ANNALS OF SCIENCE

A QUARTERLY REVIEW OF THE HISTORY OF SCIENCE SINCE THE RENAISSANCE

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VOLUME 10

1954

TAYLOR & FRANCIS, LTD.

RED LION COURT, FLEET STREET, LONDON, E.C. 4

1954

ISAAC MILNER AND THE JACKSONIAN CHAIR OF NATURAL PHILOSOPHY.

By L. J. M. Coleby, M.A., M.Sc., Ph.D.

When Richard Watson resigned the Chair of Chemistry in Cambridge on his election as Regius Professor of Divinity in 1771, there were five candidates for the vacant post, and the appointment of the new Professor became a fiercely contested issue of internal University politics, in which the great rival colleges of Trinity and St. John's took a leading part. There was some preliminary manœuvring for position. Hitherto the election had been by simple Grace of the Senate to the effect "that *A. B.* be elected Professor of Chemistry . . . ". Such a procedure was clearly unsuited for a contested election, especially when there were a number of candidates. In 1772 a proposal by Trinity that the election should be by simple majority vote was lost in the Senate, but a similar Grace was approved in November 1773 [1]. By this time three of the original candidates had dropped out, leaving Isaac Pennington, of St. John's, and William Hodson, of Trinity [2].

On the day of the election, 13 December 1773, many non-resident members of the Senate, including four peers and three bishops, came up to Cambridge to record their votes, and Pennington was elected by 148 votes to 129. The names of those voting for each candidate are recorded in the University Registrary's "Records Relating to the Professorship of Chemistry". Of those voting for Hodson, sixty-two were from Trinity and none from St. John's, while Pennington had sixty votes from St. John's and two from Trinity.

After all this, it is sad to have to record that Pennington, who held the Chair for nearly twenty years, was the only Professor of Chemistry in Cambridge who never lectured. In this he was not alone, however, for many other Professors at Cambridge in the eighteenth century treated their posts as mere sinecures. That this was possible was due to the fact that the formal teaching (mainly mathematics, with some classics and philosophy) for University examinations and exercises was in the hands of College tutors, who viewed with disapproval any poaching on their preserves. The result was, however, that in many non-examination subjects there was no teaching at all.

[1] University Grace Book *Δ*, 12.

[2] Isaac Pennington (1745–1817), M.A. 1770, M.D. 1777, Regius Professor of Physic 1793, Knighted 1796; William Hodson (1743–93), Vice-Master of Trinity College 1789–93.

Richard Davies [3] drew attention to this unsatisfactory state of affairs in his *The General State of Education in the Universities . . . Set forth in an Epistle . . . to the Reverend Doctor Hales by Richard Davies, M.D. Late Fellow of Queen's College in Camb^e^* (Bath, 1759).

Davies says :

> "The advantages, which foreign Universities have enjoyed above our own, have been chiefly owing to the number of their Professorships ; to the care their magistrates have taken, to fill their Chairs with the ablest men . . . in different branches of Science : But they chiefly excell in those of Medicine " (p. 9).
>
> "Our own Universities were never possessed of a sufficient number of Professorships to complete the circle of Science. . . . It may seem partial, to complain of the want of Professors in the particular Sciences ; when, in the present scheme of Education, the use of those that have been established, altho' not a few, is almost wholly laid aside. By what course of events this has happened, I cannot fully explain : But certain it is, that of late, the chief employment they undertake is to give an air of Dignity to the most exceptionable part of University Education : I mean, the Disputations " (p. 12).

Davies goes on to make a number of suggestions for reform : Masters and Fellows of Colleges should be relieved of the necessity of taking Holy Orders, and be allowed to retain their Fellowships on marriage. Fellowships should be limited to ten years and not be for life ; there should be more Professorships, especially in Science ; in view of the cost of apparatus for scientific work, the necessary funds should be provided from University sources. All these ideas are now commonplace, although the fulfilment of some of them had to wait for over a century.

In September 1782, there died the Reverend Richard Jackson, M.A., Fellow of Trinity College, who, in a will dated 1775 and codicil dated 1781, left to the Master and Fellows of Trinity College as Trustees certain property, the income from which was to be divided between the Head Gardener of the University Botanic Garden [4] and a Lecturer, Professor, or Demonstrator, in Natural Philosophy.

Jackson knew exactly what he wanted, and specified in great detail conditions to which the lectures should conform, the kind of experiments he wished to be undertaken, and his views on the method of treatment. He wanted to make sure that the lecturer should not neglect his duties, and to ensure this he directed that the lecturer, in order to receive his stipend,

[3] Richard Davies, M.A. 1734, M.D. 1748. Practised at Shrewsbury and later at Bath. Elected Fellow of the Royal Society 1738, but resigned two years later.

[4] A site between Bene't Street, Free School Lane, Downing Street, and Corn Exchange Street (now occupied by laboratories) had been given to the University in 1762 by Richard Walker, D.D., Vice-Master of Trinity College, for use as a Botanic Garden.

must present annually to the Vice-Chancellor a certificate, signed by at least twelve students, that the course had been duly given :

> " . . . after the whole course of thirty-six Lectures and thirty Experiments thus read and exhibited to twelve Students at least, and their respective personal signing the certificate mentioned in my Will, that the said Lecturer, before he receives the said Salary, or any part thereof, deliver, or cause to be delivered to the Vice-Chancellor of the University of Cambridge for the time being, two copies, fairly written, of one of the Lectures which had been read by him within the course exhibited that year, one of the said copies, signed and dated by the said Vice-Chancellor, and the Lecturer, to be laid up in the Public Library of the University and the other in the Library of Trinity College, annually for ever : such Lecture each year being different from that of the preceding year, and yet so connected in general as to form a rational series of experimental doctrines." [5]

The lectures were to be given in English, to be of not less than one hour in duration, and to " be read on thirty-six several days . . . yearly ". If they were omitted for a whole year, the lecturer vacated his place and another was to be elected.

The University accepted the bequest in 1783, after careful consideration of its implications. In the following year a Syndicate was appointed " to erect a building wherein the Professor of Botany and the Jacksonian Professor may deliver public lectures, providing they limit the cost of such building to £1500 ". This plain brick building faced with stone eventually cost over £1600, and consisted of a lecture-room 40 feet long and 28 feet wide, with a private room for the Jacksonian Professor at one end and a similar room for the Professor of Botany at the other [6]. It was erected at the south-east corner of the Botanic Garden, at the Downing Street end of Corn Exchange Street, and was the first University building specially designed for science teaching.

On 11 December 1783, Isaac Milner, of Queens' College, was elected first Jacksonian Professor of Natural Philosophy. Isaac Milner was born in Leeds in 1750, the third son of an unsuccessful business man. At Leeds Grammar School he showed great aptitude for classics, but on the early death of his father, Isaac was apprenticed in a woollen mill. His elder brother, Joseph, sent to Cambridge by the generosity of friends, obtained the Mastership of the Grammar School at Hull, and was able to secure Isaac as his usher. In 1770, at the age of twenty, Isaac was sent by his brother to Queens' College, Cambridge. He was Senior Wrangler in 1774, with the honourable distinction of *Incomparabilis*, took Holy Orders in 1775, was elected Fellow of Queens' College in 1776, Tutor in 1777, and

[5] J. W. Clark, *Endowments of the University of Cambridge*, Cambridge, 1904, p. 214.
[6] R. Willis and J. W. Clark, *Architectural History of the University of Cambridge*, Cambridge, 1901, vol. iii, p. 153.

