



## A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

*"To the solid ground  
Of Nature trusts the mind which builds for aye."*—WORDSWORTH.

THURSDAY, NOVEMBER 6, 1890.

### PRIESTLEY, CAVENDISH, AND LAVOISIER.

THE *Revue Scientifique* of the 25th ult. contains a translation of the address which I had the honour of delivering to the members of the Chemical Section of the British Association at the recent meeting in Leeds, to which, on the invitation of the editor, M. Charles Richet, M. Berthelot prefixes a letter, of which the following is a translation:—

"I have no direct concern in the republication of Mr. Thorpe's address which you purpose making in the *Revue*. Personally, I have not any reason to complain of his courtesy, and I should have been silent so far as he is concerned, holding that one is not bound to enter into a controversy which is purely critical, where no new fact is alleged, and where the judgment of public opinion suffices to set things in their true place; however, I comply with your request to let your readers know what my opinion is.

"To my mind, nothing is more opposed to truth and justice than the introduction of national prejudices into the history of science. All civilized nations are at one in proclaiming the glory of Newton, the greatest of astronomers, and yet the majority of English men of science, refusing to treat his rivals with equity, are not agreed to recognize Leibnitz's rights to the invention of the differential calculus: they are as prejudiced in this respect as was Newton himself. Something analogous occurs in regard to the discoveries which created modern chemistry a hundred years ago.

"Unquestionably, Priestley and Cavendish are recognized by all as great discoverers. I have myself taken pains to describe Priestley's discovery of the principal gases in terms of admiration ('La Révolution Chimique,' p. 39), and especially that of oxygen, which I unreservedly attribute to him (pp. 61-62). I have also detailed, with the encomiums which they merit, the investigations of Cavendish, 'one of the most powerful scientific minds of the last century,' and particularly his fruitful research on (to use Blagden's phrase) the artificial generation of water. But the well-merited praise accorded to these English savants does not prevent some of their countrymen from persistently denying the right of Lavoisier to the discovery and co-ordination of those general ideas on which rest our actual conception of matter, more especially in

relation to the composition of air and water. This, I venture to repeat, is an incident in the long-standing feud, continually being renewed in the history of science, between the sagacious discoverers of particular facts and the men of genius who frame general theories. The opinion of most Continental men of science seems, however, to be decided on this special point, as may be seen from the judgment given, not only by Dumas, but by Hoefer, in his 'History of Chemistry,' by H. Kopp, in his careful account of the discovery of the composition of water, and by many others. I have merely concurred with them.

"It was in this spirit that I had sought to trace the history of the discoveries which constituted the doctrine of modern chemistry, by faithfully reproducing all its phases, whilst at the same time indicating the continuity of sequence in the facts and the paternity of the ideas. I did this with an impartiality which has brought upon me the reproach that I have been indifferent to the reputation of my countryman—the very opposite to the accusations which are now directed against me.

"As regards the composition of air, it is easy to separate facts from ideas. It is certain that the discovery of oxygen is due to Priestley. But, said Lavoisier: 'If I am reproached for having borrowed my proofs from the works of this celebrated philosopher, at least none will contest my right to the conclusions, which are often diametrically opposed to his.'

"Priestley, obstinately adhering to the theory of phlogiston, regarded his new gas as consisting of the very substance of air deprived simply of its phlogiston; whilst nitrogen, according to him, was formed also of this same substance combined with a complementary portion of phlogiston. He remained faithful to this doctrine, which obscured the true nature of the greater number of chemical phenomena, until the moment when, like Lavoisier, persecuted by his countrymen, who now proclaim his fame, driven from home, his laboratory burnt by a mob, and threatened with death, he fled to America, where he died in sadness and in solitude. Even more unfortunate was Lavoisier!

"But whatever may have been the personal fate of these two great men, if it is true that Priestley discovered oxygen, it is not the less certain that the true theory of the nature of air is due to Lavoisier.

