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Phlogiston in the Air

1. When Antoine-Laurent Lavoisier sent a copy of his *Traité élémentaire de chimie* to Alessandro Volta in January 1791, he wrote, "I do not know, Monsieur, what opinion you have embraced regarding the question that divides chemists relative to the existence of phlogiston". If Volta were to give a few moments of attention to this work, however, he would be able to "judge how one can explain all of the phenomena of chemistry without recourse to a hypothetical substance whose existence has not been proven by any direct experiment".[1]

Seven years later Volta wrote to Martinus van Marum in Holland that:

> […] I had already approached very close to the new chemical theory, not only before it had acquired partisans outside of France, but even before it had been published in its entirety or had taken the form of a body [of knowledge]. I have had, therefore, no difficulty embracing it in its entirety, and I have taught it in my lessons and public demonstrations for several years. Nevertheless, I am not far from adopting the correction or addition proposed by several German chemists, notably Rühter and Gren, who think that combustibles are not solely oxidized in combustion, that they do not only acquire oxygen by a simple affinity, but exchange for it another principle which they let go, which is the base of light and for which they wish to conserve the name phlogiston.[2]

If we try to fit them into the conventional picture of the chemical revolution as a contest between two clearly aligned opposing forces, known at the time as the "phlogistonists" and the "antiphlogistonists", these two statements appear paradoxical. Volta claims to have been one of the first to approach the new chemical theory, but appears also one of the last to declare himself publicly in support of it. If we assume that Lavoisier was not ignorant in 1791 of a public position in favor of the new chemistry that Volta might already have taken, then the "several years" over which Volta claimed in 1797 to have taught it must have begun only sometime in the early 1790s. That would mean that, in the great "question" over which chemists had been divided, Volta switched over to the side of the new chemistry, not only years later than Guyton de Morveau, Berthollet, and Fourcroy, but later than Joseph Black

[1] LAVOISIER (1997), p. 218.
[2] VOLTA (1798), p. 271.

and even later than the capitulation of Richard Kirwan, the most staunch defender of phlogiston after Joseph Priestley. Even then, according to his own testimony, Volta's conversion remained incomplete, as he leaned toward a "compromise" position offered by the remaining defenders of phlogiston in Germany. It is tempting then to view Volta's claim, in a letter written to a strong partisan of Lavoisier, as a retrospective attempt to identify himself with those who had, by 1797, emerged as the clear winners in this momentous contest.

If we examine more closely Volta's trajectory during these years, however, we can be led instead to question the categories in which the chemical revolution has most often been described. The traditional view of the "overthrow of the phlogiston theory" as a cataclysmic event has been reinforced by the pervasive influence of Thomas Kuhn's ideas about the nature of revolutionary change. That the new chemistry must be incommensurable with the phlogiston theory; that the move from one paradigm to the other must, therefore, be a holistic leap analogous to a Gestalt shift; and that individual scientists make this shift at various times in an experience having resonances with a religious conversion,[3] has appeared to be amply illustrated by the way in which Lavoisier obtained converts one-by-one, and then formed with them a "school" dedicated to the conversion of the remaining phlogistonists.[4]

Within such a framework, Volta appears as an anomaly. A fervent admirer of Priestley from the early 1770s onward, he nevertheless developed strong ties with Lavoisier. In 1782 Volta came to Paris, where he, Lavoisier and Laplace conducted joint experiments on the electrical phenomena they thought to accompany evaporation, and certain chemical processes.[5] Lavoisier has been regarded as remarkably successful at converting those scientists who came under his personal persuasive power in Paris,[6] yet Volta left Paris unconverted. The extensive footnotes[7] he wrote for an Italian edition of Macquer's *Dictionnaire de chymie* during the following year retained an explanatory framework strongly oriented around phlogiston. Yet Volta's rigorously quantitative experimental style in the study of airs was closer in spirit to that of Lavoisier than to that of Priestley, and his allegiance to the phlogiston theory did not reduce his admiration for the experimental achievements of Lavoisier and his associates, in particular for their synthesis of water. He believed, in fact, that his own eudiometric experiments on the combustion of inflammable air in dephlogisticated air had led them to the threshold of this landmark event.[8]

The long trajectory that led Volta from follower of Priestley to supporter of the new chemistry of Lavoisier and his followers does not appear to have included a

[3] KUHN (1969).
[4] See especially PERRIN (1981).
[5] VOLTA (1782), pp. 274-6, ID. (1782a), pp. 296-8; LAVOISIER (1781), ID. (1781a).
[6] PERRIN (1981), pp. 50-1; DONOVAN (1993), pp. 157-87.
[7] Collected in *VO* (see Abbreviations), VI, pp. 349-436; VII, pp. 5-105.
[8] VOLTA (1798), p. 270.

compact "conversion" experience. Instead it was a prolonged passage, over much of which he occupied an intermediate position, containing elements both of Priestley's position and those of Lavoisier's theory of oxygen, as well as Lavoisier's views on the gaseous state of matter. Nor was Volta's situation unique. Each of the major participants in these events, including Lavoisier himself, moved from the traditional phlogiston theory of Georg Ernst Stahl toward the oxygen theory – not in a single mental leap but along one of various routes that included some succession of intermediate positions. By following the stages of Volta's transition we can, therefore, also begin to reassess the broader nature of the chemical revolution and of the general dynamics of revolutionary change in science.

2. To understand Volta's relation to Priestley and to Lavoisier clearly, we need first to re-examine the relations between Priestley and Lavoisier themselves, and their respective standing among their peers in the period prior to 1782. In doing so, I want to elaborate on a reassessment of that situation contained in Ferdinando Abbri's important study of the chemical revolution, *Le terre, l'acqua, le arie*. Among the important revisions of the customary story that Abbri has contributed is his assertion that:

> Between 1775 and 1781 there did *not* exist a Lavoisier cause, there was *no* true and proper debate between the alternatives of phlogistication or oxidation. Priestley dominated the science of his time, and his principle interlocutors were Landriani, Fontana, Fabbroni, Macquer, Kirwan, Scheele, and in a quite limited way, Lavoisier.[9]

Abbri attributes the surprising lack of references to the revolutionary papers that Lavoisier presented in 1777 and 1778 to the fact that these papers appeared in print only in 1780 and 1781. Lavoisier was known in the chemical community mainly through his *Opuscules physiques et chimiques*, a volume published in 1774, which he had taken care to send out to all the leading chemists of the time. This work attracted widespread attention, but was not seen as revolutionary.[10]

Abbri's view is based on his extensive survey of the contemporary literature, and is convincing. That it has not been obvious to historians is, I believe, due to their natural propensity to focus on the early trajectory of the eventual victor in such scientific debates. In Lavoisier's case the dramatic statement written in his laboratory notebook in February 1773, at the beginning of his long investigative venture, which is taken as a prediction of the revolution that he eventually achieved, has induced historians to follow him through his career as a prescient figure predestined to his success. In this well-worn story, Priestley takes the lead in the exploration of the newly discovered "airs" only until about 1776, by which time he has provided Lavoisier with several essential clues along the way to the announcement of a new general theory of combustion in 1777. From then on,

[9] ABBRI (1984), pp. 198-9.
[10] *Ibid.*, pp. 160-1.

although the way to final triumph is still long and arduous, Priestley has become the defender of the traditional phlogiston theory, while Lavoisier becomes the herald of the future "modern" chemistry.

Not only has the story been skewed by projecting its momentous outcome onto its earlier stages, but Lavoisier's own supposed initial prophecy of the outcome is, I believe, based on a misunderstanding of the oft-quoted passage in his laboratory notes. In another place I have argued that, read in its context, the ideas that Lavoisier predicted would occasion a "revolution in physics and chemistry" were not his own, but those ideas concerning the nature of airs, and in particular of fixed air, that had arisen over the preceding decades through the work of Stephen Hales, Joseph Black and their followers. At the time he published his *Opuscules*, Lavoisier asked not to be taken as the initiator of a new chemistry, but as a small contributor to a revolutionary movement already prepared by others.[11] His image of himself in these first years of his endeavor was, I believe, not unlike the perception of him by others that Abbri has suggested. He was only an able participant in a broader revolution brought about by the new pneumatic chemistry.

If we accept Abbri's view that Priestley dominated the field until after 1780, we can reconstruct the relationship between Priestley and Lavoisier in a way that is nearly the inverse of how they have customarily been portrayed. Perhaps in part because of the fact that he led the way into the revolution but ended up as its loser, Priestley has often been treated more kindly than has Lavoisier. The English natural philosopher is seen as candid, open, a genial experimenter who preferred facts to speculation, and more generous spirited than his French rival. Lavoisier is seen as more focused and methodical as an investigator, and more astute than Priestley as a theorist, but more calculating, more ambitious and more willing to exploit the work of others to his own advantage. Much of Lavoisier's reputation for borrowing from others, without acknowledging his debts, can be traced to a myth that Priestley himself initiated. In the second volume of his *Experiments and Observations on Different Kinds of Air*, published in 1776, Priestley related that after he had left Paris, where he had procured a pure calx of mercury, and had "spoken of the experiments that I had made, and that I intended to make with it, he [Lavoisier] began his experiments on the same substance, and presently found what I have called *dephlogisticated air*, but without investigating the nature of it". Elsewhere in the volume he recalled that he had "frequently mentioned my surprize at the kind of air which I had got from this preparation to Mr. Lavoisier, Mr. le Roy, and several other philosophers, [...] who, I dare say, cannot fail to recollect the circumstances".[12] Priestley genuinely believed, as he put it in a letter to Thomas Henry, that Lavoisier "ought to have acknowledged that my giving him an account of the air I had got from *Mercurius Calcinatus*, and buying a quantity of M. Cadet while I was in Paris,

[11] HOLMES (1998), pp. 137-8.
[12] PRIESTLEY (1775), pp. 36, 320.

led him to try what air it yielded, which he did presently after I left".[13] Carl Perrin has shown, however, that the view that Lavoisier obtained a critical lead from Priestley is questionable,[14] and the laboratory notebooks now show that the charge that Lavoisier began his experiments as a result of the encounter is groundless. Lavoisier carried out his first attempt to reduce mercury calx without charcoal during July 1773, a full year *before* Priestley began his experiments with the same substance.[15] In his letter, Priestley maintained that he had "barely hinted" at his complaint in his second volume. His public intimation that Lavoisier may have stolen something from him has, however, strongly shaped the attitudes of historians toward Lavoisier. Because the experiments that Lavoisier carried out with the mercury calx were crucial to the development of his theory, Priestley's allegation has darkened Lavoisier's reputation ever since.

From the time he took up the systematic investigation of airs in 1773 until the time he broke openly with Priestley's phlogiston interpretations in 1777, Lavoisier not only followed the English scientist's experimental lead at several critical junctures, but freely and repeatedly acknowledged Priestley's leadership. When he came to conclusions that differed from those of Priestley, Lavoisier took pains to express his disagreements tactfully and to surround them with deferential references to the achievements of the "celebrated" experimentalist. Priestley's references to Lavoisier's work during the same period were less consistently generous. Although he praised Lavoisier highly on at least one occasion for an experimental observation, he scarcely recognized, in these years, any significant general contribution by the younger French chemist. He fixed on the errors he believed he saw in Lavoisier's work, characterized his theoretical conclusions as "speculations", and picked out every "misrepresentation" of his own writings that he could find in Lavoisier's publications, even while significantly misrepresenting Lavoisier's positions.[16] These generalizations can be illustrated by the interchange arising from the publication, in 1776, of Lavoisier's classic paper "On the Existence of Air in Nitrous [nitric] Acid".[17]

By placing a quantity of mercury in fuming nitrous acid and trapping the air disengaged from their ensuing effervescence in a succession of vessels inverted over water, Lavoisier found that the operation produced two species of air – the nitrous air discovered earlier by Priestley, and what Lavoisier at that time was calling "the most pure air": that is, the air that Priestley and he had each obtained from the

[13] PRIESTLEY (1966), p. 153.
[14] PERRIN (1969).
[15] HOLMES (1998), pp. 108-9; PRIESTLEY (1775), p. 34.
[16] PRIESTLEY (1775), pp. 304-23. One misrepresentation of Lavoisier's views was Priestley's claim that Lavoisier considered common air to be "a simple *elementary body*", whereas Lavoisier had posed as an open question, in a paper to which Priestley referred in the same volume (p. 313), whether there were different species of air, or only a single species from which other airs were derived by modification. See LAVOISIER (1775), p. 429.
[17] LAVOISIER (1776); ID. (1776a).

reduction of mercury calx without charcoal, and which Priestley called *dephlogisticated air*. Lavoisier measured the volumes of the two airs, and by making assumptions about the densities of the two airs he calculated their weights. He did not establish that their combined weights equalled the weight of the nitrous acid consumed, but he did show that at the end of the operation the mercury that was recovered weighed "within a few grains" the same as that which he had put in. "It was evident, therefore", he concluded, that "by its combination with the mercury, the nitrous acid had been resolved into two airs". He completed his demonstration of the composition of nitrous acid by combining the two airs to "recompose" the acid. The operation through which he did so had been described by Priestley in the first volume of his *Experiments*. As in the analysis, Lavoisier did not show, in this synthesis, that the weights of the airs consumed equalled that of the acid formed. Having found three years earlier that the combustion of phosphorus and of sulfur produced respectively phosphoric and vitriolic acid, Lavoisier now proposed that every acid was composed of a "particular principle" combined with a "portion of the purest part of the air".[18]

In the second volume of his *Experiments* Priestley had reported the production of dephlogisticated air not only from mercury calx in nitrous acid, as Lavoisier now did, but also from other "metallic earths" such as red lead, and from calcareous earths. With vitriolic or marine acid, on the other hand these earths did not give off "the least air". These results, Priestley wrote, left "no doubt in my mind, but that *atmospherical air*, or the thing that we breathe, *consists of the nitrous acid and earth*, with so much phlogiston as is necessary to its elasticity".[19] The experiments and reasoning through which Priestley arrived at this conclusion were entirely qualitative.