President in 1788. He obtained the degree of D.D. in 1781, was appointed Dean of Carlisle in 1782, and was Vice-Chancellor of the University of Cambridge in 1792–3 and again in 1809–10. In 1798 he was elected Lucasian Professor of Mathematics. He died in 1820.

About 1779 Milner became very interested in chemistry but his incautious inhaling of some noxious gases led to a lung complaint from which he never entirely recovered. The following year he was elected a Fellow of the Royal Society, and in 1781 he was entrusted by Richard Watson, who had been his chemical tutor, with the carrying out of an experiment to verify Priestley's reduction of calx of lead by inflammable air (hydrogen), using a charcoal furnace as the source of heat, instead of a burning-glass [7], a variation desirable since some chemists, e.g. Macquer, had sought to identify phlogiston with light [8].

Milner was very fond of practical mechanics and in 1782 had a room at Queens' College fitted up as a workshop and private laboratory. Even Professors at that time had to find their own apparatus, by no means an inconsiderable burden. Thus we find Milner writing to William Wilberforce:

> " . . . I may as well have the office in question [Moderator] next year; especially as I don't think its profits will be found an inconvenience to me, when the bills come in for electrical apparatus, air pumps, furnaces, crucibles, &c. . . . Seymour will gravely tell you that the alchymists, notwithstanding their pretences, were always poor " [9];

and later in a letter to Ludlam in 1791:

> " I have been a great dabbler in air pumps, and have spent a great deal of money on them. I have now one by me, which cost 60*l* and upwards, exclusive of the apparatus." [10]

In 1784, Milner published *A Plan of a Course of Chemical Lectures*, and a second edition of this appeared in 1788. Milner also published *A Plan of a Course of Experimental Lectures introductory to the Study of Chemistry and other branches of Natural Philosophy*. This latter is undated, but cannot be earlier than 1784, since Milner is referred to on the title-page as Jacksonian Professor. It is probably the syllabus of a course of lectures delivered in 1782 [11].

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[7] R. Watson, *Chemical Essays*, vol. iii, Cambridge, 1782, pp. 373–5.

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[9] Mary Milner, *Life of Isaac Milner, D.D., F.R.S.*, London and Cambridge, 1840, p. 22.

[10] *Ibid.*, p. 70.

[11] Mary Milner, *op. cit.*, p. 15.

[12] Letter from Newton to Aston, a copy of which is preserved in the Letter Book of the Royal Society, vol. x, p. 28.

a society was again mooted, and this eventually led in 1784 to the foundation of "The Society for the Promotion of Philosophy and General Literature", many of the members of which were, or subsequently became, Professors [13]. It was proposed to publish original papers as "Tracts, Philosophical and Literary, by a Society of gentlemen of the University of Cambridge", but interest waned and the society was dissolved in 1786. More than thirty years later, Milner was one of the signatories to the calling of a meeting in 1819 at which the present Cambridge Philosophical Society was founded, with Professor Farish as its first President.

In May and June of 1786 Milner read before this short-lived society papers on "The Limits of Equations" and "On the Motions of the Moon's Apsides". According to his biographer these were subsequently re-read to the Royal Society and published in the *Philosophical Transactions* [14].

During the year 1786 Milner was engaged in experiments on nitric acid and the oxides of nitrogen. He communicated his results to Priestley the same year [15] and read a paper on them to the Royal Society on 2 July 1789 [16].

In the following account of Milner's experiments the contemporary nomenclature has been used. 'Nitrous Acid' was used indifferently for either nitric acid or nitrogen peroxide, which were assumed to be the same, 'nitrous air' was used for nitric oxide, and 'dephlogisticated nitrous air' for nitrous oxide. 'Volatile alkali' of course is ammonia.

That a relationship existed between nitrous acid and volatile alkali had been known for some time, and the latter had frequently been obtained from the former, but Milner knew of no case where nitrous acid had been obtained from volatile alkali.

Having heard of the production of inflammable air by the passing of steam through red-hot iron tubes, Milner tried similar experiments with

[13] Members included William Coxe, later Archdeacon of Wilts.; J. Jowett, Regius Professor of Civil Law 1782–1813; J. D. Carlyle, Professor of Arabic 1795–1804; William Farish, Professor of Chemistry 1794–1813, Jacksonian Professor of Natural Philosophy 1813–37; Richard Porson, Regius Professor of Greek 1792–1808; Thomas Martyn, F.R.S., Professor of Botany 1762–1825; Smithson Tennant, Professor of Chemistry 1813–14; Sir Busick Harwood, Professor of Medicine 1800–14; Samual Vince, Plumian Professor of Astronomy 1796–1821; and Francis John Hyde Wollaston, Jacksonian Professor of Natural Philosophy 1792–1813.

[14] "Observations on the Limits of Algebraical Equations" was read to the Royal Society 26 February 1778, and published the following year (*Phil. Trans.*, 1779, **68**, 380). Milner also published two other mathematical and astronomical papers (*ibid.*, 1779, **68**, 344, and 1780, **69**, 505). Neither of these, however, was on the motion of the Moon's Apsides.

[15] Mary Milner, *op. cit.*, p. 37.

[16] "On the Production of Nitrous Acid and Nitrous Air", *Phil. Trans.*, 1789, **79**, 300.

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nitrous acid. The latter was boiled in a small retort luted to a gun-barrel, the other end of which was immersed sometimes in water, sometimes in mercury. A mixture of nitrous air and phlogisticated air (nitrogen) was obtained in proportions varying with the degree of heat used and the rate at which the acid was boiled [17].

In order to increase the area of the surface of the iron exposed to the vapour, Milner filled the gun-barrel with iron filings, and then found that the gaseous product was almost entirely phlogisticated air, with only small amounts of nitrous air and dephlogisticated nitrous air. Milner felt sure that, if a long enough tube were employed, only phlogisticated air would be obtained.

Milner pointed out that these experiments were very similar to those of Priestley, in which nitrous air exposed to iron was converted first into dephlogisticated nitrous air and then into phlogisticated air [18], except that the process was much more rapid in Milner's experiments. That the experiments were in fact analogous, Milner showed by passing through his gun-barrels nitrous air, obtained by dissolving copper in nitrous acid, when the same result was obtained as before, but more easily. Nitrous air could be passed through a red-hot *glass* tube without chemical alteration.

In these experiments Milner frequently observed that, when the gas collected was entirely phlogisticated air, a white fume issued along with the air, and on examination he noticed the smell of volatile alkali. He recalled a similar observation by Priestley, when nitrous air had been exposed to iron, and speculated as to the possibility of reversing the process :

> " . . . as the iron turnings and the inside of the gun-barrel were left after the operation in a state of calcination, it seemed not unnatural to suppose, that by forcing volatile alkali through red hot calces of some of the metals, nitrous acid or nitrous air might be produced. It was not until May 1788 that the calx of manganese, on account of its very great infusibility, and its yielding an abundance of dephlogisticated air, occurred to me as a very suitable substance for the purpose " (*Phil. Trans.*, 1789, **79,** 305).

Milner filled a gun-barrel with the coarsely powdered calx of manganese and attached one end to a retort containing a solution of volatile alkali. On warming the latter and passing the vapour over the strongly-heated calx, he obtained considerable quantities of a highly nitrous air. Milner repeated the experiment on many occasions, and always obtained at least some nitrous air, although the actual yield varied. When the manganese was heated by itself, no nitrous air was formed, nor was any

[17] *Loc. cit.*, pp. 300–1.

