The Birth of Oxygen: Untangling the Web

Alan J. Rocke
Department of History
Case Western Reserve University
Cleveland OH 44106

Presented at the Oxygen Symposium
Madison, WI, 29 March 2003

It has been said that Isaac Newton was the most fortunate of all mortals, for there was but one universe, and so there could be but one discoverer of its laws.[1] With respect to the science of chemistry, the same might be said for the discoverer of oxygen. Like the planets to the sun, chemistry revolves around oxygen, which is *by far* the most abundant element, both in the earth's crust and in the human body. Moreover, once the nature of oxygen and oxidation was understood, it was no longer possible to maintain the ancient and unfruitful idea that earth and water were elements, for these were subsequently known to be oxides of simpler substances. A new concept of how "elements" should be understood led, in turn, to the rapid emergence of a theory of chemical *atoms*. Thus, within a generation after the discovery of oxygen, and as a direct result of that event, chemistry had emerged in the outlines of the science we know today.

But who actually discovered oxygen? This apparently simple question is at the heart of the play of the same name, and, as the play suggests, it is a surprisingly tricky question to answer. Without question, the first person to *publish a description of the preparation of what we now know as oxygen* was the English Unitarian minister Joseph Priestley. In 1770, when he started to become active in chemical research, Priestley was preaching at Mill Hill Chapel, a large congregation in Leeds. In April 1773 he was hired as librarian and literary companion to a wealthy aristocrat, Lord Shelburne, and took up residence at his estate in Calne, near Bowood, Wiltshire. It was here that

---

[1] This is a free paraphrase of the original quotation, which comes from Jean-Baptiste Delambre's éloge of Lagrange: "… aussi M. Lagrange, qui le [Newton] citait souvent comme le plus grand génie qui eût jamais existé, ajoutait-il aussitôt: *et le plus heureux; on ne trouve qu'une fois un système du monde à établir.*" J. B. Delambre, "Notice sur la vie et les ouvrages de M. le comte J. L. Lagrange" (1816), rpt. in *Oeuvres de Lagrange*, 1 (Paris: Gauthiers-Villars, 1867), ix-li, on p. xx (italics in original).

Priestley discovered the gas he always referred to as "dephlogisticated air." An ardent republican, he warmly welcomed the French Revolution, thus finding himself reviled in England (Unitarians, as dissenters from Anglican belief, were already social outcastes, and his left-wing politics only made matters worse). In 1794 Priestley emigrated to that haven of liberal politics, the United States, spending the last ten years of his life at a Unitarian retreat in the backwoods of central Pennsylvania.

Priestley was brilliant, intense, idiosyncratic, and crotchety, though in conversation he was always described as quiet and amiable, even charming. The volume and breadth of his writing is nothing short of astonishing, including over 200 books and pamphlets—"many more, Sir," Priestley once wrote to a correspondent, "than I should ever like to read."[2] His subjects ranged over theology, history, politics, pedagogy, classical languages, English grammar and linguistics, and most of the sciences. Priestley's first residence in Leeds was next to a brewery, whose fermentation vats he used to invent soda water, which he named "Pyrmont water" after a famous German natural-spring spa town. The appearance in France of a pamphlet describing this product began to establish Priestley's reputation there. This work also led him, from 1772 on, to the investigation of a startlingly broad range of "airs", several of which were new, including what we now call oxides of nitrogen and gaseous hydrogen chloride.

Then, on 1 August 1774, Priestley collected a gas driven off from heating the red calx [oxide] of mercury; this, as we now know, was oxygen. A candle burned in this new air with a vigorous and much-enlarged flame; Priestley thought that his mercury must have been contaminated by saltpeter to have produced such a display. Two months later Priestley accompanied Lord Shelburne to Paris, where he was invited to dinner at Lavoisier's home. In unpracticed French, assisted by Madame Lavoisier, he told Lavoisier about the experiment, and Lavoisier (according to Priestley's later recollection) was all ears. Back home at Bowood House the following March,

---

[2] Schofield, *Enlightenment*, p. 45.

Priestley repeated the experiment, and further study suggested that he was dealing with a newly discovered substance that supported combustion and respiration much better than ordinary air. What should he call it?

Priestley, like all chemists at that time, understood that substances were flammable if and only if they contained the principle of inflammability, called "phlogiston." It was the *loss* of this phlogiston that constituted combustion, calcination, or respiration. Since his new air supported these processes much better than common air, it must have a peculiarly large capacity to absorb phlogiston from burning objects; it must, then, be "dephlogisticated air." He published this news in the second volume of his book *Experiments and Observations on Different Kinds of Air*, which appeared late in 1775.

