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What Was the Chemical Revolution About?

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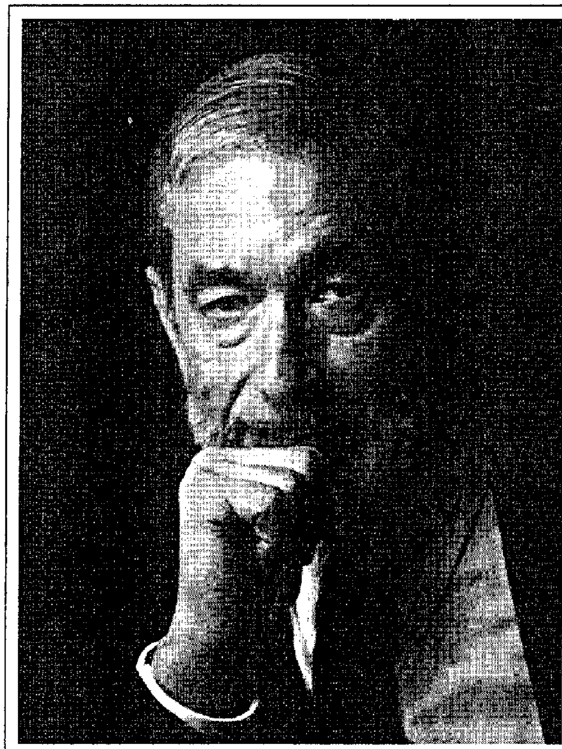
Two centuries after the death of Antoine Lavoisier, we might expect historians to be able to agree on the nature and the boundaries of the great revolution in chemistry for which he is celebrated. In 1988, however, the editor of a volume entitled "The Chemical Revolution: Essays in Reinterpretation" wrote that "there is at present a striking lack of consensus as to what happened in the Chemical Revolution." Arthur Donovan argued there that intense scholarship during the last four decades had shown the "received" view of the chemical revolution as the overthrow of the phlogiston theory to be "inadequate and misleading," but had not yet supplied a "commanding replacement" for it (1). Chemists who have never doubted that the chemical revolution was about the replacement of the phlogiston theory by Lavoisier's oxygen theory, may well wonder if Donovan's statement is an admission that historians are unable to settle anything. Are they victims of their own revisionist tendencies, perpetually reopening questions that ought to have been resolved long ago, if they only possessed the rigorous methods that scientists do? Or does the collapse of a formerly accepted story represent progress in the historical understanding of a complex set of events played out a little more than two centuries ago?



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Some of the proposals of the last three decades for redefining the chemical revolution offer new perspectives on the event and on its relations to the chemistry that preceded or followed it. Some of the changes in outlook about this particular revolution have been forced by broader recent debates about the existence of revolutions in science in general, especially in the aftermath of Thomas Kuhn's enormously influential *Structure of Scientific Revolutions*. Some of the disagreements that still engage historians, however, are continuations of debates that have existed from the time of the chemical revolution itself. They involve perennial issues about continuity and discontinuity that surround, in one form or another, the interpretation of any radical change in science, or in society at large. In the case of the chemical revolution, this debate often surfaces as one or another variation on the question, was chemistry already a science before Lavoisier's "reforms," or did it become a science only through his revolution?

Ambiguity over this question began with the leader of the revolution himself. Sometimes Lavoisier characterized earlier chemistry as so fraught with error that it would be preferable to begin anew; but on other occasions he viewed himself as continuing in directions followed by his predecessors, only carrying their analyses a step further. During the nineteenth century the question was heavily influenced by the attitudes of chemists toward the heritage that they felt they had received from Lavoisier. Because his chemical system had been perceived, by both supporters and opponents, as a "French" chemistry, nationalistic rivalries played a strong part also in these judgments (2).

