THE
DEVELOPMENT
OF THE
ATOMIC THEORY

BY

ANDREW NORMAN MELDRUM, I.E.S.
Fellow of the Bombay University

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PREFACE

It is now widely recognised that a science cannot be fully understood without a study of its history. At the same time, research into the history of the sciences is carried on under serious disadvantages. There is no special periodical for the publication of original work in this direction, at least none in Britain. It is often difficult to obtain the papers that are published, and difficult to obtain information regarding them. Even if a paper on the history of chemistry, for instance, is noticed in the *Abstracts of the Chemical Society*, one does not find that the space taken up by the abstract is always directly proportional to the importance of the paper.

Again, the Universities rarely supply genuine instruction in the history of science although this is part of the history of civilisation. A worker on the history of a science is rarely put in a position to teach what he knows, whilst anyone with an experimental knowledge of a science is at liberty to discourse on its history as he may find or make occasion. In this respect the president of a scientific society is a "chartered libertine."

The consequences of all this, in the history of chemistry at least, are that original work when published is largely ignored; that there is hardly any critical examination of results; and that errors which were exposed fifty years ago are still rampant.

Some of these observations are illustrated in the following pages.

A. N. MELDRUM.

Madhavlal Ranchhodlal Science Institute,
Ahmedabad,
Bombay Presidency, India.

THE DEVELOPMENT OF THE ATOMIC THEORY

"What is the good of giving yourself so much trouble and of composing a history when all you need do is to copy the best known ones in the usual way?....Historians copy from one another. Thus they spare themselves trouble and avoid the appearance of presumption. Imitate them and do not be original. An original historian is the object of distrust, loathing and contempt from everybody."—Anatole France, *Penguin Island.*

The fact that the address delivered to a society by its president, on grounds of etiquette, is not discussed at the time of delivery, affords a strong reason for regarding it as open to discussion later. A presidential address to a scientific society at least, if and when it is put on record, becomes as much open to comment, and, if need be, to criticism and challenge as any other scientific publication. This is demanded by the freedom of science.

The Presidential Address to the Chemical Society (*Trans.* 1917, **111**, 288) declares that "chemistry is an experimental science aiming at proving all things and holding fast to that which is good." Whilst this is true, it is not the whole truth. An experimental science has a history that lies open to study. The injunction—"prove all things, hold fast that which is good"—is to be obeyed in studying even the history of an experimental science.

The Presidential Address just cited is in part taken up with the treatment of various questions in the history of chemistry, and the opinions that it offers, particularly on the development of the atomic theory, invite and require examination. It is said that in ancient times the atomic theory was studied first by the Hindus and later by the Greeks, and was derived by the Greeks from the Hindus. (*loc. cit.*, p. 290). This view is not universally adopted by the Greek scholars of Europe, and the question where the atomic theory comes from is only a part of an immense problem, namely, the origin of the Hindu and Greek civilisations.

In considering this problem an experimental chemist is of course at a loss: he must be content simply to learn the opinions of Sanskrit and Greek scholars, whatever these opinions may be, and however they may alter from time to time. The Presidential Address relies on Daubeny, whose book on the history of the atomic theory, in its latest edition, was published in the year 1850. Sir P. C. Ray, who knows Sanskrit as well as chemistry, inclines to the view that the Hindus did communicate the atomic theory to the Greeks. He does not himself go into the merits of the question, and he relies on the authority of others—Max Müller, Colebrooke, H. H. Wilson, Macdonell—who, with but one exception, are not representative of modern Sanskrit scholarship.—(*A History of Hindu Chemistry,* 2nd ed., 1903, Vol. 1, Chap. I).

On the other hand, a modern authority on Greek says:—"No one will now suggest that Greek philosophy came from India, and indeed everything points to the conclusion that Indian philosophy came from Greece."—(John Burnett, *Early Greek Philosophy,* 2nd ed., 1908, p. 21). Again, "the Greeks did not borrow either their philosophy or their science from the East." (p. 27). "The chronology of Sanskrit literature is an extremely difficult subject, but so far as we can see, the great Indian systems are later in date than the Greek philosophies which they nearly resemble." (p. 21).

The history of a science must in the main give an account of the development of ideas: the history of chemistry must consider the evolution of the atomic theory above all, and endeavour to show how the ideas of Leucippus took shape during the centuries and became the molecular and atomic theory of the present day. The Presidential Address disposes of this task in the roundest way:—"The atomic theories of the ancient philosophers....present but little real analogy to that enunciated by John Dalton more than a century ago." "It is not possible to attach great weight to the opinions of Newton and Boyle, ingenious as are their arguments, backed up by intellects so acute."—(*loc. cit.,* p. 290). "John Dalton's ideas were entirely his own" (p. 292). The effect of these statements, taken together, is to say that John Dalton was the creator of the modern atomic theory.

