Most historians have ruled out the possibility that John Dalton was influenced by the theories of atomists William and Bryan Higgins, as well as William Austin, in developing his first table of atomic weights on 6 September 1803. I review and evaluate the case to be made for the influence of each scientist on Dalton. Contrary to prevailing views, I raise new Daltonian doubts, especially for Bryan Higgins.

Keywords: John Dalton; William Higgins; Bryan Higgins; William Austin; William Allen; atomic theory

INTRODUCTION

In 1808 John Dalton published *A New System of Chemical Philosophy*, which described principles such as the uniqueness of atoms of the same element, relative atomic masses, and the rules of chemical combination, which taken together comprise the tablets of modern chemistry. The adoption of molecular formulas based on the laws of definite and multiple proportions, and the assignment of relative atomic masses, placed the science on a new quantitative footing, enabling present-day chemists to perform a myriad of practical calculations without giving a second thought as to their origin.

In spite of the magnitude of his accomplishment, how Dalton (figure 1) came up with his theory remains a mystery that has been debated for more than 150 years. Although it is clear that his interests in meteorology and the composition of the atmosphere fuelled his investigations, did his theory arise from work on chemical combination or from investigation of solubility of gases in water? Amidst the debate regarding the origins, the possibility that Dalton was influenced by, or even aware of, the theories of atomists such as Bryan and William Higgins, and William Austin, all of whom resided in London when their key works were published, has been given little credence. One of the key reasons cited in support of this position is Dalton’s relative isolation in Manchester, an argument summarized by Kelham: ‘Dalton’s ideas were almost certainly derived independently of
this group [the London atomists], which only assisted him by paving the way for his ideas and making them readily acceptable in the metropolis.3

Bryan Higgins, who hailed from Co. Sligo, Ireland, started a school of chemistry in London in 1774, where he taught that matter consisted of globular atoms surrounded by a repelling atmosphere of caloric, the supposed substance of heat at the time. According to Higgins, atoms that repelled their own kind but were attracted to atoms of another combined in definite proportions when the forces of attraction and repulsion just balanced, a process he called saturation. Bryan addressed the union of two atoms, which he illustrated with diagrams and experiments in his lectures, using the mixing of ammonia and hydrochloric acid to form ammonium chloride as an example. His theory of saturation was first published in detail in his book A Philosophical Essay Concerning Light (1776) and was addressed in Experiments and Observations Relating to Acetous Acid (1786), Minutes of the Society for Philosophical Experiments and Conversations (1795) and also the syllabus to his lectures.4

His nephew William Higgins, who for a time was his assistant, expanded upon his uncle’s views in his 1789 work A Comparative View of the Phlogistic and Antiphlogistic Theories, addressing the union of one atom to several and illustrating the law of multiple proportions in
numerous diagrams. William believed that combination was primarily dictated by attractive forces, did not appreciate the concept of atomic mass, and cast his entire theory in the context of the phlogiston debate that was ongoing at the time, rather than presenting his theory as a unique entity.5,6

William Austin was a reader in chemistry at Oxford from 1785 to 1786, and William Higgins worked for him as a chemical operator sometime during this period. Austin stayed at Oxford for only a year, leaving in 1786 to become a physician at St Bartholomew’s Hospital, where he continued to teach chemistry. Austin’s papers included one on the composition of volatile alkali, which expressed an atomic interpretation for the specific gravities of gases based on the distances between atoms, which Austin believed had identical mass.7,8

Dalton’s law of multiple proportions was based on his first law of mixed gases,9 in which the mutual repulsion of like atoms surrounding a dissimilar atom (which they do not repel) limits the number of like atoms with which the dissimilar atom can combine. However, Dalton stated that in 1805 he switched to a second theory of mixed gases for an explanation, which involved globular atoms surrounded by caloric, the model of repulsion more in line with Bryan’s view than with William’s. Dalton developed a system of relative atomic masses, which neither Austin nor the Higginses accomplished, but his rules of chemical combination resemble those of the Higginses, regardless of which explanation he used.

For example, Dalton’s ‘rule of greatest simplicity’ is based on a binary combination of atoms, similar to Bryan’s, but applied step by step to form ternary and compounds of higher order. And his law of multiple proportions mirrors the views of William, the similarity first noted by Humphry Davy in his Bakerian Lecture of 1810,10 and repeated in his 1812 Elements of Chemical Philosophy.11 Emboldened by Davy’s statements, Higgins entered the fray in 1814, insinuating that Dalton was a plagiarist in his Experiments and Observations on the Atomic Theory,12 and continued with an assault against Dalton for the next five years. I have discussed the reasons for William Higgins’s claims to priority elsewhere,13 and Wheeler and Partington have dealt with the merit of these claims in some detail, assuming that Dalton was unaware of the work of the Higginses.14

In this paper I review and evaluate the case to be made for the influence of each scientist on Dalton, and raise new Daltonian doubts, especially for Bryan Higgins.