When Lavoisier presented his results, he was confronted with a tactical dilemma. In pursuing the experiments through which he had established the composition of nitrous air, he had depended heavily on Priestley's experimental methods, but had arrived at a result that contradicted Priestley's conception of the composition of the substances involved. The paragraph in which he dealt with this situation in his paper suggests that he made a great deal of effort to balance his claim to originality with his indebtedness to Priestley and his recognition of the esteem in which Priestley's experimental achievements were held:[20]

> Before entering into the matter at hand, I would like to begin by informing the public that some of the experiments contained in this paper do not belong to me at all; perhaps even, strictly speaking, there are none of them for which M. Priestley cannot claim the original idea. But, as the same facts have led me to diametrically opposed

[18] LAVOISIER (1776a), pp. 129-36.
[19] PRIESTLEY (1775), pp. 48-75.
[20] For a similar situation a few months later, it is possible to show, through the revisions he made in a succession of drafts, how Lavoisier struggled to balance these factors in his relation with Priestley. See HOLMES (1985), pp. 70-3.

> consequences, I hope that, if one reproaches me for having borrowed the proofs from the work of this celebrated physicist, one will, at least, not contest with me the property of the consequences.[21]

Throughout his paper Lavoisier drew attention to the experimental procedures he had used that were based on those that Priestley had carried out before him. Lavoisier's acknowledgement was, if anything, more generous than necessary. If each of his experiments was modelled on ones done by Priestley, he had not merely repeated any of them just as Priestley had done them, but modified the procedures to fit his different purposes. This is, after all, the common way in which science is pursued in an active field.

In dealing with those consequences that were opposed to Priestley's views, Lavoisier maintained his diplomatic style. "One will not fail to ask", he wrote, "if the phlogiston of the metal plays some part in this operation; without venturing here to decide a question of such great importance, I will respond that, because the mercury comes out of this operation exactly as it entered it, it appears that it had neither lost nor regained phlogiston", unless one assumed that the phlogiston passed through the vessel; but that would require a kind of phlogiston different from that of Stahl and his disciples. Here Lavoisier avoided mentioning that the conclusion also ruled out the role of phlogiston as Priestley had invoked it in this situation. Lavoisier could not evade a direct reference to his refutation of Priestley's view of the relation between the composition of nitrous air and of atmospheric air, but he again surrounded his disagreement with praise for the English scientist:

> I would like to end this paper as I began it, by rendering homage to M. Priestley for the greater part of what it contains; but the love of truth and the progress of knowledge to which we devote all of our efforts oblige me at the same time to point out an error into which he has fallen, and which would be dangerous to allow to stand. Having recognized that in combining nitrous acid with any earth whatever, he constantly obtained common air, or air better than common air, this justly celebrated physicist believed that he could conclude that atmospheric air is a compound of nitrous acid and earth. This bold idea is sufficiently refuted by the experiments contained in this paper. It is evident that it is not the air which is composed of nitrous acid, as M. Priestley thinks, but, on the contrary, nitrous acid which is composed of the air, and this remark alone provides the key to a great number of the experiments contained in sections III, IV and V of M. Priestley's second volume.[22]

It is difficult to see how Lavoisier could have treated with greater consideration the man to whom he owed much, but with whom he could not avoid a fundamental disagreement. Within the conventional interpretation of Lavoisier at this point as preparing already for the general attack on the phlogiston theory that he would launch a year later, these statements have appeared as a kind of preparatory warning,

[21] LAVOISIER (1776a), p. 130.
[22] *Ibid.*, p. 138.

presaging the broader break with Priestley that was to come. I have shown elsewhere, however, that Lavoisier was not necessarily heading steadily for that denouement, nor veiling his true intentions behind hints of broader positions still held back. Rather, in 1776, he was still living in an intermediate mental world, still thinking sometimes in terms of new conceptions of his own invention, sometimes falling in behind Priestley's interpretations.[23] His argument that phlogiston did not take part in this particular chemical change does not mean that he had already concluded that phlogiston is a hypothetical substance unsupported by any direct experiment. There is no compelling need to conclude that, at this point, Lavoisier perceived himself as moving toward a general rupture with Priestley. The tone of his paper suggests, on the contrary, that he hoped that the "celebrated physicist" would be open to persuasion on the points of disagreement between them. That such a hope would not have appeared unrealistic to Lavoisier at the time he wrote this paper is suggested by a remark by Priestley concerning his trip to Paris that has received less historical attention than his celebrated complaint about the calx of mercury. In a section in volume two of his *Experiments* concerning "air produced by the solution of vegetable substances in spirit of nitre" Priestley began with the statement that the experiments described in it "were occasioned" most immediately by:

> an experiment which I had the pleasure to see in Paris, in the laboratory of Mr. Lavoisier, my excellent fellow-labourer in these inquiries, and to whom, in a variety of respects, the philosophical part of the world has very great obligations. […] At Mr. Lavoisier's I saw, with great astonishment, the rapid production of, I believe, near two gallons of air, from a mixture of spirit of nitre and spirit of wine, heated with a pan of charcoal; and when that ingenious philosopher drew this air out of the receiver with a pump, and applied the flame of a candle to the orifice of the tube through which it was conveyed into the open air, it burned with a blue flame; and working the pump pretty vigorously, he made the streams of blue flame extend to a considerable distance. Being very much struck with this experiment, I determined with myself to give particular attention to it, and pursue it after my return to England.[24]

Given Priestley's reputation as a brilliant experimentalist and Lavoisier's reputation as a lesser experimentalist, but greater theorist, it is charming to see Priestley's enthusiasm for the experimental ingenuity of the French scientist. Given that Lavoisier is known primarily as a quantitative experimentalist, it is refreshing to see that the experiment in question was a beautiful qualitative one, of the type for which Priestley was best known.

Whatever hopes Lavoisier may have harbored for a theoretical rapprochement would have been shattered by the response that Priestley added to the Preface of the third volume of his *Experiments* just before its publication in 1777. According to Priestley's account, Lavoisier:

[23] HOLMES (1985), pp. 41-62.
[24] PRIESTLEY (1775), pp. 121-2.

> says [...] that he dissolved two ounces of mercury in spirit of nitre, and revivified the whole of it; and, in consequence of it, concludes, that pure air, procured from the red precipitate formed by it, was previously *contained in the nitrous acid*, and that no *earth* enters into the composition of it. He also says that he has no doubt but that pure air enters into the composition of *all the acids*, without exception, and that it is the air that constitutes their acidity.[25]

"Being very unwilling to suppose that so able a philosopher as Mr. Lavoisier would advance a fact of so much importance, and draw so general a conclusion from it, without sufficient foundation", Priestley "immediately went to work to repeat the experiment once more". Dissolving 17 dwts (or pennyweights, a contemporary commercial unit of weight) of "purest mercury" in an equal weight of strong spirit of nitre (nitrous acid), he heated the solution in a retort and collected all the mercury that was "revivified" in it. He found that there was "a clear loss of 1 1/2 dwt or, making every possible allowance, 1 1/4 dwt". He believed, that although if carried out under different circumstances, the amount of loss would differ, there would nevertheless, "always be more or less of loss". Consequently, he granted only that "there may be less *earth* and more *nitrous acid* in air than I had supposed".[26]

"Mr. Lavoisier candidly acknowledges", Priestley went on, "that, except this one fact, viz. the complete revivification of the mercury from a solution of it in the spirit of nitre, all the other facts, from which he reasons in this Memoir, were discovered by myself; but that the love of truth obliges him to correct the error into which I am fallen, being of such a nature, that it would be *dangerous* if it should gain credit".[27]

At this point we should pause to comment that Priestley's air of reasonableness should not mask from us the way in which he here manipulated Lavoisier's paper to his own advantage. Although Lavoisier's claim that nitrous acid is composed of pure air and nitrous air might imply that the acid contains no earth, Lavoisier had not argued that the recovery of the same weight of revived mercury that had gone into the operation ruled out the possibility that earth enters the composition of nitrous acid. Rather, he argued that the mercury had not contributed *phlogiston*. Priestley's idea that *air* is composed of earth and nitrous acid Lavoisier claimed to have been ruled out by all of "the experiments contained in this memoir". Priestley thus doubly misrepresented him as having ruled out *earth* as a component of nitrous acid rather than of air, and of having reached this conclusion solely on the basis of the recovery of the mercury without loss. Next Priestley recast Lavoisier's acknowledgment that all his experiments were derived from experiments previously performed by Priestley in such a way as to suggest that, except for the "complete revivification of mercury", Lavoisier had done no relevant experiments at all, but had merely "reasoned" on the basis of facts discovered by Priestley. Such distortions introduced

[25] PRIESTLEY (1777), pp. XXVII-XXVIII.
[26] *Ibid.*, pp. XXVIII-XXIX.
[27] *Ibid.*, p. XXIX.

by Priestley into the debate have contributed, I suspect, to the misleading historical image of Lavoisier as a lesser experimentalist than Priestley, as one who drew his theoretical conclusions mainly from the work of others. Finally, by reducing Lavoisier's experimental contribution to this one factor, Priestley could claim to have repeated Lavoisier's entire "experiment", when in fact he had repeated only one part of a much more comprehensive experiment. These maneuvers now put Priestley in a position to portray himself as the empirical scientist always ready to sacrifice theory to observation, and to cast Lavoisier in the opposite role:

> Upon this I would observe, that all that I pretend to have discovered is, that the purest air is procured in distilling to dryness a mixture of earth and spirit of nitre. This is certainly a *fact* of importance, which no person can dispute [...]. But in the *opinion* that I deduced from this fact, viz. that *air consists of earth and spirit of nitre*, I may be mistaken, and have no reason to be solicitous about it. Let others *reason* better from the facts with which I supply them if they can: I shall listen to them with attention. But I cannot forbear observing, that I should be more obliged to them for the discovery of *more* facts from which to reason. *Speculation* is a cheap commodity.[28]

Having reduced Lavoisier's investigation to a single experimental claim that he believed he had refuted, plus cheap speculation on facts that Priestley had supplied him, Priestley then turned to the subject of phlogiston. "Mr. Lavoisier's pretended discovery obliges him to deny that the *phlogiston* of the mercury, dissolved in the nitrous acid, contributes anything to the nitrous air produced in the solution", because the whole of the mercury being recovered, nothing could have been lost from it. "Now if any opinion in all the modern doctrine concerning air be well founded, it is certainly this, that nitrous air is highly charged with phlogiston and that, from this quality only, it is that it renders pure air noxious. [...] If I have completely ascertained anything at all relating to air, it is this".[29] After refuting another purported claim that Lavoisier had not actually made – that is, that "there is no *proper air* in what I have called nitrous air" – Priestley ended with the admonition:

> I wish that Mr. Lavoisier would reconsider this subject, and repeat his experiments with care (for he mentions only *one* that he made) and be very sure of the reality of a fact, which obliges him to decide contrary to what *seems* at least, to be the best established maxim relating to air, and also consider it in connexion with his opinion that all the metallic calces contain common air.[30]

This criticism appears particularly gratuitous in light of the fact that Priestley himself mentioned only one experiment he had made to refute Lavoisier's experiment. To attack the weak points in an adversary's argument without discussing the rest of it, is not uncommon in scientific controversy. To expect two

[28] *Ibid.*, pp. XXIX-XXX.
[29] *Ibid.*, pp. XXX-XXXI.
[30] *Ibid.*, pp. XXXI-XXXII.

scientists who were operating under different premises – Priestley described Lavoisier as operating "according to his own hypothesis, which is very different from mine" – to come easily to an understanding, is naive. Is this not an early stage in a typical case of scientists operating in different paradigms and, therefore, talking "through each other"?[31] I believe that such factors cannot fully explain Priestley's reaction to Lavoisier's work at a point in their relationship in which Lavoisier had not yet challenged the reigning paradigm on a general front. Following in part John McEvoy's analysis of Priestley's deeper theories of matter, Abbri attributes his negative attitude toward Lavoisier's paper on nitrous acid to the fact that Priestley's "chemical philosophy is completely opposed to that of Lavoisier".[32] Although such underlying differences did form a profound obstacle to an eventual mutual understanding between the two men, Priestley's immediate reaction to Lavoisier's paper on nitrous acid seems characterized more by open animosity than by antithetical underlying viewpoints. Priestley's dismissive attitude is striking when compared with the way in which, in the same preface, he treated other contemporaries who were engaged in experiments on the subject of airs. His friendly references to Volta, the Duc de Chaulnes, Marsilio Landriani, Felice Fontana, and others, with some of whom Priestley was also in disagreement over specific questions,[33] throw into sharp relief the relative hostility with which Priestley received Lavoisier's latest contribution to the field. His frosty reaction also contrasts starkly with the warmth of his praise for the experiment he had observed Lavoisier perform in Paris.

A possible explanation for these shifts was that Priestley's two contrasting experiences in Paris left him deeply ambivalent in his feelings about Lavoisier. On the one hand, he saw him as a gifted experimentalist and gracious host; on the other hand, as a rival who might take unfair advantage of him in any competition between them.

Priestley's response should also be assessed in the light of the domination of the field that Abbri has shown him to have enjoyed at that time. Far from feeling on the defensive, Priestley may simply not have been able to imagine the degree to which Lavoisier's conclusions challenged his own position. In his Preface he indicated that he was ready to pass the torch to the many other persons now so "assiduously employed" on the subject to whose study he had devoted the past 7 years. Confident of "the progress which is now so rapidly" taking place in "this branch of the science", he himself was ready to turn to "speculations of another nature". In the work that he was now ready to conclude, Priestley wrote, "I have been most particularly careful to distinguish *facts* from *hypothesis*". Magnanimously he asserted that it "would give me no pain to relinquish my own opinions, and adopt those of any other person that should appear to me more

[31] PRIESTLEY (1777), p. XXXI; KUHN (1969), p. 109.
[32] ABBRI (1984), p. 214.
[33] PRIESTLEY (1777), pp. IX-XXVI.

naturally to arise from the facts".[34] Like other scientists of his eminence, however, Priestley could not, in practice, maintain the degree of self-denying detachment from his own ideas that he professed. It is clear from the passages quoted above, that he regarded his "opinion" that "nitrous air is highly charged with phlogiston" as so "well founded" that he would find it painful, indeed, to recognize that he might ever have to relinquish it in favor of the opinion of someone else. Similarly, as he prepared to leave to others the further pursuit of what he had begun, he undoubtedly believed that the entire "modern doctrine concerning air", which he had mainly established, was unassailable.

The vacillation in Priestley's attitude toward Lavoisier may, therefore, have reflected his sense of himself as the acknowledged leader in a field in which he welcomed fellow workers only so long as they worked within the framework he had set out. When Lavoisier contributed an ingenious experiment on which Priestley could build further evidence for another new air, Priestley could greet him as an "excellent fellow worker". But when the ingenuity of his young colleague began to extend to revisions of the "modern doctrine", the "celebrated physicist" appeared unwilling to share the leadership that belonged to him alone. By missing the opportunity to treat Lavoisier as his equal in theoretical matters, at a time when Lavoisier was still eagerly seeking his approval, Priestley also lost the chance to avert turning Lavoisier into the formidable rival he later became. Why could contemporaries not perceive Lavoisier's position as a threat to that of Priestley until much later? Abbri's explanation that his revolutionary papers of 1777 were not published until 1780 provides part of the answer. That he did not take steps to get these papers out sooner in *Observations sur la physique*, as he had done earlier in his career, Abbri explains by the prudence Lavoisier felt about first gathering further evidence for what he now knew would be a revolution in chemistry, and his distraction by his other duties.[35] It is remarkable that, after pursuing his course relentlessly from his first investigations on airs in 1772 until the paper in which he proposed, at the end of 1777, a new general theory of combustion, Lavoisier seemed to lose, for the next three years, much of the momentum that had carried him to that point. To Abbri's explanation I would add that Lavoisier was also engaged during those years in a large joint venture with Jean-Baptiste Bucquet, the first convert to his theoretical structure, to re-examine "every part of the science of chemistry" from his new perspective. He may well have thought it best to defer further efforts to win the adherence of the chemical community to his theory of combustion, or to challenge the phlogiston theory and Priestley's positions, until he was ready to make the broader case for his views that he anticipated this work would yield. The project came to an untimely end in 1780 when Bucquet died prematurely.[36]

[34] *Ibid.*, pp. VI-XI.
[35] ABBRI (1984), pp. 199-200.
[36] HOLMES (1985), pp. 129-38, 145-7.

During these years, therefore, far from being under attack, Priestley continued to enjoy nearly undivided recognition as the leading figure in the study of the many "airs" that he had discovered, or that he had further characterized after their discovery by his predecessors from Black to Cavendish. His followers, on the continent as in England, also tended to emulate his style of investigation. As suggested above, this style was more wedded to certain strong "opinions" than Priestley himself was able to acknowledge. In practice, Priestley linked his brilliant experimentation with a theoretical bent that gave relatively free play to his speculative imagination. As Abbri has noted, the "determining influence" on Priestley was his conception of the "phlogistication and *restoring* of the air". His hypotheses concerning the phlogistication of airs were "weak and subject to continued and radical variations". Abbri considers this weakness to be due to Priestley's general conceptions of matter, which did not fit into the framework of a chemistry based on the combination and separation of elements. He thought rather in terms of the transmutation and modification of substances than of their separation and joining.[37]

This is undoubtedly part of the explanation, but I think there were reasons more directly attached to the respective experimental strategies of Priestley and Lavoisier. Lavoisier, too, in the early stages of his venture, sometimes thought of the various airs as derived from one another by modification, rather than by combination and separation of constituent parts. His commitment to what is now called his "balance sheet" method as his central investigative tool, however, had already given him, by 1776, a means to control theory by evidence that was far more powerful than Priestley's relation between "fact" and "opinion". What differentiated Lavoisier from Priestley and all other chemists of that time was not the "principle" of the balance sheet, or the as-yet implicit principle of the conservation of mass, but his growing mastery of the difficult art of applying this principle to experiments which included airs. That the substances that enter into a chemical operation should weigh the same amount as those present afterward, was not a novel insight. Nor did Lavoisier bring some prescient understanding of the primacy of the balance sheet method from somewhere else. As I have described elsewhere, he only came to appreciate its nature and potential as he began to cope, during the spring of 1773, with the problems raised by his initial experiments on combustion and calcination. Most of his experiments during the first few weeks failed, and he went through a painstaking process to learn how to control his method, and what it could do. Once he had done so, he became a more disciplined theorist than most of his contemporaries were, because his ideas about the composition of any given air, or any other substances, had to survive the quantitative experimental tests to which he could subject them.

Consequently, the fundamental differences between Lavoisier and Priestley on questions of composition were, in 1776, rooted less in principle than in practice. Priestley, too, sometimes recognized that theories of composition were to be

[37] ABBRI (1984), pp. 179, 208.

adjudicated in terms of weight relations. That is implicit in his claim that the loss of weight of the mercury observed in his version of Lavoisier's experiment supported his belief that the mercury contributed phlogiston to the composition of nitrous air. Priestley even made "an attempt", reported in volume 3 of his *Experiments*, to "ascertain the quantity of spirit of nitre in a given quantity of dephlogisticated air". The first general method he tried was to measure the quantity of earth and spirit of nitre that he used to generate the air, and the quantity of each kind of air obtained. "The loss of weight in the earth would, I imagined, determine the portion of earth in the air produced". He proposed also a way to measure the loss of spirit of nitre, "concluding that that which was deficient had entered the composition of the air". When this effort failed, because he "despaired of collecting the earth deposited", he decided that if he could "only ascertain the exact quantity of *spirit of nitre* in a given quantity of air, it would suffice for the solution of my problem. For, knowing the weight of the air, and that there was nothing to weigh in the case but the spirit of nitre and the earth, the weight of the one being known, the weight of the other would be determined of course". But after many attempts and consultations with his friends, Priestley gave up, having obtained only data that he admitted was far from satisfactory.[38]

It is not necessary to invoke the presentist judgment that the composition he proposed was wrong, to explain why Priestley did not succeed with this analysis. His problems resemble those that Lavoisier encountered repeatedly during the early months of his quest to measure the weight gains and losses produced by calcination and reduction experiments with metals in the spring of 1773. The problem was Priestley's relative lack of experience in this type of investigation. Because he did not regularly subject his theoretical views on the composition of airs to this kind of test, he could not, in the long run, compete with Lavoisier's growing mastery of this mode of experimentation. There is, therefore, a deep irony in his characterization of Lavoisier's reasoning as mere "speculation" based on facts supplied by others.

3. Priestley actively cultivated followers "in very different parts of Europe". To help ensure that their activity would conform to his own approach to the study of airs, his friend and emissary Jean Hyacinthe de Magellan, "who frequently visits and has extensive correspondence with the continent", took "pains to instruct many ingenious foreigners in the best methods of making experiments of this kind".[39] Nowhere were his contacts more lively, nor his alliances stronger, than with a group of able experimentalists in Italy. Among those with whom he kept in personal touch by correspondence, and who conducted investigations inspired by his discoveries and methods, were Marsilio Landriani, Felice Fontana and Pietro Moscati. All three were friends of Volta. Landriani and Fontana improved the method that Priestley

[38] PRIESTLEY (1777), pp. 41-54.
[39] *Ibid.*, p. VIII.

had devised to test the "goodness" of air, by mixing it with nitrous air in a closed vessel inverted over water and measuring the decrease in the total volume of the two airs. Landriani named this method "eudiometry".[40]

Volta came into contact with Priestley through their shared interest in electricity. In 1776 Volta wrote Priestley informing him of his new instrument for generating electricity, the electrophorus, which seemed to Priestley a "truly wonderful discovery".[41] Already, however, Volta had been introduced to Priestley's views on airs and his methods for studying them, as he joined in the examination of the "goodness" of the air in various localities, using the "very beautiful eudiometer" invented by his friend Landriani. Volta's dependence on Priestley at this time is revealed in his remark to Landriani, in August 1776, that he would have to "reform, amplify and correct, my exposition of airs", in view of the many new discoveries that would appear in the second volume of Priestley's *Experiments*. Because he had not yet been able to obtain that volume, Volta limited himself for then to what he could learn from volume one and from Priestley's "smaller works". Nevertheless, Volta soon established a degree of independence concerning the details of the application of Priestley's views and methods. In the first place, he far preferred Landriani's instrument to the "imperfections of the apparatus of Priestley". Secondly, after gaining some experience with the nitrous air test, he came to the conclusion that it did not measure the general "goodness" or "salubrity" of the air, as Priestley's language implied, but only its *respirability*. "Insalubrity" was caused by many factors not measured by the test, which could detect only the "presence of fixed air and the phlogistication of the air".[42]

His enthusiasm for the study of the various species of newly discovered airs led Volta to examine, in the fall of 1776, a peculiar kind of air, first noticed by his friend Father Carlo Giuseppe Campi, bubbling up from water found at the base of a hillside. Volta quickly found that he could collect this air from the marshes around a lake, from stagnant puddles and other places in which the water covered decaying matter, where it rose either spontaneously or after he stirred up the bottom. When he placed a candle at the mouth of a bottle containing the air, it burned slowly, with a lambent flame. The manner in which it burned persuaded him that it differed from the only previously known inflammable air, that obtained by dissolving metals in acids. Moreover, he soon found that it was "more" inflammable than the ordinary inflammable air, because it would burn when mixed with a much larger proportion of common air than the former could. He gave the new air the neutral name, *inflammable air native to marshes*. Volta explained the differences between the two airs in the same manner that Priestley customarily explained differences between other airs: "I imagine that such divergence [between the two airs] can arise, not so

[40] HEILBRON (1976), p. 74.
[41] PRIESTLEY (1777), p. XIX; ID. (1966), p. 157.
[42] *VO*, VI, pp. 7-13.

much from the dose of the *phlogiston* as from the diverse ways in which the latter can *combine* with these airs, and above all from the nature of the *base* with which it is combined, from the greater or lesser affinity, etc.".[43]

When he was able to get hold of a copy of the third volume of Priestley's *Experiments*, in the fall of 1776, Volta was inspired to further thoughts on the composition of inflammable air. What, he asked himself, was the "difference between *inflammable* air and air that is merely *phlogisticated*"?