A denial that the atomic theory has been evolved, equally with a denial of evolution in other directions, lies under the disadvantage that it makes but little appeal to the modern intelligence.

As will presently appear, the evolution of the atomic theory in the last hundred and fifty years depended on two things:—The existence of a prior atomic theory, and the development of knowledge regarding gases.

THE PRIOR ATOMIC THEORY*

Boyle and Newton were not the only men in the seventeenth century who concerned themselves with the atomic theory. Bacon, Descartes, Gassend, Boyle, Hooke, Newton, Locke, all men trained on the Greek and Latin classics, were interested in it. One result of their study of the theory was the conclusion that heat is a mode of motion, and Tyndall in his well-known book, *Heat—a Mode of Motion,* has amply illustrated this conclusion by quotation from seventeenth century writers. Further, Newton's ideas regarding the disintegration of atoms, derived as they are from Descartes, were thought important enough by Clerk Maxwell for quotation in the *Theory of Heat,* and are of special interest now that numerous scientific men are actively engaged in the study of atomic disintegration.

The controversy that arose between Franciscus Linus and Robert Boyle as to the nature of atmospheric pressure, bore fruit in the discovery of Boyle's law—that the density of a gas is proportional to the pressure. This law again led Newton to the first quantitative conclusion ever formed about atoms. He proved in the *Principia* that " if the density of a fluid which is made up of mutually repulsive particles is proportional to the pressure, the forces between the particles are reciprocally proportional to the distances between their centres. And *vice versa,* mutually repulsive particles, the forces between which are reciprocally proportional to the distances between their centres, will make up an elastic fluid, the density of which is proportional to the pressure. " Newton goes on :—" Whether elastic fluids do really consist of particles so repelling one another, is a physical question. We have here demonstrated mathematically the properties of fluids consisting of this kind, that hence philosophers may take occasion to discuss that question."—(*Principia,* Book II, Prop. 23).

Newton advanced the hypothesis of an elastic fluid, composed of particles which repel one another in a definite way. This hypothesis, when taken up by Bryan Higgins and William Higgins and by John Dalton, was the germ of the atomic

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* Many of the topics which are considered in what follows have been treated already by the author in a series of papers on the development of the atomic theory:—*Manch. Mem.*, **54**, No. 7, **55**, Nos. 3, 4, 5, 6, 19 and 22. (See also *Brit. Ass. Rep.*, 1908, p. 668, and *New Ireland Review*, 1910, pp. 275, 350). Where the Presidential Address treats of these topics it can be regarded as written either without knowledge of, or in answer to, the above papers.

theory of the early nineteenth century. As will be seen, the evolution of the theory can be explained in this way and in no other.

THE DEVELOPMENT OF KNOWLEDGE REGARDING GASES

The Presidential Address offers the following summary of the work that led up to the formation of a chemical atomic theory:—"The quantitative experiments of Black, Wenzel, Richter and Lavoisier undoubtedly prepared the way for a real atomic theory."—(*loc. cit.*, p. 291).

Here the names of Wenzel and Richter are open to challenge and the Presidential Address proceeds with an account of Wenzel's work to which strong exception must be taken. "Wenzel's experiments, which......were remarkably accurate, showed that when two neutral salts decompose each other, the resulting compounds are also neutral......Richter, following on these lines, drew up a table of acids and bases which respectively neutralised each other to form salts."

This account of Wenzel is unfortunate, because it is simply the vestige of an error that originated with Berzelius and that has been repeatedly exposed by the historians of chemistry, (*e. g.*, Angus Smith, *Memoir of John Dalton and History of the Atomic Theory*, 1856, pp. 160-166; Söderbaum, *Berzelius' Werden and Wachsen.* 1899, pp. 138-9). In short, it has been known for many years that Wenzel, having studied the problem of the mutual decomposition of salts, came to the wrong conclusion. His work, however, is far from being negligible. He was the forerunner, not of Richter and Proust and Dalton, but of Berthollet, of Wilhelmy, of Guldberg and Waage. He had a glimpse of the law of mass action, and his work can be understood from this point of view, and from this point of view only.†

Richter, who also studied the problem of the mutual decomposition of salts, came to the right conclusion. Even so, he had very little influence, if any, on the formation of the atomic theory. His work was done and published too late to influence William Higgins, and it had little influence on John

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† See, for instance, Mellor, *Chemical Statics and Dynamics*, 1914, pp. 4, 19, 29, 128, 178.