**The case for William Higgins**

William Henry’s 1833 memo

In 1854, W. C. Henry, son of Dalton’s lifelong friend William Henry, wrote a biography of Dalton that included a memo written by his father on 26 February 1833, as well as some statements attributed to his father, indicating that Dalton was unaware of Higgins’s Comparative View until after Davy’s Bakerian lecture.15 Wheeler and Partington, who wrote the standard reference on the Higgins–Dalton dispute in 1960, relied heavily on Henry’s 1833 memo to reiterate the same conclusion,16 and it is their repetition that is largely responsible for the prevailing view that Dalton was unaware of Higgins’s work before he developed his first table of atomic weights.
William Henry’s memo, which describes a conversation that he had with John Dalton approximately 20 years previously, begins with a description of a meeting with Dalton and some of his associates on learning of Davy’s claim for Higgins, and then continues:

It must have been after this time that, calling on Mr. Dalton, I found him in the act of reading the note to Davy’s paper in the Phil. Trans., 1811, p. 15. He expressed his surprise, and asked me if I had seen Higgins’s book. I told him that I had not only seen it, but quoted it, and lent him the volume.17

Although W. C. Henry and Wheeler and Partington accepted the memo at face value, on close examination the memo fails every key test used to evaluate the objectivity of a document. Not only is it based on Henry’s recall of an event years after it occurred, its motive is questionable, because it was written at the very moment when Henry and Charles Babbage were trying to secure a pension for Dalton.18,19 Henry was certainly not a disinterested party, being a close friend of Dalton’s; to further muddy the waters, the state of Henry’s mind when he wrote the memo is in doubt. Three years later, Henry committed suicide by shooting himself in the head, and the possibility cannot be dismissed that at the time he wrote his memo the bouts of depression and agitation that led to his death had begun to take hold.20 Henry’s son also noted in his biography that his father mentioned several times that Dalton had never read Higgins’s book. However, these statements are subject to all of the concerns that apply to the memo, and they should be treated with the same scepticism with which historians have viewed W. C. Henry’s statements about the possible influence of Richter on Dalton’s work.21

Newly located documentary evidence provides support for my arguments. As noted in W. C. Henry’s biography of Dalton, his father touched upon the issue of plagiarism on Dalton’s part in a long effusive statement written to Babbage in support of Dalton’s pension about a year earlier, on 19 February 1832.22 After dismissing the possibility of plagiarism by referring to Dalton’s honour, his independent investigative approach and his indifferent attitude towards reading the works of others, Henry stated:

He [Dalton] has drawn from observed phenomena, new and ingenious views; upon these views he has founded distinct conceptions of a general law of nature; and he has traced out the conformity of that law with an extensive class of facts, many of which he himself first revealed by well-devised experiments [italics added].23

Yet on 15 November 1805, just before the publication of the sixth volume of Manchester Memoirs, which contained the first table of atomic weights,24 Henry wrote his earliest opinion of Dalton’s experiments on record to Alexander Marcet, which directly contradicts his assessment in the pension correspondence:

In a volume of our memoirs, which is on the point of appearing, you will see several papers of Dalton on the subject of his peculiar theories. I hope I have persuaded him to give the public a clear and detailed account of his experiments and doctrines, which, in their present form, are deplorably deficient in perspicuity.25

But Dalton did not follow his friend’s advice, for the paper deleted all of the experiments supporting the development of the table of atomic weights. In fact, it was not until May 1811, after John Bostock had criticized Dalton for not providing experimental evidence that he had promised,26 that Dalton finally explained the rationale behind his theory of multiple proportions.27 Either Henry’s memory was faulty or selective after the passage of
so much time, or he intentionally misrepresented the facts regarding Dalton’s experiments in his 19 February 1832 correspondence to Babbage in support of a pension. And if Henry’s 1832 statement about Dalton’s experiments performed years previously is any guide, what confidence can be placed in his 1833 memo, recalling a 20-year-old conversation? Considering that Henry brought up the plagiarism issue to Babbage in his 1832 correspondence, it seems that the 1833 memo is connected in some manner with the effort to obtain a pension for Dalton, which he did receive four months later.