[18] J. Priestley, *Experiments and Observations on Different Kinds of Air*, London, 2nd Edn., 1775, vol. i, pp. 215–25.

produced when steam was passed over the heated calx. Milner found that the experiment worked best when a strong alkali solution was used.

The presence of nitrous air was detected by the formation of red fumes on the admission of atmospheric air. Milner pointed out, however, that no nitrous fumes were formed on the admission of air, if there were also present in the flask volatile alkali which had come over unchanged (as sometimes happened, even when the products were collected over water), because under these conditions the red fumes immediately combined with the alkali to give white clouds of 'nitrous ammoniac' (*loc. cit.*, p. 307).

As many other substances were known to afford dephlogisticated air, it was natural to conclude that these substances also would convert volatile alkali into nitrous air. The experiment succeeded with calcined green vitriol at the moment when it was yielding plenty of dephlogisticated air, but with calcined alum the product, to Milner's astonishment, proved to be a large quantity of inflammable air mixed with some hepatic air (hydrogen sulphide) and actual sulphur, and the residue of alum also smelt of hepatic air and contained particles of sulphur.

All the above experiments were repeated and confirmed when earthenware tubes were used instead of iron tubes.

Milner's explanation of these reactions was as follows. Nitrous acid was a compound of phlogisticated air and dephlogisticated air, the union of which could be brought about by an electric spark, as Cavendish had shown. Nitrous air could be converted into phlogisticated and dephlogisticated air by heat. By the action of heat or of the electric spark, volatile alkali could be converted into phlogisticated air and inflammable air. If volatile alkali is used instead of inflammable air to reduce calx of lead, phlogisticated air is left. Clearly in his experiments the dephlogisticated air from the calx combined with the phlogisticated air of the alkali to give nitrous acid or nitrous air. Since nitrous air required the addition of dephlogisticated air to become nitrous acid, evidently the latter contained more dephlogisticated air than the former.

The lack of success with alum Milner attributed to 'double affinity': in order that the phlogisticated air of the alkali should combine with the dephlogisticated air of the calx, the metal of the latter must have an affinity for the inflammable air of the alkali, i.e., be reducible.

Having given his explanations in terms of the phlogiston theory, Milner adds:

> "Those who chuse to reject the doctrines of phlogiston must make the necessary alterations in these expressions; but the reasoning will be much the same" (*loc. cit.*, p. 313).

Milner was thus the first to oxidize ammonia to nitric acid. The matter became of rather more than mere academic importance some years later, when Britain was at war with France. A large amount of the nitre used in western Europe for the manufacture of gunpowder came from Asia, and France was blockaded by the British fleet. Richard Watson had earlier pointed out the necessity of denying nitric acid as well as nitre to an enemy who had sufficient potash [19]. How was France getting the large supplies of nitre she required to keep her armies furnished with gunpowder? We find Milner asking Kirwan for information on the French method of making gunpowder. Kirwan, in a letter from Dublin dated 25 October 1795, replied that he thought it might be by the method from volatile alkali discovered by Milner [20].

Milner was also in touch with Watson who, in a letter from London dated 18 February 1796, said that, while he thought that nitre could not be made from volatile alkali as cheaply as it could be imported, nevertheless the method was worth a large-scale trial by the Board of Ordnance, since volatile alkali could always be obtained from bones, and the supply might be considerably augmented [21].

Milner was considered a very good lecturer. His chemical lectures were always well attended, and what with him, and what with his German assistant, Hoffmann, the audience was kept in a high state of interest and entertainment. Milner claimed that nearly two hundred students attended his last chemistry course. It might perhaps be inferred that his lectures were ' popular ' rather than profound. No doubt in his experimental lectures he made good use of his opportunities for striking demonstrations, but it is clear both from his own accounts of his method of teaching and from certain still extant notes of lectures that his critical examination of theories, at any rate, called for close attention and careful thought. He wrote:

> " Lecturers in natural philosophy should ever be careful to impress, on the minds of their pupils, just notions of the meaning of laws of nature, and of the methods of arriving at them. A law of nature cannot always be clearly inferred from simple, easy, experiments; nor, on the other hand, is it ever to be considered as ' merely a deduction of reason. It must be proved, either at once and directly, by some simple and decisive experiments; or, if that cannot be done, by such experiments as enable us to collect its existence by the assistance of geometry ' ".
>
> " Thus, the experiments made on what are called the mechanical powers, by consequence, imply proofs of the second law of motion."
>
> " It was in this way, namely, from experiments and observations, and analytical conclusions from them, that Sir Isaac Newton demonstrated gravity to be really a law of nature." [22]

[19] Watson, *Chemical Essays*, vol. i, Cambridge, 1781, p. 254.

[20] Mary Milner, *op. cit.*, p. 109.

[21] *Ibid.*, pp. 109–10.

[22] Milner, *Strictures on some of the Publications of the Rev Herbert Marsh, D.D.*, London, 1813, pp. 221–2. The quotation by Milner is from his paper on the " Communication of Motion by Impact and Gravity ", *Phil. Trans.*, 1778, **68**, 352 (mispaginated 252).

Milner warned his pupils against the tyranny of theories :

> " In truth, hypotheses are cheap commodities, and become dangerous from the moment any strong attachment is formed to them. It is almost their only use, that they enable us to convey intruction upon system, and to devise new experiments for some definite purpose."
>
> " Many years ago, both in my public and private lectures, and especially in those on chemistry and natural philosophy, I constantly laboured to impress on the minds of my pupils true and distinct notions concerning the laws of nature and hypotheses. . . . In natural philosophy I had Sir Isaac Newton and his disciples for sure guides ; and in chemistry I reckoned myself peculiarly fortunate in having obtained an excellent MS of the lectures of the late Dr Black ; to whom as my preceptor in that branch of knowledge, I ever looked up with admiration and reverence—though I am well known to have always controverted his favourite explanation of the doctrine of latent heat." [23]

Milner quotes with approval the words of Black :

> " And I see it always happen, that, if the application of a system to the explanation of phaenomena be very comprehensive, leaving no blanks ; and if the explanation has some feasibility, *this catches the fancy*, it *dazzles the understanding*. Nay, we think it impossible that a principle that is false, can tally in so many phaenomena. This seeming coincidence is considered as proof of its validity, and we are no longer solicitious about the *direct proofs* adduced in the beginning. . . . This kind of authority accruing to a theory from its specious and extensive application to phaenomena is always bad ; and with mere beginners in philosophy it is doing them an irreparable mischief. It nourishes that itch for theory ; and it makes them unsolicitous about the first foundations of it. Thus it forms in their minds the worst of all philosophical habits." [24]

We can see how Milner put these ideas into practice by considering his teaching on Heat and Phlogiston. It will be remembered that the founder of the Chair of Natural Philosophy laid it down that the Professor should deposit with the Vice-Chancellor each year two copies of one of his lectures. The Cambridge University Library and the Library of Trinity College, Cambridge, each contain a copy of four of these lectures, " An Essay on Heat, Parts I–IV ", and the latter has in addition " Part the First of an essay on Phlogiston ". These five lectures cover the years 1784–8.

In the first of his lectures on Heat, deposited with the Vice-Chancellor on 9 October 1784, Milner starts to consider the three main theories held at that time concerning the nature of Fire and its effects.

