



## XI. The discovery of the atomic theory claimed for Mr. Higgins

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lumn of quicksilver, under the pressure of which aqueous vapour would begin to condense at the temperature required for expelling the water out of these earthy hydrates: and this height would express the mutual affinity of the alkaline earths and water, and might be compared at pleasure with any other mechanical power. And since chemical attractions may be compared with each other, we might perhaps by degrees advance so far by these means, as to be able to express every chemical affinity in numbers, and to compare it with gravity, the universal measure of mechanical force.

We shall hereafter see that some of the weaker bases, when they take up water, retain it with a force so moderate, as to be exceeded by the expansive power of the water even at lower temperatures. Some of them thus become in some measure hygroscopic substances, since their greater or less approach to the maximum of moisture depends on the dryness of the air, and only takes place when the air exhibits a similar maximum, and the expansive power of the water at the given temperature is completely annihilated.

[To be continued.]

XI. *The Discovery of the Atomic Theory claimed for Mr. HIGGINS.* By JOHN NASH, Esq.

Merrion Square, Dublin, Jan. 7, 1814.

SIR, I BEG leave to call the attention of your readers to a publication which has recently appeared in a cotemporary Journal, and to point out a mis-statement there given to the public, injurious, in my opinion, to the scientific reputation of a learned and respected individual.

The passage I allude to is contained in No. 12 of Dr. Thomson's Annals of Philosophy, and is in these words:

“When we reflect on this cause, it appears at first evident that it must be of a mechanical nature; and what presents itself as the most probable idea, most conformable to our experience, is, that bodies are composed of atoms, or of molecules which combine 1 with 1, 1 with 2, or 3, 4, &c.; and the laws of chemical proportions seem to result from this with such clearness and evidence, that it seems very singular that an idea so simple and probable has not only not been adopted, but not even proposed before our own days. As far as I know, the English philosopher Mr. John Dalton, guided by the experiments of Bergman, Richter, Wenzel, Berthollet, Proust, and others, was the first person who endeavoured to establish that hypothesis. Sir Humphry Davy has lately assured us, that Mr. Higgins in a book published in the year 1789 established the same hypothesis. I have not seen  
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the work of Mr. Higgins, and can only notice the circumstance on the authority of Davy."

To which the learned Editor has subjoined a note of his own in the following words:

"The work of Higgins on *Phlogiston* is certainly possessed of much merit, and anticipated some of the most striking subsequent discoveries. But, when he wrote, metallic oxides were so little known, and so few exact analyses existed, that it was not possible to be acquainted with the grand fact that oxygen, &c. always unite in determinate proportions which are multiples of the minimum proportion. The atomic theory was taught by Bergman, Cullen, Black, &c., just as far as it was by Higgins. The latter indeed states some striking facts respecting the gases, and anticipated Gay-Lussac's theory of volumes; but Mr. Dalton first generalized the doctrine, and thought of determining the weight of atoms of bodies. He showed me his table of symbols, and the weight of the atoms of six or eight bodies, in 1804; and, I believe, the same year explained the subject in London, in a course of lectures delivered in the Royal Institution. The subject could scarcely be broached sooner. But about the same time several other persons had been struck with the numbers in my table of metallic oxides published in my *Chemistry*; and the doctrine would have certainly been started by others if Dalton had missed it."

That learned Editor by this publication has endeavoured to deprive Mr. Higgins of the honour due to the first author of the atomic theory, a doctrine now so generally received and universally admired, and of which Sir H. Davy, Berzelius, and others have spoken in terms of unqualified approbation.

Dr. Thomson has in a part of his note endeavoured to bestow the credit of that theory upon Mr. Dalton, and in another part to fritter away the merits and importance of the discovery. And that the discovery is important, and that merit is due to the author, I think, is fully established by the opinion of such men as I have mentioned.

I shall now proceed to show, by reference to dates and facts, that the merit of that discovery exclusively belongs to Mr. Higgins, and that Dr. Thomson was not justifiable in making the assertions in his note.