Wheeler and Partington concluded the following about Henry’s 1833 memo:

As far as we are aware, this statement by Henry is the only one available on which an opinion of Dalton’s independence of Higgins can safely be based. All the statements that Dalton read, or even knew of the existence of, the *Comparative View* before he arrived at his atomic theory in September 1803, when closely examined, are found to rest on no material facts or documentary evidence apart from statements by Higgins.28

There are so many questions regarding the objectivity of William Henry’s statement, such as his ability to recollect years later, questionable motives, and his state of mind, that no decision can safely be rendered on whether Dalton did or did not read *Comparative View* based on Henry’s memo.

*Henry’s Epitome of Chemistry*

Contrary to Wheeler and Partington, there is indeed some documentary evidence to suggest that Dalton was aware of the existence of Higgins’s *Comparative View* before he came up with the atomic theory. On 5 April 1801 John Dalton wrote a letter to his friend John Fell, stating, ‘William Henry has just presented me with his *Epitome of Chemistry*. It seems a very useful manual for one tolerably acquainted with the principles of chemistry.’29,30 On the basis of his close association with Henry and the collection of books that were auctioned off after he died,31 he seems to have followed subsequent editions as well. At the time that Dalton came up with his first table of atomic weights, the 1803 third edition was current, the preface dated 6 November 1802.

In several accounts, Dalton mentioned the importance of the oxides of nitrogen in the development of the atomic theory. Of particular note is his statement of 15 May 1811, published in *Nicholson’s Journal*, which has special significance because not only was it written at a time when Dalton was under attack by Bostock, it was also written at a time when Dalton was aware of Davy’s claim for Higgins as well, a point often overlooked by historians. Thus, Dalton would probably have chosen these words regarding the origin of his theory very carefully:

I remember the strong impression which at a very early period of these inquiries was made by observing the proportion of oxygen to azote, as 1, 2 and 3, in nitrous oxide, nitrous gas, and nitric acid, according to the experiments of Davy.32

Considering his statement, it is hard to dismiss how Dalton could have read through the section on nitrogen oxides in *Epitome* yet miss the description of Higgins’s experiment on the production of ammonia, which in the first three editions of *Epitome* immediately precedes the section entitled ‘Gaseous Oxyd of Azote—Nitrous Oxyd of Davy’, and in the second and third editions, makes reference to page 309 of *Comparative View* a few lines below (figure 2). Ammonia, which was the only compound of nitrogen and hydrogen known at the time, should have been of great interest to Dalton; indeed, his laboratory
notebook shows that he was referring to Austin’s 1788 paper on the composition of ammonia at the very moment that he came up with his first table of atomic weights.\textsuperscript{34,35}

We do know that Dalton read several foreign periodicals from the Royal Institution Library in London during his December 1803 lectures.\textsuperscript{36} Although \textit{Comparative View} was accessible at that library during his July 1803 trip,\textsuperscript{37} there is no way of knowing whether he referred to the volume.

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\section*{The Case for William Austin}

Roscoe and Harden have noted that Dalton’s laboratory notebook shows that he was referring to Austin’s 1788 paper on the composition of ammonia just a page before his first table of atomic weights appeared on 6 September 1803.\textsuperscript{38,39} His use of Austin’s data has been interpreted as a method to double-check his atomic weights, as opposed to demonstrating any theoretical insight into chemical combination that Dalton might have obtained from Austin.

But the question arises of why Dalton was using a paper written in 1788, when in his \textit{New System} he described Davy’s analytical results on ammonia, published in 1800, as follows: ‘This conclusion [Davy’s study] was so nearly agreeing with the previous determination of Berthollet, that both have justly been held up as specimens of the accuracy of modern chemical analysis.’\textsuperscript{40} Thomson reached the same conclusion in 1802, noting in his \textit{System of Chemistry} that the ammonia results obtained by Berthollet ‘have been still farther confirmed by those made more lately by Mr Davy.’\textsuperscript{41} In spite of this, Dalton waited for two years, not making the switch to Berthollet’s data until 1805.\textsuperscript{42}

In fact, Austin did not report any experimental data of his own—he \textit{calculated} the composition of ammonia from the specific gravities reported by Kirwan for the gaseous
compound and its constituents. At the time, researchers could not obtain ammonia by the direct combination of nitrogen and hydrogen, and Austin’s approach using physical data to demonstrate chemical combination should have held a strong appeal for Dalton the physicist and meteorologist, in comparison with Davy’s and Berthollet’s decomposition of the compound. Austin’s conclusion to his paper suggests why Dalton initially chose to use it over Davy’s or Berthollet’s results:

If we consider the great difficulty of obtaining these specific gravities with exactness, we must be pleased to find so near a concurrence [with Berthollet’s results], and place more confidence in experiments on the specific gravities and combinations of æ¨riform bodies, than has generally been given them.43

In his paper, Austin attributed the distance between the particles, which he based on the specific gravities of the gases, as the obstacle to the direct combination of nitrogen and hydrogen in the gaseous state. The particles were simply too far apart, the attraction between the particles too weak, to overcome the force with which the particles held their repelling caloric. Austin stated:

The specific gravity of inflammable air being eleven times less than that of phlogisticated air, the distance of its particles must be greater than the distance of the particles of phlogisticated air in the proportion of \( \sqrt[3]{11} \) to 1, if the elementary particles of the two airs be of equal magnitude [italics added]…44

And as if Dalton were responding to Austin’s remarks, following the heading ‘Observations on the ultimate particles of bodies and their combinations’ on page 244 of his notebook, he stated the following on page 246:

Though it is probable that the specific gravities (sic) of different elastic fluids has some relation to that of their ultimate particles, yet it is certain that they are not the same thing [italics added]; for the ult. part. of water or steam are certainly of greater specific gravity than those of oxygen, yet the last gas is heavier than steam.45

Two pages later, Dalton came up with his first table of atomic weights. Meldrum cites another comment in Dalton’s laboratory notebook, which Thackray dates to September 1805,46 providing further support to the notion that Dalton was replying to Austin in September 1803. In this instance, Dalton specifically referenced the cube-root formula used by Austin and repeated his 1803 observation that water vapour is lighter than oxygen. After first posing the proposition that all gaseous atoms have the same weight, a reiteration of Austin’s position, Dalton continued:

According to the first [the proposition that all gaseous atoms have the same weight] the gases of greatest specific gravity are those whose particles are closest and the diameters of the elastic particles will be as the cube root of the sp. gr. This cannot be true for nit. gas which is made up of azot and oxygen is lighter than oxygen itself; and so is aq. vapour than oxygen one of its constituents.47

In the context of Austin’s paper and the heading on page 244 of his notebook, it seems that the concept of relative atomic masses sprang from a consideration of the use of specific gravities to investigate chemical combination, not the solubility of gases in water as suggested by Nash.48 Once Dalton realized the implications of Austin’s paper, that the specific gravities of the particles and gas were different, he seems to have applied this concept to his other research problem, the solubility of gases.49
THE CASE FOR BRYAN HIGGINS

The issue of whether Dalton was aware of Bryan Higgins’s work has received much less attention, overshadowed by his nephew William Higgin’s claims to the atomic theory that Bryan never pursued. However, in 1939 Partington stated the following about Dalton’s theory of chemical combination and multiple proportions in relation to Bryan’s theory of caloric and saturation, a one-to-one combination of atoms:

Dalton’s theory is, therefore, not based on arbitrary rules but on the Newtonian atomic theory; it is an extension of that of Bryan Higgins and makes use of similar ideas, although there is no reason to think that Dalton was indebted to Higgins.50

But contrary to Partington, on the basis of details uncovered in the diary of William Allen—Dalton’s friend, fellow Quaker and chemist—there is indeed reason to suggest that Dalton may have been influenced by Bryan’s work before he came up with his first table of atomic weights. The diary, published in 1846, mentions a meeting between Allen and Dalton in London on 10 July 1803 during Dalton’s summer holiday,51 a trip of which little is known and that occurred at a crucial moment in the narrative of the origin of the atomic theory.

Breakthrough on heat and a trip to London

From November 1802 to April 1803 Dalton was working on experiments related to the solubility of gases and the combinations of oxides of nitrogen. On 6 April 1803 he wrote in his notebook, ‘End of Expts. of this sort till after midsummer 1803.’52 The reason for the break is reflected in a letter that Dalton wrote to a fellow Quaker chemist, probably John Fell, on 12 April 1803. After describing his work on the solubility of gases and the publication of his works in Europe, Dalton wrote:

I am just upon the point of discovering something superior to any of those already published, & which may be of as much importance to science as that of Gravitation itself—I mean the nature of Heat & all its combinations with substances.53

Thackray has suggested that Dalton’s switch in research priorities might have been triggered by the appearance of a response in April to an earlier communication he had submitted on absolute zero to Nicholson’s Journal.54 Dalton was a lifelong believer in the existence of caloric, and the rest of the entries in his notebooks involved experiments on heat, the expansion of liquids and the properties of steam, reflecting his new line of investigation, which he continued until he took his annual vacation in the mountains of the Lake District some time after 5 July. However, before visiting Helvellyn, he took time to travel to London to meet his friend Allen (figure 3) in July 1803.55