Priestley thought that inflammable air consisted chiefly, if not wholly, of the union of an acid vapor with phlogiston. It was the phlogiston which made the air inflammable.[44] Simple phlogisticated air, the residue of air in which combustion had taken place, could support no further combustion or respiration, because it was saturated already with phlogiston. In his "Speculations Concerning the Constituent Principles of the Different Kinds of Air" placed at the end of his first volume of *Experiments*, Priestley had juxtaposed his interpretations of the two airs:

> Acid air and phlogiston constitute an air which either extinguishes flame, or is itself inflammable, according, probably, to the quantity of phlogiston combined in it, or the mode of combination. When it extinguishes flame, it is probably so much charged with the phlogistic matter, as to take no more from a burning candle, which must, therefore, necessarily go out in it. When it is inflammable, it is probably so much charged with phlogiston, that the heat communicated by a burning candle makes it immediately separate itself from the other principle with which it was united, in which separation *heat* is produced, as in other cases of ignition; the action and reaction, which necessarily attends the separation of the constituent principles, exciting probably a vibratory motion in them.[45]

This was as far as Priestley was accustomed to go in the interpretation of the phenomena underlying the diverse roles that phlogiston played in his doctrine of airs. That the presence of the same principle could have such opposite effects posed conundrums for followers like Volta who sought to elaborate on Priestley's "opinions".

For Volta the solution to this puzzle lay in specifying the difference between the "modes of combination" of phlogiston with the two airs. In phlogisticated air, he decided, the phlogiston was only loosely associated with the aerial particles, whereas in inflammable air the phlogiston was tightly combined with them. That explained why when one mixed common air with phlogisticated air the former is immediately vitiated, whereas inflammable air does not vitiate common air unless the two are ignited. The more loosely held phlogiston was easily transferred to the common air, whereas the tightly bound phlogiston of inflammable air could only be transferred by means of a violent decomposition. Drawing on Priestley's studies of nitrous air, which burned less vigorously than inflammable air, Volta inferred that the degree of attachment between the phlogiston and the aerial particles was intermediate between

[43] *Ibid.*, pp. 19-31.
[44] PRIESTLEY (1774), p. 242.
[45] *Ibid.*, pp. 260-1.

that and the other two airs. The analogies between the composition of inflammable air and the well-known combustible substance, sulfur, induced Volta to propose that inflammable air consisted of an aerial acid intimately combined with phlogiston, "from which results our *aerial sulfur*". Fanciful as the details of Volta's explanations were, they were faithful extensions of Priestley's general views on the composition of these airs. "Since you have read by now all of the second volume of Priestley on different sorts of air", Volta wrote his friend Campi in November, "tell me, in the midst of so many decisive experiments on the composition of salubrious air, that is *nitrous acid, or a modification of it* (which I would rather call *aerial acid*) *and earth*, is there any room left to doubt [this theory]"? Volta resolved any doubts he might have, by explaining in terms of these ideas such further phenomena as the lightness of inflammable air relative to common air, and the intermediate weight of phlogisticated air. His hypothesis suggested that inflammable air ought to be generated from phlogisticated air by pressing the phlogiston of the latter into closer combination with the aerial particles. This prediction he verified to his satisfaction by burning a form of phosphorus in a small enclosed space and producing some inflammable air in addition to the usual phlogisticated air.[46]

Turning from what he acknowledged in his next letter to Campi were mere conjectures, to a new experimental venture, Volta tested another conjecture that had occurred to him while he collected his inflammable air native to marshes: that was the idea that the mysterious light sometimes seen flitting over marshy ground, and known as *ignis fatuus*, might be caused by lightning igniting the inflammable air. He exposed the mouth of a bottle containing some of the air to his apparatus for generating electric charge, the electrophorus. A spark discharged from the apparatus set the air in flames. He then found that he could, in the same way, cause ordinary inflammable air to burn in a succession of small explosions. He now began to vary his new method by discharging an electric charge from an ordinary static generator through two conductors that ended in small spheres, varying the location and separation of the spheres inside the jars that held the air.[47]

On December 10 Volta wrote Priestley a letter in which he described his discovery of the inflammable air native to marshes. He claimed also to have "collected many new facts" about the inflammability of airs. Briefly he related that he had been able to ignite the new inflammable air by means of an electric spark ("even when the electricity was very moderate"), with hot charcoal (without flame), and with red hot iron. All these experiments also "succeeded very easily" with ordinary inflammable air. Volta briefly communicated his explanation for the phenomenon of *ignis fatuus*. Part of his letter he published at the same time in the *Giornale dei letterati*.[48]

[46] *VO*, VI, pp. 36-42; quotes, from pp. 41, 42.
[47] *Ibid.*, pp. 49-53.
[48] *Ibid.*, pp. 61-2.

His new discoveries opened for Volta exciting new vistas, ranging from grand speculations about the causes of the aurora borealis and other meteorological phenomena, to the practical possibility of developing an "inflammable air pistol". Having recently read in volume 2 of Priestley's *Experiments*, about the "marvelous kind of air recently found by him, which he calls *dephlogisticated*", among whose properties was that of communicating to inflammable air "the virtue of igniting and exploding with great force", Volta imagined that his new method of igniting the inflammable air with an electric spark could provide the means to harness that force in a formidable weapon. But he was also gripped by a more fundamental question, raised by the remarkable fact that inflammable air could be so easily ignited by a "weak electric spark". All other inflammable substances, including the paradigmatic sulfur, required fire itself, or at least a means to apply large amounts of heat, for a measurable period of time. The electric spark not only contained little or no heat, but was present only for an instant. "Our air", he wrote Campi in January, 1777, "beats all other inflammable substances". The rapidity with which it burned, after a mere momentary contact with the various means of ignition, led him to believe that it "is all inflammability". Volta now proclaimed that:

> One must say that inflammable air is the unique substance endowed with such a virtue; and that all other substances to which we give the name inflammable have that virtue from it, and are resolved into it before they go into flame. There is nothing in this assertion that cannot be proven through the secure support of experiment.

Volta was certain that he was entering a "very vast field" for further investigation.[49]

To claim to have identified the unique inflammable substance required Volta to confront a history of prior identifications of that principle. In his next letter to Campi, Volta gave a series of arguments for his view, attempting to show that in the combustion of solids, liquids and vapors it is always a constituent inflammable air that is burned in the process. Re-examining Hermann Boerhaave's idea that pure alcohol is the inflammable principle, Volta tried to reconcile that claim also with his views by interpreting alcohol itself as composed essentially of inflammable air. He supported with some new experimental observations Boerhaave's belief that the fumes given off in the combustion of other substances contain inflammable air. Claiming it was not necessary to be either "a zealous supporter of such transformations, or a partisan of the new doctrine of airs" to settle such questions by appeal to the facts, Volta nevertheless based his own latest account of the nature of inflammable air on "the same author to whom we are indebted for the rapid and grand progress that this beautiful part of the Natural Sciences, the Chemistry of the Air, has made during these last years".[50] Priestley had found that an electric spark discharged in many different substances produces inflammable air. In addition to the common acids, he had been able "to get

[49] *Ibid.*, pp. 35, 57-65; quotes from p. 65.
[50] *Ibid.*, pp. 69-80.

inflammable air from the *volatile spirit of sal ammoniac*", as well as from what he called alkaline air.[51] Broadening his earlier interpretation, Volta now abandoned the term *aerial sulfur* that he had recently coined for the acid he had believed to combine with phlogiston in inflammable air, and defined the latter instead as a "compound of phlogiston tightly joined with a kind of aerial salt". This salt, he stressed, must "possess already the aeriform state" before it is joined to phlogiston. That such substances existed he thought was supported by the fact that Priestley had got inflammable air from alkaline air. When inflammable air burned, there was a "forceful decomposition of the phlogiston from its base, and a continuous transfer of the former into common air". Unlike the combustion of a solid or liquid, when inflammable air burns, the phlogiston is transferred "*from air to air*". Consequently there was no other phenomenon to observe but the beautiful flame. Volta offered detailed descriptions of the movements of the particles he imagined took part, to explain how, in contrast to liquids and solids that burn only at the surface, a mass of inflammable air can burn simultaneously through its whole mass.[52] Despite these richly imagined visual images of the processes, Volta said nothing about the fate of the aerial salt from which the phlogiston had been transferred. This is one of the typical pitfalls that Priestley's casually stated "opinions" on the composition of the airs which he studied set for his followers. When enthusiasts such as Volta tried to fill in the details left unexplored in the lapidary formulations of their leader, they were easily led into contradictions hidden from them by their allegiance to Priestley's general "doctrine of airs".

A summary of Volta's seven letters to Campi was published in 1777 in Milan in the journal *Scelta di opuscoli interessanti*.[53] Volta spent much of the spring of that year constructing the inflammable air pistol that he had imagined during the previous fall. He contrived an arrangement in which he could produce an electric spark through two wires inserted into a strong glass chamber containing a mixture of inflammable and dephlogisticated air. The force of the explosion drove a lead ball out through a tube. He experimented with various forms of the device, with methods to load the chamber with the airs, and with ways to discharge the mixture from a distance safe enough to protect him from the powerful explosions that resulted. After successful initial trials he had built a glass apparatus that could be carried in a pocket and actually resembled a pistol. With it he could fire shots strong enough to put a dent in a table, hoped eventually to be able to fire a hole through one, and carried out impressive public demonstrations. With visions of eventually revolutionizing the arsenals of warfare, he considered experiments on a larger scale, conscious that he was poised on a delicate balance between success and calamity.[54]

[51] PRIESTLEY (1774), pp. 242-7.
[52] *VO*, VI, pp. 79-84.
[53] *Ibid.*, pp. 105-20.
[54] *Ibid.*, pp. 125-56.

Volta's inflammable air pistol contributed less to the advance of weaponry than to his knowledge of the process he had hoped to exploit for this purpose. Abandoning his original plan to use dephlogisticated air, because of the violence of the result, Volta resorted to mixtures of inflammable and common air. To find the best mixture, he varied widely the proportions of the two airs. When the inflammable air did not exceed one fourth of the total mixture, he found, the volume of the airs diminished by more than that of the inflammable air. When it was greater, the decrease was less than that of the inflammable air. Carrying out the operation in a closed tube, he realized, offered an opportunity to discover the "other principles" contained in inflammable air:

> Since the inflammable air in its burning and total decomposition passes the phlogiston, with which it was joined, to the common air, and both enclosed airs lose their aerial form, it is necessary that [the other principles of the inflammable air are precipitated as a result of the reaction). Why then can we not collect and examine them?