What Priestley could not know is that this air had already been prepared and carefully studied by a Swedish pharmacy assistant named Carl Wilhelm Scheele. Employed in a pharmacy in Uppsala, Scheele was able to pursue his investigations only in bits of spare time, although many of his discoveries were known to the world-famous professor of chemistry at Uppsala University, Torbern Bergman. About 1771 Scheele discovered a remarkable new air that vigorously supported combustion, recording the event in a laboratory notebook that still survives. This was the earliest documented observation of the gas, which Scheele named "fire air" (Feuerluft). He also prepared and studied the gases now known as nitrogen, hydrogen, chlorine, and several others. Pressed by his daily duties, not a master of his own time, he delayed publication of this extraordinary research. Heroic historical efforts have only recently succeeded in deciphering Scheele's nearly illegible penmanship, so that we now know some of the details of the discovery.

In 1775 Scheele got a break, when the town of Köping, 100 miles to the west, lost its only pharmacist. Scheele not only acquired the pharmacy, but took the pharmacist's widow and son under his care, as well. Scheele remained in this small town the rest of his short life. In a letter to his father written shortly after moving to Köping, he professed his intent to marry the widow Pohl (who was then just 24, by the way). He did exactly that, but only 11 years later, on his deathbed, simply to ensure that Sara Margarethe would retain ownership of the pharmacy. Was he too busy? Unattracted by widow Pohl? Of an incompatible sexual orientation? Simply clueless? Or did widow Pohl resist his advances? We will never know.[3]

Scheele was aware of the similar work of both Priestley and Lavoisier. He knew that he had been the first to prepare the amazing new gas, and was sensitive to the issue of priority. Soon after moving to Köping, before the end of 1775, he finished writing up his research for publication in the form of a pamphlet in German. But for the better part of two years Bergman delayed delivering an all-important preface endorsing the little-known author. When the work finally was published in 1777, most of Scheele's novelties were already old news.

Like Priestley, indeed like virtually all chemists, Scheele believed in the existence of phlogiston, and incorporated his discovery into the phlogiston theory in a clever fashion. According to Scheele, his new fire air supported combustion by attracting and chemically combining with the phlogiston that left the burning object; if fire air was not present, the phlogiston would not leave the object, and so combustion would not occur. According to Scheele, the combination of fire air and phlogiston constitutes the matter of heat, and indeed heat is evolved in all combustions and calcinations.

Scheele and Priestley were extraordinarily skilled practical chemists; both were also energetic, inventive, and resourceful. What they failed to do was sufficiently to ponder the implications of their discoveries for the prevailing body of chemical theory. This was not simply, perhaps not at all, a failure of intellect or imagination; it was directly connected with their laboratory *methods*. Dealing with gases as Scheele and Priestley did, it was natural to use *volume* as

---

[3] In February 1886 Scheele complained in a letter to Wilcke of gout. There exists an undated MS in Scheele's hand, labeled "Onanism. Self-Defilement." This MS argues for the belief that celibacy causes gout due to the absorption of sperm by the body, and that sexual activity or masturbation relieves the symptoms. It also prescribes remedies. See Uno Boklund, *Carl Wilhelm Scheele, His Work and Life* (Stockholm: Roos, 1968), pp. 330-33, 480-81.

their consistent measure of amount of material. Neither man thought of systematically using *weights* of their gases, nor of pursuing the next step—carefully following these weights through the various chemical reactions in which the substances took part.

Enter Lavoisier. Raised in a wealthy Parisian family, educated for the law, from an early age he was fascinated by chemistry. He was elected to the elite Academy of Sciences at the age of twenty-five, a measure of how fast he acquired the reputation of a leading figure in the French scientific world. His life was devoted to science, but he required income; therefore he bought a share of the "General Farm," a private agency that contracted with the French government for collection of certain indirect taxes. This move may be regarded as analogous to buying a seat on a commodities exchange today; it was much more (and much less) than tax collection. Later, as Director of the Gunpowder Commission and Chairman of the Board of the Discount Bank, he became much involved with economic and political reforms at the end of the Old Regime. Ethical and progressive in all his dealings, he welcomed the early stages of the French Revolution. However, his connection with tax farming brought him to grief in the Reign of Terror. Convicted by a kangaroo court of corruption and oppression of the people, he was executed in May 1794, at the age of fifty.