Bryan Higgins’s influence on Allen

Allen was the son of a silk weaver and ended up running a chemical business at Plough Court with a laboratory at Plaistow.56 His interest in chemistry was fuelled by his association with Bryan Higgins. In a recapitulation of the year 1793, he notes in his diary, ‘I have attended some of Higgins’s lectures,—learnt something of short-hand, and the new system of chemistry, and instituted a plan for my future studies.’57 Allen must have been sufficiently impressed by the lectures because in the following year he became a member of Higgins’s
Society for Philosophical Experiments and Conversations, and at the close of 1794 Allen volunteered to serve on the Committee of Publication to oversee the distribution and expense of the minutes, which were written up by Higgins.58 Higgins’s society was disbanded by 1797, at which time several of its members, such as William Babington and Thomas Young, signed a letter requesting the Society of Arts to house the group, an effort that failed. Eventually, many of the members of Higgins’s society, including Allen, Babington and Young, migrated to the Royal Institution before Dalton arrived in London.59

The nature of Dalton’s and Allen’s relationship
At the time of their meeting on 10 July 1803, Allen had evidently been a friend of Dalton’s for several years, both having strong ties to the Society of Friends. Allen notes that in May 1790, he attended a Society of Friends meeting with John Gough,60 who was Dalton’s mentor. Three years later, Allen was a subscriber to Dalton’s Meteorological Observations.61 On the basis of a letter that Allen wrote to Dalton on 7 February 1809, their correspondence dealt not only with their experimental investigations, such as Allen’s and William Pepys’s classic work on respiration,62 but also with scientific news, books and the price of reagents.63 A letter written by Dalton to Allen in July 1809 provides a further indication of the closeness of the friendship, with Dalton confiding his uncertainty about an experimental finding related to his respiration investigations in comparison with
Allen’s results: ‘I cannot make up my mind upon it. I was glad to see your results on charcoal, &c., so clear and definite.’

After writing this letter, his indecision lasted for 16 months until he finally read an appendix to an earlier paper on respiration before the Manchester Literary and Philosophical Society in November 1810.

Seeking advice: a proposition to lecture at the Royal Institution

In January 1802, Allen was asked by William Babington, a physician and fellow member of Higgins’s society, for assistance in presenting chemical lectures at Guy’s Hospital, which he had given for more than a dozen years on his own. The Babington–Allen lectures were evidently a success, the 1802 syllabus was issued, and Allen received a letter from Humphry Davy on 4 July 1803 asking him whether he would repeat the Guy’s Hospital lectures at the Royal Institution as a solo lecturer. Allen evidently trusted Babington’s opinion, for on 7 July he consulted his co-lecturer, who advised him to accept Davy’s offer.

Three days later, Allen met with John Dalton, who was also under consideration for a lecture course, and the two discussed Davy’s offer. His diary entry for 10 July 1803 reads, ‘John Dalton of Manchester here at tea—conversed with him about the proposition from the Royal Institution to lecture there, he being one of those applied to.’ From a diary entry in 1802 Allen was anxious about his performance as a lecturer, which is why he sought out the advice of Babington and Dalton. However, he actually had little to be concerned about: as noted in his diary, he was able to provide an on-the-spot summary of the particles of matter at one lecture on the elements, which was very well received by his audience.

There is no way to know exactly what was discussed between the two men. However, in general terms, I think it fair to conclude that Allen’s meeting with Dalton probably covered the following: methods of lecture presentation, such as what level to offer the course at and whether to read off the syllabus, topics covered in similar meetings between Allen and William Farish in 1805; something of the content of Allen’s 1802 lectures, which landed Allen the proposal from the Royal Institution in the first place; and, on the basis of Dalton’s correspondence with Fell and Allen, his latest scientific activities, most certainly on heat, which he believed could be as significant as the law of gravity, and probably his studies on gas solubility and nitrogen oxides as well. As to the specifics of what Dalton might have learned from Allen regarding these subjects, I turn to the 1802 syllabus and the minutes of Higgins’s society, which provide some clues about Allen’s views and knowledge.

Oxides of nitrogen

Allen believed that nitrogen and oxygen combined in fixed proportions to form several compounds, as evidenced in the following entry in the 1802 syllabus:

Of the properties of Nitrous Gas—The most remarkable of these its re-producing Nitric Acid by the addition, and furnishing pure Azote by the subtraction of Oxygen.

The different forms of the Nitric Acid therefore considered as resulting from the union of these principles in different proportions.