The most famous of these opinions was expressed in 1869 by the French chemist Adolph Wurtz, in the introduction to his *History of Chemical Doctrines since Lavoisier* (3):

> Chemistry is a French science: it was constituted by Lavoisier, of immortal memory. For centuries chemistry was only a repository of obscure, often misleading recipes used by alchemists, and later by iatrochemists. A great mind, Georg Ernst Stahl, had tried in vain, at the beginning of the 18th century, to give it a scientific foundation. His system could not withstand the test of the facts and the powerful criticism of Lavoisier. The work of Lavoisier is complex: he was both the author of a new theory, and the creator of the true method in chemistry.

In his chapter on Lavoisier, Wurtz described knowledgeably both the phlogiston theory and the main features of Lavoisier's "new doctrine (4)". For contemporaries, however, these details were overpowered by his emotionally charged initial declaration that Lavoisier had single-handedly constructed the science of chemistry, and that its origin was, therefore, French. The corollary, implied in the declaration itself, but also made explicit in the rest of the that I have included in the preceding quotation, was that chemistry before Lavoisier was a rudimentary, pre-scientific affair; that the only previous attempt to make it scientific, Stahl's phlogiston theory, had been a failure.

To some German chemists, Wurtz's claim appeared outrageous. Several of them wrote historical rebuttals, emphasizing continuities between the doctrines of Stahl and later chemistry, and diminishing the contributions of Lavoisier (5). In the midst of these polemics the great German historian of chemistry, Hermann Kopp, published a one volume history of *The Development of Chemistry in Modern Times*, in which he assessed judiciously, and with remarkable freedom from national bias, the question (6):

> How far had chemistry advanced, up to ... the time at which Lavoisier acted so powerfully on its further development and led it onto the track pursued continually since then? Had chemistry at that time already secured the claim to be regarded as a science, or did it, according to what it strove for and had achieved, not yet merit that designation? Did what a later time brought forth as the so-called modern chemistry come from the further development of what was already known and scientifically integrated, or does chemistry as a science really date from Lavoisier? Very different answers have been given to this question.

In treating this question himself, Kopp noted first that by the end of what he called the era of the "domination of the phlogiston theory," all chemists were in agreement that chemistry was "the doctrine of the composition of bodies: how they are composed, and how they become composed (7)". This criterion was especially important for Kopp, because, as the quoted statement suggests, he defined a science in general according to its goals, not merely its completed achievements (8). The knowledge already acquired by the end of that era was, however, impressive enough. After enumerating the metals, earths, salts, and acids newly identified during the last decades of this era, Kopp commented on "how rich in discoveries of particular substances the era preceding the overthrow of the phlogiston theory was." These discoveries relied on a general understanding that "certain substances are contained in other composed substances as constituents, in which they continue to exist. Such constituents were not merely hypothetical elements, but actually producible [*darstellbare*] substances." Most of these pre-Lavoisier views of composition were afterward translated into the language and viewpoint of the chemistry of Lavoisier (9).

This "wealth of knowledge" was, Kopp conceded, "for the most part empirical knowledge," but the era did not lack "more general perspectives" capable of leading to "more comprehensive, important views." As one approaches the most basic levels of chemical thought of the time, however, one encounters in greater degree, opinions that were "erroneous." In particular, "with respect to the elementary composition of bodies, and most especially their ultimate constituents, erroneous representations dominated." He reviewed the four element theory, a commonplace of chemical textbooks near the end of the phlogiston era. This theory represented a revival of the Aristotelian elements of 2000 years earlier. Unlike some more recent historians, Kopp emphasized that under the names of these elements 18th century chemists thought of very different entities than the Greek philosophers had. "The meaning of these names was adapted, above all else, to what later chemical investigations appeared to reveal (10)".