It is well known, that for several years the attention of the chemical world was directed to the consideration of the phlogistic and antiphlogistic doctrines. Whilst these questions were agitated by their respective advocates, Mr. Higgins, without adopting either the one theory or the other, commenced, upon the true ground of experiment and analysis, to examine the foundation of both, and the atomic theory was one result of that investigation.

vestigation. And accordingly Mr. Higgins, in his work printed in the year 1790, gave that theory to the world, ten years, at least, before either Mr. Dalton or his learned panegyrist even published a hint of such a doctrine. That work which Mr. Higgins styled *A comparative View of the Phlogistic and Antiphlogistic Theories*, and which Dr. Thomson calls (and I think disingenuously) a work upon Phlogiston, was very generally read, and is now to be found in the library of every scientific society and individual, with that date for its publication prefixed to it.

Reference to dates establishes priority; and reference to the works of Mr. Higgins and Mr. Dalton will fully establish this, that Mr. Higgins left the atomic theory fully as perfect as Mr. Dalton; and I would with confidence call upon the most zealous advocate of the latter gentleman, to show in his work any one position, or principle, which is not founded upon, or deducible from, the doctrine first discovered and established by Mr. Higgins.

Dr. Thomson in the note alluded to asserts, that "*when Mr. Higgins wrote, metallic oxides were so little known, and so few exact analyses existed, that it was impossible to be acquainted with the grand fact, that oxygen, &c. always unites in determinate proportions which are multiples of the minimum proportion.*" To set the Doctor right on that head, I will beg leave to refer him and the reader again to the *Comparative View*, page 295 &c. where they will find that treated of which the Doctor asserts to be then unknown; and what he calls the *grand fact*, will be found to be the greatest leading principle which Mr. Higgins endeavours to establish in that work, particularly in that part where the molecules of different acids are represented by diagrams, with their respective number of particles of oxygen and bases; and it was that which first gave a clear idea of definite proportions.

I apprehend the Doctor is not more fortunate in the assertion, "*that the atomic theory was taught by Bergman, Cullen, Black, &c. just as far as by Higgins.*" Now I have looked over carefully the works of those chemists, and also an accurate manuscript note or report of Black's Lectures; and I affirm, there is not in any of them the slightest mention of the atomic theory. Indeed I must here remark, that the Doctor has not attempted to support any of his assertions by quotations: it was prudent not to make the attempt, he could not succeed.

In contradiction to what the Doctor says, "*Mr. Dalton first generalised the doctrine, and thought of the weight of atoms of bodies,*" I need only refer the reader to Mr. Higgins's work, pages 15, 37, and particularly to pages 80 and 81. As to that remark of the Doctor's with which he closes his note, that "*the doctrine would be started by others if Dalton had missed it,*" I must

must say that it is most disingenuous : first, because it indirectly makes Mr. Dalton the starter (to use the Doctor's metaphor) of that whereof he was only the pursuer, and when it appears, and that to the Doctor's knowledge, that Higgins put up the game ; and next, because that sentence conveys a sentiment unworthy of any scientific man, equally disparaging to merit of any discovery whatsoever, the best answer to which is to remind the Doctor of the story of Columbus and the egg.

I wish it to be clearly understood, that I by no means attempt to attribute the learned Doctor's mis-statement to any unworthy motive ; nor is it any part of my intention to enter into a vindication, or explanation, of the theories and the positions laid down in the production of Mr. Higgins. Such were I even capable of performing would now be superfluous, as I understand there is at present in the press a work on that subject, from the pen of the learned Professor himself. I mean no more than to claim for Mr. Higgins the merit of being the original author and promulgator of the Atomic Theory.

I am, sir,

Your obedient humble servant,

To Mr. Tilloch.

JOHN NASH.

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XII. *Preparation of the lately discovered new Substance called IODE, which possesses the singular Property of becoming converted into a beautiful violet-coloured Gas by the mere Application of Heat. Communicated by Mr. FREDRICK ACCUM in a Letter to the Editors.*

SIRS, YOU will receive with these lines a small glass tube, containing a specimen of the extraordinary substance lately discovered in France, which possesses the singular property of becoming converted into a beautiful violet-coloured gas, by the mere application of a gentle heat, and condensing again unaltered into a solid state, resembling plumbago or black lead, when suffered to grow cold.

As this substance, to which the name of iode has been given, has within these few weeks arrested the attention of chemists, and as the mode of obtaining it has not yet been published in this country, I take this opportunity of stating, that it may be procured by distilling, with a very gentle heat, the uncrystallizable saline mass which is obtained, or left behind, after separating all the crystallizable salts from a lixivium or solution of kelp, or Spanish barilla of commerce.

For the purpose of experiment or exhibition in a Lecture-room, the following easy process answers exceedingly well :

Take