100 parts of the Dry Nitric Acid said to consist of 68.06 Nit. Gas and 31.94 Oxygen.—
100 parts of the liquid Acid, of 90 Acid and 10 Water, and 100 parts of Nit. Gas of 46.6 Azote and 53.4 Oxygen, the cubical inch of the Gas weighing 335 thousandths of a grain.
The data on the composition of nitric acid and nitrous gas reported in the 1802 syllabus seem to come from experimental results described in Davy’s *Researches,* which Dalton stated influenced him early on in his investigations. Dalton, like most English chemists at the time, was certainly aware of Lavoisier’s ideas on the varying degrees of oxygenation of the nitrogen oxides as expressed in *Elements of Chemistry.* However, Davy concluded in *Researches* that Lavoisier’s analyses of several compounds consisting of nitrogen and oxygen were inaccurate, showing a greater proportion of oxygen than Davy had found. He tactfully noted that Lavoisier had conducted his experiments ‘at an early period of pneumatic chemistry.’

Babington’s and Allen’s lectures were geared to medical students requiring a practical knowledge of chemistry, and they adopted the prevailing view of affinity-based saturation, similar to Guyton de Morveau’s theory of varying proportions of acid–base saturation. As discussed by Mauskopf, Thomas Thomson addressed combining weights in an 1801 *Encyclopaedia Britannica* article and in the 1802 edition of his text *System of Chemistry,* which should have been familiar to Dalton. Thomson’s views on combination were ambivalent at the time of Dalton’s meeting with Allen, at first expanding on Guyton’s views of varying proportions of saturation in support of Proust’s theory of definite proportion in his 1801 article; however, by the publication of his 1802 text, he was shifting towards a theory of combination of indefinite proportions espoused by Berthollet.

Similarly to Thomson, Dalton seemed to be just as indecisive regarding combination in fixed proportions before his meeting with Allen, as least as far as the nitrogen oxides were concerned. On 21 March 1803 he stated: ‘Nitrous gas—1.7 or 2.7 may be combined with oxygen, it is presumed.’ And on 1 April 1803, just before he gave up the study of such combinations until after his holiday, he conducted several experiments investigating the effects of slow or rapid mixing on nitrous gas and common air, ending with the statement: ‘Query, is not nitrous air decomposed by the rapid mixture?’ I suggest that Dalton would have respected his friend Allen’s scientific opinion on the fixed proportions of the nitrogen oxides, as reflected in their scientific correspondence on respiration.

**Caloric and saturation**

Because the 1802 syllabus was oriented towards medical students, theoretical discussions of heat seem to be limited to a few sections, for example ‘Of the various opinions which have been entertained with regard to the nature of Caloric.’ This heading seems to mark the point in the lectures where Allen would have addressed the theories of Bryan Higgins, such as those discussed when he attended the meetings of Higgins’s society.

A quick review of the headings and subheadings for many of the 21 meetings of Higgins’s society held during the 1794 lecture season shows how important the subject of caloric was to the group: about half of the sessions discussed propositions dealing with the nature of caloric and its combinations. For example, of particular significance to Dalton’s new investigations was the proposition ‘Caloric, whose Parts are repellent of each other, combines with divers Bodies’, which was discussed for three weeks at the opening of the meetings, because the concepts of caloric and saturation were fundamental principles that laid the foundation for the ensuing discussions of the society. Even at the meetings in which the proposition did not specifically mention caloric, the nature of caloric and its combinations with matter, the very breakthrough that Dalton was pursuing, were intricately woven throughout the entire course of meetings, not only in the discussions but
also in the numerous experiments that were conducted for illustration. It is no wonder that Partington stated, ‘The Minutes of the Society dwells in tedious detail on the nature of “caloric.”’ Allen, who attended some of Higgins’s lectures, was a member of the society and served on its publication committee; he would have been thoroughly conversant with Higgins’s theories on caloric and saturation.

Higgins’s treatment of saturation as expressed in the minutes was relevant to Dalton’s investigations on heat, on the combination of nitrogen oxides and, significantly, on gas solubility as well. For example, after citing the combination of ammonia with hydrogen chloride in the minutes, an example that Higgins used frequently to illustrate the concept of saturation as a one-to-one combination of atoms, he further described saturation as one that is applicable to various phenomena:

This was exemplified in the mixture of ammoniacal gas and Carbonic acid gas, of nitrous gas and oxygen gas; in absorptions of gasses by water [italics added]; in combinations of acids with kali, natron, earths and metals; in all which, considerable quantities of Caloric were liberated from the portions combined, but not from the superfluous portions. Although Higgins’s examples of saturation dealt with binary combinations of atoms, in at least one instance—which, significantly, involved the absorption of gases by water—he did allude to combination of more than two particles to form ternary or higher compounds:

As to the gasses which thus act on each other spontaneously, or enter water to the exclusion of the described quantity of Caloric [italics added]; it is to be remarked, that their central parts are not ultimate particles, but that they consist of two or more particles of different kinds combined [italics added] and encompassed with Caloric.