> " . . . The first supposes that fire in its pure and disengaged State is a fluid, material substance consisting of a collection of simple,

[23] *Ibid.*, p. 224.

[24] *Ibid.*, p. 227. The original is in Black's *Lectures on the Elements of Chemistry*, ed. Robison, Edinburgh, 1803, vol. i, p. 548.

homogeneous particles, which are infinitely small, and have no sensible adhesion to each other, and are agitated continually in all directions with a very rapid motion " (*ff*. 5–6).

" The same particles of elementary fire are conceived capable of entering into the composition of bodies and becoming a constituent part of them. In this case the Igneous particles are supposed to lose their extreme mobility and by forming an intimate union with other parts of matter to become fixed. Then by reason of this new combination the fire is deprived of the properties which characterise it, and no sensible Heat is generated in the compound, which is formed by its admission; but instead of heat and expansion, and the other Effects of Fire, various properties of a different nature are now communicated, which in all cases are supposed to depend on its presence in this State of fixation. The igneous matter thus combined . . . is . . . the Phlogiston of the Chemists . . . " (*ff*. 9–10).

Milner then devotes some eleven pages to a consideration of the experimental evidence for the phlogiston theory.

The second theory of heat given by Milner is that fire consists of minute material particles (as before), but that these are incapable of entering into chemical combination with other bodies, and if present always exhibit the properties associated with sensible heat. According to this theory different bodies have different capacities for heat, so that the addition of the same quantity of heat may cause different increases of temperature. The lecture concludes with an account of early experiments on thermal capacities by the method of mixtures.

In " Part the second of an Essay on Heat ", received by the Vice-Chancellor on 31 December 1785, Milner continues his account of this second theory of heat. Those who hold it consider phlogiston to be " a very subtil matter of a very different nature from fire and *sui generis* ". While a good deal of evidence could be adduced in favour of the existence of phlogiston, as well as of its identity in all circumstances, it did not necessarily compel us to assume phlogiston to be the same thing as the element of Fire.

It was possible, for example, that the light and heat generated when a body burned in air came not from the body itself, but from the surrounding air, which was essential for the combustion. Milner referred to the work of Crawford [25] who, in experiments to discover the different capacities of bodies for containing fire (specific heats), found that dephlogisticated air

[25] A. Crawford (1748–95), physician at St. Thomas's Hospital, London, and later Professor of Chemistry at the Royal Military Academy, Woolwich, worked for a time in Glasgow under Irvine, and published in 1779 *Experiments and Observations on Animal Heat*, which included an account of much of the work of Black. For an account of this and other contemporary work on heat see D. McKie and N. H. de V. Heathcote, *The Discovery of Specific and Latent Heats*, London, 1935.

had a higher specific heat than atmospheric air, and this in turn than phlogisticated air.

> " In general, Crawford thinks, that the capacities of bodies for containing fire are diminished by the addition of Phlogiston and increased by the separation of this principle, for besides what has just been observed of Air, the calces of Iron, lead, tin and antimony were found to have greater heat capacities for fire than their Metals . . . " (*f.* 6).

Since, according to the phlogiston theory, a burning body gave up its phlogiston to the air that supported its combustion, the above results could be used to explain the heat liberated in the combustion, on lines similar to those used by Irvine in the case of latent heat, namely, that it was, as it were, squeezed out of the air when the heat capacity of the latter was diminished by the addition of phlogiston.

Milner then goes on to discuss the third theory of heat :

> " *Heat* consists in a vibrating motion of the parts of bodies, and *Fire* is a body so heated as to emit light copiously. The arguments against this Theory have of late Years been esteemed so numerous and weighty, that it has almost been given up by Philosophers ; nevertheless as I cannot but think it the most probable of any, and as some of the facts which are thought utterly inexplicable upon the System appear to me to admit of a more easy and natural Solution in this way than in any other, I could wish that somebody else had endeavoured to shew the truth of what I advance, and the probability of this doctrine of the cause of heat, by contrasting it with the two former opinions, and particularly the latter, which is now grown so fashionable among the chemists and is supposed to rest upon Experiments that directly overturn the hypothesis that heat consists in Motion.—However as nobody that I know of since the Publication of Dr Crawford's system has attempted this, I shall venture to produce what I have to observe on the Subject as clearly and as briefly as possible. . . . I am far from thinking the hypothesis I defend to be more than a conjecture . . . at present I am of opinion that some of our best natural Philosophers have rejected this doctrine of heat a little too precipitately, and embraced hypotheses, which are less probable " (*ff.* 9–11).

Proceeding to the evidence in favour of the theory, Milner begins by referring to the formation of heat by friction and percussion, and notes that it is also produced by chemical reactions in solution :

> " In the former instances, the heat was produced by strong frictions and percussions, and in these latter by the union of the small particles of bodies, but in both it seems probable that an intestine motion of the substances should happen ; for the very same powers, which in some instances produce these frictions and percussions, in other circumstance of bodies rubbed or struck are known to produce sensible motions ; therefore we have the greatest reason to believe from analogy that numerous and rapid motions are still produced among the constituent

> particles of the bodies, though the size of the particles of matter moved, and the very short spaces in which the Vibrations are performed may render these Motions imperceptible to the Senses " (*ff.* 14–15).

After considering further arguments in favour of the theory, such as the simple explanation it afforded of thermal expansion, and the experimental fact that the weight of bodies did not appear to be increased by heat, however great, Milner considered various objections against the theory :

> " (i) That such vibrations have never in reality been proved to exist and although their Existence were allowed, they would not agree with the Phaenomena (ii) That Motions pass very quickly through the whole mass of bodies and even in a moment when the bodies are elastic, whereas heat moves more slowly like a liquid (iii) A given quantity of heat ought to be divided among bodies in proportion to their quantity of matter, which is contrary to fact. 4th Another Objection is, that in general Heat is by no means proportional to motion " (*ff.* 20–1).

To the first of these objections Milner replied that the ability of a given hypothesis to explain a particular phenomenon was to a considerable extent a matter of personal opinion. He himself thought the motion theory would explain this better than any other, as he hoped would be evident when he had considered the phenomena in detail. He thought that the evidence for the existence of molecular motions was as good as that brought forward for the existence of a " material igneous substance " (*f.* 21). To the second objection Milner replied :

> " I think it not true that motions in all cases pass very rapidly through the mass even of an Elastic body : Are not motions communicated gradually to the Air the most elastic body in Nature ? And if it be said that the Air is not a simple body, but consists of many bodies, and that therefore it requires time to communicate the motions from one to another ; I ask, why not may other bodies, as well as the Air, be considered as made up of a number of lesser elementary bodies, and why may not time be necessary to communicate motions of a particular sort from some of them to others, which are removed to a considerable distance ?—I say Motions of a particular sort ; for it is absolutely necessary here to make a distinction : If I strike the head of a nail, however long it may be, with a hammer and force it into a piece of wood ; as long as the nail continues to enter the wood, the Motion communicated to the head of the nail by the hammer is instantly communicated to the point, and to all parts of the Nail, which in this case move all together ; but then we are to observe little or no heat is communicated to the nail by these percussions ; Now let the Nail be driven up to the head, and then let it receive some smart blows from the hammer ; In these circumstances, when the nail can enter no farther, why may not the same power which before produced an instantaneous motion of the whole Nail, now produce a vibratory motion of its minute parts, beginning at the head and gradually proceeding forwards ? And in fact the nail now grows exceeding hot as Mr Boyle long ago observed. It is in this Vibratory or oscillatory

motion, and not in any motion that heat is supposed to consist. Nor is such a distinction merely imaginary. . . . May not very considerable Motions, for example, be communicated to the Air without much sound being produced ? and again may not very considerable sounds be produced by vibrations of a particular sort, which cause not much sensible agitation of the Air ? " (*ff.* 22–5).