Lavoisier's marriage was fortunate in every respect. Marie Lavoisier was young, beautiful, accomplished, and intelligent. She drew the diagrams for her husband's books, translated and interpreted from foreign languages, kept his correspondence, and assisted in the laboratory. In April 1776, when Antoine joined the gunpowder commission, the couple moved to an elegant apartment adjoining a well-equipped laboratory in the Paris Arsenal, near the Bastille. Lavoisier could afford the best custom-built apparatus, and he also attracted a stable of dedicated assistants. But his own intensity, stamina, imagination, and intelligence was ultimately the driving force of his research program.

In 1772 Lavoisier carved out a research subject for himself—the participation of air in all imaginable chemical and physiological processes. He soon convinced himself that burning and calcining objects absorb air and increase in weight; conversely, when substances are reduced they lose air and weight. This suggested that the phlogiston theory was at least incomplete, and perhaps even incorrect, for how could matter lose phlogiston yet gain weight, and how could it gain phlogiston yet lose weight? But Lavoisier was initially unable to determine just *what kind of air* was involved in these reactions.

In April 1774 a Parisian chemist named Pierre Bayen discovered that the red calx of mercury could be reduced to the metal without charcoal, simply by heating it strongly. Lavoisier's attention was drawn to this phenomenon, for the simplicity of the reduction suggested that it could help clarify a confusing situation. A commission appointed by the Academy of Sciences that included Lavoisier investigated this reaction that summer and fall, but apparently without studying the nature of the gas given off. This was exactly when Priestley was isolating oxygen for the very first time, and by exactly this same method.

It was at this point, October 1774, that Priestley visited Paris and told the Lavoisiers about his new air. Virtually simultaneously—it must have been literally within days of this dinner—Lavoisier received a letter from Scheele describing the preparation of a gas that was similar if not identical to Priestley's.[4] There is no question but that Lavoisier was already familiar with the reaction, if not with the properties of the gaseous product. There is also no question but that he took inspiration from these hints from his rivals. By the following March, Priestley understood for the first time that he was dealing with a new gaseous substance, which he began calling dephlogisticated air. That same month, Lavoisier carried out an extensive study of the gas. His conclusion was that it was "pure air," or "the air itself entire and undecomposed."

---

[4] The most detailed discussion of this letter and the history associated with it is ibid., pp. 42-47, 364-92.

Lavoisier published this immediately, in May 1775, and Priestley read the paper. When Priestley's discussion of the same reaction was printed a few months later, he took the opportunity to correct Lavoisier. Dephlogisticated air is not pure air or the air itself, Priestley wrote; it is a unique chemical species, mixed with another portion of the atmosphere that does not support combustion or calcination. Supplied with this second hint from his rival, Lavoisier carried out new and even better experiments. His language began to shift: the air from mercury calx was now described as "the purest portion of the air." He gradually came to agree with Priestley, that the gas was a distinctive chemical entity. By 1779 further research had produced the conviction that it was a constituent of all acids. In a revision to a paper of that year Lavoisier coined the word "oxygen" or "acid-former," in order to avoid any circumlocutions in the future.

Lavoisier's research pathway led through nearly trackless wilderness to a comprehensive theory embracing combustion, calcination, reduction, respiration, the composition of water, the nature of heat, acidity, and the gaseous state, a new concept of chemical elements, and a reform of chemical nomenclature. The journey took him from 1772 until 1785, and the result was a total refutation of phlogiston. By 1792 nearly all professional chemists agreed that Lavoisier's evidence was convincing and that his system was true. Scheele was already dead by then; Priestley held out in support of phlogiston, almost alone, isolated in backwoods Pennsylvania, until his death in 1804.

Lavoisier's performance was one of the most brilliant tour de forces in the entire history of science. In addition to dazzling experimental skills, profound theoretical imagination, indefatigable energy, and the good fortune to command resources and wealth, he had one more crucial advantage, namely his devotion to a simple principle, apparently overlooked by Scheele and by Priestley. This principle was that in order fully to understand any chemical reaction, one must carefully identify *all* reactants and *all* products, in other words every chemical entity that plays a role, whether solid, liquid, or gas; and further, that one must achieve a precise mass balance, so that the sum of the weights of all the reactants precisely equals the sum of the weights of all the products. As *simple* as this prescription sounds, it is fiendishly difficult to achieve, especially with gases; and as *obvious* as the principle may seem to modern ears, in the context of the 18th century it was not obvious at all. But it was the indispensable key to the door leading to modern chemistry.