Higgins held a fundamental belief in the material nature of heat, particles of which surround a central particle to form a globular atmosphere of repellant caloric, which is responsible for the elasticity of gases. For example, Higgins included the following statements about caloric at the very first technical meeting of his society:

The general laws of attraction and combination, and all the known phenomena of Caloric, lead us to this inference; that attractive particles engage around them into a state of combination, as much Caloric as the mutual repulsions of its parts will permit to be thus approximated towards each other [italics added]; and also engage further portions around the former, but with forces and densities decreasing, as the distances from the central particles increase.

Dalton’s explanation for combination based on his first law of mixed gases, which he detailed in his 1811 response to Bostock, bears a resemblance to Higgins’s attractive particle surrounded by caloric particles, except that Dalton added repellant atoms B to a central atom A, each addition causing the repellant atoms to be situated as far as possible from one another, ‘...till the repulsion of the atoms of B...refuse to admit any more.’ For example, Dalton believed that two atoms would be located 180° apart, three atoms 120° apart, and continuing until the repulsive forces become too large to accommodate any more additions.

Higgins described gases that would not react spontaneously when mixed as follows:

But there are gasses, whose bases have not hitherto been decomposed, or found to consist of more than one kind of matter. Such bases are Azot, Hydrogen and Oxygen; each particle of which engages Caloric on all sides. These gasses presented to each other in
any proportions, will not combine to the exclusion of any part of their Caloric, until something is done to blend the charges of it.91

According to Higgins, in a mixture of hydrogen and oxygen the attractive forces are insufficient to overcome the repulsive charges until an outside agent such as an electric spark is applied to initiate combustion, resulting in combination to form water, and the exclusion of caloric to emit heat and light. This is similar to the explanation that Dalton employed for the combination of hydrogen and oxygen in his 1811 response to Bostock.92

On the basis of these examples, Dalton would have been introduced to Higgins’s views on caloric in July 1803, which, as noted by Fox,93 were similar to those adopted by Dalton to formulate his second law of mixed gases, which Dalton stated he did not adopt until 1805.94 If my suggestion is correct, Dalton stuck to his own first law of mixed gases to explain combination for two years, hesitant to abandon it because of its ‘beautiful’ application,95 until switching to a Higgins-like theory to explain its drawbacks. This scenario is reminiscent of Dalton’s handling of the composition of ammonia, sticking with Austin’s calculated results based on specific gravities before switching to Berthollet’s data, which he should have been aware of for two years.

However, in 1811, when Dalton provided his first public explanation of his theories of combination in response to Bostock, he used both his first and second laws, the first law to explain his rules of combination and the second to illustrate the reaction between hydrogen and oxygen. As noted by Fleming96 and Cole,97 this is an inconsistent and irreconcilable approach, the first law assuming no attraction between unlike particles, and the second law being based on the opposite stance. This seems to be another example of Dalton’s inability to make up his mind, as illustrated in his letter to Allen on respiration, and the lack of clarity in his theories and experimental work as noted in Henry’s letter to Marcet in 1805.

CONCLUSIONS

The cases for William Austin’s and Bryan Higgins’s influence on Dalton are stronger than that for William Higgins. Austin’s paper, which we know was in Dalton’s hands at a crucial moment in the development of the atomic theory, may have sparked Dalton’s interest in relative atomic masses. As for Bryan, unlike his nephew William Higgins, there is no memo written by Henry to be explained away, nor is it necessary for Dalton to have accessed any of Bryan’s books, although they were in the collection of the Royal Institution Library during his visit to London. Dalton may have known about William’s Comparative View, but there is no evidence that he read it.

Three months before his meeting with Allen, Dalton believed that his new work on the combination of heat with matter could be as important as the law of gravity, and he certainly would have discussed his new research agenda with his friend Allen, just as he had mentioned his investigations to Fell. Allen attended Bryan Higgins’s lectures, was a member of his society, and joined the society’s publication committee, which oversaw the distribution of Higgins’s caloric-focused society minutes. Allen had the ability to summarize key concepts on the spot, as noted in his diary entry for his 1802 lecture, and his scientific correspondence with Dalton showed a free exchange of information. Considering all of these factors, it is difficult to argue how Dalton could emerge from his meeting with Allen without learning something of Higgins’s theories on caloric and saturation, which were directly related to Dalton’s new research on heat. The meeting
seems to have had some impact on Dalton, whether out of friendship, respect for his friend’s opinions or scientific import: Dalton kept a copy of the 1802 syllabus in his library until his death, 41 years after his meeting with Allen.  