"The history of the composition of water is more complicated. In reality, the discovery of the facts belongs neither wholly to Cavendish—who undoubtedly played a most important part, inasmuch as he gave the impetus towards the definitive solution—nor to Lavoisier, who

first established a knowledge of the facts by his public experiments and his published writings—nor even to the two combined. They had predecessors, and at the moment even when the light came, Monge played an essential part in the rigorous demonstration of which Mr. Thorpe apparently has no suspicion. Thus each man's share in this history cannot be settled by a word: we require to follow exactly the gradual progress of experiment and publication. But here, again, if Lavoisier is not the principal discoverer of the facts, it is he who has the incontestable merit of having furnished the exact interpretation of the phenomena, freed from the mists of phlogiston, to which Cavendish seems to have remained faithful to the day of his death.

"I have elsewhere laid bare all these facts, and I have no intention of reproducing here the details of a controversy exhausted even in Lavoisier's time, and in which Mr. Thorpe does no more than reproduce the unjustifiable imputations of Blagden, who, impelled by passion, went so far as to interpolate and falsify, with his own hand, the manuscript memoirs of Cavendish, in order to gain arguments in support of his accusations.

"Moreover, nothing more decisively establishes the part played by Lavoisier, and his right to the institution of our modern theories, than the letter of a contemporary English *savant*, Black, as celebrated for his discoveries in physics as in chemistry, and who might have put forward claims on his own account. In 1791 he wrote to Lavoisier, in a letter equally honourable to both:—'The numerous experiments which you have made on a large scale, and which you have so well devised, have been pursued with so much care and with such scrupulous attention to details that nothing can be more satisfactory than the proofs you have obtained. The system which you have based on the facts is so intimately connected with them, is so simple and so intelligible, that it must become more and more generally approved and adopted by a great number of chemists who have long been accustomed to the old system. . . . Having for thirty years believed and taught the doctrine of phlogiston as it was understood before the discovery of your system, I, for a long time, felt inimical to the new system, which represented as absurd that which I had hitherto regarded as sound doctrine; but this enmity, which sprang only from the force of habit, has gradually diminished, subdued by the clearness of your proofs and the soundness of your plan.'

"We can but hope to see the day when the scientific men of England will conform to the opinion of one of the most illustrious of their countrymen.

"M. BERTHELOT,  
"of the Institute."

I quite agree with M. Berthelot that nothing is more opposed to truth and justice than the introduction of national prejudices into the history of science. It was for that reason that I felt compelled, in the Leeds address, to protest against the spirit and bias of the accounts of the discovery of the facts relating to oxygen and the composition of water given in "La Révolution Chimique." Although M. Berthelot's letter somewhat confuses the issues, there is, in reality, but small difference between us. What I ventured to criticize was the general tone and tendency of M. Berthelot's argument, which seems to palliate, and even to justify, Lavoisier's pretensions to a discovery in which he has no right even to be considered as a participator. M. Berthelot now tells us, in his letter that he attributes the discovery of oxygen unreservedly to Priestley. So far so good. It is something gained to have thus secured such an unqualified statement from one who occupies the position of authority in the world of

chemistry in France that is enjoyed by the present Perpetual Secretary of the Academy. We may well hope, therefore, that this particular question has been finally set at rest.