Who discovered oxygen? A complicated question; here are some straight answers. Chronologically speaking, Scheele was the first to isolate oxygen and study its properties; in this sense he is the earliest discoverer, but he delayed the publication or dissemination of the event. Without any knowledge of Scheele's work, Priestley followed in his footsteps soon thereafter, and became the first to publish the discovery. Now, where exactly does this leave Lavoisier? This is a more difficult question, and one that calls for definitional as well as value judgments. Oxygen gas, considered solely as a discovered substance, was simply one fact among many. What should one *do* with this new fact? Lavoisier used it to build an entirely new system of chemistry, and in the process he built the foundations of the science we know today.

I mentioned Isaac Newton at the beginning of this talk, and a comparison with his career can be instructive. Historians of science are well aware, even if laypeople are not, that most of the specific constitutive elements of Newtonian physics originated one or two generations before him. His first law of motion, rectilinear inertia, was stated first by Descartes; a specific crucial instance of Newton's second law of motion was thoroughly explored by Galileo; inertia and relativity of motion was likewise stated by Galileo; Kepler provided the foundation of planetary kinematics; and the inverse-square concept of gravitation was explored by Edmund Halley, Christopher Wren, Robert Hooke, and others, sometimes even in print, before Newton. Newton was well aware of all of this. Why, then, do we always speak of "Newtonian physics"?

We do so with justice. To take nothing away from Newton's predecessors, the achievement represented by his masterwork, the *Principia*, is so extraordinary as to be almost miraculous. What had been isolated ideas, inconsistent applications, half-finished demonstrations, unfruitful theory, and solitary facts, were now integrated into a rigorous and consistent mathematical structure for celestial and terrestrial mechanics that met nearly all empirical tests, and moreover provided a reliable guide for further exploration of the unknown.

The parallel to Lavoisier is by no means perfect, but I think it does suggest something of the truth. Lavoisier, like Newton, did not always back the right horse, from an anachronistic modern point of view; one such example is his enthusiastic retention of caloric, a material fluid of heat that had much in common with phlogiston. Yet it is also true that in borrowing extensively from his predecessors and rivals, and then brilliantly developing the science both in the laboratory and in the quiet of his study, Lavoisier, like Newton, laboriously constructed a magnificent scientific edifice, an edifice that in essential respects still stands today.

But one might still inquire, did Lavoisier borrow, or did he steal; that is, did he properly credit his rivals? The question is not easy to answer, but let me conclude with some reflections on this question. First of all, Lavoisier could certainly have been more generous in the first instance with his rivals. He never answered Scheele's 1774 letter, and his early references to Priestley were more grudging and incomplete than one would have liked to see. Clearly Priestley himself, at least, was not satisfied by his treatment.

And yet, Lavoisier repeatedly published high words of praise of the Englishman. In 1776 he conceded that Priestley had been right in claiming that the gas was a unique substance. Before doing so, he went so far as to admit that "some of the experiments described in this paper were not performed first by me; indeed, strictly speaking, there is not one for which M. Priestley could not claim the first idea. However, since the facts have led us to diametrically opposite conclusions, I hope that if I am criticized for having borrowed experiments from this celebrated scientist, at least the conclusions will be regarded as original with me." The following year, 1777, he drew the lines even sharper, even though he still had not yet named the new gas. "According to M. Priestley and many other scientists," he wrote, "the air in which one has burned candles is partly phlogisticated. I think, on the contrary, and I believe I have proven, that this air is only atmospheric air partially deprived of its pure, respirable portion. … Besides, since I am at the point of attacking the entire doctrine of Stahl concerning phlogiston, and of undertaking to prove that it is erroneous in every respect, if my opinions are well founded, M. Priestley's phlogisticated air will find itself entangled in the ruins of the edifice."[5]

Now, is this not some very interesting wording? As Frederic Holmes has noted, the passage "suggests … that Lavoisier was attempting to portray Priestley not as the perpetrator of erroneous views, but as a victim trapped within the ruins of the system of Stahl."[6] Holmes argues persuasively that Lavoisier warmly admired the Englishman, and was moved to dispute his conclusions only slowly and regretfully. According to Holmes, Lavoisier's high regard for Priestley's scientific brilliance is one of the reasons that he did not push to establish his complete system of antiphlogistic chemistry until the mid-1780s. It seems so, to me, as well. It is pleasant to conclude this brief review of the complex and fractious history of the discovery of oxygen, on a note of such sweet reason and generous human feeling.

---

[5] Holmes, *Lavoisier and the Chemistry of Life*, p. 106; Donovan, *Lavoisier*, p. 144.

[6] Holmes, ibid.