Why, in spite of the wealth of knowledge about other aspects of composition that he credited to pre-Lavoisier chemistry, did Kopp characterize the era by precisely that theoretical system which failed to survive the chemical revolution? One answer that he himself gave was that, "For a long time chemistry had viewed its chief task as to understand and explain the action of fire on various bodies (11)". Kopp understood thoroughly both the strengths and limitations of the phlogiston theory. On the one hand, "In the recognition of which substances are intermediate between others, and in which order, chemistry was considerably advanced under the influence of the phlogiston theory (12)". Much of the knowledge organized in this way "was directly translated into the new system." By the end of the era, however, efforts to hold to the theory in the face of changing circumstances had already led, before Lavoisier intervened, to "considerable divergence, and rapid changes of views. ... The recognition of one error maintained until then led one only to tumble into a new error."

Despite these negative notes, Kopp had no doubt that the answer to his question, "whether chemistry, already conceived then as a science, was pursued scientifically," was "Yes." Much of the knowledge acquired before Lavoisier provided foundations for the system that Lavoisier formulated. Chemistry as a science did not date from Lavoisier, even if he had proven that the previous answers to "fundamental questions" had been wrong, and even if he had created a new method of investigation. The problems of chemistry remained the same, but the methods to solve these problems were "perfected by Lavoisier", and a new doctrine of the composition of bodies and their most important processes introduced. "A transformation of views was caused by Lavoisier within an existing science (13)".

Hermann Kopp had studied 18th century chemistry more thoroughly than any historian before him had, and knew more about the subject than most historians since him. He was able to conclude that pre-Lavoisian chemistry was a genuine science, even though he distinguished "correct" knowledge from "error" in typical 19th century fashion. Current historians of science believe that past scientific ideas and knowledge should be judged according to the standards of their own time, not that of later eras. It is ironic that they seem, nevertheless, to have more reservations than Kopp had about the scientific character of chemistry before the chemical revolution.

There are, I believe, a number of explanations for why the question of whether chemistry before Lavoisier was a science has remained an unresolved issue. One reason is that historians of science, like scientists themselves, often forget their own past. Until well into the twentieth century, Hermann Kopp was known among historians of chemistry as a towering scholar in their field. In 1932 the journal *Archeion* published an appreciation of Kopp as a historian, by Edmund von Lippmann, which concluded that "Until the present day no younger scholar has combined the talent, the knowledge (of the subject and the languages) and the hard work to such a degree" as Kopp had done to produce his massive studies in the history of chemistry (14). The generations of historians of science who have come of age during the 1950s and later, however, tend to treat the older histories written by scientists about their own fields as pre-professional and outmoded. In the recent *Osiris* volume on reinterpretations of the chemical revolution, to which I alluded at the beginning of my talk, I found only one reference to Kopp, significantly by a Dutch historian who has been in the field longer than the rest of the contributors to that volume (15). I confess that I, too, ignored Kopp until recently, and now realize that some of what I have written about 18th century chemistry that I thought to be novel, he had already discussed. Undoubtedly those participating in debates about the meaning of the chemical revolution today would not fully agree with Kopp's position, but current discussions have, nevertheless, been impoverished by the loss of insights that he brought to the field more than a century ago.

Another reason that the same questions continue to be reopened is that the criteria by which they are answered have changed with changes in the structure and aims of the history of science. During the post war period, as the field was moving away from the model of the scientist-historian, and toward attachments with professional historians, there was a strong tendency to view the history of science as a branch of intellectual history. One consequence of such alliances was a focus, never so exclusive as later portrayed, but nevertheless strong enough, on scientific ideas. From this perspective, much of the work of eighteenth century chemists that someone like Kopp could treat with empathy, appeared barren. Moreover, the pre-Lavoisier chemists were very difficult to read, because they wrote in a language that had been made opaque by the reformed nomenclature which emerged during the chemical revolution. Historians began during the 1960s to define 18th century chemistry less in terms of the activities of practicing chemists than of the views of influential figures of the period who wrote about chemistry, or topics related to chemistry. A good example was a pioneering paper written in 1963 by Maurice Crosland. Having been requested to "discuss the development of chemistry in the eighteenth century with particular reference to the history of ideas," Crosland found it "confining" to restrict his "attention to men who might reasonably be called chemists and ignore others whose chief interests were in other branches of science or even outside science altogether." Among the ideas that appeared to Crosland to have affected the development of chemistry most significantly were the atomic view of matter, elective attractions, schemes of classification, the "physical approach to chemistry," and the phlogiston theory. Only the latter emanated primarily from chemists. The "influence of Newton" played a predominant role, and the attitudes of the philosophes toward chemistry became more visible than the views of chemists whose careers took place in the laboratory (16).