Milner admitted that the ratio of the specific heats of two substances was not in general the same as that of their densities, but argued that in any case it did not necessarily follow that the ratio of the specific heats of two bodies had to be proportional to the amounts of vibratory motion in these bodies. Moreover specific heats were determined by the method of mixtures, on the assumption that one body gained exactly as much heat as the other lost. This assumption might not in fact be true. Milner continued :

" To any one who makes the fourth objection, I grant very readily that Heat is not observed to be proportional to motion in general, nor even to every kind of vibratory motion ; but then as very different sorts of vibratory motion will easily be conceived to exist by any one sufficiently versed in mechanicks to know the Effects of the combinations of attracting, repelling and impulsive forces, I deny that there are any facts which prove that it is not proportional to a particular kind of oscillating motion in which heat may essentially exist " (*f.* 29).

To meet the difficulty of the absence of heat when oil or grease is used to prevent friction, Milner argued :

" Here, I suppose, the moving forces produce little or none of those tremors or agitations in which heat consists, but on the contarary they communicate motions of a very different sort to the particles of these slippery substances."

To the objection that, according to this account, these oily and greasy matters are incapable of receiving vibratory motions and consequently are incapable of being heated, Milner replied :

" . . . this account only shows that the same causes acting in different circumstances may produce different results . . . an attrition or collision, which produces a great quantity of igneous Vibration and perhaps very little of any other sort of motion or vibration, when oil or grease is not made use of, may when such Substances are added produce the opposite effects viz a great quantity of other sorts of Motion or Vibrations and little or none of that particular Sort in which Heat consists " (*ff.* 34–6).

Milner quoted the objection of Bergman that owing to resistance all kinds of motion on the earth were liable to decay, so that the small motion made use of to ignite a spark could not increase to burn not merely a house but even a whole town. In reply he pointed out that the same objection arose even on the fluid theory, since the small quantity in the spark was assumed to be capable of liberating large quantities from other bodies :

> " I reply—That the Spark consists of a very small quantity of matter, the parts of which are agitated with prodigiously rapid oscillations and vibrations, that similar oscillations are communicated by it to a particle of an inflammable body, which according to our Theory instantly conceives an intense Heat ; and that in this state of Heat the particle lets loose to the Air its Phlogiston ; That the act of suddenly discharging to the Air a quantity of Phlogiston from the particle agitates its parts with new and increased oscillations by the action of the attractive and repulsive powers of the inflammable bodies and of the air ; and that by repeating this process, a combustion of any magnitude may be raised " (*ff.* 40–1).

Returning to the objection that all motion decays on earth, Milner admitted that the sensible motions did decay, but that it was necessary to consider *how* the resistances acted in destroying motion :

> " A Resistance merely from the Vis inertiae of matter is acknowledged to be attended with a communication of Motion to the parts of matter ; and it is obvious that in the resistance from the attrition of a Solid or the Tenacity of a fluid, a communication of the same nature must often happen though in a different degree " (*f.* 42).

Mathematicians knew the difficulty of computing the result of forces between even a few bodies simultaneously. How difficult was the case with innumerable attracting and repelling particles :

> " . . . can we doubt . . . that the sensible motions of bodies appear to us to decay and even to stop, in many instances when they are only resolved or compounded and melted down into motions of very different sorts, very rapid and at the same time imperceptible on account of the short spaces in which they are performed ? . . . Newton in his 31st Query, when he Enumerates the Tenacity of fluids and the Attrition of their parts, together with the Elasticity of Solids as reasons, why Motion is much more apt to be lost than got, and is always upon the decay, does not appear to me to consider those very minute motions, tremors and oscillations of different sorts, which may remain in great quantity after all the greater and obvious motions have entirely ceased " (*ff.* 43–5).

Milner began the Third Part of his Essay on Heat (deposited with the Vice-Chancellor on 31 December 1786) by considering the objection that, if fire were a form of motion, it could not spread, since the quantity of motion in a system of bodies was not affected by their mutual actions and collisions, one body always losing what another gained. To this he replied :

> " . . . it is only the quantity of motion estimated in a given direction, that remains invariable in all collisions and mutual actions of bodies upon each other . . . quantity of absolute motion may be conceived to increase prodigiously during their mutual actions ; whilst its quantity estimated in a given direction remains the same. . . . Newton observes, that if two globes joined by a slender rod revolve about their common center

of gravity with a uniform motion, while their Center moves on uniformly in a right line drawn in the plane of their circular motion The sum of the motions of the two Globes as often as they are in a right line described by their common center of gravity will be bigger than the Sum of their Motions when they are in a line perpendicular to that line, and by this Instance, (says he) it appears that Motion may be got or lost."

"These reflections seem justly applicable in this place to weaken Professor Bergman's argument, though I still think that the full refutation of it depends on estimating the effects of the attractive and repulsive powers, which intercede the very small particles of bodies" (*ff*. 2–4).

Experiments showing the passage of heat through a vacuum were thought by many to be unanswerable in terms of the kinetic theory of heat. Milner pointed out that in the first place there were weaknesses on the purely experimental side: it was impossible to get a perfect vacuum—some air must be present and possibly exhalations from other bodies. Moreover, the thermometer had somehow to be suspended:

> "I have frequently thought it remarkable that this Experiment should be quoted so often on the present occasion from Newton and no mention be made of the conclusion which that eminent Philosopher drew from it. Is not the heat, says he, of the warm room conveyed through the Vacuum by the vibrations of a much subtiler Medium than Air, which after the Air was drawn out remained in the Vacuum? and Is not this Medium the same with that Medium, by which light is refracted and reflected, and by whose Vibrations light communicates heat to bodies, and is put into fits of easy reflexion and easy transmission, and do not the vibrations of this Medium, in hot bodies contribute to the intensity and duration of their heat?" (*ff*. 6–7).

If supporters of the material theory of heat objected to the assumption of such a medium (an assumption which in any case was not *essential* to a mechanical theory of heat) or pointed out that it was just as big an assumption as their own concept of a particular fluid, Milner retorted that the hypothetical medium had a much wider field of use, since it could be employed to explain a great many other phenomena of nature—gravity, reflexion and refraction of light, vision, and so on:

> "I have no objection in Philosophy to a bold conjecture, which with some appearance of Probability accounts for a prodigious number of Phaenomena; but I have a very great one to such Conjectures, when with an equal probability they only account for particular facts or operations of nature—Newton was too much of a sound Experimentalist to indulge himself often in Conjectures; and when he did, he has directed us by his example to frame an hypothesis as extensive as possible in its application" (*ff*. 8–9).

Milner pointed out that his theory of heat was independent of any particular hypothesis as to the nature of phlogiston, except only that phlogiston could not be heat itself in its fixed state. Hence the theory could be held by phlogistonists or anti-phlogistonists alike—only the most trivial alteration in the explanation would be necessary (*f.* 11).