M. Berthelot need not ask British men of science to conform to the opinion of Black. They already do so. That to Lavoisier, and to Lavoisier alone, belongs the merit of having effected the overthrow of the theory of phlogiston, and of having to that extent laid the foundation of modern chemistry, is not questioned on this side of the Channel. So far as I know, it has only been among Lavoisier's own countrymen that any doubt on this point has been raised. We all remember the passionate scorn with which Lavoisier repudiated and protested against the attempts of his compatriots to rob him of his rights:—"Cette théorie n'est donc pas comme je l'entends dire—la théorie des chimistes français; elle est la *mienne*, et c'est une propriété que je réclame auprès de mes contemporains et de la postérité!" It is true, as M. Berthelot implies, that Black has claims. Lavoisier himself admits as much. It would be easy, if it were not beside the points at issue, to match the letter which M. Berthelot quotes, by others from Lavoisier in which he ascribes to Black the germs of his doctrine. M. Berthelot, I repeat, confuses the issues. This particular point was never raised by me in the address. What I said was:—"Two cardinal facts made the downfall of phlogiston complete—the discovery of oxygen, and the determination of the compound nature of water. M. Berthelot's contention is, that not only did Lavoisier effect the overthrow, but he also discovered the facts." I, in common, I venture to assert, with every British chemist, admit unreservedly that Lavoisier effected the overthrow, but we deny that he discovered the facts. It is altogether beside the question for M. Berthelot now to say in effect:—"Have I not praised your men of science, and thereby drawn down upon myself the wrath of my countrymen? And yet you are not satisfied!" We are sorry for M. Berthelot: he is in the position of the man with many friends, and his friends for the moment are a little angry. He has either not the courage of his convictions, or he has halted between two opinions—with the usual consequences.

With respect to the discovery of the compound nature of water, M. Berthelot now takes up a different position from that which he occupies in "La Révolution Chimique." His contention there was that by every legitimate canon the experiment of June 24, 1783, gives to Lavoisier the priority of discovery. He now admits that Cavendish played "un rôle capital—car il donna le branle aux esprits vers la solution définitive." But how was this possible when Cavendish's memoir was not published until January 1784? There is really only one answer—it was given simply by the intervention of Blagden. I repeat that Blagden told Lavoisier of Cavendish's researches and of his conclusions, and that it was in the light of that knowledge that the experiment of June 24, 1783, was made. There can be no question of this. Blagden's testimony, as given in the letter to Crell, is as direct and decisive as it is damning. It was never contradicted by Lavoisier, nor by Laplace, Vandermonde, Fourcroy, Meusnier, or Legendre, who were present on the occasion when Lavoisier himself admits that he received the information.

M. Berthelot does not contradict it, but, instead, he asperses the moral character of Blagden. This method of treating a witness whose evidence cannot be rebutted is apt, when unsuccessful, to recoil on him who attempts it. It is perfectly true that Blagden interpolated the famous passage in Cavendish's memoir :—

"During the last summer, also, a friend of mine gave some account of them [the experiments] to M. Lavoisier, as well as of the conclusion drawn from them. . . . But at that time so far was M. Lavoisier from thinking any such opinion warranted that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water."

This passage, however, was inserted with Cavendish's knowledge and consent, and by his assistant and amanuensis, who happened to be the very man who had a personal knowledge of the facts. Assuming the statement to be true, where is the immorality of the proceeding?

Everything that we can learn authoritatively concerning Blagden goes to show that he was an upright and honourable man. Sir Joseph Banks has testified to his abilities and integrity. Dr. Johnson spoke of his copiousness and precision of communication, with the characteristic addition : "Blagden, sir, is a delightful fellow." Laplace, Cuvier, Berthollet, and Benjamin Delessert, were among his friends.<sup>1</sup> He was rich, and was understood to have speculated to profit in the French funds. For thirteen years he was a Secretary of the Royal Society, and in 1792 he was knighted for his services to science. Every year he spent a considerable time on the Continent, and was frequently in Paris. The gossip of the period states that he aspired to the hand of Madame Lavoisier, who preferred Count Rumford. He died in Berthollet's house at Arcueil, on March 26, 1820. In an obituary notice in the *Moniteur* of September 22, 1820, M. Jomard testifies to his benevolence and generosity, and states that "none of his countrymen have done more justice to the labours and discoveries of the French, or have contributed more than he to the happy relations which have subsisted for six years (1814-20) between the *savans* of the two countries." By his will he provided liberally for his scientific friends : Berthollet, the daughter of Madame Cuvier, and the daughter of Count Rumford, each received £1000; and Laplace £100, "to purchase a ring." M. Berthelot asperses the character, not only of Blagden, but also of his countrymen by his insinuations. Would he have us believe that men like Berthollet, Cuvier, and Laplace, would extend their friendship to, and receive pecuniary benefits from, one whom they believed had foully stabbed their compatriot in the back? It is surely incumbent on M. Berthelot, on every ground, either to substantiate his implications or to withdraw them.

