 1793, they were arrested. Lavoisier and the other accused persons, including his father-in-law, were imprisoned in the Prison of Port-Libre. There followed long delays, and attempts to obtain the release of the accused. On May 8, 1794, the trial took place—that trial at which the President, Coffinhal, declared: *"La République n'a pas besoin des savants, il faut que la justice suive son cours."* On the same day the great injustice was done. "Only a moment to cut off his head", said Lagrange, "and perhaps a hundred years before we see such another."

The Revolution had accomplished its most criminal act, and the science of respiration had to wait over forty years for its next forward step.

I am indebted to Professor Charles Singer and to Dr. Douglas McKie for reading the typescript of this paper.

The weights and measures used throughout are, unless otherwise stated, those of the pre-Revolutionary French system.

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