If he dried the tube to exclude drops of water or powder, as he habitually did in the preparation of an explosion of the pistol, he thought he would be able to detect any vapor deposited on the sides in the operation, or any solid or liquid formed. The great difficulty would be that the very small quantities of the airs that he could use in the closed tube could be expected to leave a minute product. In his first efforts Volta "could not collect anything". Suspecting that the other substance was an acid, Volta tested whether a drop of tincture of sunflower placed in the tube would be turned red by the operation. "The effect did not correspond" to his expectation, but did not cause him to give up the idea.[55]

Neither did these results conform to the conjectures about the composition of inflammable air that Volta had recently elaborated in such detail. Carrying out the operation in an enclosed space had quickly taught him that this was not just a transfer of phlogiston "from air to air", but a transfer that diminished the quantity of air present. His response shows that, whatever theories of matter he or Priestley might entertain, in practical situations he, too, assumed implicitly that matter could not be created or destroyed, that what had disappeared as airs must be accounted for as solids or liquids.

On June 6, Priestley wrote Volta to thank him for the receipt of his "elegant treatise on the *Native inflammable air of marshes*". Perusing it had given Priestley "very great satisfaction", and he took the "liberty" of publishing part of Volta's letter to him as an appendix in the third volume of his *Experiments*. Lamenting the difficulty of correspondence between "philosophical persons living at a distance from each other", he informed Volta that, to save expense, he had arranged with Marsilio Landriani to convey letters and small parcels to him. "As you are in so happy a train", Priestley added:

[55] *Ibid.*, pp. 149-50.

> I hope you will persevere in it, and I doubt not that, in so fruitful a field, and with so happy a genius, you will continue to make valuable discoveries. I shall always rejoice to hear of your success. Since I have got your books, I have several times amused myself in verifying your curious observations, and have never failed to collect inflammable air whenever I have sought for it. I often think I should be very happy to accompany you in the excursions you so well describe.[56]

Obviously delighted with this response from the person he so admired, Volta quickly wrote Landriani, on July 15, to find out if he had the third volume of the *Experiments* that Priestley hoped Volta had already received. Proudly Volta related that Priestley had expressed "great astonishment" at the discovery of *inflammable air native to marshes*, had "solemnly praised me, and truly made a grand cause of it".[57]

Accustomed as historians of the chemical revolution are to stressing the skill with which Lavoisier organized a group of followers around him in Paris during the 1780s, we need to balance our perspective on the campaign Lavoisier then led by noting how adroitly Priestley had, with the help also of his tireless advocate Magellan, already built an international network of experimental "philosophers" who looked to him as the founder of the modern doctrine of airs.

Volta repaid Priestley's praise with a loyal defense of one of Priestley's views that was currently contested within the family of his generally supportive allies in Italy. Landriani and Moscati had found in 1776 that dephlogisticated air could be produced not only by the action of nitrous acid on metals, as Priestley had reported in the second volume of his *Experiments*, but also with several other acids. Consequently they questioned his claim that dephlogisticated air is composed of nitrous air and earth.[58] In his letter to Landriani, Volta objected to his claim to have produced dephlogisticated air without nitrous acid. That acid was probably present as an impurity in the sublimate, the minium, and other substances that Landriani had used. Volta had convinced himself by "many attempts" to produce dephlogisticated air with vitriolic, marine, or vegetable acid and with metals free from nitrous acid, that the only airs that could be obtained in this way were fixed and inflammable air. "Nitrous acid is, therefore, truly an essential ingredient of respirable air. The other acids are ingredients of the non-respirable airs. Thus respirable air is, as I maintain with Priestley, nothing but nitrous air combined with earth. In summary, that air is a true aerial niter".[59] Ten days later Volta revised his defense strategy. Conceding that three salts of mineral acids that Landriani used in his experiments to produce dephlogisticated air did not contain nitrous acid, Volta now suggested either that the nitrous acid came from the air, or, resorting to the idea "supported by the more profound chemists, and founded on the grand idea of one primitive acid", he

[56] PRIESTLEY (1966), p. 159.
[57] *VO*, VI, p. 159.
[58] ABBRI (1984), pp. 238-9.
[59] *VO*, VI, p. 160.

suggested that all acids could be converted to nitrous acid. These possible explanations, he now wrote Landriani, suffice to oppose "your experiments with which you believe you destroy the theory of Priestley". Volta's own experiments provided ample evidence that nitrous acid enters directly into the composition of dephlogisticated air.[60]

Meanwhile, Volta's own experiments on the decomposition of inflammable air were taking him in a new direction that also led him back to Priestley. Focusing now on the diminution in the volume of the mixture of inflammable and common air that he had observed when the airs were ignited in a closed tube, he returned to the dephlogisticated air that had originally inspired his idea for an inflammable air pistol. When he ignited two measures of inflammable air mixed with one of dephlogisticated air, the diminution was so large that "of the whole volume of three measures, there remained scarcely half a measure". Nothing, he wrote could be "more astonishing". Such results reminded him of the great decrease in volume that Priestley had attained in his "nitrous air" test when he applied it to dephlogisticated air. Soon Volta convinced himself that everything proceeded in the same way for nitrous air as for inflammable air, "with the sole difference" that the degree of diminution of the heat released, and the vivacity of the process, were greater with the latter.[61]

Volta now set to work to build an apparatus designed to "measure accurately the diminution", resulting from the ignition of a mixture of inflammable air and common air, so that he could find out how that quantity varied when he mixed the two airs in different proportions. The instrument consisted of a glass tube, open at both ends and flared at the bottom, with a scale on which one could determine changes in volume by changes in the level of the water over which the airs would be enclosed. The top he closed tightly with a cork stopper through which were inserted two wires that extended into the interior of the tube. The tube being inverted over water standing in a small basin, the airs were introduced into the tube, then ignited by passing an electric spark between the ends of the wire. The apparatus was, as Volta wrote, extremely simple, but nevertheless "furnishes the means to make a very great number of experiments". During the summer of 1777 he concentrated his efforts particularly on establishing the two extreme proportions between which the airs which could be ignited, and the proportion that caused the most vigorous inflammation. To insure that his results would be "constant and invariable", he carefully considered each of the factors that must be controlled in order to conduct multiple experiments under the same circumstances. These included the strength of the electric spark; the properties of the inflammable air, which depended on the way in which it was generated, and the "goodness of the common air". The same inflammable air, he found, "required, to burn, a greater dose of common air, in the same measure that the latter was less good". This observation suggested to him that

[60] *Ibid.*, pp. 163-4.
[61] *Ibid.*, pp. 159-60.

his method and apparatus could serve also as a "new way to test the respirability of the air". Introducing the inflammable air into the tube first, he then added common air one bubble at a time, until the electric spark was able to ignite the mixture. The "number of bubbles" required became a measure of the "degree of vitiation, or of irrespirability, of diverse airs". In this way Volta came to regard his instrument as a "new kind of eudiometer".[62]

At the beginning of September, Volta finally received his copy of Priestley's long awaited volume three. Immediately he wrote to the author, expressing his gratitude at the "honor" of having part of his previous letter published as an appendix. Less happy for him was another appendix in which Priestley published a letter from John Waltire, an English lecturer in natural philosophy. Waltire described an experiment in which he had burned inflammable air enclosed in a curved phial hung over a tub. The water rose into the open end of the phial and showed that "about as much inflammable air vanishes as is equal to the bulk of the common air". Chagrined to learn that he had not been, as he thought, the first person to burn inflammable air in a closed space and observe the diminution, Volta now had to assure Priestley that he had not been aware of the earlier experiment, and could now claim only that his experiments were more varied and accurate than that of Waltire. He then proceeded to describe his new apparatus in great detail, summarize the experiments he had performed with it, and enumerate for the originator of the nitrous air test for the goodness of air, the advantages over that method possessed by his new mode of eudiometry.[63]

In September Volta made a long journey through Switzerland, where he met many famous men, including the Bernoulli's in Basel and Albrecht von Haller in Bern, and was even granted half an hour with Voltaire. In Geneva he met the naturalist Jean Senebier, with whom he quickly established a strong tie. He showed Senebier how to use his new eudiometer, and in turn took an interest in Senebier's ideas about the nutrition of animals and plants. It gave him some satisfaction to learn that the Swiss naturalists who tried to use Landriani's eudiometer found it quite unsatisfactory. Landriani had told Volta that Volta's apparatus was inconvenient and inexact, and should be rejected. With Senebier he made arrangements to have his own eudiometer constructed for use in Geneva.[64]

Back in Como for the resumption of his teaching responsibilities by the beginning of October, Volta also resumed his experiments on the decomposition of inflammable air. Continuing to refine his apparatus and methods, he spared no effort to make his measurements as exact and reliable as possible. Concentrating on the use of his instrument as a eudiometer to determine the "goodness of respirable air", he tested various airs ranging in goodness from pure dephlogisticated air to common

[62] *Ibid.*, pp. 175-84.
[63] *Ibid.*, pp. 175-7, 182-3; PRIESTLEY (1777), pp. 367, 381-3.
[64] *VO*, VI, pp. 167-9.

air to air saturated with phlogiston. As part of his effort to find the limits of accuracy attainable, he determined the largest amount of each of these airs in which he could ignite a given quantity of inflammable air. To his surprise, the proportion was the same, 14:1, whether he used dephlogisticated, common, or fully phlogisticated air. To explain this unexpected result, he conjectured that it was the extent to which the inflammable air is "diluted" in the other air that sets this limit.[65]

Measuring the diminution caused by each of the bubbles of the air that he introduced into the inflammable air, he found that when the former was dephlogisticated air, the proportion of the total volume of the airs introduced that disappeared was very large. He sought to increase the diminution ever further by adding as many portions as possible of the least amounts of dephlogisticated air that could produce successive small ignitions.[66]

In mid-December, Volta reported to Senebier an experiment in which he had added 13 measures of dephlogisticated air to one measure of inflammable air and attained a weak inflammation with the electric spark. He was able to repeat this process 19 times, at the end of which all 19 measures of the inflammable air had disappeared, and the 13 of dephlogisticated air had been reduced to 6. "This experiment", he wrote, "in which so large a volume of air is destroyed in a closed tube, made me hope to be able to find what it is that precipitated, whether it is an earth, or acid: for that purpose I want to repeat the experiment using mercury in place of water".[67]

When he had attempted unsuccessfully to identify the precipitate in the spring, it was the small quantity of air "destroyed" that he believed had made the task too difficult. The much greater proportions of decrease he could now obtain thus revived his earlier hope.

To develop his case for the close analogy he had seen between the actions of inflammable air and nitrous air on dephlogisticated air, Volta repeated with nitrous air all of the variations in proportions and procedures that he had carried out with inflammable air. The parallels seemed striking to him. In both cases there was a minimum and maximum proportion of one air to the other within which the airs acted on one another. For both cases there was one proportion between the air and dephlogisticated air, "by means of which one could obtain the most vigorous *effervescence*". The differences between the two processes were limited to gradations in heat released, the violence or gentleness of the effects, or the amount of volume lost.[68]

In the closing lines of his letter to Senebier, Volta wrote:

> My experiments have made evident the proposition that inflammable air is not composed solely of phlogiston, and you have touched on some of the proofs. Vegetation does not

[65] *Ibid.*, pp. 187-99.
[66] *Ibid.*
[67] *Ibid.*, pp. 252-3.
[68] *Ibid.*, pp. 199-200.

> separate phlog[iston] from infl[ammable] air. According to Priestley, agitation in water does, but I doubt it, and I have given you the reasons and one of my experiments.[69]

John Heilbron has suggested that "Volta's style of physics altered" around this time, as "his professional opportunities and acquaintances increased".[70] In his willingness here to doubt Priestley's interpretation of a result that was central to his view of the phlogistication and restoration of air, we may here see signs of a similar change in the style of his chemistry. His new contacts in Switzerland, and especially with Senebier, may have begun to liberate Volta from his virtual apprenticeship to Priestley in this field, without reducing his admiration for the Englishman.

In January 1778, Volta wrote another letter to Priestley continuing the communication of his research on inflammable airs. The first and last parts of the letter described in detail the construction and use of the several different designs of his eudiometer that he had developed for different uses, and summarized the results of the experiments he had conducted with them. These parts of his paper included several tables showing the quantities of dephlogisticated air added successively to the quantities remaining of inflammable air, or the inverse, and the residual quantities left after each addition. They graphically display Volta's quantitative style of experimentation and his search for accuracy and for optimal conditions to achieve his experimental goals.[71]

The middle portion was a long argument to support his belief that the close analogy between the actions of inflammable and nitrous air on dephlogisticated air implied that their compositions were also analogous. The argument was, he acknowledged, "theory and conjecture". It was mainly qualitative and framed within the bounds of Priestley's views on the phlogistication of airs. Even in the experimental section he had stopped at one point to remark on how "marvelously the phenomena fit with the theory of the air and of phlogiston".[72] But one can sense a change in tone since his previous letter to Priestley. In describing what he now referred to as his own propositions, Volta seemed more assertive and less deferential to the originator of the theory on which he was enlarging.