Within the framework of this genre of history of scientific thought, the question of whether chemistry before Lavoisier was "still more of an art than a science" was displaced from criteria such as Kopp had used, to those resting on the status of an intellectual structure largely defined by outsiders to, or dilettantes within, chemistry itself. A similar orientation can be seen in other prominent historical studies of this period, including, for example, Arnold Thackray's monograph *Atoms and Powers*, published in 1970, whose subtitle was *An Essay on Newtonian Matter-Theory and the Development of Chemistry (17).* By such standards it was easy to find 18th century chemistry lacking in the kind of theoretical structure one would expect of a "modern science." The wealth of discoveries that had so impressed Kopp were readily passed over as merely empirical.

In 1955 the French historian of chemistry Maurice Daumas pointed out that 18th century physicists and mathematicians believed that chemistry had stagnated in outmoded doctrines. It could only be saved, according to them, by applying mathematical methods and by explaining chemical reactions by extending the principle of universal attraction to the molecular scale. They also condemned chemical conceptions of heat as a substance. But what they had to substitute was of no use to chemists. "The chemists remained true to themselves. Practitioners above all else, they kept their faith in the only guide that had led them so far, that of experiment (18)." Daumas's view can serve also as a warning to historians not to lean too heavily on the writings of Newtonians, natural historians such as Buffon, or the philosophes, to characterize the chemistry of that time. Relying on the testimony of outsiders and amateurs to the field, historians risk missing the internal dynamic of what was taking places within its laboratories, in the Academies in which the leading chemists of the age participated, and in the specialized literature within which its advances were recorded.

Another displacement of the question, when did chemistry become a science, began in the 1970s with the emergence of a vigorous social history of science. Within this framework historians looked neither for evidences of the progressive acquisition of empirical knowledge, nor for the advent of a strong theoretical structure, but for the formation of a "discipline-oriented community." In the best known study of this type, Karl Hufbauer found that by the 1770s a national community had coalesced in Germany. Its signs were the number of active chemists holding institutional positions, and regular communication among them, particularly with the founding of the first specialized chemical journal (19).

One of the main obstacles to reaching a consensus about what happened in the chemical revolution is a persistent conflation of two images of Lavoisier. From his own time he was recognized, by followers and opponents alike, as the leader of a great revolution in chemistry. During the nineteenth century he came to be viewed by many also as the "founder" of modern chemistry, an image conveyed most vividly in the patriotic hues of Adolph Wurtz. These ought to have been separable viewpoints. The chemical revolution was a historical event, bounded in time, whereas the foundation of modern chemistry is a far more complex phenomenon, dependent on variable judgments about the essential features of modern science in general, of chemistry in particular, and of its differences from what had preceded it. The distinction has, however, attracted little notice.

Nineteenth century writers sometimes used the term "revolution," as Lavoisier himself did, to describe what he had introduced into chemistry, but did not give priority to the term. Kopp, for example, headed his chapter on that topic "the reform of chemistry by Lavoisier (20)," and Wurtz talked about the "triumph" of the "system of Lavoisier" over that of Stahl (21). Twentieth century historians have fixed on the phrase "chemical revolution" and have, in spite of the questionable status of all "founder" myths, continued to treat it as the defining event in the formation of modern chemistry. Why has that happened?