Dalton. Roscoe and Harden have shown from Dalton's note-books that his chemical atomic theory was formed in the year 1803, and that the earliest reference to Richter in the note-books bears the date April 19th, 1807.—(Roscoe and Harden, *New View of the Origin of Dalton's Atomic Theory,* pp. 46, 79, 91).

Thus, of the workers named in the Presidential Address as having prepared the way for an atomic theory, there are left for consideration only two—Black and Lavoisier. It is said that "the experiments of Black on "Magnesia Alba" marked a new departure in the mode of attacking chemical problems."—(*loc. cit.*, p. 291). The Address goes on to quote a passage from Black which makes it plain that, instead of claiming to have made a departure in chemical method, Black regarded himself as having proceeded on the same lines as previous workers. "Chemists have often observed .... that part of a body has vanished from their senses........and they have always found upon further inquiry, that subtle part to be air, which having been imprisoned in the body, under solid form, was set free, and rendered fluid and elastic by the fire. We may therefore safely conclude that the volatile matter lost in the calcination of magnesia is mostly air."

Black, in studying the mild and caustic alkalies, used the balance mainly as an indicator of loss or gain of carbon dioxide. He made no effort to attain extraordinary accuracy, and he did not use his results to arrive at the composition of the carbonates by weight.

Because Black made an advance in knowledge of the carbonates one need not infer that he was the first chemist to buy a balance. The opinion that it was Black who introduced the use of the balance into chemistry is just as false as the opinion that it was Lavoisier. This has already been pointed out in the Alembic Club Reprint of Black's paper:—"The introduction of the quantitative method into chemistry...... did not by any means originate with Black."—(*Alembic Club Reprints,* No. 1, Preface by L. D.). Quantitative experiments on combustion had been made a hundred years before the time of Black. The *Essai* of the year 1630, in which Jean Rey surmised that a metal increases in weight because the "air is thickened and rendered adhesive to the metal," indicates that Rey made few if any experiments of his own on the subject. For the fact of increase of weight on calcination he relied chiefly on the work of others—

Brun, Carden, Scaliger, Fachsius, Caesalpinus, Poppius, Libavius, and some of these workers had observed the fact many years before. (*Alembic Club Reprints,* No. **11**, pp. 5, 36, 37, 39, 41, 43, 49).

There is no reason to think that the use of the balance made the difference between the old chemistry and the new. The balance was used for centuries without much light being thrown on chemical problems. Lavoisier, who used it and reached a point in his work at which a knowledge of oxygen became indispensable, did not discover oxygen.

Hopkins, in pointing out the importance of qualitative knowledge in biological chemistry, remarks:—" We all know that to arrive at the mathematical form is the ultimate goal of all real scientific knowledge ; but at a given moment in the history of a science, qualitative knowledge maybe as important as the consolidation of other knowledge in a mathematical form." (*Chem. Soc. Ann. Rep.*, 1916, **13**, 193).

Chemists might make quantitative experiments, but in the absence of knowledge regarding the gases, oxygen above all, they were simply groping in darkness. The prologue to the systematic study of the gases is found in the work of Hales, albeit Hales was greatly indebted to Mayow and to Muschenbroeck. " His experiments," says Priestley, " are so numerous and varied that they are justly esteemed to be the solid foundation of all our knowledge of this subject. " (*Experiments and Observations on different Kinds of Air,* 2nd ed., 1775, p. 4). He showed that gas is given off in numerous chemical changes, and he concluded that the gas had been present,—" fixed, " using Mayow's expression—in the original substances. In his experiments he produced carbon dioxide, nitric oxide, hydrochloric acid, ammonia, oxygen, etc., the remarkable thing being that he never realised that one gas is a distinct substance from another.

Hales was the " father in chemistry " of Black and of Priestley. Black showed in the year 1755 that by depriving a mild alkali of " fixed air " a caustic alkali is formed, and that by combining a caustic alkali with " fixed air " a mild alkali is formed. Thus he proved that in well known chemical changes a particular gas takes part. For many years chemical thought had been poisoned by the confusion of one gas with another, and of all gases with ordinary air. The general effect of Black's work was to show the importance of studying gases. He says this himself :—" Curious chemists tried to produce new airs, as they were called, by every possible means, in the expectation of singular results and discoveries. And thus has arisen a new species of Chemistry which may be called Pneumatic Chemistry." Black left the development of the subject to others. Cavendish had measured the volume of the gas given off by the action of acids on carbonates and the density and solubility of the gas. The subsequent work of Cavendish on gases is well known, as well as the work of Rutherford, Scheele and Priestley.