"No one can doubt", Volta asserted, "that one of the principal ingredients of both airs is *phlogiston*", because the action of each air on dephlogisticated air saturated the latter with phlogiston. That the other ingredient of nitrous air is nitrous acid is manifestly obvious, because the action of that air on respirable air precipitates the acid. "Analogy now leads us to believe that pure acid enters also into the composition of inflammable air". The immediate objection one might make, Volta acknowledged, was that in the decomposition of nitrous air, the acid appeared. Why then does no acid appear in the decomposition of inflammable air? His explanation

[69] *Ibid.*, p. 253.
[70] HEILBRON (1976), p. 72.
[71] *VO*, VI, pp. 187-215.
[72] *Ibid.*, p. 195.

was that a given volume of inflammable air contains more phlogiston than an equal volume of nitrous air, a view supported by the fact that in his eudiometer experiments one quarter more nitrous air than inflammable air was required to saturate the same measure of dephlogisticated air. To illustrate his contention, he considered what would happen if a quantity of nitrous air =33 contained a quantity of "matter of phlogiston" = 24, and of acid = 9, whereas the corresponding quantities for inflammable air = 33 were phlogiston = 32 (that is 24 x 1 1/4), and acid = 1. It would not be surprising if such a minute quantity of acid went undetected in the experiments so far performed.[73]

In the rest of his argument Volta embellished his view that the acid in inflammable air may also be nitrous acid, delving more deeply into the idea that a universal acid may account for its origin. The similarity of the actions of the two airs led him to argue that their compositions differed only in some "simple *modification*" of the way in which the acid and the phlogiston are combined in them.[74] Rather than to follow Volta further in these arguments, the second of which was merely another elaboration of Priestley's general view that the different properties of various phlogisticated airs can be explained by their different modes of union, I want to focus on the quantitative "thought experiment" outlined above, because here we can see Volta beginning to diverge from Priestley's style of reasoning in a potentially subversive way. The style is balance-sheet reasoning, closer in spirit to the way Lavoisier thought than to the way Priestley thought. But the concepts of composition Volta treated this way were ones that Priestley had originated without recourse to such quantitative reasoning. To subject phlogiston to balance-sheet reasoning implied that its role in chemical operations might also have to be judged according to the scrutiny of the laboratory balance that it had so far eluded.

To make this argument Volta had also to differ with Priestley in a less subtle way. He resorted to it in part "because you, Signore, hold the opposite opinion"[75] concerning the relative quantities of phlogiston in the two airs. In the volume of his *Experiments* which Volta had recently received, Priestley described an experiment in which he introduced inflammable air into an inverted vessel containing strong nitrous acid. When left standing overnight, the quantity of air increased, and when he applied a candle flame to it, it exploded just like a mixture of inflammable air and dephlogisticated air. "It is easily inferred from these experiments", he wrote:

> that the strong yellow spirit of nitre, which contains the most acid with the least phlogiston, supplies the inflammable air with a species of *vapour*, that, by readily uniting with its phlogiston, promotes the ascension of it, and thereby increases the force of its explosion; whereas the weaker and phlogisticated acids seem to impart to it an additional quantity of phlogiston, making it to be, in part, nitrous air. And indeed this experiment

[73] *Ibid.*, pp. 202-3.
[74] *Ibid.*, pp. 203-6.
[75] *Ibid.*, p. 202.

> seems to make it probable that nitrous air contains more phlogiston than inflammable air; as also appears probable *a priori*, considering the much greater affinity of nitrous than of the other acids for phlogiston.[76]

This was typical of the ease with which Priestley could "infer" general principles through semi-quantitative reflections on single experiments, protecting himself against the possibility that later observations might not conform to the reasoning by claiming only that the conclusions "appear probable". That Volta was ready to challenge such "opinions" with stronger inferences drawn from his more rigorously quantitative experiments indicates that he had gained sufficient confidence to write to Priestley as his theoretical equal. But for how long could Volta contain his style of reasoning within a conceptual structure built upon Priestley's more casual attitude toward the "speculations" that he allowed himself while insisting that it was really only the "facts" that counted?

In the section in which he discussed the "prodigious diminutions" that he had been able to obtain in his experiments with inflammable and dephlogisticated air, Volta repeated the plan he had already outlined to Senebier, but with more emphasis on its underlying principle. Considering these results, he did:

> not despair of being some day able to make sensible that which separates from such airs and falls down; because it is clear enough that these airs do not annihilate each other and no material is lost; but only that much of their volume disappears because several parts abandon the aerial form and, clustered together, appear constituted in drops, powder or some other form.

He proposed to carry out the experiment on as large a scale as possible, over mercury, and assured Priestley that he would inform him in another letter if he were able to discover anything.[77] Volta had come a long way from the time, nine months earlier, when he had formulated an explanation of the process as the transfer of phlogiston "from air to air" without thinking about what might become of the other "ingredient" of inflammable air.

4. After sending his second letter to Priestley, Volta continued to refine his experiments on inflammable air. In Geneva, Senebier began using a eudiometer designed for him by Volta, but better constructed than Volta's own, to repeat Volta's experiments. In April 1778, Senebier wrote that his results differed from those of Volta, and asked for some clarifications about the procedures involved. Volta replied that the difficulty probably lay in Senebier's inexperience with the techniques, and the fact that the inflammable air he used probably contained some common air. Even a small amount of the impurity would substantially alter the

[76] PRIESTLEY (1777), pp. 262-3.
[77] *VO*, VI, pp. 196-7.

results by misleading the observer about the proportions in which the inflammable and dephlogisticated airs were mixed. Volta detailed the precautions he took to ensure the purity of the inflammable air. He always applied the nitrous air test, which should show no reduction in volume if no common air is present. By May, Senebier was getting better results. Volta acknowledged that he "believed that there is nothing more difficult than what I have recently encountered in obtaining inflammable air without the least mixture of common air". Those difficulties would, he feared, lead to widespread doubts about the exactitude of his eudiometer, but he saw no alternative than to "reduce the inevitable inexactitude to the smallest possible limit". The purity of the inflammable air was particularly critical to his ongoing effort to obtain the greatest possible reduction in volume in the ignition of inflammable air in common air. By the beginning of May, he had been able to reduce the volume remaining at the end of the experiment to 1/25 of the original total. He still aimed to do better.[78]

As he guided Senebier in the practical use of his eudiometer, Volta began also to communicate his theoretical ideas about airs to his Swiss collaborator rather than to Priestley. As he sought ever more exactitude in the experimental study of the airs, Volta encountered questions about their composition that pressed him to probe ever more deeply into the implications of the general ideas about the ubiquitous role of phlogiston imparted to him from Priestley.

One of these questions arose from some experiments with inflammable and dephlogisticated air in which Volta believed he had reduced the volume of the airs to such an extent that "I have entirely destroyed the dephlogisticated air, and the residue was solely inflammable air". The evidence for his conclusion was that the nitrous air test produced no further diminution. Pondering on this result, he asked himself:

> On the supposition that the dephlogisticated air was destroyed in its entirety, is common air [really] of the same nature, and formed from the same principles, with the sole difference that it is already phlogisticated up to a point? If that is so, why is common air not also destructible in its entirety, why can it only be diminished by 1/5 or at most 1/4? […] But if common air is not of the same nature as deph[logisticated] air, with the sole difference being in its greater or less charge of phlogiston, what is it? Whence comes its respirability, and how could it, in all of its properties of supporting fire and flame, the calcination of metals, respiration and the life of animals, so closely resemble dephlogisticated air which one has deliberately vitiated up to a certain point? It is not so easy to answer all of these questions and I will not undertake to do so here.[79]

It is probably no accident that Volta chose to discuss these questions with Senebier. He may have sensed that if he were to ask the author of the views under question, Priestley would have no answers.

[78] *Ibid.*, pp. 257-66.
[79] *Ibid.*, pp. 262, 264.

For nearly a year, Volta's pre-occupation with the inflammable air pistol and its evolution into a eudiometer had diverted him from the further study of his *inflammable air native to marshes*. Now he turned back to the air he had discovered and subjected it to the same kinds of experiments that he had been pursuing with ordinary inflammable air. The result enabled him to give more precise meaning to his earlier observation that the new inflammable air was "richer in inflammability" than was common inflammable air. Because in a comparable experiment, the former occasioned a diminution of volume four times as great as the latter, he concluded that the "inflammable air of marshes contains at least four times as much phlogiston as the inflammable air from metals". To give further qualitative support to this conclusion, he showed that the inflammable marsh air burned with a longer flame than inflammable air from metals, and that that phenomenon was due to the greater abundance of phlogiston transferred from the decomposing inflammable air to the surrounding dephlogisticated air.[80]

While pursuing quantitative rigor in his experiments, Volta was simultaneously building up his own set of ideas on the origin of airs, in a fashion that echoed the qualitative style of inference characteristic of Priestley's doctrine of airs. The starting point for Volta was his growing attachment to the traditional idea that there is a "single, universal, acid salt, of which all other acids or alkalis are nothing but particular modifications". All acids are properly airs, and owe their liquid states only to their great miscibility with water. This was not a theory of transmutation, because Volta believed that the particular acids formed were combinations of the universal, or aerial acid, with earth and phlogiston, the differences in their properties depending on the different proportions and the tightness or looseness with which these principles were combined. Although Volta attached these ideas wherever he could to his experimental observations, and, although his theoretical reasoning was more sustained, sought greater consistency than Priestley customarily did in his fragmented conjectures, Volta thought about all these problems still within the broad confines of the English philosopher's doctrine of airs. For now, the piercing questions that he had posed in the immediate context of his experimental observation of the complete destruction of dephlogisticated air left no mark on the principles on which he founded his "system of the different species of airs".[81]

Volta sent the two letters on "The inflammation of inflammable air in enclosed vessels" that he had written to Priestley, also to Senebier, who translated them into French. They were published, the first in 1778, the second in 1779, in *Observations sur la physique*, where they attracted a great deal of attention. Abbri has described how the publication of these letters caused the study of inflammable air to become

[80] *Ibid.*, pp. 265-6, 271-6.
[81] *Ibid.*, pp. 280-6.

"a constant element of chemical Europe" during the next two years, and made Volta a central figure in the new chemistry of airs.[82]

Early in 1779, Priestley published a volume of *Experiments and Observations relating to Various Branches of Natural Philosophy; with a Continuation of the Observations on Air*. Despite the change in title, the subtitle more accurately reflected its contents. In the preface he explained that he had not had to leave his "philosophical pursuits", as he had intended to do at the time his previous volume appeared, because the other activity that he had planned to take up "did not happen to engage so much of my time as expected". The new volume was similar in character to its predecessors. Priestley allowed his readers to follow him step by step through the experiments he had recently carried out, which consisted mainly of refinements or additions to the studies of the same phenomena he had previously reported. The greater number of them were ways to generate and examine the various species of airs he had earlier discovered. Phlogiston and its appearances in these phenomena were woven deeply into his experimental narratives. He treated its existence and nature as unproblematic, and included no further general discussions of its properties.[83]

Cheerfully accepting the discovery by Landriani that, in contradiction to his own earlier observations, vitriolic acid contributes to the production of dephlogisticated air, Priestley repeated Landriani's experiments and also repeated with vitriolic acid his own previous experiments on all the metallic and other earths from which he had been able to produce dephlogisticated air with nitrous acid. Nor did he have trouble adjusting his idea of the composition of dephlogisticated air to the new situation. Previously, having moistened red lead with each of the three mineral acids, and found that the experiment with nitrous acid "yielded plenty" of the air, the other acids "none at all", he "had no doubt but that it was the nitrous acid that it had imbibed". The new results had since given him "reason to suspect that hypothesis, plausible as it appears; and at present I am inclined to think that, though, besides *earth*, some acid enters into the composition of air, it is not necessarily the *nitrous* acid, but, in some cases, the *vitriolic*; or at least in the processes by which this air is procured, they are converted into one another, or into some other acid, or substance that bears an equal relation to them both". Some of his "late experiments", he added, "would lead me to conclude, that there is no acid at all in pure air", but those made with mercury and nitrous acid "seem to be decisive in favour of the contrary".[84]

These vacillating views, hedged by phrases such as "inclined to think", or "seem to be decisive", did not embarrass him. Rather than to strive for greater certainty, he made it a virtue to change his mind easily and to keep more than one possibility open. Such shifts only reinforced his image of himself as one who preferred facts to speculation. He used this example to illustrate his assertion that:

[82] ABBRI (1984), pp. 282-9.
[83] PRIESTLEY (1779).
[84] *Ibid.*, pp. 197-203.