One explanation I would propose is that the chemical revolution was fitted into a more encompassing story of how modern science in general emerged from what has come to be known as "The scientific revolution." David Lindberg has recently surveyed the historical process through which the belief of seventeenth century scientists, and of spokesmen for science such as Francis Bacon, that they were making a sharp break from the past, evolved into the twentieth century notion of a great scientific revolution. The characterization of this revolution was in part a rebuttal to the assertions of Pierre Duhem, who had found evidence of continuities between Galilean physics and ideas of the late middle ages, and claimed that modern science had evolved continuously out of medieval science. A culmination of the reassertion of discontinuity was expressed in Herbert Butterfield's *The Origins of Modern Science* (22), which began with the famous statement that the scientific revolution "outshines everything since the rise of Christianity and reduces the Renaissance and Reformation to the rank of mere episodes." According to Butterfield, that revolution was "the real origin both of the modern world and of the modern mentality (23)." Encompassed within this overarching event was the formation of the modern sciences.

To account for so much, Butterfield had to enlarge the boundaries of a development "popularly associated with the sixteenth and seventeenth centuries," to extend from 1300 to 1800 (24). The latter date enabled him to include within its scope what he called "The postponed scientific revolution in chemistry."

"It has often been a matter of surprise," he wrote, "that the emergence of modern chemistry should come at so late a stage in the story of scientific progress." He attributed much of the delay to the "hurdle" raised in the eighteenth century by the "interposition of the phlogistic theory." After recounting the canonical story of the events, from Joseph Black to Lavoisier, which finally permitted chemists to get around this "block" to "scientific progress," Butterfield concluded, "the chemical revolution which he [Lavoisier] set out to achieve was incorporated in the new terminology as well as in a new treatise on chemistry. ... Over a broad front, therefore, he made good his victory, so that he stands as the founder of the modern science (25)."

First published in 1949, and in a second edition in 1957, Butterfield's little volume provided an accessible overview of the momentous events which shaped the emergence of "modern science" in just that period in which the history of science was coalescing, particularly in the United States, as an academic discipline. It appears to have played a disproportionate role in shaping the early historical perceptions of the young scholars who began entering the field at that time, and despite the extensive new scholarship in each of the areas it covers, it is still used in introductory courses. It is reasonable to surmise that Butterfield's account of the chemical revolution has helped to condition both the negative image of pre-Lavoisier chemistry that is still prominent in the field, and the close association still assumed between victory in the chemical revolution and the foundation of modern chemistry.

The consequences of the retrospective expansion of the meaning of the chemical revolution to somehow cover the foundation of modern chemistry as a whole can be plainly seen in recent revisionist reinterpretations of the revolution. There is not time here to enumerate the specific redefinitions of its scope and themes that have been proposed in the past few years, but I think that Donovan summarized their general thrust accurately when he wrote that they make it (26):

> clear that focusing on the overthrow of the phlogiston theory provides too narrow a base for investigations of any of the major developments associated with the Chemical Revolution, investigations such as those that attempt to reconstruct Lavoisier's research career in chemistry, to describe the theoretical revolution he effected, to explain the ways in which his new theories were received, and to construct a more comprehensive account of the founding of modern chemistry.

The pitfall here is that to account for all of these developments, in particular the founding of modern chemistry, as aspects of the chemical revolution, is to impose a crushing interpretative overburden on the meaning of the historical event that was from the beginning seen by contemporaries as a "revolution in chemistry." What has happened is that historians have inverted the part-whole relationship that should apply. The founding of modern chemistry is not contained within the chemical revolution, but is a much larger story, in which the revolution plays its part.