M. Berthelot makes the gratuitous assumption that I am ignorant of the work of Monge. Whether I am or not is altogether beside the mark. There is, indeed, no question of Monge. Monge distinctly disclaims priority

<sup>1</sup> Many of the letters of Berthollet to Blagden are still in existence. In one of these, dated "19 Mars, 1785," he writes from Paris :—"L'on s'est beaucoup occupé ici ces derniers tems de la belle découverte de Mr. Cavendish, sur la composition de l'eau : Mr. Lavoisier a tâché de porter sur cet objet toute l'exactitude dont il est susceptible. . . . Mr. Lavoisier veut répéter l'expérience en faisant brûler l'air déphlogistiqué dans le gaz inflammable, et il y a apparence qu'alors on n'aura point d'acide nitreux, selon les belles observations de Mr. Cavendish." Is this language consistent with the belief that Berthollet, who must have known the facts, regarded Lavoisier as the real discoverer of the compound nature of water?

to Cavendish, nor did he attempt to establish a right to be considered an independent discoverer of the true nature of water. In his memoir, "Sur le Résultat de l'Inflammation du Gas inflammable et de l'Air déphlogistiqué dans les Vaisseaux Clos," he tells us that the experiments recorded in it were made in June and July 1783, and repeated in October of the same year. "I did not then know," he adds, "that Mr. Cavendish had made them several months before in England, though on a smaller scale; nor that MM. Lavoisier and Laplace had made them about the same time at Paris in an apparatus which did not admit of as much precision as the one which I employed." I fail to see what M. Berthelot gains by his reference to Monge.

M. Berthelot reproaches Priestley and Cavendish for their adherence to phlogistonism. I say it with all respect, but is it seemly for M. Berthelot, of all men, to cast this stone? Is not he himself an exemplification of that conservatism which he deplores? A generation ago the doctrine of Avogadro became the corner-stone of that edifice of which M. Berthelot asserts that Lavoisier laid the foundations. Indeed, the introduction of that doctrine effected a revolution hardly less momentous than that of which Lavoisier was the leader. But what has been M. Berthelot's consistent attitude towards this teaching? We can illustrate it by a single example. He is the sole teacher in Europe of any position who continues to symbolize the constitution of that very substance of which he claims that Lavoisier discovered the composition by a formula which is as obsolete as any conception of phlogistonism.

T. E. THORPE.

#### A HAND-BOOK OF PHOTOGRAPHY.

*Handbuch der Photographie.* Part I. Fourth Edition. By Prof. Dr. H. W. Vogel. (Berlin : Robert Oppenheim, 1890.)

THIS is the latest edition of a work which has been known in Germany for ten years, and of which the author is the Director of the Photochemical Laboratory of the Imperial Technical High School in Berlin. The existence of such a post as that occupied by Dr. Vogel in one of the foremost technical schools of Germany is as much an indication of the advanced state of technical education in that country as the non-existence of such specialists in the technical schools of this country is a sign of our comparatively backward condition in the field of chemical technology. The subjects comprised under this heading are so wide in their range and so difficult to grasp, excepting by actual personal contact with the chemical industries, that no instruction likely to be of any great value to those preparing for, or engaged in, the latter can be given, unless the instructor has this qualification. Nor can the student properly avail himself of the instruction thus offered, unless he on his part is well grounded in the general principles of the science which underlies his subject. When such a ground-work has been laid, and the student thus equipped is passed on to the specialist, the result is a chemical technologist who is likely to be of real use to his country. The Germans have realized this long ago—the machinery exists both for laying the foundation and for raising the superstructure of specialized knowledge. In this country, so far as