After referring to Macquer's theory that phlogiston might be the material of light [26], Milner gave his own view of the nature of light, which he assumes, with Newton, to consist of very small particles. The impact of these particles on other bodies could excite the vibrations of heat, and this, Milner thought, was the only real and substantial connexion between heat and light:

> "The rays of light striking bodies with vast Velocities, may like other causes of motion, as frictions, Collisions, or the attractive Energies exerted in chemical affinities, excite oscillations in bodies, that may heat them; and in return, Bodies, when sufficiently heated, that is bodies, whose parts are sufficiently excited, and put into a vibrating state, either by the rays of light, or by any of the aforementioned causes may emit light and shine, and this emission may be performed by the vibrating motion of their parts; and thus bodies and light may act mutually, on each other, Bodies upon light in emitting it and Light upon bodies in heating them" (*ff.* 16–7).

In support of the view that bodies received much of their activity from light, Milner pointed out that black bodies conceived heat from light more readily than bodies of other colours because less light was reflected [27], and that the light of the sun concentrated by a large mirror produced no sensible heat in the air:

> ". . . it plainly proves that the particles of light are not the same thing with the proper cause of heat, so it demonstrates likewise that heat does not consist in motion of every kind: We know that the rays of light move with immense velocities, and yet no heat seems to be excited by them till these rapid motions are stopped and converted into motions of a different sort, into tremors and vibrations; and this power of producing these subtile movements by receiving and modifying the motions of the particles of light, the Air seems to possess in a very small degree; But as soon as the particles of light reach the surface of the Earth and impinge upon it, immediately this tremulous motion is generated; partly by the percussive forces of these rays, which are reflected after impact, and partly by the diversified action of other rays, which are absorbed and suffocated. . . . Hence we may understand that the sensible heat of the surrounding atmosphere near the surface of the Earth is produced not immediately by the action of the Sun, but by the Earth itself, which is being heated by the rays of the Sun" (*ff.* 19–21).

The remainder of Part Three of Milner's Essay was devoted to a clear and detailed account of the phenomena of latent heat and of the views of Black on the nature of latent heat.

[26] P. J. Macquer, *Dictionnaire de Chimie*, Paris, 1778, Art. "Phlogistique", vol. iii, p. 123.

[27] Richard Watson had made experiments in Cambridge in 1772 on what was, in effect, a black-bulb thermometer (*Phil. Trans.*, 1773, **63**, 40).

Part Four of the Essay was received by the Vice-Chancellor on 12 October 1787. It opens with an account of the freezing mixture of nitric acid and snow and of its explanation in terms of latent heat. Milner then goes on to describe latent heat of vaporization, including instances at boiling points under reduced pressures. Having first given Black's views on these phenomena, he proceeds to describe briefly Irvine's explanation of them :

> "That different bodies have different capacities for containing heat seems a principle sufficiently proved from the Experiments related under the description of the 2nd opinion concerning heat—Now if we can solve all these Phaenomena, which are ascribed to the operation of latent heat from a principle, which is allowed to exist, we ought to prefer this method of solution to that, which invents a new hypothesis."
>
> "By conducting experiments . . . it may be collected that water has a much greater capacity for heat than Ice has—And thus all the Phaenomena, which we usually ascribe to the conversion of sensible heat into latent heat admit of a very easy solution without supposing any portion of heat to become imperceptible.—For example, as a particle of Ice is heated to the degree of 32° by much less heat than what can produce the same sensible temperature in a particle of Water, it is impossible that the particle of Ice should ever change into a particle of water without a great apparent loss or diminution of heat, and yet no particle of heat can properly be said to be latent in the drop of water. The water indeed requires more of the cause of heat to heat it to the same temperature, but the heat which it contains is not to be distinguished into two parts, one sensible and the other latent ; Every part of its heat is producing a proportional part of the sensible heat which it possesses . . . " (*ff.* 10–14).

Milner then examines how latent heat could be explained in terms of motion :

> "As very little is known, on *any* hypothesis, concerning the *manner*, in which heat produces its Effects, we may always admit, that what we advance on this head, is little more than Conjecture. . . ."
>
> "In our present knowledge or rather ignorance of the constitution of nature, we are not to pretend to enumerate all the causes, which can produce heating motions, much less to explain the manner in which any one of them acts—But experience supports us in supposing Motions of various kinds very often to be productive of heat—Great and sensible Motions of bodies are often converted into lesser and imperceptible Motions . . . and therefore are probably often converted into those minute oscillatory motions in which heat consists, and when a body has its parts once put into vibration, that is, when it is once heated, it is observed to communicate heat to surrounding bodies. . . ."
>
> ". . . when a body is exposed to be melted, whatever be the cause of this internal vibration, it continually increases until the body arrives at the melting point, and when this happens, the sensible heat can increase no farther ; because the heating cause is now employed in separating the minute parts of the body and producing Fluidity. The heating cause continues to act, but its action produces a different

Effect, because circumstances are altered:—As we supposed it to produce Motion before, so it seems to produce Motion still; for fluidity cannot be conceived to take place without considerable Motion:—but the Motion is of a different kind and produces no sensible heat:—We can give no reason why the body should begin to melt at such a particular degree of sensible Heat—It is the Constitution of the body—and no other hypothesis affords a more instructive answer.—But it seems not unnatural to expect a melting body to grow no hotter, during the fusion of its different parts; because the fusion itself is an Effect, and an Effect of the same kind with the heating Vibrations of the body; at least the two Effects agree in this, that they both consist in the motion of the parts of the body, and may therefore easily arise from the action of the same cause, which in some circumstances produces rapid oscillations, performed in extremely short spaces, or in other words, heating Motions, may in other circumstances produce Motions of a different nature, performed in greater Spaces, the consequence of which may be Fluidity—According to the hypothesis of a *matter* of Heat, The Fire enters the body and produces sensible heat, for a certain time, till the body begins to melt, and then the fire is absorbed, becoming latent and produces Fluidity. Does such a supposition appear less arbitrary and more agreeable to analogy? In the hypothesis of Motion, there seems to be *some relation* between sensible heat and Fluidity, considered as Effects, or different sorts of Motion, but nobody has pretended to show, How a matter of Fire may sometimes produce sensible Heat and sometimes Fluidity. Or if anyone prefer Irvine's explanation of these facts, he is still in similar difficulty and obscurity —He can give no answer to either of these questions—1st why does the body begin to melt at such a particular degree of sensible heat and 2nd Why should a melted body require more heat to heat it than the same body in a state of congelation.—Now according to the hypothesis I am defending, many persons think they can perceive a glimpse of Reason why a melted body should be *less* capable of receiving igneous vibration. It may require a stronger action of the heating cause to produce equal vibrations in a melted body, because its particles can move so easily among one another, and therefore can admit the production of motions of a different nature; whereas the parts of a Solid cohere with greater firmness, naturally resist the impulse of external matter, and by this resistance may produce tremulous Motions among themselves . . ." (*ff.* 17–25).

During the process of solidification of a liquid, a quantity of sensible heat was certainly liberated, and supporters of material theories of heat claimed that this could not be accounted for in terms of a kinetic theory. Milner writes:

"Never since I was acquainted with this valuable fact, did I perceive any particular difficulty in accounting for it upon the hypothesis that heat consists in motion; but I have often wondered what could be the reason our adversaries laid so great Stress upon this Phaenomenon. . . . Now it appears to me that the argument, we are considering, originates in an obscure belief that the same quantity of Motion always exists in the world, and therefore that Motion never begins *de novo*. . . . But this reasoning will have little force with those, who know that the quantity of absolute Motion in the world is continually varying, and that natural bodies are endowed with numerous attractive and repulsive forces.—Perhaps these forces may generate no Motion *estimated in a given direction*, on account of the Equality of action and reaction; but can it be doubted that they generate a great deal of *absolute* Motion? Or that the Sum of the *absolute* Motions of a system of bodies is often increased by their mutual actions upon each other?"