Once we free the chemical revolution from the burden of explaining too much, then, I believe it becomes quite clear that the older view that it centered on the overthrow of the phlogiston theory was a realistic one. I have elsewhere argued that Lavoisier and his most strategic convert, Guyton de Morveau clearly had that in mind when they invoked the word revolution to describe what had transpired (27). If, by revolution we mean a radical break with the past, a struggle to replace authoritative positions with something new, and a victory in this struggle, then there can be no doubt that the contest was between the phlogiston theory and the theory of combustion with which Lavoisier sought to supplant it. All of the prominent opponents of Lavoisier's new chemical system resisted it as defenders of phlogiston. It is no accident that they named Lavoisier and his followers "antiphlogistonists."

Historians have long maintained that Lavoisier also overthrew the prevailing views about chemical elements. In the famous preliminary discourse of his *Traité élémentaire de chimie*, Lavoisier dismissed "the tendency to see all natural bodies composed of three or four elements," as a metaphysical prejudice dating from the Greek philosophers. He replaced such ideas with his pragmatic definition of elements as "all those substances that we have not yet been able to decompose by any means (28)." It is true that in the period just before Lavoisier entered the field, there had been a revival of the four element theory, but, as Kopp recognized, its basis was much different from the original Greek foundations. Moreover, it was, in the eyes of the eighteenth century chemists who favored it, only a tentative scheme, subject to the same criterion of validity that Lavoisier later asserted. In the article on the "elements" in his *Dictionnaire de chymie*, Pierre Joseph Macquer named in 1766 "the purest fire, air, water, and earth" as elements, because "all the efforts of the art" had so far been "insufficient to decompose them." But he quickly added that "It is very possible that these substances, although reputed to be very simple, are not, that they are even very composed, that they result from the union of many other, simpler substances (29)." These "reputed" elements did disappear in the chemical revolution. Because they had never been firmly embedded within the operating structure of pre-Lavoisier chemistry, as phlogiston had been, however, they were not defended with the tenacity that phlogiston was, and their demise was more a by-product of, than a central issue of the chemical revolution.

I said earlier that I have come belatedly to the realization that there are strong resemblances between the way I have recently written about eighteenth century chemistry and what Hermann Kopp wrote about it long ago. To summarize my view of the chemical revolution I could well adopt his statement that, "A transformation of views was caused by Lavoisier within an existing science." There is, however, a distinction between Kopp's treatment and my position that I believe is significant, the exploration of which can reveal further insights about the nature of the pre-Lavoisier era and recast the boundaries of the chemical revolution itself.

The wealth of discoveries of acids, bases, earths, metals and neutral salts about which Kopp wrote with such deep familiarity appeared to him to constitute "empirical" knowledge. For the "broader views" of the era he turned to eighteenth century ideas about the elements, affinities, and above all, the phlogiston theory. I have argued that the systematic experimental investigations through which the discoveries Kopp describes were made were guided by a simple, but powerful theoretical structure.

The emergence of the concept of what was known at the beginning of the century as the "middle salt," and by the middle of the century as the neutral salt, had profound consequences for the chemistry of that era, and for chemistry ever since then. The idea of the neutral salt was, in part, an empirical generalization growing out of the gradual realization during the seventeenth century that acids could be combined with the various known alkalis, metals, or calcareous earth (the only "earth" recognized at the time to have this property) to produce salts which did not display the properties of these constituents, but from which the constituents could be recovered with their original properties restored (30).

The reason that this generalization was <u>more</u> than empirical, however, is that it required chemists to conceive of the two constituents of the salt as present within it, even though the properties by which the constituents were defined and identified were not present. That was not only counter-intuitive, but in conflict with the general theories of composition prevailing in the seventeenth century. The four element theory of that period, which was, unlike its eighteenth century revival, directly traceable to its Aristotelian roots, the Paracelsian salt, sulfur, mercury triad, and the various compromise systems derived from them during that century, all attributed the generic properties of tangible substances—their fluidity or solidity, volatility or non-volatility, combustibility or non-combustibility, their sharpness or insipidness of taste and other qualities—to one or more of the "principles" which entered into them.