> whenever I have drawn general conclusions too soon, I have been very ready to abandon them. [...] I have also repeatedly cautioned my readers [...] that they are to consider new *facts* only as discoveries, and mere *deductions* from these facts, as no kind of authority; but to draw all conclusions, and form all hypotheses, for themselves.[85]

Historians as well as contemporaries have generally been sympathetic to the personal credo Priestley stated so strongly here and elsewhere. He appears open-minded and democratic, committed to a kind of science in which everyone can participate, and no one has particular authority. But, whether he could recognize it himself or not, Priestley was professing principles that he did not, in fact, fully practice. As Abbri has noted, Priestley could so easily accept the correction necessitated by Landriani's discovery, because it "did not pose a theoretical problem for the English chemist". Only a minor adjustment was required.

Priestley was flexible only within the limits of the broad "modern doctrine of airs" he had initiated. The small modification required here did not threaten his general principle that the properties of airs are to be explained by their various degrees and modes of combination with phlogiston, because his theoretical structure was not tight enough to put up resistance to such minor upheavals. But the example of Priestley's reaction to the conclusions that Lavoisier had drawn from the Englishman's "facts" in his memoir on the composition of nitrous acid reminds us how selective he was in admitting others to the circle of natural philosophers whose opinions he could accept. Nor did Priestley concede anything essential to Lavoisier in the new volume. Returning to the same subject, he again mentioned that Lavoisier denied the "presence of earth in dephlogisticated air". He went so far as to repeat the experiment in which he claimed that mercury is lost in the process, but obtained, as before, a "deficiency in the weight of mercury after the experiment". Therefore, "I still conclude, that there is some earth in the air; but I do not say whether this earth be essential in its constitution, though I suspect it to be so, or only *dissolved* in it, and foreign to it, like water in air".[86] In a section reviewing a new edition of Macquer's *Dictionnaire* which included an article on "gas" (which Macquer introduced to replace the current extension of the term "air" to the newly discovered "elastic fluids"), Priestley mentioned Lavoisier twice, in each case concerning an assertion that Macquer also accepted. To Lavoisier's supposition that "phlogiston, combined with common air, converts it to fixed air", he retorted that this was not "*my* opinion, or one that is agreeable to fact". The second assertion was "that metallic calces with the addition of combustible substances, yield fixed air, a mistake on which I have animadverted before".[87]

For Volta, Priestley had warmer words. In a section entitled "Whether inflammable air or nitrous air contain more phlogiston", he wrote that "many schemes have

[85] *Ibid.*, p. XI.
[86] *Ibid.*, pp. 260-3.
[87] *Ibid.*, pp. 446-8.

occurred to me to ascertain the proportions of phlogiston that each of them contain". He described, however, only one of them, suggested by the experiment of Waltire that he had included in an appendix to his previous volume. Burning inflammable air under a receiver standing in water, he examined the air afterward with his nitrous air test and found that the decrease in volume was about the same as when he mixed the same amount of nitrous air and common air in the vessel. "Consequently, equal measures of nitrous and inflammable air contain about equal quantities of phlogiston". Since then, however, he continued, "I have obtained a more accurate solution from the mode of experimenting introduced by that excellent philosopher Mr. Volta; who fires inflammable air in common air, by the electric spark, and consequently can determine the exact proportion of the inflammable air decomposed in a given quantity of common air". Volta's result, Priestley claimed, agreed with his own. Describing an experiment similar to that of Volta that he himself had then performed, Priestley reported that this result, too, showed the inflammable air had been phlogisticated to "exactly the state" that nitrous air was.[88]

As we can see from the contents of Volta's second letter to Priestley discussed above, this story of perfect harmony between Priestley and one of his loyal followers in Italy is, unfortunately, not an accurate representation of what had taken place. In his previous volume, Priestley had reported, not that nitrous and inflammable air contain about the same amount of phlogiston, but that the latter contains less than the former does. Volta had concluded, on the other hand, that inflammable air contains more phlogiston than does nitrous air, and had given Priestley arguments intended to dissuade him from his "contrary opinion".

I have focused on what may appear mainly negative aspects of Priestley's views, not to diminish his stature, but to expose weaknesses in his position that may help to explain the subsequent course of events. It is important to remember Abbri's conclusion that Priestley was, at this point, not on the defensive. Although Lavoisier had already presented in the Academy of Sciences the paper on the general theory of combustion that provided the first general alternative to the phlogiston theory, that paper was not yet published, probably little known outside of Paris, and Priestley was either not yet aware of it, or did not yet grasp its import. He was at the peak of his influence as the founder of the modern doctrine of airs, not as the defender of a conservative Stahlian orthodoxy. He was the much admired leader of an active network of experimentalists in England and on the continent, who sought to advance, under his guidance, the field they believed he had created.[89]

What kind of guidance then did he offer? Through the warmth of the personal connections he maintained, he won their esteem and their friendship. Through his openness to differences of opinion within the broad "doctrine" to which they collectively adhered, he allowed them room for independence of thought and of

[88] *Ibid.*, pp. 378-82.
[89] For a different view of Priestley's democratic vision of science, see GOLINSKI (1992), pp. 145-8.

direction. But his preference for spontaneity over discipline, for facts over speculation, covered what amounted to an unwillingness to confront either the gaps within his own doctrine or the challenges provided by someone like Lavoisier whose viewpoint differed more sharply from his own. Having advanced rapidly into an open field by the virtuosity with which he performed simple experiments and by his uncanny ability to turn accidental observations into new opportunities, Priestley gave new life to a venerable principle of inflammability. Phlogiston explanations of the many new phenomena he brought to light proliferated in his fertile mind. But it was time for less ingenuity and more control. Unless probable interpretations of individual phenomena and of particular experiments could be tied together into a coherent whole; unless the cursory discussions of the general principles underlying his interpretations of the composition of the airs he studied so assiduously could be deepened by more sustained reasoning, his doctrine would remain a ramshackle structure. It could dominate the new field only so long as no formidable competing structure should appear. Instead of leading his network of followers into a new stage in the development of his doctrine, Priestley seemed to retreat to the discovery of the new "facts" that he continued to gather in profusion.

Even the new facts were, however, becoming less relevant, because Priestley continued to rely on the same simple qualitative and semi-quantitative methods that had served him so well, at a time when the further advance of the field called instead for more precise experiments, with instruments and apparatus capable of greater quantitative accuracy. It was not only Lavoisier who was at the time building such instruments and performing such experiments. While Lavoisier was quietly preparing the challenge to come, a member of Priestley's own camp, Alessandro Volta, was keeping pace with him in the design of better instruments and in the pursuit of quantitative adequacy.

In 1779 it was not inevitable that Priestley's leadership in pneumatic chemistry be supplanted by that of Lavoisier. Perhaps the directions in which Priestley himself seemed unprepared to move could be taken by someone within his own network of supporters. One of his followers in Italy was both thinking more deeply about the theoretical structure that Priestley had inspired and pursuing more effectively the experimental pathways that Priestley had opened for him. As the use of his elegant eudiometric methods spread through Europe, the man who appears to have been best prepared for such a role was Alessandro Volta.

5. Volta not only pondered over the origin and composition of airs, but engaged in theoretical discussions with other members of the network of Priestley's followers. With Senebier, in particular, he debated whether respirable air contains an earth, as he believed with Priestley, or whether, as Senebier and Fontana held, it was "nothing but an aeriform acid vapor".[90] There is not space here to enter into the arguments involved, but further study of their exchanges would be rewarding. Without questioning the reality of the general view of the phlogistication of airs prescribed by the founder of their field, they nevertheless pressed persistently and thoughtfully to refine the repertoire of principles of composition and change they had inherited, in the aim of constructing a more coherent "doctrine", more consistent with their growing body of experimental observations. The picture they were building nevertheless remained qualitative, speculative, and beyond direct confirmation or refutation, at a different level of analysis from that at which they sought experimentally for ever greater quantitative precision and reliability.

Volta continued to devise new modifications of his eudiometric methods. One ingenious adaptation of the instrument he designed specifically to enable him to "discover if in a volume of respirable air there is the least quantity of inflammable air".[91] Another design, which he described to Senebier in October, 1778, enabled him to introduce very small quantities of respirable air successively into a quantity of inflammable air without dismantling the apparatus. By simply manipulating three stopcocks repeatedly, he could rapidly produce "20 [to] 24 inflammations or still more, and to reduce that many measures of air to almost nothing". That his ultimate aim was still to discover the products of the inflammation is suggested by the comment he added: "and can one not, by this means, receive the matter of these decomposed airs"?[92]

His progress was, however, interrupted in 1779 by his fresh appointment as Professor at the University of Pavia. To Senebier he explained in August that the main reason for "the intermission in my experiments on airs", was that although there was a "fine cabinet of machines there, I lack all the instruments required for this new branch of physics". He had received some apparatus from London, but he needed also a large quantity of mercury, which he had not been able to procure. "As you well know", he added:

> many experiments must be made with the mercury apparatus: particularly for inflammable air, whose constituent parts I am searching to discover, it is necessary above all, in order to make sensible and to collect that which precipitates when this air is inflamed, to carry out the process over mercury, not over water. That is what I have not been able to do so far, and what has prevented me from going further in my researches.[93]

Volta was apparently never able to carry out the experiments he here envisioned. In any case, he did not discover what he sought. In the notes he prepared in 1783 for the Italian translation of Macquer's *Dictionnaire*, Volta wrote, following a description of his experiments on the inflammation of inflammable air:

[90] *VO*, VI, pp. 287-90.
[91] *Ibid.*, pp. 293-7.
[92] *Ibid.*, pp. 298-300.
[93] *Ibid.*, p. 303.

> Let us pause for a moment over these results. What becomes of the air which disappears in the inflammation? Is some acid or earth precipitated? Not at all. And yet, from the inflammable air, whose decomposition loads its phlogiston onto the respirable air, some other principle constituting the base of this phlogiston must remain, and be rendered sensible. The portion of respirable air which receives this phlogiston must, however, as in other such mutations, be changed into fixed air. But why does it not suffer this mutation and is instead destroyed, or at least vanishes, without our knowing where it goes and what becomes of it? Does it form heat, as Scheele says? But I do not understand how the air, which cannot pass through glass, can, by supercomposition with phlogiston [combining with phlogiston in excess], become so subtle as to penetrate this and all the other solid substances through which heat can pass; besides many other reasons militate against this theory. Perhaps it loses the aeriform state and is changed into vapor, into that light fume that I have observed to remain after the inflammation? And of which of the two airs is that fume the residue, the inflammable, or rather the dephlogisticated? These are questions which neither Mr. Volta, nor anyone else, have yet known how to resolve.[94]

Besides revealing his continued puzzlement over a question that had now preoccupied him for nearly 5 years, this passage reflects several significant revisions that Volta made between 1778 and 1783 in his view of the composition of airs. In another part of the note Volta succinctly stated the view that he had now reached. When inflammable air has been burned in dephlogisticated air in appropriate proportions, the latter is almost completely destroyed. But in common air only, at most 1/5 of the latter disappears, the other 4/5 forming a residue, which "has entirely or nearly lost its respirability, which species of air, which Priestley designated *phlogisticated air*, and which we think, with Scheele, Bergman, Lavoisier, is nothing else than the air of the atmosphere deprived of its portion of dephlogisticated air, the sole and unique respirable fluid".[95]

The view that Volta supported here resolved the deep contradictions that his experimental destruction of dephlogisticated air had raised for him in 1778 concerning the relation between dephlogisticated and common air. It represents also a major move away from the position Priestley had maintained, and toward the conceptual structure that Lavoisier had already reached in 1778. There is no sense, however, here or in any other of Volta's writings up until this time, that he was switching sides on a controversial question, or even that a divide existed that one might have to cross or not to cross. The lack of any sign of conflict in Volta's mind when he here chose the view of Bergman, Scheele and Lavoisier over that of Priestley fits Abbri's interpretation that there was no significant public debate between Priestley and Lavoisier until after 1781.