"Those who consider these things then, will not be surprised, that when a body fixes, a quantity of heat should suddenly appear; for in the change of a body from a fluid to a solid state, there is a new arrangement of parts; This new arrangement cannot happen without Motion of the parts, nor does it seem unnatural to suppose, that when they have attained their proper situation, and cease to move easily among one another, there should remain for a time considerable quantity of tremulous or vibratory Motion."

Milner then considered the latent heat of vaporization and, while discussing this, pointed out that, although the fact was not always explicitly stated, those who opposed the idea that heat was a form of motion held that it was a material substance, and Milner found it difficult to conceive of *matter* becoming latent (*ff.* 31–2).

At the boiling point, Milner held, the particles of water, from some unknown property in their constitution, were disposed to recede from each other, and this separation of parts was produced by the same causes which in other circumstances would produce heat. During the condensation of steam, he said:

> ". . . the particles of steam must approach one another with rapid Motions . . . there is a certain limit to their approach; for they never make a body denser or specifically heavier than water."
>
> "Then does it seem unnatural to suppose vibratory and intestine motions to be generated as soon as the approach of the particles is stopped by the action of repulsive forces?" (*ff.* 35–6).

Milner's "Essay on Heat" ends with some miscellaneous observations, and also a reference to Scheele's views on the nature of heat:

> "Some years ago, Mr Scheele proposed an ingenious new opinion concerning the nature of fire—according to him Fire is a compound of pure air and Phlogiston."
>
> "In the Essay I have not considered the particular reasons by which this Philosopher supports his opinion, because I am not aware that they tend in any degree to prove that Fire is matter."
>
> "I have explained at large two distinct opinions, in which Fire is considered as matter, and from these many others may probably be derived and supported with plausible arguments, if we allow ourselves to consider this *matter* a Compound" (*ff.* 40–2).

Scheele's experiments on the reflexion and refraction of heat are mentioned as being of great interest "if confirmed".

The last of Milner's Jacksonian Lectures, preserved in the Library of Trinity College, is entitled " Part the first of an Essay on Phlogiston " and was received by the Vice-Chancellor on 25 October 1788. Milner begins by pointing out the advantages of a study of heat before considering the phlogiston theory. He goes on :

> " The Science of Chemistry consists only of a multitude of detached facts, unless these be well arranged according to some Theory. Stahl's theory of Phlogiston has long been thought sufficiently established ; and chemical authors in their explanation of facts constantly refer to it : whether we are convinced of the truth of the theory or not is perhaps of little consequence ; but unless we *understand* it, we shall be in a condition to receive small instruction from the most celebrated writers on Chemistry."
>
> " Philosophers, who reject the doctrine of phlogiston, are many and the number of them seems to increase daily : the controversy has on both sides been ably conducted ; and given rise to the finest modern discoveries. Yet even the writings of the Anti-phlogistians cannot be understood, unless we first inform ourselves what they mean to oppose, and with what view they produce their experiments " (*ff*. 4–5).

Defining phlogiston as " the principle of Inflammability, in its pure, simple and uncombined state ", Milner proceeds to give a general account of the theory, and then considers the objection that no one has ever been able to exhibit phlogiston in its pure state :

> " To this Objection however the Chemists have been prepared with plausible answers—Some of them have maintained that the Phlogiston is the pure matter of Fire ; therefore in its nature, when disengaged from chemical combination, *incapable* of restraint or confinement : that it is let loose during the Inflammation of bodies ; but cannot be separated or confined in any Vessels whatever : That it enters into fresh Combinations and gives new properties to the bodies, with which it happens to combine ; and by so doing prevents any increase of sensible heat or redundency of disengaged fire, in the Universe."
>
> " Others, and perhaps the greater number have supposed the Phlogiston to be a very subtle matter *sui generis* : according to them, this matter abounds in inflammable bodies, and is let loose during their Inflammation, it combines again with other bodies, which happen to be near it, and for which it has an attraction ; but they do not conceive Phlogiston to be the matter of Fire in any state ; and in fact, several seem to have believed the Existence of Phlogiston according to this last Explanation, who have rejected the doctrine of igneous *Matter*."

Milner then describes Stahl's preparation of sulphur from sulphuric acid by reducing potassium sulphate with carbon [28].

[28] Stahl gives an account of this experiment in several of his works. See " Experimentum Novum Sulphur Arte Producendi " in the first of his series of monthly *Observationes Chymico-Physico-Medicae Curiosae*, Francofurti & Lipsiae, July, 1697, reprinted in the collected work, *Opusculum Chymico-Physico-Medicum*, Halae, 1715.

"What particularly deserves attention here is, that Charcoal is by no means essential to the success of the Experiment. The same thing happens with fats, oils, resins, or any other inflammable body. . . . Nor is there the least difference in the nature of the Sulphurs thus formed by the help of different bodies: a remarkable circumstance! and one which first induced Stahl to conclude that the pure principle of Inflammability was the same thing in different Inflammables" (*ff.* 18–19).

That in this experiment phlogiston passed from the charcoal to the sulphate could be argued from similar experiments on the calces of certain metals:

"Two metallic substances Iron and zinc are inflammable at a sufficient heat. These burn away like other inflammable bodies and leave an incombustible residue called a calx, which can recover its metallic properties by mixing with charcoal dust and heating sufficiently."

"It is to be observed that in these cases of revivication of metals, as well as in the former one of the composition of Sulphur, no inflammation of the inflammable body made use of in the Experiments takes place. The Experiments succeed in close Vessels, and bodies cannot burn without a supply of fresh Air:—Therefore the Phlogiston seems to pass from the inflammable body . . ." (*ff.* 28–9).

Milner next considers the view that the matter of light was phlogiston. That the matter of light actually combines with bodies.

". . . seems very probable from the Observations of S^r I. Newton; and there are several reasons to believe that it communicates, in some cases, the properties which are usually ascribed to the presence of Phlogiston—1^st Vegetables contain much Phlogiston; and vegetables will not thrive without light; Heat is not sufficient for this purpose. . . . Lastly the Calces of the noble metals are reduced to their metallic state without the addition of any phlogistic Substance, except light. This reduction is effected by a strong heat, and in vessels which are pervious to no known substance except light" (*ff.* 30–32).

According to Milner, nothing had tended to establish the phlogiston theory so much as the experiments recently made on inflammable air (hydrogen). He describes Priestley's reduction of red lead to the metal with inflammable air, using a burning-glass, and his own variation of the same experiment, using a charcoal furnace as the source of heat (see p. 237):

"These Experiments proved most acceptable to the advocates of Phlogiston, who now began to defend their doctrine by arguments that seemed direct and irresistable. They had been repeatedly ridiculed for believing the existence of a substance, which they could neither exhibit to the senses, nor demonstrate to be possessed of any one known property of Matter. But this reproach seemed now entirely done away. The inflammable principle was capable of being weighed and measured like other bodies; it was confined in bladders and other vessels; and though it could not be represented as an object of sight, its Existence seemed as certain as that of the Atmosphere we breath. However some new and unexpected discoveries concerning the nature of inflammable air enabled the Anti-phlogistions to contest this important conclusion with great plausibility and consistent argument " (*ff.* 35–6).