As is well known, this general conception persisted long after the seventeenth century schemes in which it had been embodied had receded into the past. Its most prominent manifestation in the eighteenth century was the principle of flammibility, phlogiston. But Stahl and other chemists of the time looked for others, such as that of acidity, under the guidance of the deeply embedded idea that every prominent property common to a class of substances should ultimately be traceable to one of its generic principles (31).

Some historians argue that the chemical revolution replaced this traditional view of composition with a "combinatorial" one, in which the levels of composition were defined by the tangible substances that could be separated by successive layers of analysis, regardless of the relationship of the properties of the constituents and those of the bodies composed of them. Maurice Daumas emphasized, on the other hand, the continuity between Lavoisier and his predecessors in this respect. "The chemistry of principles" with which Lavoisier came into contact in his youth was represented in new forms in his "principle of acidity," his presumption that a corresponding "principle of alkalinity" would be found, and in caloric (32).

I would like to argue that there was also a continuity across the revolutionary rupture that was the inverse of this one - that is, that there was a strong precedent for the "combinatorial" view of composition thought to be introduced by Lavoisier. This view was expressed with particular clarity in the article "Mixte et Mixtion" in the influential French *Encyclopédie* in 1765 (33):

> An essential characteristic of the chemical mixt, at least in its most perfect form, is that the particular properties of each of the principles that unite in the formation of the mixt perish, or are at least so far masked that it is as if they did not exist at all, and the mixt is truly a new substance, specified by its own properties, and different from those of each of its principles.

The prime support for this generalization came from the observed relations between acids, bases, and the neutral salts that they formed. I would submit, however, that this was no simple generalization of laboratory experience. It was a hard-won understanding of that experience. Seventeenth century chemists had generally believed that the acid and alkali which reacted together so energetically destroyed each other in the process, because those very distinctive properties of the reactants could no longer be detected in the product. In the early eighteenth century, chemists in the French Academy of Sciences carried on a heated debate about whether the iron extracted from plant matter could have been preformed in it, or created in the process. The main obstacle to their acceptance of the first option was their difficulty in conceiving how the iron could be present without imparting magnetic properties to the plant.

In the face of such a mental barrier, the most forceful countervailing force in eighteenth century chemistry was the success of the chemistry of salts. As the number of known salts, acids, and bases grew, research guided by this compositional framework flourished on an ever accelerating scale. It was the principal source of that great "wealth of discoveries" that so impressed Hermann Kopp. Lavoisier had already recognized as much in 1778, when he wrote that "the theory of neutral salts, which has fixed the attention of chemists for more than a century, ... is so perfected today that one can regard it as the most complete part of chemistry (34)."

Viewing the chemistry of salts, not as Kopp did, as empirical knowledge alone, but as Lavoisier had seen it, as a domain of chemistry built around a theory of neutral salts, enables us to define the old question of continuity and discontinuity in the chemical revolution in a new way. Within a well-defined problem domain, defined by Lavoisier as "combustion, the calcination of metals, and in general all the operations in which there is an absorption and release of air (35)," he came into sharp confrontation with those who believed that the phlogiston theory adequately explained these phenomena. He attacked the defenders of the prevailing view, overwhelmed them with his reasoning, his experimental evidence, his effective rhetoric, and an organized campaign to win over those who resisted. He believed that his revolution was nearly completed by 1790, because chemists all over Europe were "gradually dropping the doctrine of Stahl (36)."

> When Lavoisier looked at the chemistry of salts, however, he did not see himself in opposition to his predecessors, but as the follower of a long tradition. "In accordance with the state in which the science of chemistry has been transmitted to us," he wrote in 1778 (37):
>
> It remains for us to do for the principles comprising neutral salts what the chemists who were our predecessors did for the neutral salts themselves: to attack the acids and bases, and to push back by another degree the limits of this type of chemical analysis.