After leaving London, Dalton travelled to the peak of Helvellyn, where he took meteorological measurements on 27 July; a few days later he was back in his Manchester laboratory. On 4 August 1803, about three weeks after his meeting with Allen, he wrote the following in his notebook, an entry that has been the focus of considerable debate over the years: ‘It appears, too, that a very rapid mixture of equal parts com. air and nitrous gas, gives 112 or 120 residuum. Consequently that oxygen joins to nit. gas sometimes 1.7 to 1, and at other times 3.4 to 1.’

Historians, such as Partington, Nash and most recently Usselman et al., have reconstructed Dalton’s experiment, concluding that Dalton must have been seeking evidence of multiple proportions to come up with his results so quickly; Rocke has suggested that his ‘great idea’ of multiple proportions might have occurred to him in the summer of 1803—a date that coincides with Dalton’s meeting with Allen.

Assuming my arguments to be correct about Dalton’s knowledge of Bryan Higgins’s theories, it is conceivable that Dalton might have expanded Bryan’s views on caloric and saturation, a one-to-one union of atoms, to develop the theory of multiple proportions, similarly to but independently of William, who also knew of Bryan’s work. Regardless, there is no uncertainty in rendering a verdict on Partington’s and Kelham’s positions that ‘…there is no reason to think that Dalton was indebted to Higgins’, or ‘Dalton’s ideas were almost certainly derived independently of this group [the London atomists] …’, respectively. Dalton’s ideas did resemble Higgins’s in many respects, and Dalton’s meeting with Allen might represent a ‘missing link’ in the development of the atomic theory, a ‘Daltonian doubt’ regarding Bryan Higgins, but one not as readily resolved as those raised 50 years ago by Guerlac and Siegfried about the influence of Richter and James Smithson, respectively.

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NOTES


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14 Wheeler and Partington, *op. cit.* (note 5).

22 W. Henry to C. Babbage, 19 February 1832, Add. MS. 37186, ff. 260 and 262, Babbage Papers, British Library; referenced with permission.
26 J. Bostock, ‘Remarks on Mr. Dalton’s hypothesis of the manner in which bodies combine with each other’, Nicholson’s J. 28, 280–292 (1811).
29 Quoted in Thackray, op. cit. (note 21), p. 150.
30 W. Henry, An epitome of chemistry, 1st, 2nd and 3rd edns (J. Johnson, London, 1801, 1801 and 1803); 4th edn (J. Johnson, Edinburgh, 1806).
33 Henry, op.cit. (note 30), 1st edn, p. 64; 2nd edn, p. 65; 3rd edn, p. 77.
34 Austin, op. cit. (note 8). Austin’s paper refers to William Higgins’s experiment on ammonia described in Epitome and Comparative View without mentioning Higgins by name, but it is unknown whether the reference has any significance for Dalton’s awareness of Higgins’s work.
37 Comparative view was presented to the Royal Institution Library by Frederick Kannmacher on 19 March 1800. In addition, Bryan Higgins’s Experiments and observations relating to acetic acid was donated by Rumsford as part of the original library. Personal communication from Jane Harrison, Royal Institution, 12 and 14 June 2013.
38 Roscoe and Harden, op. cit. (note 2), p. 85.
39 Austin, op. cit. (note 8).
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42 Roscoe and Harden, op. cit. (note 2), pp. 83 and 85.
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44 Ibid., p. 382.
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46 Thackray, op. cit. (note 9), pp. 44–46.
48 Nash, op. cit. (note 2).
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61 J. Dalton, Meteorological observations and essays (W. Richardson, London, 1793), at p. xi.
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68 W. Babington and W. Allen, A syllabus of a course of chemical lectures read at Guy’s Hospital (W. Phillips, London, 1802).
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82 Ibid.
84 B. Higgins, op. cit. (note 58), pp. 13–70.
87 Ibid., p. 35
89 B. Higgins, op. cit. (note 58), pp. 21–22.
93 Fox, op. cit. (note 88), pp. 11 and 111–112.
94 Roscoe and Harden, op. cit. (note 2), p. 16.
95 Ibid.
100 Roscoe and Harden, op. cit. (note 2), p. 38.
106 Kelham, op. cit. (note 3).
107 Thackray, op. cit. (note 9).