At this stage Milner proceeds to give a general account of the way in which the anti-phlogistonists explained the facts adduced in favour of phlogiston, and the main arguments on which their own views were founded :

> " The notion of a substance, which could not be obtained pure and uncombined, the existence of which was *inferred* only from the Effects, which it was supposed to produce and which in its passage from one body to another could never be rendered an object of sight, always appeared obscure and exceptionable ; and to many philosophers a proper subject of ridicule. The Phlogiston, it was said, was always at hand, a sure resource in any difficulty ! it was present or absent just as suited best the purposes of the Chemists, and it was in vain to attempt a confutation of doctrines supported by such a creature of the Imagination."
>
> " It must be owned however that these objections came principally from men of Speculation and retirement ; and who for that reason could not be supposed well acquainted with the leading facts, upon which the chemists had grounded their opinions. It is true they had heard of the formation of vitriolic acid and the revivication of metals ; but the bare *recital* of Experiments usually makes very slight impression on the understanding of persons not accustomed to operate themselves— It was impossible therefore for the Chemists to communicate what they *felt*. They daily made Experiments in the Laboratories, and saw the change produced in metals and their calces, and though they could not accurately perceive the *manner* in which charcoal and other phlogistic substances produce their Effects ; yet the conviction of their important use in Operations seemed just and reasonable ; and the mind of the practical chemist was by a repetition of experiments almost brought to the same degree of acquiescence and belief, as if he had seen and felt the very substance upon which his theory was founded ; and to the influence of which he attributed the most curious and extraordinary transmutations."
>
> " The clearest Expressions and best arrangement of the Subject could not affect the understandings of speculative Philosophers in the same manner."
>
> " In this state of the Controversy while the Existence of Phlogiston was attacked only by strokes of humour and ridicule ; the Chemists easily proved an overmatch for their adversaries ; they frequently convicted them of ignorance or misrepresentation, they exhorted them to labour and make trials themselves at the furnace ; and with an air of triumph referred them to the actual examination of operations and contrivances, which, they said, must for ever appear unintelligible to indolent and captious objectors."
>
> " There was however one well known fact, which to reasonable men always appeared to militate against the doctrine of Phlogiston. I mean the increased weight of Calces of metals.—This extraordinary fact seems to have been ascertained even by the old chemists, and to have afforded

matter of objection against the hypothesis of the absence of some real substance. When the imperfect metals were burned in the fire to fine powder; these calces were found heavier than the original metals; and yet the Phlogiston of the metal was supposed to be separated by this process—What answer could be given to so formidable an objection? Is the Phlogiston real matter? Is this material endowed with a principle of Levity? In fact some celebrated chemists have actually gone so far as to assert the probability of this, rather than give up their favourite hypothesis; whilst others conceiving the Idea of a tendency in a body to ascend upwards, too bold and unwarranted, ascribed the increase in weight of calces to the accession of igneous particles. An hypothesis almost as unphilosophical and unsupported: a matter of fire has never been proved, nor has the presence of fire ever been shewn to increase the weight of a body."

"The last mentioned argument, though obviously powerful, does not seem to have much effect in shaking the credit of the Phlogistic hypothesis, till lately, when Philosophers begun to make their experiments with more accuracy; and in particular the Chemists begun to attend to the production and absorption of aerial fluids; and the changes they underwent. M. Lavoisier seems to be the first, who carefully examined the circumstances, which accompany the calcination of metals, and also the formation of acids and as the experiments of this philosopher have been thought so complete and satisfactory as to decide in a scientific [manner] the controversy concerning the existence of Phlogiston it will be proper in this place to give a particular detail of some of them" (*ff*. 37–41).

Milner then explains carefully, emphasizing the quantitative aspect, how Lavoisier calcined mercury in a closed vessel containing air, with the result that the air diminished in volume by one-sixth and the residual air had the properties of 'phlogisticated air'. The calx of mercury obtained in this experiment having been skimmed off and distilled without addition, metallic mercury was obtained and about the same quantity of air was liberated as had been absorbed in the first part of the experiment. The air liberated had the properties of 'dephlogisticated air'. From these experiments Lavoisier deduced:

"1st That Atmospherical Air consists of two distinct fluids; one of which is the respirable part; and is also capable of supporting the combustion of bodies; The other is totally unfit for these purposes; and that these very different species of Air exist in the Atmosphere nearly in the proportion of 1 : 5 or 1 : 4 as he afterwards more accurately determines. 2d That in the Calcination of Metals the pure respirable part of the Air and this only, is absorbed by the metal; and that the union of this Air with the metal forms what is commonly called the calx of the metal."

"Thus the antient hypothesis of Phlogiston seems overturned at one Stroke, and a new and simple theory substituted in its place—a Theory founded on direct and satisfactory Experiments, and which at the same time gives a clear account of the increase in weight of the calces of metals . . ." (*ff*. 42–3).

As further evidence in support of Lavoisier's theories Milner describes Lavoisier's experiments on the combustion of phosphorus and of sulphur. When the former (according to the phlogistonists a compound of a particular acid with phlogiston) was ignited in air in a closed vessel, by means of a burning-glass, it yielded a white powder, weighing more than twice the weight of the phosphorus burned. The increase in weight answered 'pretty accurately' to the weight of air absorbed as measured by the decrease in volume. Similar results had been obtained in the case of sulphur. Milner comments :

> "The above experiments seem to have been well conducted, and have induced M: L^r. and his followers to believe that phosphorus and sulphur are the simple principles ; and that the respective acids are the Compound ones—Instead of affirming Sulphur and Phosphorus to consist of peculiar acids united with Phlogiston ; we are taught to reject this last Substance, as an imaginary being and to suppose that the Phosphoric and Sulphureous acids to be compositions of Phosphorus and Sulphur united respectively with the pure part of the Atmosphere."
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> "In this explanation therefore, the antient doctrine of Stahl is entirely set aside, and his celebrated Experiments concerning the formation of sulphur reckoned inconclusive" (*ff.* 45–6).

In his will, Jackson directed that his Professor should deposit each year the manuscript of one lecture, and that the lectures so deposited should, if possible, form a connected series. It seems clear, therefore, that in each of his annual courses Milner devoted four lectures to the theory of heat, and presumably at least two to phlogiston. "Part the first of an Essay in Phlogiston" must have been delivered early in 1788, and in view of the revolution in chemical thought occurring about this time it is unfortunate that we have no record of any second or subsequent Part of this Essay.

Milner was excused from lecturing in 1790 on account of ill-health, and was given leave to find a deputy in 1791. In 1792 he resigned the Chair of Natural Philosophy on his appointment as Dean of Carlisle, although he continued to reside in the University. He maintained an interest in science throughout his life, and kept up a correspondence with several scientific workers, and in particular with Professor Leslie of Edinburgh. In the Michaelmas Term of 1813, at the age of sixty-three, he attended the first and only course of professorial lectures on chemistry given by the ill-fated Smithson Tennant. It is unfortunate that Milner's many college, university, and ecclesiastical responsibilities prevented him from undertaking any serious researches of his own, for he was undoubtedly one of the most able minds the University ever produced.

In conclusion, I should like to express my thanks to the Librarian of Trinity College, Cambridge, for permission to study the manuscripts of Milner's lectures, and other Jacksonian Papers.