That is just what he achieved. It was through his oxygen theory of combustion that the acids and bases were found to be composed each of a particular constituent combined with oxygen. Where there had formerly been established a single level of composition in terms of what Kopp called "presentable" component substances, there were now two successive levels understood in the same manner. The whole of the second part of Lavoisier's *Traité* is testimony to the ease with which the simpler levels of composition that he had defined could be integrated with the previous knowledge and theory of neutral salts.

My version of Kopp's statement, therefore implies that the revolution Lavoisier led took place not only within an established science, but in a bounded domain within a broader science. Kopp viewed the phlogiston theory as the dominant theoretical feature of that established science because of the great importance traditionally attached to explanations for the action of fire on substances. I view that concern as less dominant by the second half of the eighteenth century, when distillation and other analyses depending primarily on heat no longer dominated the operations of the chemical laboratory to the extent they once had. The chemistry of salts relied more and more on solution and precipitation, differential solubilities, purification and identification through crystallization, and the expanding capacity to define a specific salt, acid, or base through a broad combination of physical and chemical properties. In short, chemistry was no longer a narrow, monolithic set of concepts and procedures, but a complex science comprising an array of increasingly specialized knowledge and problems. Lavoisier clearly recognized that state of the field when he referred to the various "parts" of chemistry.

To reaffirm that the chemical revolution was about the overthrow of the phlogiston theory is not to imply that the changes Lavoisier introduced into chemistry were limited to that domain. The list of his achievements that had major impacts on the future development of the various subfields of chemistry which emerged during the nineteenth century is astonishingly long. They range from his thoroughgoing quantitative style of experimentation and the introduction of a whole new level of complexity of instruments and apparatus, to calorimetry, the elementary analysis of organic substances, and a theory of fermentation which provided the first description of a chemical process as a balanced equation, to the list of pragmatically defined elements from which all subsequent tables of the elements have evolved, and the reformed nomenclature.

Some of these achievements changed the theory and practice of chemistry rapidly, others took many years to exert their full effects. The more dramatic effects might be called "revolutionary," if we follow the popular tendency to describe highly visible, rapid changes of any kind as revolutions. To do so as historians, however, only blunts our use of the language and diminishes the precision of our interpretations. It is useful to distinguish scientific revolutions which require ruptures involving radical change and the overthrow of something essential to the pre-revolutionary state, from other kinds of major scientific transformations. Lavoisier caused a revolution focused on the phlogiston theory. He produced or laid the ground work for many additional transformations in chemistry. But some well established parts of chemistry, in which there was rapid progress contemporary with Lavoisier, but to which he contributed little, continued to develop in the same directions in which they had been heading when he arrived on the scene. They too played their parts in the founding of modern chemistry.

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COMING EVENTS

214th ACS Meeting, Las Vegas, NV, September 7-11, 1997

Fifth Chemical Congress of North America, Cancun, Mexico, November 11-15, 1997

215th ACS Meeting, Dallas, TX, March 29, April 2, 1998

International Symposium, "The Science and Technology of Rudolfinian Time," National Technical Museum, Prague, Czech Republic, August 24-28, 1997. For details contact: Dr. Jaroslav Folta, "R-11" Nat. Tech. Museum, Kostelni 42, 170 78 Prague 7. iso@ntm.anet.cz; FAX: ++ 420 2 379151.

50th Anniversary Conference, British Society for History of Science, "The History of Science as Public Culture?" September 9-11, 1997, University of Leeds. For details contact Dr. Frank A. J. L. James, Royal Inst. Centre for the History of Science & Technology, Royal Institution, 21 Albemarle Street, London, W1X 4BS. fjames@ri.ac.uk; FAX: ++ 0171 629 3569.

7th Italian Congress in History of Chemistry, L'Aquila University, Abruzzo, Italy: October 8-11, 1997. For details contact: Prof. Giorgio Corradini, Dipartimento di Chimica, Ingegneria Chimica e Materiali, Monteluco di Roio, L'Aquila, Italia. FAX: + 39 862 434203.