Priestley studied specially nitric oxide, hydrochloric acid, ammonia and oxygen gases, each of which Hales had had under observation. His discovery of oxygen in the year 1774, as a gas in which a candle burns vigorously, was the starting point of modern chemistry. He happened to communicate this observation almost at once to Lavoisier, and Lavoisier was in a better position than any other man in the world to see its importance. The qualitative chemist had come to the aid of the quantitative. In Priestley's discovery Lavoisier found the clue to his own patient and exact work. Henceforth his task as a chemist consisted in the study of oxygen, in showing that oxygen plays in nature, on a vast scale, the part that carbon dioxide plays in the chemistry of the mild and caustic alkalies. " Vast intellectual and material continents lay for the first time displayed, opening fields of thought and fields of enterprise of which no one could conjecture the limit."

THE CHEMICAL ATOMIC THEORY

Speculation is excited by a striking discovery, and speculations concerning the nature of matter have often arisen from discoveries regarding the gases. As has already been shown, Newton, in order to account for Boyle's law, was led to form the hypothesis of an elastic fluid composed of particles which repel one another in a definite way, this hypothesis being the earliest known instance of the exact treatment of the atomic theory.

The Presidential Address never mentions Priestley, whose work not only was the starting point of Lavoisier's system of chemistry, but gave a stimulus to the formation of a chemical atomic theory. In the first place a preliminary atomic theory arose out of Priestley's discovery that hydrochloric acid and ammonia can exist as gases. Bryan Higgins, who was a student of Newton's works, applied the doctrine of " particles mutually repulsive " to the case of these two gases. He thought that on combination with one another the gases must unite particle with particle, and in this way only. He reasoned that two particles of ammonia could not combine with one of acid, for, if the three were to meet, the two of ammonia must repel one another and one of them must be driven away from the acid atom. For a similar reason, two atoms of acid could not combine with one of ammonia.

Bryan Higgins attempted to explain other facts in terms of this theory. He took it as a general rule that when a salt crystallises from water which contains acid, the salt does not carry down acid with it. The cause was, he thought, that the particles of acid in the salt repel the particles of acid in the water. This, right or wrong, illustrates the fact that Bryan Higgins, as a follower of Newton, had formed precise views about the combination of atoms.

Once Lavoisier knew how to prepare oxygen the fate of the phlogiston theory was sealed. Bryan Higgins, however, continued for years to believe in the theory and thus, of necessity, he was confined to incorrect views regarding the composition of matter and prevented from improving his atomic theory. His nephew, William Higgins, whom he trained in chemistry, became an early convert to Lavoisier's doctrines and published in the year 1789 a book entitled *A Comparative View of the Phlogistic and Antiphlogistic Hypotheses.* This book has two remarkable features: it expounds Lavoisier's teaching against phlogiston, and it contains a much improved atomic theory. The Presidential Address disparages the atomic theory of William Higgins:—" His suggestions were involved and hidden in much phlogistic matter, apparently without any clear ideas underlying them as to the nature of compounds "—(*loc. cit.*, p. 292). These words encourage the suspicion that they are based on an imperfect acquaintance with the book referred to, for they make the suggestion that Higgins was an ignorant believer in phlogiston. On the contrary, William Higgins deserves all the credit of having written the earliest book in the English language against the phlogiston theory. Further, he found the germ of an atomic theory in his uncle's work, and Lavoisier's teaching enabled him to develop the theory from the germ. The nephew's theory was an improvement on the uncle's, and both were based on Newton's doctrine of an elastic fluid composed of mutually repulsive particles.

It is all in the natural order of things. The Presidential Address justly says that " the numerous and accurate experiments of Lavoisier......gave abundant data and prepared the ground for theoretical explanation." William Higgins, learning from Lavoisier that the element oxygen may combine with another element in more than one proportion, supposed that these elements tend to combine first in the proportion atom to atom. The next possible combination was two atoms of oxygen to one of the other element, then three to one and so on. Because like atoms repel one another, the most stable combination was 1 to 1, then 2 to 1, then 3 to 1, and so on. His views as to water can be expressed in the formula OH, as to oxides of sulphur by OS and O_2S, as to the oxides of nitrogen by ON, O_2N, O_3N, O_4N, O_5N. It was unfortunate for chemistry that the importance of William Higgins' ideas regarding atoms was not perceived at the time he published them. His theory "fell on a heedless world", just as Newland's system of the elements fell on a heedless Chemical Society.