In all his writings, published and private, speculative and experimental on the subject of airs from 1776 until after 1780, Volta rarely mentioned the name of Lavoisier. His constant point of reference, even when he differed with him on

[94] *Ibid.*, p. 397.
[95] *Ibid.*, p. 394.

specific questions, was Joseph Priestley. An outline for a course on the "Differenti specie d'arie" that Volta taught at Pavia in 1783 began:

> Dr. Priestley has nearly created a new science of all the factitious airs. He distinguishes *fixed air* properly so-called, *nitrous air*, *inflammable air*, *phlogisticated air*, *dephlogisticated air*, *acid airs* and *alkaline air*. Mr. Scheele and Mr. Bergman have added *hepatic air*.[96]

Working within the general framework of the "new doctrine of airs" founded by Priestley, Volta was nevertheless moving, by 1783, in directions that tended toward convergence with the views of Lavoisier. In contrast to Priestley's earlier views and in common with Lavoisier, Volta now held that the atmosphere was composed of a respirable and a non-respirable portion. The respirable portion could be converted by combustion into fixed air, a mutation in which the non-respirable portion played no part. Had Volta been able to move further along this trajectory undisturbed by factors extrinsic to his experimental pathway and his ongoing thought about the nature of airs, his views and methods and those of Lavoisier might have evolved toward a synthesis to which both would have contributed significant elements. But both men were embedded in social networks that could be merged less easily than could their science.

Volta's orientation was fixed not only by the initial direction that his thought and experimentation on airs had derived from Priestley, but also by the ties of communication and loyalty that held him and his close friends in Italy within the orbit of the eminent Englishman. Through these years he had little or no contact with Lavoisier's circle in France. That situation changed when Volta came to Paris, in the spring of 1782, on the first stage of an extended scientific tour. When he arrived in April, he found Lavoisier and Laplace attempting to show that when liquids and solids pass into the vapor state or return from it, they "give signs of negative or positive electricity". Volta had brought with him a new condenser capable of detecting much weaker electrical charges than had hitherto been possible. After he had shown them some experiments with it, they entertained hopes that they might succeed with the evaporation experiments using it. After Lavoisier ordered the construction of a larger condenser with a marble plane, the three men tried the experiment, but without success. Lavoisier and Laplace took the apparatus to "the country", where they were "attended with success". That result incited them, back in Paris, to carry out, together with Volta, similar experiments on the combustion of charcoal and the solution of metals in vitriolic acid. In each case Volta's condenser became charged, sometimes sufficiently to discharge an electric spark. Lavoisier closed his report of these experiments to the Academy with the remark that Volta had been present for the last of them, and that "the presence and witness of this excellent physicist can only inspire confidence in our results".[97]

[96] *Ibid.*, p. 333.
[97] VOLTA (1782), pp. XXIX-XXX; LAVOISIER (1781a).

During the time that Volta was with them, Lavoisier and Laplace were also engaged in their historic experiments with the ice calorimeter,[98] an event in which their Italian visitor took great interest. Volta thus remained long enough and participated actively enough with Lavoisier and Laplace to establish personal bonds of mutual esteem. Volta was, however, not ready to switch his allegiances. Travelling on to London he collaborated equally well with his British colleagues, including Richard Kirwan,[99] who had just elaborated a new version of the phlogiston theory.[100]

A few weeks after Volta left Paris, Lavoisier and Laplace passed measured quantities of inflammable air and oxygen into a closed chamber, ignited them with an electric spark similar to those that Volta had been using for over 5 years for the same purpose, and collected pure water from the bottom and sides of the vessel. When Volta received the news back in Italy later in the year, he was devastated.

The reasons for Volta's dismay are apparent to anyone who has learned of a great discovery which he or she might, but for some unforeseen circumstances, have discovered sooner. Over and over in his correspondence and lectures afterward, Volta searched for ways to claim some part in the discovery without denying credit to Lavoisier and Laplace for their momentous achievement. In a letter written in October 1783, he described to his correspondent what he had heard about Lavoisier's experiment, then added:

> Concerning the synthesis, I understand how he has obtained this water by igniting the mixture of these airs. I understand, I say, because I have come very close, having discovered that when the inflammable air of metals disappears by inflammation. it does not convert any portion of the dephlogisticated air into fixed air, as all other inflammable airs and other phlogisticating processes do. But it brings about the destruction of a volume of dephlogisticated air equal to one half its own volume; which destruction is accompanied by the appearance of a nebulous and humid fume or vapor. Read the note that I have made on the article on inflammable air in the Dictionary of Macquer, and you will see that I speak often about this vapor, into which a mixture of inflammable air and dephlogisticated or common air is resolved. And although I doubted that the vapor was purely aqueous, because it resisted condensing into drops, I nevertheless excluded acid or salt of any sort.

He had proposed, ever since 1777, to examine the inflammation of large quantities of inflammable air and respirable air over mercury:

> with the object of recovering what precipitated. The misfortune is that I never had enough mercury to be able to do it. If I had it, I do not doubt that I would soon have found what Mr. Lavoisier has now found, I imagine, using an apparatus similar to the

[98] HOLMES (1985), pp. 162-83.
[99] VOLTA (1782), p. XXXI.
[100] ABBRI (1984), p. 292.

> one that I demonstrated before him and all of the Academy to burn airs in a closed space, an apparatus that was not yet known in France.[101]

Close as he believed he had come to Lavoisier's discovery, Volta was not ready to accept the theoretical consequences that Lavoisier drew. "I think", he wrote, not "that the water is a compound of the inflammable air and the dephlogisticated air, but that the water is a simple element, or at least simpler than the two airs; that it is the water that is contained in these, not these that are contained in the water".[102] Volta remained open, however, to further proof that Lavoisier might bring in favor of his interpretation.

Here we can see the Italian physicist caught in a web of unresolved tensions, involving priority and fairness, personal loyalties and national alliances, but also theoretical orientations and experimental criteria. His proposed interpretation would make the discovery consistent with other phlogistic processes, but flew in the face of the information he had already received that Lavoisier had collected a weight of water equal to that of the two airs lost. Here Volta's strong commitment to reasoning in the phlogiston tradition came into conflict with his more pragmatic experimental reasoning. We have seen that Volta himself had thought that no matter could be lost in the destruction of the airs, so that what disappeared must be recoverable in some form of precipitate.

It must be left for another occasion to trace the slow, perhaps tortuous process through which Volta eventually resolved these contradictory pulls on him in favor of the "new chemistry". I will end instead by posing a tantalizing counter-factual possibility. If Volta had been able to procure his mercury, if he had been able to carry out the large-scale experiments at which he had been aiming for so many years, if he had come to Paris in the spring of 1782 with the knowledge that that elusive fume in his apparatus after the inflammation was *water*, how might the events so well known after that time have been different?

In the long-run, of course, the outcome of the chemical revolution would not have been changed. The discovery of the synthesis of water, whether made by Lavoisier and Laplace, by Volta, by Cavendish, by Monge, or by someone else, would still have provided Lavoisier with the clue he needed to complete his theoretical structure. But the alliances and the dynamics through which the subsequent events were played out might have been quite different. The question that divided chemists "relative to the existence of phlogiston" had not yet, in 1783, become a contest between the party of the "phlogistonists" and the "antiphlogistic" camp of Lavoisier's followers in France. Had Lavoisier and Laplace learned from Volta of the appearance of water in the inflammation of inflammable air when he arrived in Paris, they would undoubtedly have conducted together with him experiments to verify and quantify his discovery,

[101] *VO*, VI, pp. 410-1.
[102] *Ibid.*, p. 411.

rather than those experiments through which they believed themselves to have found a new electrical effect. In that case, Alessandro Volta, the deeply loyal follower of Joseph Priestley, would have been joined with the emerging leader of the "new chemistry" in a great discovery. As he went on from Paris to London, he would have been placed in a perfect position to mediate, almost before it began, the fiercely competitive struggle that separated the chemists of Europe: and that has made it appear ever since that there was then no place to stand between two incommensurable ways of practising chemistry.

BIBLIOGRAPHY

ABBRI, FERDINANDO (1984), *Le terre, l'acqua, le arie: La rivoluzione chimica del Settecento*, Bologna: Il Mulino, 1984.

DONOVAN, ARTHUR (1993), *Antoine Lavoisier: Science, Administration, and Revolution*, Oxford: Blackwell, 1993.

GOLINSKI, JAN (1992), *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760-1820*, Cambridge: Cambridge University Press, 1992.

HEILBRON, JOHN L. (1976), "Volta, Alessandro Giuseppe Antonio Anastasio", in GILLISPIE, CHARLES COULSTON ed., *Dictionary of Scientific Biography*, New York: Charles Scribner's, 1976, XIV, pp. 69-82.

HOLMES, FREDERIC L. (1985), *Lavoisier and the Chemistry of Life: An Exploration of Scientific Creativity*, Madison: University of Wisconsin Press, 1985.

ID. (1998), *Antoine Lavoisier: The Next Crucial Year, or the Sources of his Quantitative Method in Chemistry*, Princeton: Princeton University Press, 1998.

KUHN, THOMAS S. (1969), *The Structure of Scientific Revolutions*, 2nd ed., Chicago: University of Chicago Press, 1969.

LAVOISIER, ANTOINE-LAURENT (1775), "Mémoire sur la nature du principe qui se combine avec les métaux pendant leur calcination, et qui en augmente le poids", *Observations sur la physique*, 5 (1775).

ID. (1776), "Mémoire sur l'existence de l'air dans l'acide nitreux", in *Recueil de mémoires et d'observations sur la formation et la fabrication du salpêtre*, Paris: Lacombe, 1776, pp. 601-17.

ID. (1776a) [reprint of LAVOISIER (1776)], "Mémoire sur l'existence de l'air dans l'acide nitreux", in LAVOISIER (1864-93), II, pp. 129-38.

ID. (1781), "L'électricité qu'absorbent les corps qui se réduisent en vapeurs", in *Mémoires de l'Académie des sciences*, (1781), p. 292-4.

ID. (1781a) [reprint of LAVOISIER (1781)], in LAVOISIER (1864-93), II, pp. 374-6.

ID. (1864-93), *Oeuvres de Lavoisier ...*, Paris: [ed. divers.], 1864-93, 5 vols.

ID. (1997), *Oeuvres de Lavoisier. Correspondance 1789-1791*, BRET, PATRICE ed., Paris: Académie des Sciences, 1997, vol. VI.

PERRIN, CARLETON E. (1969), "Prelude to Lavoisier's Theory of Calcination and some Observations on *Mercurius Calcinatus per se*", *Ambix*, 16 (1969), pp. 140-51.

ID. (1981), "The Triumph of the Antiphlogistians", in WOOLF, HARRY ed., *The Analytic Spirit: Essays in the History of Science in Honor of Henry Guerlac*, Ithaca: Cornell University Press, 1981, pp. 40-63.

PRIESTLEY, JOSEPH (1774), *Experiments and Observations on Different Kinds of Air*, London: J. Johnson, 1774, vol. I.

ID., *Experiments and Observations on Different Kinds of Air*, London: J. Johnson, 1775, vol. II.

ID. (1777), *Experiments and Observations on Different Kinds of Air*, London: J. Johnson, 1777, vol. III.

ID. (1779), *Experiments and Observations Relating to Various Branches of Natural Philosophy; with a Continuation of the Observations on Air*, London: J. Johnson, 1779.

ID. (1966), *A Scientific Autobiography of Joseph Priestley (1733-1804)*, SCHOFIELD, ROBERT E. ed., Cambridge, Mass.: M.I.T. Press, 1966.

VOLTA, ALESSANDRO (1782), "Del modo di render sensibilissima la più debole elettricità sia naturale, sia artificiale", *Philosophical Transactions*, 72 (1782), pp. 237-80; [English translation], *ibid.*, Appendix, pp. XII-XXXIII.

ID. (1782a) [reprint of VOLTA (1782)], "Del modo di render sensibile la più debole elettricità sia naturale, sia artificiale", in *VO* (see Abbreviations), III, pp. 271-300.

ID. (1798), Letter to Martinus van Marum, Milano, 26 November 1798, in *VO*, VII, pp. 269-72.