John Dalton's chemical atomic theory was formed about fourteen years later. The first known table of atomic weights, as Roscoe and Harden have shown (*op. cit.*, p. 28), appears in Dalton's note-books under the date September 6th, 1803. The atomic weight tables which he drew up later differ much in details from the first, but they are based on the same principles. Hence a full explanation of how this first table arose would be an account of the origin of Dalton's chemical atomic theory.

Dalton had formed a physical atomic theory previously to the chemical; it arose out of his study of the gases that had been discovered in the atmosphere. The fact that the gases in the atmosphere are uniformly mixed, although they have different densities, had led to the almost universal belief that they existed there in a state of chemical combination with one another. Dalton's instincts and his experiments led him to the contrary belief, that the atmosphere is a physical mixture. In the year 1801 Dalton proved that the pressure in a mixture of gases is the sum of the partial pressures, so that each gas in a mixture exerts its own pressure as if the others were absent. Other discoveries followed on this. Dalton studied the vapour pressure of liquids, particularly of water, and was thus enabled to explain evaporation and the dewpoint. In 1802 he established the fact of gaseous diffusion, " that a lighter gas cannot rest upon a heavier," which Priestley had doubted.

In 1803 Henry showed that the amount of a gas which dissolves in a liquid is proportional to the pressure, and Dalton followed at once with the observation that in a mixture of gases exposed to a liquid each gas dissolves according to its partial pressure.

In the year 1801 Dalton advanced a theory of mixed gases which he stated as follows :—" When two elastic fluids, denoted by A and B, are mixed together, there is no mutual repulsion amongst their particles, that is, the particles of A do not repel those of B, as they do one another." This theory, as being an obvious attempt to extend Newton's hypothesis regarding a single elastic fluid to the case of a mixture, proves that Dalton was a Newtonian.

When his chemical theory is examined Dalton is found to be a Newtonian still. The fundamental rule of this theory is that different atoms tend to combine in the proportion of atom to atom. When only one compound of two elements was known it was presumed to be binary, *e.g.*, water was formed by the union of one atom of oxygen with one of hydrogen, ammonia by the union of one of hydrogen with one of nitrogen.

Afterwards, when Dalton was challenged to justify this rule, he pointed out that if an element A unites with an element B, the repulsion of the atoms of B for one another must tend to the formation of a binary compound. " Binary compounds must first be formed, then ternary, and so on, till the repulsion of the atoms of B refuse to admit any more."—(*Nicholson's Journal,* 1811, **29**, 147). Thus Dalton's physical theory and his chemical theory have a common basis in Newton's doctrine of mutually repulsive atoms. Although the Presidential Address says that " it is not possible to attach great weight to the opinions of Newton" and that " Dalton's theory was entirely his own," there is no room for doubt that Dalton was a Newtonian, as Bryan Higgins and William Higgins were before him.

THE RECEPTION GIVEN TO DALTON'S CHEMICAL THEORY

The Presidential Address says " the new theory was very rapidly welcomed and adopted in this country especially, and owes its rapid acceptation very largely to the energy and enthusiasm of Professor Thomas Thomson..........the great influence possessed by W. H. Wollaston ......contributed largely to its immediate acceptance amongst scientific men." —(*loc. cit.*, p. 294).

There is confusion of thought here in two directions. In the first place the Presidential Address ignores the distinction between law and theory,—between the law of combination in multiple proportions and the theory based upon it. Wollaston was a believer in the law and he adduced in the year 1808 cases of it which he had observed amongst salts. But he ought not to have been named as a direct supporter of the atomic theory. Like Davy, he was sceptical about atoms, and he was the advocate of "equivalents" instead.

Again, the Presidential Address ignores the difference between making a theory known to scientific men, and inducing them to embrace it. Thomas Thomson's enthusiasm over the theory was genuine and his efforts to make it known were successful, yet Dalton and he remained for many years the only chemists in Britain who proved their faith in the theory by work.

The address says that Dalton, "having clearly stated the theory, proceeded to establish it on a thoroughly sound experimental basis."—(*loc. cit.*, p. 293). This statement is ambiguous. It is true as a statement of what Dalton attempted to do. If it means that Dalton succeeded in his attempts, this is the precise opposite of what all people who know about him believe. Thomson, again, was not happy in his attempts to establish Prout's hypothesis regarding atomic weights. In fact, Dalton was a much less accurate worker than Thomson, and Thomson a much less accurate worker than Berzelius. It was Berzelius who, by the exercise of energy, skill and judgment, put the atomic theory on a thoroughly sound experimental basis, and thereby, far more than any other chemist, brought about its acceptance in the scientific world.

THE QUESTION OF PRIORITY BETWEEN HIGGINS AND DALTON

The Presidential Address touches on the question of the priority of Higgins over Dalton in the matter of the atomic theory. It admits that Higgins' essay of the year 1789 "seems at first sight to contain and set forth a theory of matter closely resembling that put forward by Dalton a few years later."—(*loc. cit.*, p. 292). This admission is to be taken for what it is worth : it amounts to first impressions, and first impressions only, of a book that was published more than a century ago. This is trifling with the question. A man must have perfect mastery of a question if he can trifle with it and at the same time say the last word upon it.

The Address makes no attempt fully and finally to assess Higgins' claims: it avoids this by proceeding to deprecate "acrimonious and fruitless discussions as to priority." After all, from the scientific point of view, acrimony is merely irrelevant to discussion. Questions of priority are essentially intellectual. Chronology is the indispensable basis of history, and the history of science turns on matters of priority. Questions of priority in science might be discussed absolutely without feeling, if human nature would allow scientific papers to be published anonymously,

People go into the questions that interest them. In the address the President asserts his own priority on two matters :—(1) The atomic weight of nitrogen, which, in a paper read in 1901, he showed to be exactly 14 (14·000 or 13·999) ; (2) the atomic weight of iodine which he found to be higher than Stas' value. "Still more remarkable was the publication five weeks *afterwards* * by Ladenburg, who arrived at a number almost identical with that given by me......the higher value has been thoroughly established by work on other ratios by Baxter and others of the Harvard school."—(*loc. cit.*, p. 308).

Thus a president can make use of his position to assert his own priority and to deprecate discussion regarding the priority of a worker who cannot speak for himself. It is to be noted that stress is laid on a priority of five weeks over Ladenburg, whilst the interval between Higgins' atomic theory and Dalton's—fourteen years at least—is alluded to as "a few years."

The controversy which arose over the claims of Higgins and Dalton to the chemical atomic theory, and which continued between the years 1810 and 1818, did not prove entirely fruitless. W. H. Wollaston took the side of Higgins at the time, and after the controversy had died down other men of weight, Thomas Graham and Sir John Herschel, for instance, took occasion to assert the priority of Higgins. Herschel introduces the subject in his dialogue *On Atoms.* (*Popular Lectures on Scientific Subjects*, 1868, p. 453).

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* The italics are in the original.

Hermione.—" Do tell me something about these atoms. I declare it has quite excited me; specially because it seems to have something to do with the atomic theory of Dalton."

Hermogenes.—" Higgins, if you please."

Coward and Harden in a recent paper afford proof that the essential identity of Higgins' theory and Dalton's can no longer be denied. The object of their paper is to give an account of the lecture-sheets that Dalton prepared to illustrate the atomic theory. The authors describe " Sheet 12 " thus :—" Five oxides of nitrogen, represented as compounds of one atom of nitrogen with one, two, three, four and five atoms of oxygen." On this they make the following comment :—" It is improbable that Dalton himself ever adopted these formulae for the oxides of nitrogen. The sheet was perhaps used to illustrate some contemporary views on the subject......or possibly even those of William Higgins, *A Comparative View of the Phlogistic and Antiphlogistic Hypotheses,* 1789, pp. 132-5."—(*Manch. Mem.,* 1915, **59**, No. 12, p. 52).

Evidently, then, the doctrine of chemical combination in multiple proportions is embodied in William Higgins' atomic theory of the year 1789. Coward and Harden make the admission, intentional or unintentional, that Higgins' and Dalton's chemical atomic theories are essentially the same. Hence it follows that Higgins forestalled Dalton.

The resemblance between William Higgins' ideas and John Dalton's is indeed so complete that it can be accounted for only on one or other of two suppositions :—(1) That Dalton plagiarised from Higgins. There is, however, no necessity for adopting this supposition. (2) That Higgins and Dalton each started from the same hypothesis, namely, Newton's doctrine of an elastic fluid composed of mutually repulsive particles, followed much the same train of thought and reached essentially the same conclusions.

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